writer conjectures it to be a hybrid between *lanata* and *populifolia*, var. *leucanthus*.

If any one will look at the plates to which I have referred, he may satisfy himself of the astonishing diversity of these forms. In *Gard. Mag.* 1839, p. 430, is an early record of the appearance of the new seedlings at shows. At the Caledonian Horticultural Show, the Cinerarias "were very brilliant, and partook of novelty." The names of the seedlings successful, including *waterhousiana*, are given. At the beginning of the forties the named kinds became very numerous, and were at first offered at high prices in the trade advertisements. Henderson and Ivery were the two chief English cultivators at that time.

During this period, 1830–1840, the progress was very rapid, and there can be no doubt that the florists' Cinerarias came into existence within some ten or twelve years. Such a plate as that in *Jour. d'hort.* Gand, 1846, shows the ordinary kinds much as we know them. From those plants up to the perfected plants of ten years ago, the change was undoubtedly slow and gradual. The alterations have consisted chiefly in increase in size and symmetry of the flower, and in promotion of compactness of habit (see, *e.g.*, Glenny, *Ann. of Hort.* 1850, p. 37, also *Gard. Chron.* 1879 (1), p. 532).

The next point is of some interest. As compared with other "improved" herbaceous plants, the Cineraria is a little peculiar in the fact that it is now generally raised from seed. This is done partly to ensure that the plants shall not be overgrown, and partly to avoid green fly, a pest to which these plants are specially liable. In consequence of this, the old "named" kinds, that is to say, kinds propagated by asexual methods, went out of fashion, though till lately they still had supporters. It was found that seeds of good strains could be fairly relied on —not, of course, to reproduce the form of their particular parents, but to give fine plants. For instance, Henderson, *Scot. Gard.* i. 1852, p. 22, says: "in raising seedlings you should select three or four dwarf varieties, which number is quite sufficient to produce all the different colours." In *Gard. Chron.* 1887 (1), p. 549, are some interesting particulars of the methods used by Mr. James, to whom the later improvement of the plant in England is largely due. The plants of each colour are grouped in blocks, and the bees are freely admitted to the houses. It is not found necessary to separate the plants further, and in saving seed all the colours are mixed together. In the case of the Cineraria therefore, as in that of Calceolarias, Begonias, and other plants much grown from seed, it is desirable not only to create a fine variety of which the stock can at once be multiplied asexually, but also to raise a good strain of which the seedlings come fairly true. The latter process may undoubtedly often take time.

Even in recent times a "sport" has been recorded. In *Gard. Chron.* 1880 (1), p. 277, it is stated that Mr. James "has succeeded in obtaining a new 'break' that promises to be the forerunner of another host of new flowers. The colours of the flower do not shade off into one another, as is usually the case, but are arranged in bold and well-defined belts. . . . We understand that it flowered for the first time last season, and that it has reproduced itself from seed." A figure is given.

To these particulars might be added many more, relating to the origin of double varieties, variations in the foliage, and other matters. The foregoing notes of the history must, I think be taken to show (1) that the modern Cinerarias arose as hybrids derived from several very distinct species; (2) that the hybrid seedlings were from the first highly variable; (3) that "sports" of an extreme kind appeared after hybridisation in the early years of the "improvement" of these plants; (4) that the subsequent perfection of the form, size and habit has proceeded by a slow process of selection. Mr. Dyer's statement that the modern Cinerarias have been evolved from the wild *C. cruenta* "by the gradual accumulation of small variations" is therefore, in my judgment, misleading, for this statement neglects two chief factors in the evolution of the Cineraria, namely, hybridisation and subsequent "sporting."

I have ventured to deal with this case because it seems to be generally supposed by those not acquainted with the facts, that the origin of the modern florists' flowers has in general been very gradual. As a matter of fact it would, I believe, be more true to say that the new departures have in general been at first very rapid, subsequent improvement being commonly slow. "Sporting," usually after hybridisation, has been the chief factor in the production of these new developments, just as in the case of the Cineraria. To speak of no more, I may refer to the new forms of Begonia, of Gladiolus, and of Erica now so familiar. With what special propriety the Cineraria was chosen by Mr. Dyer to support his contention is not evident to me.

Whether any of these sports exhibit the phenomenon of organic stability I cannot now discuss. W. BATESON.

St. John's College, Cambridge, April 17.

The Age of the Earth.

IN Dr. Hobson's letter on this subject, he confuses the argument by the introduction of a new factor (never alluded to in the former discussion, or in my theory as stated in "Island Life"), the *bulk* or *volume* of the matter deposited. This has nothing whatever to do with the practical problem, because it is admittedly impossible to form *any* estimate of the total bulk of all the stratified rocks of the earth during all geological time; while it is equally impossible to form any estimate of the total bulk of the denuded matter, since we have no clue whatever to the number of times the same areas have been again and again denuded. But the maximum *thickness* of the same rocks, compared with the average *rate* of denudation, and the coincident maximum *rate* of deposition, *do* furnish materials for an estimate, since they can all be approximately determined from actual observation; and the result is what I have given. If Dr. Hobson had referred to the former discussion he would have avoided imputing to me "fallacies" which I never made. I never said a word about "equal bulks" of material being deposited in less time than they were denuded. But, as the only available data are those of *thickness*, not bulk, then it is clear that, if the area of deposition is one-nineteenth of the area of denudation, the *rate* of deposition of a known *thickness* of rocks will be nineteen times as great as the known *rate* of denudation. It was necessary for me to point this out when first discussing the subject, because one eminent writer had made the rate of deposition *less* than the rate of denudation, because the water-area is greater than the land-area of the globe; while an eminent geologist has quite recently taken the rates of denudation and deposition as being *equal*. If, however, the area of deposition is very much *less* than the area of denudation, which is now admitted to be the fact, then the *rate* of deposition *per foot of thickness* will be many times *greater* than the *rate* of denudation.

I should not have thought it necessary again to state this very obvious conclusion, had not Prof. Sollas, while so clearly pointing out Dr. Hobson's misconception as to the area over which the maximum thickness of the strata extended, omitted to refer to the confusion he has now for the first time introduced into the problem, by references to the *bulk* or *volume* of the sedimentary rocks, a factor which all previous writers have seen to be wholly beyond even an approximate determination.

ALFRED R. WALLACE.

So little is really known about the earth's age that any additional mode of approximating to it, however rough, may possess some value. The following method of finding a lower limit is, with one or two alterations, the same as that given in a paper in the *Geological Magazine* for 1887 (p. 348). It depends, not on the rate of denudation, but on the rate of subsidence within the area of sedimentation.

Part of the sediment brought down by a river is used for keeping the surface of the delta close to the level of the sea; and the fact that the deposits formed from it are generally shallow-water deposits, shows that the amount of sediment is, as a rule, sufficient or more than sufficient for the purpose. The remainder of the sediment is carried out seawards, and enlarges the delta laterally.

If there were no surplus sediment, it is evident that the mean rate of subsidence over the delta would be obtained by dividing the volume of the sediment brought down annually by the river by the area of the delta. But if there be an excess of sediment, then the same quotient will give a value greater than the mean rate of subsidence, for only part of the sediment is used for keeping the delta-surface in shallow water. In the case of the Mississippi, the amount of sediment brought down annually is 7,459,267,200 cubic feet, and the area of the delta 12,300 square miles, or 342,204,320,000 square feet; so that the mean rate of subsidence is not greater than $\frac{1}{4\cdot6}$ of a foot per year, or 2·18 feet per century.

Prof. Sollas estimates the total maximum thickness of the different layers of sediment since the beginning of Cambrian

times at 164,000 feet (NATURE, vol. li. p. 534). If these layers tapered off uniformly in either direction from the region of maximum deposit, the total mean thickness would be half this, or 82,000 feet; and if the mean rate of subsidence were never greater than 2·18 feet per century, the total time required for the accumulation of Cambrian and post-Cambrian rocks would be not less than $3\frac{3}{4}$ millions of years. But there may have been long unknown gaps in the process of their accumulation; the outer margin of the deposits may have extended far beyond the area of subsidence, and the mean rate of subsidence may have been at all times considerably less than the upper limit given above. On these accounts, as well as on others that might be mentioned, it seems possible that much more than $3\frac{3}{4}$ million years has elapsed since the beginning of the Cambrian period.

Birmingham, April 8. C. DAVISON.

The Burmese Chipped Flints Pliocene not Miocene.

IN the *Geological Magazine* for November of last year, p. 525, is a review by Prof. T. Rupert Jones, of the important paper, published in the *Records of the Geological Survey of India*, by Dr. Fritz Noetling, the Palæontologist of the Survey, "On the occurrence of Chipped (?) Flints in the Upper Miocene of Burma." Another paper, by Prof. T. R. Jones, on "Miocene Man in India," appeared in *Natural Science* for the same month.

From the fact that the mammals *Rhinoceros perimensis* and *Hipparion antelopinum*, of which bones were found associated with the flint chips, have only been found in India in Pliocene beds, and from a slight acquaintance, gained, it is true, more than thirty years ago, with the Burmese strata in which Dr. Noetling's most interesting discovery was made, I felt assured that there must be some error in believing that the flint chips occurred in Miocene deposits, and I wrote to Dr. Noetling on the subject. I have just heard from him in reply. In a letter from Upper Burma of March 11, he tells me he has now definitely ascertained that the bed containing the chipped flints is Pliocene.

Further particulars will, I hope, be published before long by Dr. Noetling; and I should not have written on the subject but that a serious error is caused by its being supposed that "Miocene Man" has been shown to have existed in India, and it is desirable that this error should be corrected without delay. The importance of the discovery is in no way diminished by the correction of the geological date to which the flint-bearing stratum is referred. W. T. BLANFORD.

April 17.

The Mandrake.

WITH regard to Prof. Veth's exhaustive account of the mandrake (referred to in NATURE of April 11, p. 573), it may be useful to students of folklore to call their attention to the occurrence in the Chinese literature of a similar superstition, wherein *Phytolacca acinosa* (Shang-luh) takes the place of *Mandragora officinarum*. Sie Tsai-Kang's "Wu-tsah-tsu," written about 1610 (Japanese edition, 1661, tome x. p. 41), contains the following passage:—"The Shang-luh grows on the ground beneath which dead man lies; hence its root is mostly shaped like a man.[1] . . . In a calm night when nobody is about, the collector, offering the owl's flesh roasted with oil, propitiates the spirit of the plant until *ignes fatui* crowd about the latter; then the root is dug out, brought home and prepared with magic paper for a week; thus it is made capable of speech. This plant is surnamed 'Ye-hu' (*i.e.* Night Cry) on account of its demoniacal nature.[2] There are two varieties of it: the white one is used for medicine; the red one commands evil spirits, and kills men when it is internally taken by error." KUMAGUSU MINAKATA.

April 16.

[1] Here the author says: "It is popularly called 'Chang-liu-Kan' (= Witch-tree-root)." The name shows that the root was used in witchcraft, similarly with that of the Mandragora (*cf.* Hone, "The Year-Book," *sub.* "December 28").

[2] Another explanation suggested for this name is that, as long as the fruit of the Phytolacca remains unripe, the cuckoo continues to cry every night (Sie Tsai-Kang, *ubi supr.*). However, seeing that the belief in the shrieks of the Mandragora was once current among the Europeans ("Encyclopædia Britannica," 9th ed. vol. xv. p. 476), it would be more just to derive the Chinese name "Night Cry" from an analogous origin.

A Claim for Priority.

I SEND you, under separate cover, a copy of an address, "Radiant Matter," &c., delivered at the International Electrical Exhibition, held in Philadelphia in 1884, reprinted from the *Journal* of the Franklin Institute, September 1885, and would call your attention to the description of the method of preparing films of gold and other metals of extreme thinness, far exceeding in tenuity those described in NATURE as novelties in metallurgical methods (prepared in identically the same manner), and exhibited at a conversazione of the Royal Society, June 13, 1894. The first published note regarding this subject may be found in the *Proceedings* of the American Phil. Soc., vol. xcix. February 16, 1877. Later and fuller notices will be found in *Journ. Franklin Institute*, April 1877, June 1877, September 1885, and September 1894. In addition to the above, the process was fully described in *U.S. Patent*, 198, 209, December 18, 1877. ALEX. E. OUTERBRIDGE.

Philadelphia, April 5.

AN IMPROVED METHOD FOR THE MICROSCOPIC INVESTIGATION OF CRYSTALS.

A MEMOIR of considerable importance to all who are interested in the microscopic determination of the characters of crystals, is contributed by Prof. Klein to the *Sitzungsberichte* of the Berlin *Akademie der Wissenschaften* for January 31, 1895. The two essential points of the communication are that a form of stage goniometer is described, which permits of the most complete examination of many of the principal zones of the crystal with one and the same setting of the crystal upon its holder, and that the crystal is immersed during the observations in a liquid whose refractive index is about the mean of the refractive indices of the crystal. The idea of the "Universaldrehapparat," as the new stage goniometer is termed, appears to have suggested itself almost simultaneously to Prof. Klein and to Herr von Federow, for the former described an earlier form of it in the *Sitzungsberichte* of April 1891, while the latter published a description of an "Universaltischen" for the microscope in the *Zeitschrift für Krystallographie* of May in the same year. Herr von Federow had previously contributed to the *Zeitschrift* a remarkable memoir concerning a theodolitic universal goniometer, and the application of the principle of that instrument to the microscope goniometer followed naturally therefrom. The present memoir of Prof. Klein affords so admirable a description of the improved instrument, which has been constructed for him by the well-known Berlin crystallographical optician, Herr Fuess, and likewise of the mode of employing it in connection with the immersion method, that readers of NATURE may find a brief account of it not uninteresting. Unfortunately this description cannot well be illustrated, as Prof. Klein's illustrations are photographic reproductions which are unsuitable for further reproduction.

The microscope should of course be one of the petrological type, fitted with the usual accessories for the examination of crystals in parallel and convergent polarised light. The particular instrument constructed for Prof. Klein is somewhat similar to the largest Fuess model. It is so arranged with respect to the centre of gravity that it can be rotated into the horizontal position whenever desired, a point of some importance with regard to the use of an immersion liquid. The stage is of course circular, and is divided so as to read with the aid of a pair of verniers to single minutes; it is further provided above with two graduated rectangular traversing movements, one of which is supplied with a micrometer registering 0·01 m.m., while the other is capable of much more rapid motion. The advantages of the simultaneous rotation of the polarising and analysing nicols, as adopted in the microscopes made by Mr. Swift under the direction of Mr. Allan Dick, have been so well appreciated by