With this word it is easier to state the theorems, "two line-directions determine one plane-direction," and its reciprocal, than with the other. "Two directions determine one aspect," is hard.

If this discussion has not gone on too long perhaps some of your correspondents will criticise this suggestion and compare it with "aspect." It is desirable that the best word possible should be chosen. J. M. W.

Science and Art Examinations

THE subject of Science and Art Examinations by the Department of Science and Art is one which really requires looking up, and I wish to make one or two suggestions and remarks as to the mode of examination.

In the first place, take the examination itself. The candidates make their appearance at the appointed time and place. Their forms are given them, and their places assigned to them. Now the candidate is told to write on both sides of the form, thus leaving no back pages on which to do his rough calculation. Blotting-paper in 1870 was not allowed; but in 1871 the Department fixed a sheet to the bottom of each form in such a position that it was very difficult to make use of it; much time—time that was of the utmost consequence to the candidate—being lost in doing so. This, of course, stopped him from doing so much work, and so lessened his chance of success. This may be all very well for the Department so far as it affects grants on results; but what about the unfortunate student who is made the victim of this very arbitrary custom?

Then again for the questions set. In all the papers the questions set were very difficult. "The Department" having, without any notice, raised the standard of examination, the subjects of questions set in the first stage of mathematics were placed in the syllabus a stage higher, viz., the second stage Then in chemistry (inorganic) the standard was considerably raised. The questions in this subject are very unfair in the opinion of many persons who have seen them. Take the following:—

"HONOURS 1871

"Describe the process of manufacturing sulphuric acid, as carried on in an alkali works, illustrating the various chemical changes by equations, and, as far as possible, the constitution of the compounds formed by graphic formulæ."

Now about the sulphuric acid part, or about the equations, I have nothing to say; but when the question requires a knowledge of graphic formulæ I protest against it. Graphic formulæ are not in sufficient use to warrant their introduction into an examination—thus enforcing their general adoption whether right or wrong; and I do not think the examiner should be allowed to enforce his peculiar views—the views taken by himself and a few other chemists—into the great system of Science examination in the country, thus compelling it to be learnt by any person wishing to compete.

Now for the results. The results of the examinations for 1871 are very unsatisfactory, and a very high ratio is shown of failures, and second classes to first classes obtained. This, of course, must lessen the amount of money to be paid on results by the Department, and a report was circulating a short time ago, to the effect that "The examiners, after having made their reports, had the papers returned to them, with an instruction to reduce the number of successful candidates, as an intimation had been given by a right hon. gentleman that the amount of grant due upon those papers must be reduced 20,000*l*. The examiners were thus obliged to eliminate half the names from their lists." The question was asked by Mr. Dixon, M.P., in the House of Commons, whether this was or was not true, and Mr. Forster, M.P., denied it. But, previous to that, a provincial local secretary, hearing the rumour, wrote to ask the Department if it were true, and received a reply saying it was true, and that instead of the amount being 20,000*l*. it was 40,000*l*. (The Department's letter can be produced.) Now I would suggest that the Department reform these matters referring to the forms, blotting-paper, questions, and results, and that if they do not do so that the House of Commons take the matter up and do justice to Science teachers and students. HENRY UHLGREN

New Zealand Forest Trees

IN the last number of NATURE is a paragraph relating to some New Zealand woods, which the writer observes are "deserving of a better fate than to be cut down wholesale and used as firewood." Five timber trees are mentioned, of which the native names only are given.

Knowing that it is the province of NATURE to give as accurate information as possible on all points with which it deals, I send you the botanical names of four of these New Zealand trees. The Rimu or red pine is probably *Dacrydium cupressinum* Soland, a tree 80 or more feet high, the fleshy cup of the fruit of which is eatable. *D. laxifolium* Hk. fil., a small creeping bush, is also known occasionally as Rimu. The Mataii or black pine is *Podocarpus spicata* Br., likewise a large tree, and having an eatable fruit. The Totara is *Podocarpus totara* A. Cunn., a tree about 60 feet high, producing a durable and close-grained wood much valued in the islands, and, like the others, having an eatable drupe. These trees are all more or less abundant in the Northern and Middle islands, and all belong to the natural order Coniferæ, though we are told in the paragraph referred to that "none of them are Coniferæ."

The Rata, "that wonderful vegetable production forming itself out of numberless vines," &c., is referable to some species of *Metrosideros*. *M. robusta* A. Cunn, and *M. florida* Sm., are both known as Rata, but the hard and very dense wood usually known under that name is mostly derived from *M. robusta*. This, however, is not a climbing plant, but an erect tree 50 or 60 feet high; therefore the plant referred to in the paragraph before us is probably *M. florida*. The Makia I do not know, but its extreme hardness would seem to indicate it as belonging to the same order as the last, namely the Myrtaceæ.

JOHN R. JACKSON

Kew, Nov. 7

The Glacial Drift at Finchley

A FURTHER examination of the railway cutting at the Finchley and Hendon Station shows that the glacial beds now revealed there have a greater thickness and range than I at first imagined. On Saturday last I visited the place in company with Dr. Hicks, of Hendon, a gentleman well-known for his researches in the Cambrian formation. Above the blue clay, and right up within a few inches of the vegetable soil, we found drift fossils. With an interruption here and there from the underlying London clay, these chalky glacial beds, consisting of blue (Oxford?) clay, blueish clay with flints, marl, sand, and gravel (in no regular descending order), have an average thickness of 30 feet. They are open for about 500 yards, and they might perhaps be traced farther north-west, towards the Dollis Brook Viaduct. Dr. Hicks and I afterwards visited Mr. Plowman's Manor brick-fields, a little south-east of the railway station; here too we found fossils in the brick-earth.

From what has transpired during the last few weeks, it would seem that the Muswell Hill deposit need no longer figure in geological literature as an outlier, at a long distance from the general deposit; and Londoners may in future find glacial drift without much difficulty about Highgate, Finchley, Whetstone, and Barnet. I am indebted to Professor Morris for the information that the Great Northern Cemetery at Barnet lies almost wholly in the glacial clay. The forthcoming Survey memoir upon the drift in this district is looked for by London geologists with much interest. HENRY WALKER

100, Fleet Street, E.C., Nov. 7

*ON THE ORIGIN OF INSECTS**

THE metamorphoses of this group have always seemed to me one of the greatest difficulties of the Darwinian theory. In most cases the development of the individual reproduces to a certain extent that of the race, but the motionless, imbecile, pupa cannot represent a mature form. Fritz Müller considers that the wingless Blattidæ probably most closely represent the original insect stock; Haeckel is inclined rather to the Pseudo-Neuroptera. I feel great difficulty in conceiving by what natural process an insect with a suctorial mouth like that

* Abstract of a paper read before the Linnean Society, Nov 2, 1871, by Sir John Lubbock, Bart., M.P., F.R.S.

of a gnat or butterfly could be developed from a powerfully mandibulate type like the Orthoptera, or even from the Neuroptera. M. Brauer has recently suggested that the interesting genus *Campodea* is, of all known existing forms, that which probably most nearly resembles the parent insect stock. He considers that the grub form of larva is a retrograde type, in which opinion I am unable to concur, though disposed to agree with M. Brauer on the first point. M. Brauer in coming to this conclusion relies partly on geological considerations; partly on the fact that larvæ, more or less resembling *Campodea*, are found among widely different groups of insects. I think there are other considerations which offer considerable support to this view. No one, so far as I know, has yet attempted to explain, in accordance with Mr. Darwin's views, such a life history as that, for instance, of a butterfly, in which the mouth is first mandibulate and then suctorial. A clue to the difficulty might, I think, be found in the distinction between developmental and adaptive changes, to which I called the attention of the Society in a previous memoir. The larvæ of insects are by no means mere stages in the development of the perfect animal. On the contrary, they are subject to the influence of Natural Selection, and undergo changes which have reference entirely to their own requirements and condition. It is evident then that, while the embryonic development of an animal in the egg gives us an epitome of its specific history, this is by no means the case with species in which the immature forms have a separate and independent existence. Hence, if an animal when young pursues one mode of life, and lives on one kind of food, and subsequently, either from its own growth in size and strength, or from any change of season, alters its habits or food, however slightly, immediately it becomes subject to the action of distinct forces; Natural Selection affects it in two different, and it may be very distinct, manners, gradually leading to differences which may become so great as to involve an intermediate period of change and quiescence.

There are, however, peculiar difficulties in those cases in which, as among the Lepidoptera, the same species is mandibulate as a larva and suctorial as an imago. From this point of view, however, *Campodea* and the *Collembola* (*Podura*, &c.) are peculiarly interesting. There are among insects three principal types of mouth, firstly, the mandibulate, secondly, the suctorial, and thirdly, that of *Campodea*, and the *Collembola* generally, in which the mandibles and maxillæ are attached internally, and though far from strong, have some freedom of motion, and can be used for biting and chewing soft substances. This type is intermediate between the other two. Assuming that certain representatives of such a type found themselves in circumstances which made a suctorial mouth advantageous, those individuals would be favoured by Natural Selection in which the mandibles and maxillæ were best calculated to pierce or prick, and their power of lateral motion would tend to fall into abeyance, while, on the other hand, if powerful masticatory jaws were an advantage, the opposite process would take place.

There is yet a third possibility—namely, that during the first portion of life the power of mastication should be an advantage, and during the second that of suction, or *vice versâ*. A certain kind of food might abound at one season and fail at another; might be suitable for the animal at one age and not at another: now in such cases we should have two forces acting successively on each individual, and tending to modify the organisation of the mouth in different directions. It will not be denied that the ten thousand variations in the mouth parts of insects have special reference to the mode of life, and are of some advantage to the species in which they occur. Hence no believer in Natural Selection can doubt the possibility of the three cases above suggested, and the last of which seems to explain the possible origin of species which are mandibulate in one period of life and not in another. The change from the one condition to the other would no doubt take place contemporaneously with a change of skin. At such times we know that, even when there is no change of form, the temporary softness of the organs often precludes the insect from feeding for a time, as, for instance, is the case in the silkworm. When, however, any considerable change was involved, this period of fasting would be prolonged, and would lead to the existence of a third condition, that of pupa, intermediate between the other two. Since other changes are more conspicuous than those relating to the mouth, we are apt to associate the pupa state with the acquisition of wings, but the case of the Orthoptera (grasshoppers, &c.) is sufficient proof that the development of wings is perfectly compatible with continuous activity. So that in reality the necessity for rest is much more intimately connected with the change in the constitution of the mouth, although in many cases no doubt the result is accompanied by changes in the legs, and in the internal organisation. It is, however, obvious that a mouth like that of a beetle could not be modified into a suctorial organ like that of a bug or a gnat, because the intermediate stages would necessarily be injurious. Neither, on the other hand, for the same reason could the mouth of the Hemiptera be modified into a mandibulate type like that of the Coleoptera. But in *Campodea* and the *Collembola* we have a type of animal closely resembling certain larvæ which occur both in the mandibulate and suctorial series of insects, and which possesses a mouth neither distinctly mandibulate nor distinctly suctorial, but constituted on a peculiar type capable of modification in either direction by gradual changes without loss of utility.

If these views are correct, the genus *Campodea* must be regarded as a form of remarkable interest, since it is the living representative of a primæval type from which not only the *Collembola* and *Thysanura* but the other great orders of insects have all derived their origin.

CHARLES BABBAGE

DIED THE 20TH OF OCTOBER, 1871

THERE is no fear that the worth of the late Charles Babbage will be over-estimated by this or any generation. To the majority of people he was little known except as an irritable and eccentric person, possessed by a strange idea of a calculating machine, which he failed to carry to completion. Only those who have carefully studied a number of his writings can adequately conceive the nobility of his nature and the depth of his genius. To deny that there were deficiencies in his character, which much diminished the value of his labours, would be useless, for they were readily apparent in every part of his life. The powers of mind possessed by Mr. Babbage, if used with judgment and persistence upon a limited range of subjects, must have placed him among the few greatest men who can create new methods or reform whole branches of knowledge. Unfortunately the works of Babbage are strangely fragmentary. It has been stated in the daily press that he wrote eighty volumes; but most of the eighty publications are short papers, often only a few pages in length, published in the transactions of learned societies. Those to which we can apply the name of books, such as "The Ninth Bridgewater Treatise," "The Reflections on the Decline of Science," or "The Account of the Exposition of 1851," are generally incomplete sketches, on which but little care could have been expended. We have, in fact, mere samples of what he could do. He was essentially one who began and did not complete. He sowed ideas, the fruit of which has been reaped by men less able but of more thrifty mental habits.

It was not time that was wanting to him. Born as long ago as the 26th of December, 1792, he has enjoyed a