the study of ornithology presents many phenomena of far deeper interest than the mere search after the oldest name, resulting, as it does too often, in the unearthing of some utterly unknown title, to the confusion of the student. The same principles of nomenclature which we tried to follow in earlier days are those of the A.O.U. now, which result in *Urinator lumme* as the name of the Red-throated Diver. And it is not as if there will be any finality about this nomenclature, for we have seen the treatment of too many monographs to make us believe this. When an ornithologist takes up a group of birds and monographs it, he spends months or even years of study on this particular group, obtains a grasp of his subject, and does his level best to give finality to his work. Does he succeed? Seldom, if ever. We hold it as an absolute canon that the nomenclature of monographs should be followed, unless a definite reason is given why a name should be altered. But, instead of this being done, we find, over and over again, that the author of a small paper or of a faunal list will, by altering generic names and so re-shuffling the specific names, give a totally different aspect to birds which have only just before been carefully monographed with a hope of finality in their nomenclature. So will it probably be with the A.O.U. "List," when some ornithologist in America will rise up and (as we expect to see before long) declare the trinomial system unworkable or the nomenclature of the "List" too complicated, and will re-shuffle the names, and attain temporary renown.

We think, however, that, now that the two leading Ornithological Societies of England and America have spoken with authority on the subject of the nomenclature of the birds of their respective countries, the British Ornithologists' Union should endeavour, if possible, to confer with the sister Society in America, and see if a common ground of agreement cannot be arrived at. If these two bodies came to a settlement, the whole matter could be laid before an Ornithological Congress, and there would be some hope of unanimity for the future. The points of divergence in practice between English and American ornithologists are less than might be supposed. The two principal ones are the adoption by the A.O.U. of the 10th edition of Linnæus's "Systema Naturæ" instead of the 12th edition, and the employment of trinomial nomenclature. So many English ornithologists are now using the latter mode that there ought to be no difficulty in conceding the latter point if any ornithologist like the method. Formulated as it is in the A.O.U. "Code," there is no difficulty in understanding what is meant by the trinomial titles, and the American Committee have given a clear definition of their object in Canon XI., though the difficulties which have been pointed out on this side of the water are still not disposed of. "In a word, intergradation is the touchstone of trinomialism. It is also obvious that, the larger the series of specimens handled, the more likely is intergradation between forms supposed to be distinct to be established, if it exists." So says the canon above quoted; but, we would ask, if two forms absolutely intergrade, why are they not of the same species? and why will not a binomial title be sufficient? and again, what name is to be given to the specimens collected at the point of contact? Or again, if a larger series of specimens proves that two species do not intergrade, as they were at first supposed to do, then they will each once more bear a separate specific name. Further, are trinomials to be used for insular forms, as is done by Mr. Allen for *Loxigilla noctis sclateri* from Santa Lucia, as there is no chance of intergradation between it and *L. noctis* from the neighbouring islands? Trinomial nomenclature has, however, taken such a place in American ornithology, and is adopted by so many naturalists in the Old World, that the principle must be conceded to all who like to avail themselves of it. The question with regard to the tenth edition of Linnæus's "Systema" might also be got over, but the A.O.U. will have greater difficulty in convincing European naturalists that it is advantageous to the progress of ornithological science to alter established nomenclature by introducing *Chelidon* as the generic name for the Chimney-swallow instead of the feather-legged Martins, which are to be henceforth *Hirundo*. This radical change is to be adopted in homage to Forster's "List of British Birds," a mere list of names without a character for a single genus. Although similar lists have sometimes been accepted for specific names, their recognition in the case of genera is rare, although in many instances long-established usage has rendered some of them familiar.

The few objections which we have made above must not be supposed to lessen our respect for the general tenour of the work now issued by our American *confrères*, whose labours deserve our most careful consideration, while it cannot be doubted that the publication of this "Code and Check-List" will have great influence on the future of zoological nomenclature.

R. BOWDLER SHARPE

PROFESSOR NEWCOMB'S DETERMINATION OF THE VELOCITY OF LIGHT[1]

THE method selected for the important experiments described in the present memoir,[2] is that known as Foucault's. The idea fundamental to it is that of the determination of the interval occupied by light in flashing from a revolving to a fixed mirror and back, by the amount of deviation produced in its return path through the change meantime effected in the position of the revolving mirror. The angle of deviation of the ray is double the angle of displacement of the reflector; to this angle corresponds (since the mirror rotates at a known rate) a definite fraction of a second, which is the time of luminous transmission across twice the measured distance between the mirrors.

But this theoretically simple means of ascertaining the velocity of light is complicated, in practice, with innumerable difficulties. A choice demanding the utmost nicety of judgment must be made between various conflicting conditions; sacrifice in one direction is the price of advantage in another; a balance has to be struck, giving the largest sum-total of facilities, with the fewest and least intractable drawbacks. The plan finally decided upon by Prof. Newcomb was the result of much anxious deliberation: we hope to render it, in its main outlines, intelligible to our readers.

A fundamental condition of the problem is to get an image of the light-source absolutely coincident with the light-source itself, *so long as the movable mirror is at rest*. And this, whatever be the position the mirror is at rest in, provided only that it be such as to permit the rays sent out by it to return, after due triple reflection, to the eye. This requisite is secured by locating the centre of curvature of the distant concave mirror in the axis of the revolving plane one. All rays emitted from this point towards the former will return along the same paths; differences of direction due to differing positions of the movable mirror will be eliminated by the return reflection; and there ensues a "stationary image" of the light-source, occupying, when visible at all, an invariable situation.

So far, all the operators by Foucault's method have been unanimous; but in the placing of the lens indispensable for the management and concentration of the light employed, a material distinction obtained between the

[1] "Measures of the Velocity of Light made under direction of the Secretary of the Navy during the years 1880–82," by Simon Newcomb, Professor, U.S. Navy. Astronomical Papers prepared for the use of the American Ephemeris and Nautical Almanac, vol. ii. parts iii. and iv. (Washington: Bureau of Navigation, 1885.)

[2] For the historical notice serving as an introduction to it, see NATURE, May 13, p. 29.

plan of experiment chosen by Prof. Newcomb, and that pursued by Prof. Michelson in his similar investigation at the Naval Academy in 1879 (see NATURE, vol. xxi. pp. 94, 120). Fig. 1 represents in principle the arrangement adopted by the former, which was also that used by Foucault. In it the lens, L, is placed between the light-source, S, and the revolving mirror, A. Fig. 2 shows the disposition preferred by Michelson, in which the lens is interposed between the revolving and fixed mirrors. In both equally, S and M are, and for the purpose in view necessarily must be, in conjugate foci of the lens.

A disadvantage of the first form is that the measurement of any considerable deviations will be attended by uncertainties caused by the oblique passage through the lens of the return beams. It was, however, obviated in the experiments under consideration, by the use of *two* lenses—one for the outgoing, the other for the incoming rays. The second method (Michelson's) promises increased brilliancy of the image; which may, nevertheless, be regarded as outweighed by atmospheric and other impediments to its distinctness, as well as by the illumination of the field of view produced by the passage through it of some part of the lens with every revolution of the mirror. The method exemplified in Fig. 1 was then chosen by Prof. Newcomb as affording more or less calculable conditions; while No. 2 involved all the uncertainties of definition habitually besetting astronomical observations.

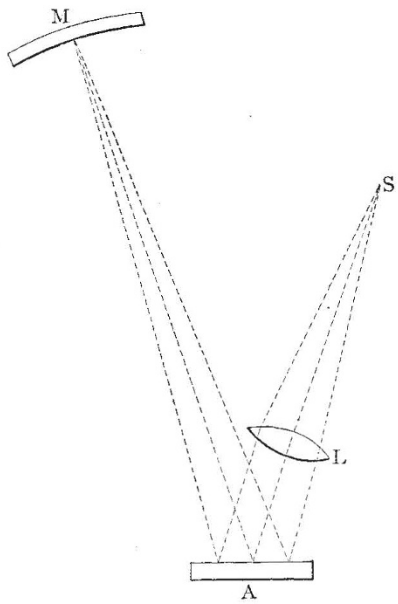

Fig. 1.

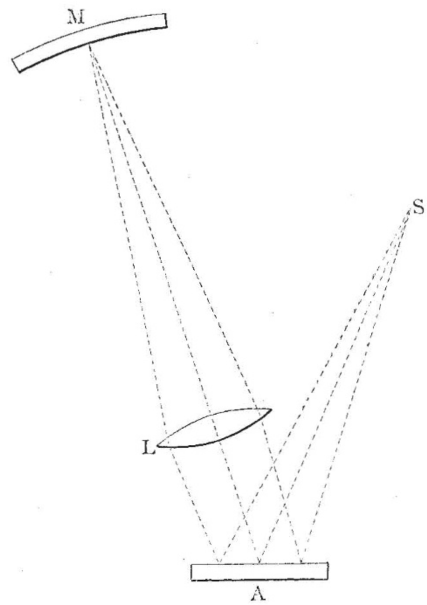

Fig. 2.

Let us now endeavour to realise the nature of the experimenter's immediate task. The precise measurement of an angle actually constitutes it. From the mirror A, so long as it remains at rest, an image is reflected in a certain direction; but no sooner is A set rapidly rotating, than the same image is reflected in a slightly different direction. The amount of this difference—in other words, the angle of deviation—is the object to be ascertained.

Obviously, the first desideratum is to render the inevitable error of measurement comparatively small, by making the quantity to be measured large. Two roads are open towards this end. A high velocity can be given to the mirror A; or a great distance can be interposed between A and M. By the first means, the angle rotated through in a given time will be augmented; by the second, the time available for the displacement of the reflector will be prolonged by the lengthening of the journey imposed upon the rays to be reflected. The difficulties hampering increased speed are purely mechanical, though none the less formidable; those in the way of a lengthened path are optical.

The preservation of light enough to keep the image bright and distinct is of paramount necessity for the avoidance of ruinous uncertainties in its measurement. Now, in Foucault's experiments, the object affording the image was the line of a reticule. It was dark upon a bright ground; a platinum-wire relieved against a sheaf of sunbeams. But no perfectly defined image of such an object could be formed at any considerable distance; and we find, accordingly, that the utmost length by which he ventured to separate his mirrors was twenty metres. His entire apparatus was, in fact, contained in a single room. Hence, notwithstanding a speed given to his mirror of from 600 to 800 revolutions per second, the actual linear deflection of the return ray amounted to no more than seven-tenths of a millimetre. Chiefly by employing as his light-source an illuminated slit, the lucent image of which on a dark ground bore the enormous loss of light ensuing from the transportation of the fixed mirror to a distance of close upon 2000 metres, Michelson was enabled to augment this deflection some two-hundred-fold. The resulting velocity for light of 299,910 kilometres per second was proportionately trustworthy, the error of the angular measurement upon which it immediately depended being estimated to be one hundred times less than in Foucault's determination. Prof. Newcomb's improvements carried him still further towards absolute accuracy.

The details of construction of his "phototachometer" were decided on in the summer of 1879, and the instrument was completed by the Messrs. Clark in May 1880. It consisted essentially of four parts—a sending and a receiving telescope, a revolving and a fixed mirror. Sunlight, thrown from a heliostat through an adjustable vertical slit at the eye-end of the sender, passed down the tube, which was bent at right-angles to get it out of the way of the observing telescope, and after reflection by a plane mirror at the elbow, passed out through the objective towards the revolving mirror. This was formed by a rectangular prism of polished steel, 85 millimetres in height, and with a cross-section of 37·5 square millimetres. The vertical faces constituting the reflecting area were nickel-plated, and proved of a remarkably durable though not very high polish. Motion in opposite directions at will was communicated by two air-turbines, acting one at the top, the other at the bottom of the mirror, and serving, by a simple contrivance, each for the regulation of the contrary velocity imparted by the other. A wheel-work arrangement, by which an electric current was broken once for every twenty-eight revolutions of the mirror, gave the means of obtaining a chronographic record of its rate of going. Two fixed mirrors, mounted side by side on cast-iron stands, were employed to return the light sent to them by the revolving mirror. Each was about 40 centimetres in diameter, and had a radius of curvature of some 3000 metres. The object-glass of the receiving telescope was (in the first instance) placed immediately under that of the sender, the former thus directly facing the lower, the latter the upper section of the movable mirror. The two *tubes*, however, owing to the "broken" form given, as already mentioned, to that of the sender, made with each other an angle of 90°. Horizontal movement round a vertical axis coincident with that of the rotating mirror, was possessed by the observing telescope, to which was attached a pair of microscopes for reading off the divisions on a horizontal divided arc fixed to a stiff frame at its further end. The amount of this horizontal motion of the telescope measured the deviation of the thrice-reflected sunbeam, and, by an immediate deduction, its velocity.

The site chosen for the erection of the apparatus was Fort Myer, on the south side of the Potomac, overlooking the city of Washington. The stationary mirrors, to and from which the carefully guarded rays performed their trips, were placed, to begin with, in the grounds of the Naval Observatory, at a distance of 2551 metres from Fort Myer; but were in 1881 removed to a point at the base of the Washington Monument, at a distance increased to 3721 metres. Some tentative experiments were undertaken on June 22, 1880; after a few days' trial, however, it was found that the wheel-work for counting the revolutions of the mirror was destroyed by the rapidity of the impressed movements. New wheels wore out almost before a set of readings could be obtained with them; until at length the Messrs. Clark, finding that no metal would stand the inflicted wear and tear, substituted *raw hide* as the material for the first wheel, a device which proved wholly successful. With the instrument thus modified work was begun on August 9, and continued without interruption until September 20. The transportation of the fixed mirrors to the Monument station in the spring of 1881 postponed the commencement of operations to August 8; and their effective prosecution was then impeded by the discovery of a source of systematic error in a "torsional vibration" of the rotating mirror. That is to say, the steel prism employed to reflect the light, no longer, when its speed attained a certain point, revolved as an absolutely rigid whole, but *tended towards* the possession of different velocities in its different parts. Hence a slight twisting of its mass producing vibrations round the axis of rotation, the effect of which was visible in the breaking up of the image of the slit into four separate images, one due to each of the faces of the prism. The persistence of this baffling symptom compelled a modification of the instrument, by which the sending and receiving telescopes could be respectively depressed and raised so as to alternate their positions, and the portions of the mirror they were directed towards. The mean of any two complete sets of observations made with the telescopes thus interchanged would be free, as Prof. Newcomb shows, from the effects of any probable form of torsional vibration.

No such effects, however, were apparent in the observations of 1882. This last series extended from July 24 to September 5, and were so nearly free from accidental differences that the probable error of a complete determination was scarcely more, under good conditions, than the ten-thousandth part of the whole. Upon these, accordingly, the chief reliance was placed in the final discussion of results.

The announcement that Messrs. Forbes and Young had detected a difference of 2 per cent. in the rates of transmission of red and blue light prompted, at Fort Myer, a most careful watch for traces of colour in the reflected image of the slit. But although, from a discrepancy of even one-twentieth that amount, a spectrum 15″ in breadth must have ensued, the iridescent edges which would infallibly have betrayed its presence were not seen.

An important novelty in Prof. Newcomb's method was his use of opposite rotations and their accompanying opposite deviations. In his instrument the mirror, as already stated, could be made to revolve at pleasure, either from right to left or from left to right. Instead, then, of measuring, as had always previously been done, the deflection produced in the return ray by the change from rest to an ascertained rate of rotation, the object of his determinations was the total deflection due to extremes

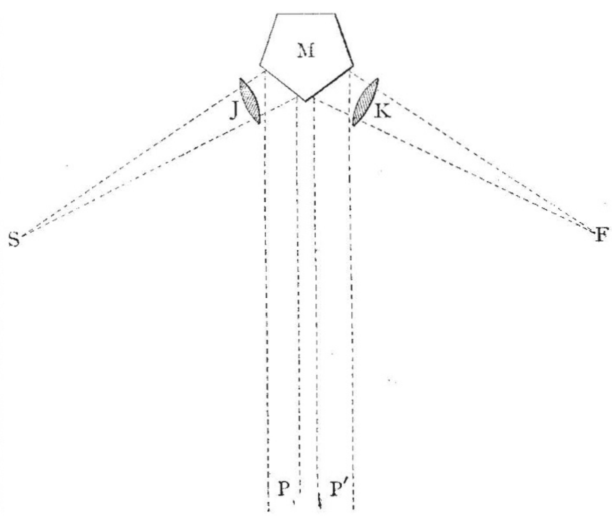

FIG. 3.

of contrary movement. The mode of experimenting was briefly as follows.

First, the valve was opened to the air-blast giving *negative* rotation, the receiving telescope being set upon some division near one extremity of its arc; the image of the slit was then accurately fixed, by the regulating agency upon the velocity of the mirror of the opposing air-current, upon the middle wire of the micrometer; the chronograph made its record of the rate of going, and the microscopes were read. This constituted what was called a "run," and occupied two minutes or upwards. The telescope was next unclamped, and directed near the opposite end of the divided arc. *Positive* motion was given by opening the other valve, and the process of fixing the image and reading off repeated. A comparison of the two sets sufficed to determine the time spent by the light in passing to and from the mirrors on the other shore of the Potomac.

This method of contrary deviations is most strongly recommended by Prof. Newcomb to future investigators. It combines the two advantages of doubling the angle to be measured, and of abolishing possible errors in the determination of the zero-point. In the present series, velocities, alternately in opposite directions, rarely exceeding 230 revolutions per second,[1] gave a total change of direction of nearly 8°. And this largeness of the measured angle materially contributed to enhance the accuracy of the results. Highly effective, also, for the same end were the elaborate precautions for darkening the telescopic field of view, and thus rendering the image of the illuminated slit more distinct. As their upshot, daylight was reduced to about one-thousandth its normal intensity. What was left only just sufficed to show the spider-lines without artificial light. The necessity for such precautions may be estimated from our author's statement that a concave mirror, of which the diameter should be one decimetre for each kilometre of distance, would receive only 1/60,000 part of the light reflected from the revolving mirror; while of that 60,000th part only a small fraction could be practically turned to account, owing to the many sources of loss in reflection and transmission. Since, however, *two* fixed mirrors, each four decimetres across, placed at a distance of less than four kilometres, were employed in the operations at Fort Myer, the proportion of light there returned was rather more than double the above estimate. Prof. Newcomb appears to have been, on the whole, eminently successful in his optical arrangements. The imperfect definition which was the besetting difficulty of Michelson's experiments gave him little trouble.

The recent American determinations of the velocity of light, justly considered as of far superior precision to any others yet executed, give the following results:—

Michelson, at Naval Academy, in 1879	299,910 km.
Michelson, at Cleveland, 1882	299,853 „
Newcomb, at Washington, 1882, using only results supposed to be nearly free from constant errors	299,860 „
Newcomb, including all determinations	299,810 „

To these are added for comparison:—

Foucault, at Paris, in 1862	298,000 „
Cornu, at Paris, in 1874	298,500 „
Cornu, at Paris, in 1878	300,400 „
The same, discussed by Listing	299,990 „
Young and Forbes, 1880-81	301,382 „

Prof. Newcomb's finally-concluded result is that light travels *in vacuo* at the rate of 299,860 ± 30 kilometres per second. And the probable error of thirty kilometres, small as it is, has been liberally estimated. A determination so satisfactory of this important element goes far towards solving the problem of the sun's distance. Combined with Nyrén's constant of aberration, 20″·492, it gives, for the solar parallax, the value of 8″·794. The corresponding distance of 149·61 million kilometres, or 92,965,020 miles, agrees quite closely with Dr. Gill's result from the opposition of Mars in 1877, and exceeds by only 165,020 miles the mean deduced by Mr. D. P. Todd from earlier determinations of light-velocity. No information as to the dimensions of the solar system which we are ever likely to get from a transit of Venus can approach in reliability the present conclusion.

Prof. Newcomb is so far from believing that the *ne plus ultra* of accuracy has been reached in his own remarkable experiments, that he appends to the detailed description of their method some valuable suggestions for its improvement. He had hoped, indeed, he tells us, to reach a concluded value exact to between five and ten kilometres, which, after repeated verification, might be available as a test of the invariability of standards of length. The further prosecution of the inquiry, however, he now leaves to any physicist who may be invited to the task by the promise of his advice and co-operation.

Fundamentally, he holds that the system pursued at Fort Myer in 1880-82 is preferable to any other yet tried. No known expedient for ascertaining the rate of transmission of light is comparable to that of its deflection, after a measured journey, by a moving mirror. The apparatus by which this plan was realised admits, however, in his opinion, of some amelioration in detail. The disadvantageous necessity, for instance, of appropriating a separate section of the reflecting surface to the outward- and homeward-bound rays could be removed by the substitution of a pentagonal for a quadrangular prism, as shown in Fig. 3, where M is a section of the revolving mirror, J the object-glass of the sender, receiving light from the slit S, and throwing it in the direction P towards the distant reflector. On its return along the path P′, the ray is reflected from an adjoining face of the revolving mirror into the receiving telescope, K.

The closing words of the paper under review attest the unappeased aspiration towards accuracy characteristic of the successful investigator.

"A still further perfection of the method," its author writes, "which would lead to a result of which the precision would be limited only by our means of linear measurement is, I conceive, within the power of art. It consists in placing the fixed mirror at so great a distance that the pentagonal revolving mirror would move through an arc of nearly 36° while the ray is going and returning. If a speed of 500 turns per second could be attained, the required distance would be thirty kilometres. Then, in opposite directions of rotation, the return ray would be reflected at phases of the mirror differing by the angle between two consecutive faces. The result would be that the receiving telescope would need to have but a small motion, and all the observer would have to measure would be the small angle by which the difference of positions of the mirror when the flash was received in opposite directions of rotation, differed from 72°. In the Rocky Mountains or the Sierra Nevada no difficulty would be found in finding stations at which a return ray could be received from a distance of thirty, forty, or even fifty kilometres, with little more dispersion and loss than at a distance of four kilometres through the air of less favoured regions. It is true that the surface of the distant reflector would have to be increased in proportion to the distance, but it would not be necessary to make a single reflector of great size. A row of ten reflectors, each six or eight decimetres in diameter, might be sufficient to insure the visibility of the return ray." A. M. CLERKE

[1] Michelson's revolving mirror executed 256 turns in a second.

NOTES

AT a meeting of the Royal Society of Edinburgh on June 7, medals were presented as follows:—To Mr. John Aitken (Darroch), the Keith Prize for 1883–85, for his paper on the formation of small clear spaces in dusty air, and for previous papers on atmospheric phenomena; to Edward Sang, LL.D., the Makdougall-Brisbane Prize for 1882–84, for his communication on the need for decimal subdivisions in astronomy and navigation and on tables requisite therefor, and generally for his recalculation of logarithms both of numbers and of trigonometrical ratios; to Mr. B. N. Peach the Neill Prize for 1883–86, for his contributions to the geology and palæontology o Scotland.

THE organising committee of Section A has arranged that a special discussion shall be held, at the Birmingham meeting of the British Association, jointly with Section D, on the physical and physiological theories of colour-vision. The discussion will be opened by Lord Rayleigh, and Dr. Michael Foster will also take part in it. Persons who wish to contribute papers bearing on the subject of discussion are requested to send their names to the Recorders of Sections A or D, at 22, Albemarle Street, W., not later than August 1.

THE death is announced, in his seventieth year, of Mr. Llewellyn Jowett, the well-known archæologist.