This was, leaving out of account the Sudan in the south and the wild Libyan tribes to the west, the world as known to the Egyptians. Of Italy, they probably had as yet no knowledge.

Towards the end of the reign of Amenhotep III. a revolution broke out in Syria and Palestine. Shubbiluliuma or Suppilulius, king of the Hittites, a monarch full of guile, aspired to oust Egypt from the control of Syria and to destroy Mitanni. He found tools ready to his hand in certain discontented and rebellious Amorite princes of the Lebanon and in the Phœnicians of Arvad, and stirred up strife. Amenhotep quelled the revolt for a time, but it broke out again, and when his extraordinary son, Akhenaten, ascended the throne, the whole country seethed with turmoil.

The new king was interested only in his project of reforming the Egyptian religion; he was a man of art and of peace, and for the first time in history, perhaps, a great king refused to go forth to war, and allowed his dominions to fall away from him.

Palestine and Syria were in chaos. Wandering tribes, among them those Khabiri who have been credibly identified with the Hebrews, overran the land, the Hittite princes of the south revolted, and with them certain chiefs of Aryan (? Mitannian) origin who also had settled there under the Egyptian dominion. The Canaanite chiefs and Phœnician princes who remained faithful were gradually borne down in the absence of help from Egypt, and at the end of Akhenaten's reign the whole country had fallen away from the king.

In Tutankhamen's day the great prince Huy may represent himself on the walls of his tomb, as he does, bringing Semitic chiefs to offer tribute to his majesty, but we see that this can have been but a farce: the king's writ ran no farther than the coast of the Shephelah, probably. In the north the Amorites had but exchanged one master for another, for they now became the vassals of the Hittites, albeit under a looser control than that of Egypt. The Hittite control of Syria continued unchallenged till the days of Seti I. and Rameses II., fifty years later, when Egypt essayed to reimpose her yoke on the Semites. Long wars ensued, waged directly by Egypt against the Hittites, until about 1279 B.C. a peace of exhaustion was concluded between the protagonists, a peace of which we have the full protocol, signed and sealed by the Great King of Egypt and the Great King of Khatti, couched in diplomatic and legal phraseology that might have issued from a modern chancellery. It was a com-

promise: of her old Asiatic dominion Egypt retained only Palestine; Syria fell to the Hittites and remained theirs till, eighty years later, the invasion of the Philistines and their seafaring allies from the North overthrew the Hittite kingdom and tore Palestine itself from Egypt.

Tutankhamen, then, was confronted across his attenuated frontier by a far more formidable foe than the Babylonian could ever be. Mitanni was gone—destroyed by Shubbiluliuma after all help from Egypt had proved vain. Assyria, trusting in the prowess of her soldiers, kept her independence of both Babylon and Khatti; Shubbiluliuma seems prudently to have let her be. Her king, Ashur-uballit, was a long-lived and probably a politic as well as a doughty ruler. Somewhat later, in the time of Rameses II., Shalmaneser, king of Assyria, was a much more powerful monarch than the Babylonian Kadashman-turgu, and it was partly in apprehension of his power, probably, that Rameses and Khattusilis, the Hittite ruler, finally compromised their differences.

The collision of different national civilisations at this time produced none of the mutual approximations that might have been expected. Only Egypt began to show signs, more accentuated later, of Semitic influence in her culture. Babylon, however, shows no signs of Egyptian influence, the Hittites perhaps a little, the Mycenæans more. But there is no landslide in any direction anywhere. Each people remained faithful to its traditions. There were colonies of Mycenæan artists, as of Semitic and even Hittite craftsmen in Egypt. But though the Egyptians prized and used Greek products, we find no direct imitation of Minoan art even in the free and untrammelled Egyptian art of Akhenaten's time, though the works of the Minoan artists must have appealed to the realistic and truth-loving king. There is no trace of Minoan or of Mesopotamian influence yet in any of the objects of Egyptian art discovered in Tutankhamen's tomb of which photographs have been published: the weird heads, for example, of one of the gilded couches that have been thought to be Mesopotamian in aspect are merely heads of the Egyptian goddess Thoueris in her fierce and typhonic character. We should, in fact, expect Mesopotamian influence less than Minoan or even Hittite. The Thoueris-head was adapted by the Minoans for the heads of their water-demons.

Such, in brief survey, are the main characteristics of the outer world known to Tutankhamen and his people, and of Egypt's relations with it.

Recent Advances in Photographic Theory.¹

By Dr. C. E. K. MEES.

THE study of the physico-chemical relations on which depend the form in which a precipitate is produced has been developed by a number of workers in recent years, and its application to the precipitation of silver halide has been studied by Sheppard and Trivelli. In his earlier work Trivelli made a large number of photomicrographs of emulsions taken from standard photographic plates and films,

one of which is reproduced in Fig. 1. It will be seen that the silver bromide grains, of which the emulsion is composed, are of very varied sizes, there being present a large number of small grains, down to the limit of those visible with a microscope, and a smaller number of large grains, including some of very much greater area than the smallest grains present. The largest grains are all polygons, with angles of 60° and 120°. There is a tendency to round off the corners and edges of the small grains, so that the smallest grains appear to be more or less spherical.

¹ Communication No. 165 from the Research Laboratory of the Eastman Kodak Company. From a lecture delivered before the Franklin Institute of Philadelphia on December 7, 1922.

A study of these small spherical grains by R. B. Wilsey, however, using the methods of X-ray crystal analysis, shows that even the smallest grains are still definitely crystalline and have the same structure as the large grains, the crystalline form being a cubic lattice.

So long ago as 1915 it was realised that the distribution of the different sizes of grains in an emulsion might play a very important part in determining the characteristics of that emulsion. The problem was to measure the distribution of the grains; that is, the number of grains of a given size which occurred in an emulsion and the variation of the number with the size of the grain. This problem has often arisen in scientific work. It has been studied in connexion with suspensions of all kinds. Various indirect methods of attacking the problem have been suggested. It is possible to get determinations by settling the emulsion, taking advantage of the fact that the larger particles will settle most rapidly according to Stokes's law, but the direct method is, clearly, to spread out

FIG. 1.

a thin layer of the emulsion and to count the different sizes of grains occurring in it. Trivelli photomicrographed a thinly coated emulsion at an enlargement of 2500 diameters, enlarged the negatives to 10,000 diameters, outlined all the grains of these enlargements, and then planimetered the grains and obtained tables showing the areas of the different grains present, at least a thousand grains being counted for each emulsion. Sheppard and Wightman obtained the same results by the use of the camera lucida instead of photomicrography. From these tables curves were obtained showing the relation between the size of grains and the number present for several standard emulsions. Fig. 2 shows the results for the portrait film and slow-lantern emulsions. It will be seen that the curve shows a distribution of sizes of grain which corresponds approximately to a probability curve, the maximum number of particles being of a diameter of approximately 0.5μ , the particles both smaller and larger than this being fewer, until we have very few particles indeed of larger size than 2.7μ and also few of smaller size than 0.2μ . On this small side no particles can be measured less than 0.2μ , because this is the limit of the resolving power of the microscope.

It is probable, however, that curves showing diameters will not be of real value, because the con-

trolling factor will not be the diameter of the particles, but their projective area, as shown in Fig. 3.

Svedberg investigated systematically the relation between the size and sensitiveness of grains in photographic emulsions. He prepared emulsions so thinly

FIG. 2.

coated that the grains were all in single layers, and counted the grains of different sizes by classifying them into four classes. The emulsions were then exposed and developed, and the developed silver removed, the remaining grains, representing those which had not been made developable by the action of light, being counted. In this way curves could be obtained showing the sensitiveness of the grains of each class, and it was found, as might be expected, that the larger grains were much more sensitive than the smaller grains.

Svedberg next assumed that the product of the light action in the halide grain—that is, the substance of the latent image—consists of small centres distributed

FIG. 3.

through the grain or through the light-affected part of the grain, and that these centres are distributed according to the laws of chance. If a plate be developed for a very short time the grains show these centres as small black spots upon them. This was shown by Hodgson as early as 1917 (Fig. 4).

Not only did Svedberg demonstrate the existence of these centres, but he also made plain their relation to the silver bromide grains by photographing the grains

before development by deep red light, then developing for a short time and removing the undeveloped halide. On the plate there are then left small dots, and comparisons with the first plate showed these to correspond with the silver halide grains originally present.

Svedberg has shown that the number of centres produced in this way by initial development increases with the exposure in accordance with the usual photographic laws, and it might be assumed that the discovery of these centres produced during development is a proof of discreteness in the action of light upon the grain, and that they must result from a structure in the silver bromide grains existing either before exposure or produced during exposure, and corresponding, for example, to spots of sensitiveness. While the evidence for this seems very great, it must be remembered that we know nothing about these centres until development takes place, and that even if the whole grain were equally affected by the action of light and changed to the same extent, we should still expect development to take place first at some local

FIG. 4.

spot corresponding to slight surface differences in the grain. A sheet of metal immersed in acid, for example, will not be attacked uniformly all over the surface. Because of the impurities, action will start at individual points.

In a very important paper, Toy has given measurements showing that the number of nuclei produced on initial development are proportional to the number of grains which become developable on complete development, and that the larger grains not only have more nuclei on account of their size, but that these nuclei are also more sensitive to light than those in the smaller grains, the sensitivity of a grain being the sensitivity of its most sensitive nucleus. Svedberg considers that the number of developable centres per unit area of grain surface is a measure of the light sensitivity of the silver halide of the emulsion. From Toy's work it would seem to be doubtful whether we can speak of the light sensitivity of the halide itself in terms of the nucleus theory, since this will vary with the size of the grain.

Recently, a number of phenomena have been observed which are very difficult to explain by the use of the classical wave theory of light, and it seems not unlikely that it may be necessary to turn to a theory having some analogy to the corpuscular theory. As a first

step towards this, Max Planck suggested his now well-known quantum hypothesis, according to which an atom radiating energy liberates it in discrete quanta, the amount of energy corresponding to each quantum being a constant multiplied by the frequency of the light. Bohr adopted Rutherford's theory of the structure of the atom, considering the atom to consist of a nucleus containing an electron carrying a positive charge of electricity, and to be surrounded by one or more electrons carrying a negative charge, the electrons revolving about the positive nucleus itself. He imagined that the electrons revolve without radiating, but that when an electron suffers some violent shock it gives up energy, and this energy is radiated and has the value of Planck's quantum. Thus, if an electron, by the sudden impact of another electron, for example, is thrown out of an atom and is attracted back to its place by the nucleus, then, as it falls back, it will send out a pulse of energy, and it will be seen at once that, if light is produced by such a behaviour of electrons, it is inherently probable that it will be radiated in pulses rather than continuously. Since, according to Bohr, the frequency of the vibration emitted is exactly proportional to the energy which the electron releases, Planck's quantum condition is fulfilled, and we have the famous equation,

$$Ve = h\nu,$$

where V is the voltage acting on the electron charge e , ν is the frequency, and h is Planck's constant.

In an X-ray tube the discharge of electricity is in the form of a stream of corpuscles travelling with a very high velocity, which depends upon the voltage of the electric current applied to the tube. When these corpuscles strike the target their energy is radiated in the form of X-rays, and we know that these X-rays partake very closely of the nature of light, except that the length of the waves is about one-thousandth of those of light, or, what is the same thing, their frequency is a thousand times as great. It is to this that they owe their great penetrating power.

On the classical wave theory of light, then, we should imagine that an X-ray tube having its target bombarded by the stream of corpuscles produced by the current would emit waves of X-rays spreading into space, just as waves of light are imagined to spread from a source; but now comes a great difficulty. When these X-ray waves travelling out pass through a gas and are absorbed, they cause the molecules of the gas to emit electrons, and these electrons are emitted with almost exactly the same velocity as the electrons in the tube which produced the X-rays themselves. The extraordinary nature of this phenomenon is well illustrated by Sir William Bragg in a recent article. He takes as an analogy the dropping of a log of wood into the sea from a height of one hundred feet. A wave radiates away from where it falls; the wave spreads; its energy is more and more widely distributed; the ripples die away; at a short distance, a few hundred yards, perhaps, the effect will apparently disappear. If the water were perfectly free from viscosity, and there were no other causes to fritter away the energy of the waves, they would travel indefinitely, always diminishing in their height. Now, at some point, say a thousand miles away, these now

microscopic ripples encounter a wooden ship. We should expect that they would produce no effect, especially as they may have passed many other ships without having affected them, but, for some reason, as these tiny ripples reach the ship, a plank of the same weight as the log is hurled out of the ship to a height of exactly one hundred feet, and the whole energy which was originally supplied by the log falling into the water is concentrated upon the ejection of the plank. It will be seen at once how inadequate the wave theory is to account for this phenomenon. Similar difficulties occur in connexion with photo-electricity or the liberation of electrons under the influence of light.

The method by which a photographic emulsion adds up light during a long exposure has always been a great problem when it is considered from the point of view of the classical wave theory. If we accept the idea that the grains of silver halide in an emulsion are exposed to a continuous flood of light from a distant star, for example, then each grain must be imagined to be integrating light until it has received enough to make it developable. Since the exposure required in astronomical photography is frequently very long, we must consider that the grains continue to integrate the light for many hours, and it is difficult to imagine any mechanism which would enable them to do this. The difficulty is enhanced by the fact that even a very brief exposure continues to produce an effect after an interruption of a long period, so that if all the grains have been affected by the first exposure, they must be capable of storing energy quite insufficient to make them developable and to hold this energy for a long period, and then resume its accumulation at the level where the interruption occurs. In the same way, when we study the exposure of the individual grains, even if we could imagine some mechanism by which the grains could store up the energy falling upon them until they became developable, we should expect that all the grains of the same size would become developable at the same time, unless, indeed, we assume the process of exposure to be autocatalytic in nature. When grains are examined under the microscope, however, some of them are found to have been affected before others. If we imagine that they all have become exposed to a uniform flood of light, we must consider that these grains differ in sensitiveness among themselves, and that the possibility of change on exposure, so that they become developable, is due to the presence of a sensitiser. This may be either concentrated unequally in the different grains or may form centres of sensitiveness similar to those supposed to exist by Svedberg and other workers in the field, who think that the centres found at the beginning of development are the origin of sensitiveness, and are present from the time of making the emulsion.

If we had no prior knowledge of the wave theory of light, however, it is clear that the simplest explanation of the sensitiveness of different grains would be that, instead of a continuous flow of light in the form of waves on to sensitive films, the light was falling upon it as a rain of projectiles, and that these projectiles made developable any grains that they hit, the grains that were missed not being developable, but being

hit later if they continued to be exposed to the radiation. Naturally, the bigger the grains the more likely are they to be hit, so that a calculation can be made of the relation between the size and the percentage number of grains which will become developable after a given exposure.

Silberstein suggests that the projectiles, rather than being called "corpuscles," which gives the idea that they are round, should be called "light darts," and should be imagined to consist of a long train of waves of very small diameter travelling with the velocity of light.

It is obvious that this theory of light darts would meet the difficulties which are offered by the phenomena of X-rays and photo-electricity to the idea of a continuous wave front, while not excluding the possibility of the formation of interference and diffraction effects. At first sight it would seem to offer a solution of the problem of the integration of exposure by the silver halide grains of the emulsion, since we might assume that, instead of a grain integrating energy falling upon it until it had received enough to make it developable, it was not affected at all until struck by a quantum of light, and then became developable completely. If this was so, however, we should expect that the amount of energy necessary to make a grain developable would be, on the average, one quantum, and at most a few quanta, more than one being necessary because of the chance that a fresh grain would not be struck by every "light dart" falling upon the emulsion, some falling between the grains and others striking grains which were already developable.

In some work which has just been started in our laboratory we are getting results from which I think we may conclude as a preliminary statement that, for high-speed emulsions, several hundred quanta of violet light are necessary per grain in order to make the grain developable. If this is confirmed, the light dart hypothesis would seem to be scarcely sufficient by itself to explain the integration of energy by the emulsion, and we are thrown back on to the idea of differential sensitiveness among the grains, or of spots of limited area on the grains, so that of the hundreds of quanta striking a grain only one may be considered to be operative, the rest falling upon the insensitive portions of the grain. Suppose that the fraction of a grain which is sensitive is ϵ , and this consists of an average of $\bar{K}$ spots of ω area, then

$$\bar{K}\omega = \epsilon a.$$

Now, if a grain has no spots, it will be quite insensitive and will not be developable, no matter how long it is exposed, so that the value of $\bar{K}$ and ω can be determined experimentally by counting the grains left over after a very prolonged exposure.

In any case, a question of great importance in connexion with the latent image is the amount of energy required to make the silver halide developable. If the new determinations show that several hundred quanta of violet light per grain are necessary, then a revision of ideas relating to the latent image itself will follow, as compared with those ideas derived from the belief that the energy available is only one quantum per grain, in which case it is clear that the latent image must depend upon a change occurring in a single atom of silver or

halogen, since the only work we can imagine one quantum capable of doing is to release a single electron from an atom. If several hundred quanta per grain are available, then it is clear that not one atom of silver per grain may be affected, but that several hundred atoms may be changed, and that an appreciable, though very small, amount of chemical decomposition may be effected by the energy available.

More important still, quantitative differences in the amount of latent image present in a grain become possible. If only one quantum per grain is available, a grain is either exposed or not exposed, but if energy corresponding to an amount of several hundred quanta is used, we might imagine that a grain could become partly exposed, so that, for example, it might be developable by a developer of high reduction potential

but not by one of lower potential. Moreover, grains might clearly be of different degrees of sensitiveness—that is, they might require different amounts of energy to make them developable—and the whole idea of quantitative differences in sensitiveness and exposure, which is so difficult if we imagine one quantum of energy per grain to be sufficient to produce a complete change in the grain which will make it developable, becomes perfectly intelligible. On the other hand, the division of the sensitive area into a number of small sensitive spots, which accords with the ideas both of Silberstein and those of other workers such as Svedberg who have located sensitiveness in “centres,” would still enable us to retain the idea that a single quantum of energy is sufficient for exposure if it reaches one sensitive spot.

Obituary.

DR. JAMES GOW.

THE lamented death on February 16 of Dr. James Gow, formerly headmaster of Westminster School, and author of “A Short History of Greek Mathematics,” calls for notice in NATURE. Educated at King’s College School, Gow went to Trinity College, Cambridge, in 1871, and was 3rd Classic and Chancellor’s classical medallist in 1875. He was elected fellow of Trinity in 1876, the year in which it was observed as a curiosity that the four fellows of Trinity then elected all had monosyllabic names and mustered no more than fifteen letters between them: Cox, Hicks, Lord, Gow. Three of them, including Gow, rowed for the historic Second Trinity Boat Club, now extinct.

Gow’s mind was alert, quick, and versatile; he could have succeeded at almost anything he undertook. The son of an artist, he had himself decided talent in the same direction; he was, as an undergraduate, devoted to music. But his main work was in classics, and even there his interests were very varied. His fellowship dissertation was on the origin of grammatical gender; he edited the Odes and Epodes and the Satires of Horace; and he produced one of the most useful books ever written for schoolboys, a “Companion to School Classics”—a pioneer work which gave a lead to more ambitious and bulky handbooks since issued from the University Presses and elsewhere.

The “Short History of Greek Mathematics” is another proof of Gow’s versatility. His original intention was to write a history of the city of Alexandria. He contemplated a chapter in that work which should deal with the mathematical school from Euclid to Diophantus. But this project led him insensibly to more general mathematical topics, such as the development of numeral systems, Egyptian arithmetic, Greek calculation and Greek theory of numbers, with the result that the material accumulated became too extensive for a chapter in a more general history, and he decided to make Greek mathematics the subject of a separate work. Such a book was very much wanted; here, too, he was breaking new ground. There were three recent and important German works by Bretschneider, Hankel, and Moritz Cantor, but no book in English at all comprehensive. The under-

taking was the more arduous in that Gow had made no special study of mathematics since his school-days, and it is no small proficiency in mathematics that is required for the compilation of such a history. The work proved a little uneven owing to the fact that the arithmetical portion was written on a scale too large to allow of the history of geometry being treated with equal fulness if the whole work was to be in a reasonable compass; and Gow realised that with “a history like this . . . the utility will no doubt vary as the brevity” (p. 145).

The best possible test of a book is, perhaps, the impression that it makes upon a reader who takes it up thirty or forty years after its publication. This book stands the test well. It is true that the mass of the material that must be included appeared to Gow at times to be overwhelming; for he speaks in his preface of the labour having often been dreary. But this certainly would not be gathered from the finished work, which is from first to last anything but dreary. Many things in it have necessarily been superseded as the result of subsequent researches, but the book can still be read with the same pleasure as it aroused on its first appearance.

T. L. H.

REV. WILLIAM WILKS.

HORTICULTURE is the poorer by the sudden death on March 2 of the Rev. William Wilks, vicar of Shirley, Croydon, 1879–1912. The son of Dr. G. F. Wilks, born at Ashford, Kent, on October 19, 1843, William Wilks was educated at Clapham and Pembroke College, Cambridge, where he took his degree in 1864. He was intended to follow his father’s profession, but forsaking that course, after studying at Wells, he took orders, and was appointed curate of Croydon in 1866. The rest of his life, except for his annual holiday on the Continent, or latterly in Scotland, was spent in that neighbourhood, and when in 1912 he resigned his vicarage he went to live and garden next door at the “Wilderness” which he had built, and where he died.

There is no need to speak here of Mr. Wilks’s parish work—the concourse of local people at his funeral at Shirley showed how it was appreciated—but rather