nature or colour, every trace of its passage being swept out of form by reason of its swift displacement. The impression therefore remains both uniform and colourless. Things change, however, as soon as we pour in mercury behind the plates, or otherwise provide for a mirror being in contact with it. The presence of the *mirror* changes the propagated waves into *standing waves*. The reflected ray is, namely, thrown back on the incident ray, and interferes with its motion, both rays having equal and opposite velocities of propagation. The result is a set of standing waves—that is, of waves surging up and down each in a fixed plane. Each wave impresses the sensitive film where it stands, thus producing one of these photographic strata above alluded to. The impression is latent, but comes out by photographic development. Of course the distance between two successive strata is the distance between two neighbouring waves; this, theory shows, is exactly half the wave-length of the impressing light. In the case of violet, for instance, the wave-length being $\frac{1}{100000}$ millimetres, half the wave-length in the above quoted distance of $\frac{1}{200000}$ millimetres; this, therefore, is at the same time the interval between two standing waves, in the case of violet light, the interval between two successive photographic strata, and at last it is the interval required to exist, according to Newton's theory, for the said strata reflecting violet rays, and making these alone apparent, when illumined by white light.

The colours reflected by the film have the same nature and origin as those reflected by soap-bubbles or Newton's rings; they owe their intensity to the great number of reflecting strata. Suppose, for instance, the photographic film to have the thickness of a sheet of paper (one-tenth of a millimetre), the fabric built in it by and for a violet ray is five hundred stories high, the total height making up one-tenth of a millimetre. Lord Rayleigh, in 1887, has proved *à priori* that such a system is specially adapted to reflect the corresponding waves of light.

How are we now to prove that the above theory is really applicable to the colour photograph you have seen? How can we demonstrate that those bright colours are due not to pigments, but to the interference, as in the case of soap-bubbles? We have several ways of proving it.

First of all, we are not bound to the use of a peculiar chemical substance, such as Becquerel's subchloride of silver; we obtain colours with a variety of chemicals. We can, for instance, dispense entirely with the use of a silver salt; a film of gelatine or coagulated albumen impregnated with bichromate of potash, then washed with pure water after exposure, gives a very brilliant image of the spectrum.

Secondly, the colours on the plate are visible only in the direction of specular reflection. The position of the source by which we illumine the photograph being given, we have to put the eye in a corresponding position, so as to catch the regularly reflected rays. In every other position we see nothing but a colourless negative. Now, as you are aware, the colours of pigments are seen in any direction. By projecting again a photograph of the spectrum, and turning it to and fro, I can show you that the colours are visible only in one direction.

Thirdly, if we change the incidence of the illuminating rays, that is, if we look at the plate first in a normal direction, then more and more slantingly, we find that the colours change with the incidence exactly as they do in the case of soap-bubbles, or of Newton's rings; they change according to the same law, and for the same reasons. The red end of the spectrum turns successively to orange, yellow, green, blue, and violet. The whole system of colours, the image of the spectrum, is seen to move down into the part impressed by the infra-red. This is what we expect to happen with interference colours, and what again we cannot obtain with pigments.

Fourthly, if while looking at the film normally, we suffer it to absorb moisture—this can be done by breathing repeatedly on its surface—we see that the colours again change, but in an order opposite to that above described. Here the blue end of the spectrum is seen to turn gradually green, yellow, orange, red, and finally infra-red, that is, invisible. The spectrum this time seems to move up into the ultra-violet part of the improved film. By suffering the water to evaporate, the whole image moves back into its proper place; this experiment may be repeated any number of times.

The same phenomenon may be obtained with Newton's apparatus, by slowly lifting the lens out of contact with the plane surface. The explanation is the same in both cases. The gelatine swells up when imbibing moisture. If we consider, for

instance, the v of the spectrum, the small intervals between the strata, corresponding to violet rays, gradually swell up to the values proper for green, and for red, and for infra-red; green, then red, then infra-red, are therefore successively reflected.

We will wet this photograph of the spectrum with water, project it on the screen, and watch the colours coming back in the order prescribed by theory.

It is necessary to use a transparent film, since an opaque one, such as is commonly in use, would hide the mirror from view; the sensitive substance must be grainless, or, at least, the grains must be much finer than the dimensions of the strata they are intended to form, and therefore wholly invisible. The preparation of transparent layers gave me at first much trouble; I despaired for years to find a proper method for making them. The method, however, is simply this: if the sensitive substance (the silver bromide, for instance) be formed in presence of a sufficient quantity of organic matter, such as albumen, gelatine, or collodion, it does not appear as a precipitate; it remains invisible; it *is* formed, but seems to remain dissolved in the organic substratum. If, for instance, we prepare a film of albumeno-iodide in the usual way, only taking care to lessen the proportions of iodide to half per cent. of the albumen, we get a perfectly transparent plate, adapted to colour photography.

We want now to go a step further. It is very well for physicists to be contented with working on the spectrum, since that contains the elements of every compound colour; but we all desire to be able to photograph other objects than the spectrum—common objects with the most compound colours. We have again but to take theory as a guide, and that tells us that the same process is able to give us either simple or compound colours. We have then to take a transparent and correctly isochromatised film, expose it with its mercury backing, then develop and fix it in the usual way; the plate, after drying, gives a correct coloured image of the objects placed before the camera. Only one exposure, only one operation is necessary for getting an image with every colour complete.

A plausible objection was offered at first to the possibility of photographing a mixture of simple colours. The objection was this: a ray of violet gives rise to a set of strata separated by a given interval; red light produces another set of strata with another interval; if both co-exist, the strata formed by the red are sure to block out here and there the intervals left between the strata formed by the violet. Is it not to be feared that one fabric will be blurred out by the other, and the whole effect marred? The confusion would be still worse if we consider the action of white light, which contains an infinity of simple components; every interval here is sure to be blocked up.

Mathematical analysis, however, shows this objection to be unfounded; we have great complexity, but not confusion. Every compound ray, both coloured and white, is faithfully rendered. As an experimental proof of this, we will project on the screen photographs of very different objects, namely, stained-glass windows, landscapes from nature, a portrait made from life, and vases and flowers.

That the colours here observed are due to interference, and not to the presence of pigments, can be shown in the same way as with the spectrum. Here, again, we observe that the colours are visible only in the direction of specular reflection, that they change with the angle of incidence, that they change and disappear by wetting, and reappear by drying. Pigments remain equally visible and unaltered in colour under every incidence. If we attempted to touch up one of our photographs with oil or water-colours, the adulterated place would stand out on a colourless background by merely obscuring by diffused light. It is therefore impossible either to imitate or touch up a colour photograph made by the above described interferential method.

THE INFLUENCE OF ATMOSPHERIC AND OCEANIC CURRENTS UPON TERRESTRIAL LATITUDES.¹

ALTHOUGH the following theorem should be implicitly contained in the formula for the rotation of a spheroid carrying a fluid on its surface, I have nowhere seen it explicitly stated.

Theorem.—Let an unconstrained, rigid sphere, with equal moments of inertia, be in a state of free rotation:

Let this sphere bear on its surface a sheet or sheets of continuous movable matter:

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Let this movable matter be kept in a state of steady motion relative to the sphere, by actions and reactions between it and the sphere, without the action of any external force :

Let P be the pole of the axis of rotation of the sphere, which will also be its momental axis :

Let Q be the momental axis of the total motions of the movable matter relative to the sphere :

Let I be the moment of inertia of the sphere, and M the total moment of momentum of all the movable matter around the axis Q :

Then shall the sphere take such a motion that the pole P , while remaining in a fixed direction in space from the centre of the sphere, shall move steadily relatively to the matter of the sphere around the pole Q , with an angular velocity M/I .

The application of this theorem to the case of the earth surrounded by its oceanic and atmospheric envelopes can now readily be seen. To obtain the value of M , we may roughly estimate the ratio of the moment of inertia of the earth to that of the ocean as 2600, and to that of the atmosphere as 1,000,000.

Observations, as discussed by Chandler, and interpreted by theory, indicate an annual change in the pole of the earth, which would be produced by a primary oscillation back and forth through a length of ten feet, or a revolution in a circle having a diameter of five feet. The former motion would, according to the theorem, be the necessary result of a general motion of the oceans on the two sides of the earth, which, at the point where the motion was a maximum, would be 2600 times as great. Approximately this motion would be represented by a continuous flow of the central parts of the Pacific ocean toward the pole of about 150 feet per day, with a correspondingly larger motion of the Atlantic in the opposite direction ; followed by an opposite oscillation during the other six months. If, as may seem very likely, there cannot be so great a differential flow as this through Behring Strait, and between the American and Asiatic continents, it will be necessary to suppose a more rapid flow elsewhere, or a sufficient vortex in the currents of the Atlantic and Pacific oceans. Whether the currents in these oceans are deep enough to produce the observed effect must be left to hydrographers to decide.

Passing to the atmosphere, the excess of motion through one season over that of the other season, to and from the north direction, amounting to 4000 miles in six months, or say twenty miles per day, would also account for the observed change. In these statements respecting the required motions of the earth and atmosphere, I have presupposed a motion around an invariable momental axis. If the motions are such that their momental axis moves around the earth in the course of a year, the required differential motions between the opposite seasons would be only half as great.

In what precedes I have spoken of the earth as a sphere, and considered only differential motions. The actual earth being a spheroid, the motion of the pole already described would not be continuous. The actual effect of oceanic and atmospheric currents of a permanent character on the terrestrial spheroid would be to displace the mean pole of the earth from its pole of figure to such a point of equilibrium that the motion described in the theorem would be neutralised by an equal and opposite Eulerian motion, due to the ellipticity of the spheroid. The actual effect would be a revolution of the terrestrial pole, according to the known laws of rotation, around the central point of equilibrium thus fixed in 427 days. Just what the displacement is can be only a matter of guess-work ; from the known magnitude of the ocean currents they might produce a displacement ranging from ten to twenty feet.

A brief statement of the character of the theoretical variations of the latitude, due to these causes, may not be inappropriate. Since the directions of the currents of the air and ocean go through an annual period, we should expect a corresponding period in the latitude. Since, however, the amount of the annual change varies irregularly from year to year, though remaining constant in the general mean, the amplitude of the annual term should be subject to small variations from time to time, while preserving its mean value unchanged from age to age.

On the other hand, the amplitude of the Eulerian motion being permanently increased or diminished by every meteorological change, may be expected to vary its amplitude in a slow and irregular manner from decade to decade. The Eulerian motion, having a period of 427 days, ought to be nearly circular,

unless the equatorial moments of inertia of the earth differ much more than we can suppose probable. The annual motion may differ somewhat from a circle, and be somewhat less regular. There can be no strictly periodic changes in the latitude but these two, but it is quite possible that, owing to secular changes, or changes continued through several years, in the currents of the ocean and atmosphere, corresponding changes of irregular long period may be found in the latitude.

It will be seen that these conclusions are accordant with Chandler's results as regards the double period, but do not fully agree with them in other details. SIMON NEWCOMB.

THE PAST, PRESENT, AND FUTURE WATER SUPPLY OF LONDON.¹

IN a discourse to the members of the Royal Institution on the subject of the metropolitan water supply nearly thirty years ago, I stated that out of every thousand people existing upon this planet three lived in London ; and, as the population of London has, in the meantime, doubtless grown at a more rapid rate than that of the rest of the world, it will probably be no exaggeration to say that now, out of every thousand people alive on this earth, four live in London ; and therefore, any matter which immediately concerns the health and comfort of this vast mass of humanity may well merit our most earnest attention. Amongst such matters, that of the supply, in sufficient quantity, of palatable and wholesome water, is certainly not the least in importance.

It is not therefore surprising that this subject has received much attention from several Royal Commissions—notably from the Royal Commission on Water Supply of 1867, presided over by the Duke of Richmond ; the Royal Commission on River Pollution and Domestic Water Supply of Great Britain, presided over by the late Sir William Denison, of which I had the honour to be a member ; and, lastly, that of 1892, of which Lord Balfour of Burleigh was the chairman.

The Royal Institution has, for nearly three-quarters of a century, been prominently connected with the investigation and improvement of the metropolitan water supply ; no less than four of our Professors of Chemistry have been successively engaged in this work, namely Profs. Brand, Odling, Dewar, and myself, whilst three of them have been members of the Royal Commissions just mentioned. I may therefore perhaps be excused for bringing the subject under your notice again for the third time.

On the present occasion, I propose to consider the subject from three points of view, viz. the past, the present, and the future ; and, for reasons which will appear hereafter, I shall divide the past from the present at, or about, the year 1883, and will not go back farther than the year 1828, when Dr. Brand, Professor of Chemistry in the Royal Institution, Mr. Telford, the celebrated engineer, and Dr. Roget, Secretary of the Royal Society, were appointed a Royal Commission to inquire into the quality and salubrity of the water supplied to the metropolis.

The Commissioners made careful examinations and analyses, and reported as follows. "We are of opinion that the present state of the supply of water to the metropolis is susceptible of, and requires, improvement ; that many of the complaints respecting the quality of the water are well founded ; and that it ought to be derived from other sources than those now resorted to, and guarded by such restrictions as shall at all times ensure its cleanliness and purity. (At this time the water was pumped from the Thames between London Bridge and Battersea.) To obtain an effective supply of clear water free from insects and all suspended matter, we have taken into consideration various plans of filtering the river water through beds of sand and other materials, and considering this, on many accounts, as a very important object, we are glad to find that it is perfectly possible to filter the whole supply, and this within such limits in point of expense as that no serious objection can be urged against the plan on that score, and with such rapidity as not to interfere with the regularity of the service."

Before the year 1829, therefore, the river water supplied to London was not filtered at all ; but after the issue of this report the companies set themselves earnestly to work to improve the quality of the water by filtration.

¹ A discourse delivered by Dr. E. Frankland, D.C.L., LL.D., F.R.S., at the Royal Institution on February 21.