The plan and elevation of this building, which will serve as a type for those installed in most of the lighthouses, is shown in Fig. 7.

The Planier is a full horizon light. Its characteristic is that of three white flashes separated by a red flash. Its range, like that of all the new lights in the Mediterranean, is twenty-seven nautical miles for fourteen-fifteenths of the year.

We have mentioned that the transformation of the Palmyre light is also in progress. This, unlike the Planier, will throw a beam in one direction only; and the arrangement of the lantern is therefore slightly different. It is shown in Fig. 8. The general disposition resembles, up to a certain point, that of la Hève. The optical apparatus for the new fixed lights will have a diameter of 0.6 met., instead of 0.3, as was formerly employed. With the revolving cylinder of vertical lenses, this diameter will reach 0.7 met.

CRITICISM OF PROFESSOR HUBRECHT'S HYPOTHESIS OF DEVELOPMENT BY PRIMOGENITURE.

EVOLUTIONISTS have hitherto been puzzled to find a full and satisfactory explanation of the persistency of certain types, such as the familiar Lingula and others, through long periods of the earth's past. Prof. A. A. W. Hubrecht of Utrecht has offered, in his inaugural address, an hypothesis which he thinks adequate to solve this problem. The address is published in full in Nature, nos. 690–691. We may pass over the first part, which contains familiar matter only, and which, therefore, we venture to advise scientific readers to skip. The presentation of the author's own views begins near the bottom of the first column on p. 302. The habit of needless diffuseness in writing is a very grave encumbrance to scientific literature, and ought always to encounter the critic's emphatic condemnation.

The theory which Professor Hubrecht has advanced appears to us not only untenable, but unscientific; we think it might be characterized as pure speculation of that reckless quality which of late years has crept into zoölogy, considerably to the discredit of the science. To justify this condemnation, we will first state the author's hypothesis, and afterward the objections to it.

The hypothesis may be summarized as follows: 1. In many animals the period of reproduction is a prolonged one; so that there are young born of young parents, others of old parents, and, of course, of parents of intermediate age. A distinction therefore exists between first-born and last-born posterity. 2. Similarly, these first-born will likewise have first- and last-born; so, also, will the last-born; consequently there will be one set of generations of the first-born, and another set of the last-born. 3. In the first series the generations will follow rapidly, in the second series slowly, upon one another; hence, from a given pair, there will be in time numerous descendants; "a small number of these being descendants in a direct line of the first-born of every successive generation, another small number being the descendants in a direct line of the last-born of every successive generation." Consequently, of the contemporaneous generations, the individuals of the first set would have numerous ancestors; those of the second set, not nearly so many. 4. The age of the parent affects the character of the progeny. Of this, Hubrecht is able to bring forward only one example, — apparently the only one known to him; namely, that Stone found in the McCloud River that the eggs of young salmon are smaller than those of old salmon. 5. "I must now call your attention to the second cardinal point. . . . Heredity has, indeed, invested them [the progeny] with peculiarities, part of which show themselves in their organization; another part remaining latent, and only attaining development in following generations. Such a latent potential energy towards eventual modification of the individual or his progeny must needs find more occasions to unfold itself in the first-born, *simply because these are possessed of a larger number of ancestors*" (the italics are ours). 6. Asexual reproduction is accompanied by less variation than sexual.

From these premises, the deduction: that the first-born of sexual generations are the principal variants, and *ergo* the principal source of new species; and the last-born, *per contra*, the representatives of stability.

In rejoinder to this plausible but specious argument, our contention is, *first*, that we cannot assume that there are really any series of first- and last-born; *second*, that, granting the distinction between them, it cannot be assumed that one is more variable than the other; *third*, granting both these premises, the facts of zoölogy cannot be made to show that the permanence of types is derived from the last-born, nor that the evolution of new species depends on primogeniture to any considerable extent.

First, Any succession of first-born would depend upon both parents being first-born; and the probability of both parents so being for any considerable number of generations is so infinitely small that it might be called zero. Let us take a species which pairs (a bird, for example), and where the male fertilizes only one female. Let us assume that in a given locality there are ten of each sex, and of various ages, and that there is an equal chance of any two pairing; then the probability of the first-born male pairing with the first-born female would be 1 in 100. The chances of the next set pairing in the same manner would be also 1 in 100, if we further assume, what is the usual case, that the number of individuals remains constant. The chances of both pairs being first-born would be 100×100, or 10,000. In nine generations the chance of their being all first-born would become 1 in 1,000,000,000,000,000,000 (one million million million). Now, for birds which become mature in one year, these are the chances for nine years. Birds are known first from the Jurassic, which we will call for convenience 1,000,000 years ago; so that it might prove laborious to write out the chances for that period, the chance being the last term of a geometrical progression of which one million is the number of terms, and one hundred the ratio. Yet we have taken a case exaggeratedly in favor of Hubrecht's view. It were possible to adduce many arguments to show that the habits of animals often render the existence of a series of first-born improbable; but the previous calculation sufficiently disposes of Hubrecht's fundamental assumption. And, moreover, every such calculation would lead to essentially the same result, whatever the figures chosen to start with might be, because the chance is the last term of a geometrical progression. If Pro-

fessor Hubrecht finds mathematics unconvincing, we would beg him to consult genealogical records, by which he could ascertain the carefully registered contradiction of his assumption that there is a series of the first-born, or even an approximation to it.

Second, We cannot accept the assertion, that a large number of ancestors increases the tendency to variability, because the direct influence of the progenitors upon the production of variations very rapidly diminishes as the number of generations increases. And, on the other hand, it is well known that long-inherited characteristics are the most constant. The more ancient a feature is, the greater its fixity: hence we might as well assume the opposite of Hubrecht's assertion; viz., that the greater the number of ancestors, the more fixed the qualities of the young. Here it may be noticed, that although it is very probable that the parents' age causes modifications in the young, yet Hubrecht mentions only one fact to support the assertion, and that fact is the only one brought forward to support any portion of his hypotheses. We certainly have no sufficient reason for agreeing with the assumption that first-born would be more variable than last-born.

Third, If we admit the two previous premises, we should still have to show that they have given us the determination of the real causes. If evolution by primogeniture were a real cause, then the most variable animals, or those classes where there are most species, would, in consequence of inherited habit, produce young while themselves young, and the stable types would have acquired the characteristics of reproducing very late. Such, however, is not the case. Insects, the most variable of types, reproduce, for the most part, at the end of their lives; while the permanent type, Lingula, reproduces while young. Further objections might be added; but sufficient has been said to explain, and, it is believed, to justify, the condemnation of the hypotheses involved in the author's generalization.

Professor Hubrecht, by his able morphological researches on various subjects, notably on the anatomy of nemertines, has earned a well-deserved esteem: and it is a matter of regret to have to criticise any writing of his severely; but the tendency to draw a maximum of conclusion from a minimum of fact is one to which we feel impelled to object most strenuously. Hubrecht (p. 279) speaks almost sneeringly of what he is pleased to call the school of scientific zoölogists,[1] or those who have sought to elevate zoölogy above mere systematic work. The cause of his *animus* we do not know, but feel that he is hardly just, and not likely to wish to be called an *unscientific* zoölogist himself. Of his hypothesis of development by primogeniture, our opinion has been expressed.

CHARLES S. MINOT.

NOTES ON THE GEOLOGY OF JAPAN.

WE are permitted, by the courtesy of M. Jules Marcou of Cambridge, to make use of the following extract from a letter addressed to him from Tokio by Dr. C. Gottsche, professor of geology in the Tokio *daigaku*, or imperial university.

Since you published, seven years ago, the *Explication d'une carte géologique de la terre*, much has been changed in Japan. Lyman's flying surveys in Yesso and Japan expired in 1879. A new geological survey has been established, under the superintendence of Dr. E. Naumann; geology has been taught for more than six years, both in the university and at the engineering college of Tokio; and travellers are allowed to cross the interior in every direction. A mass of information has been procured in this way; and I suppose you will find valuable materials in the notices, and in the little sketch-map my friend and countryman, Dr. Naumann, is just preparing for you. Nevertheless, I take the liberty to furnish you with some additional remarks on facts or specimens which I have recently examined, and which might be overlooked by him.

The upper Devonian system is indicated by half a dozen specimens of Spirifer disjunctus de Verneuil, which I met in several old Japanese collections, and which partly originate from the provinces Tosa (on Shikoku) and Ise (on the main island). This fossil has not yet been met with *in situ*.

The carboniferous system is only represented by marine limestones, which are exposed in seventeen localities along the eastern coast of Japan, from 39° 10′ N. L. to 31° 20′ N. L. The fauna is very scanty; but everywhere the limestones are characterized by the common occurrence of Fusulina and Schwagerina, which in many cases are accompanied by Endothyra, Textilaria, and Trochammina. Among other fossils, I mention only Bellerophon (?) hiulcus Sow., Favosites, and Poteriocrinus.

The limestones correspond, in my judgment, to the whole carboniferous system, the upper productive series included. My reasons are: 1°. The different paleontological character of the lower carboniferous mountain-limestone of Lo-ping in China (cf. Kayser, *Zeitschr. deutsch. geol. gesellsch.*, 1881, 351); 2°. The common occurrence of the genus Schwagerina, which I think is confined to the uppermost carboniferous and lower dyassic systems of Nebraska, Russia, and Austrian Alps; 3°. The researches of V. von Möller, who states that the marine carboniferous limestones of Russia also represent the entire system. From the second point, it might seem that our Japanese deposits correspond only to the uppermost series, which in China is really productive.

The dark triassic shales, with Monotis salinaria, var. Richemondiana Zittel, which Dr. Naumann discovered near Sendai (*Jahrb. k.-k. reichsanst.*, 1881, 519), now extend from 40° N. L. (Niageba, province of Ugo) to 33° N. L. (Kinkaisan, province of Higo). This will be the more interesting to you as special care is devoted in your Explication to the Monotis strata. Very similar dark shales from Okatzumura and Minatomura, district of Ojikagori, province of Rikuzen (about 38° 30′ N. L., 141° 20′ E. long., Greenw.), are lower liassic. I recognized within them Arietites bisulcatus Brug., Arietites of rotiformis Sow., and Lytoceras sp. of the group of L. fimbriatum. The two Arietites are characteristic for the Ammonites Bucklandi-zone of Oppel.

The middle Jurassic is only represented by plant-bearing shales. Dr. Geyler of Frankfurt described already sixteen species from the Tetorigawa valley, in the province of Kaga (*Palaeontogr.*, xxiv. 221, 5 pl.), mostly identical with Jurassic species from Amuria, eastern Siberia, and Spitzberg. In the mean while the number of localities and fossils has somewhat increased. The said strata have been met with again at Nozirimura, province of Echizen; Ogamigo, district of Onogori, province of Hida; Midzutani, near Yuasa, province of Kishiu; and Tannomura, province of Awa, on Shikoku. The leading fossil is everywhere Podozamites lanceolatus

[1] Scientific zoölogy (*wissenschaftliche zoologie*) has had, since the establishment of Siebold and Kölliker's *Zeitschrift für wissenschaftliche zoologie*, a special significance to professional naturalists.