own, and one of his (*i.e.* Gould's) early associates, that I first, about 20 years ago, became aware of his discovery of many evidences of glaciation in Tasmania."

I have recently found a Parliamentary Paper, issued in 1860, in which Gould describes his recognition of Glacial action in some of the high valleys of central Tasmania. The passage is as follows ("A Report of the Exploration of the Western Country by Mr. Gould," Parl. Pap., Tasmania, 1860, No. 6) :—

"In the Cuvier Valley I was struck, both in going and in returning, by the similarity to the terminal moraine of a glacier presented by an enormous accumulation of boulders which chokes the lower end of the valley, and, somewhat like a dam, extends completely across it, with the exception of the point where it is broken through by the river."

I am glad, therefore, to be able to give to Gould the credit of having published the discovery, which in my paper I could only quote as a verbal tradition.

The Cuvier Valley is one day's journey west of Lake St. Clair. A hut, five miles due west of the top of Mount Arrowsmith, occurs in it. J. W. GREGORY.

The University, Melbourne, Victoria, April 25.

The Origin of the Horse.

IN your issue of May 19 (p. 53) Prof. T. D. A. Cockerell refers to *Equus caballus celticus*, Ewart, as "still surviving in the pure state in Iceland." Prof. Ewart, in his paper on "The Multiple Origin of Horses and Ponies," says that "the few pure specimens of the Celtic pony survive" in the north of Iceland. I take it that Prof. Ewart does not mean that the northern Icelandic breed of ponies is a pure one, but only that certain individuals of this breed exhibit the "Celtic" characters in a very marked degree. In a recent paper (*Proc.* Camb. Phil. Soc., vol. xii., part iv.) Mr. F. H. A. Marshall and I have brought forward both historical and zoological evidence for the mixed origin of the Icelandic pony. It is perhaps worth noting that the people of north Iceland still claim a social superiority over those of the south as being descended chiefly from the second body of colonists which reached the island. In considering the origin of different breeds of the domestic animals ethnological considerations are often important, and, conversely, the examination of local breeds may sometimes throw light on ethnological problems. For example, in the Malay Peninsula the breed of dogs owned by the majority of the jungle tribes usually classed as Sakais differs from that of the Malay pariah, which has recently been adopted in some cases by Semang tribes and also by those Sakais who live in close intercourse with the Malays. The pariah seems likely to oust the Sakai dog completely, and I am not aware that any zoologist has yet made a detailed examination of the latter, which shows certain resemblances to the local race of *Cyon rutilans*.

Of course, investigations into the ethnological distribution of animals must be made with the very greatest care, for not only may one breed oust or swamp another, but the characters of a single individual may prove so dominant that they may prevail in a great number of cross-bred descendants, and so change the character of a breed in a very short time. This has recently happened in the Færöe Isles. As we know from the statements of Landt ("Description of the Faroe Islands," 1798), there were at least two distinct breeds of dogs in these islands at the end of the eighteenth century, one resembling the modern Danish hound, but smaller; the other a short-legged, rough-haired terrier. The two breeds can still be traced on some of the islands, notably on Naalsoe; but in the neighbourhood of Thorshavn, the capital, great alteration has taken place quite recently. Some ten or twelve years ago a Danish governor introduced a well-bred dachshund dog, which inter-bred with the native bitches. In 1903 I could hardly find a single dog in the town which did not show traces of dachshund ancestry—short, bent legs, long body, &c.—more or less marked. The in-bred highly specialised individual has proved prepotent when crossed with the more or less generalised types which, judging from the statements of Lucas Debes (1623–1670) and Landt, have been somewhat cross-bred for at least two and a half centuries. We are apt to forget factors of the kind when discussing the breeds of domestic animals, and also when investigating the different races of men, but it should be remembered that they are of the very greatest importance in both lines of inquiry. NELSON ANNANDALE.

34 Charlotte Square, Edinburgh.

Insular Races of Animals and Plants.

IF we accept the view that species are such by virtue of segregation, and consider subspecies to be groups as yet imperfectly segregated, we seem logically bound to regard insular forms as valid species. According to this way of looking at the matter, a subspecies is in biology what a peninsula is in geography, while a species corresponds to an island. Hence it follows that many subspecies are far more widely distributed and for most purposes more important than many distinct species; just as many peninsulas are more important than the small islands off their coasts.

While it appears illogical to treat insular races as subspecies, there are difficulties in the way of regarding them all as distinct species. In former years, the most distinct were so recognised, and the others were simply ignored. This practice, while it smoothed the way for the systematist, deprived us of the use of a large body of facts of the greatest possible interest to the evolutionist, and the time has come when it must be given up. As a result of the new methods, the number of "species" recognised is increasing very rapidly, as shown, for example, by the description of seventy new Malayan mammals in a single paper by Mr. G. S. Miller, jun. Many of the "species" described in this paper are excessively similar and yet distinguishable, and inhabit different islands. It is evident that one could take a map of the Malay Archipelago and prophesy with some degree of accuracy the number of insular species of Mus and some other genera awaiting discovery by simply counting the islands, eliminating those too closely adjacent. In mountain regions something of the same sort is found, the tops of the mountains or mountain ranges serving the same purpose as islands. For freshwater organisms, lakes and river systems afford similar phenomena, as shown, for example, by the races or species of Salmonidæ.

The objections to the recognition of all these isolated forms as valid species are two. First, their extreme similarity in many instances, and second, the specific name does not indicate the immediate relationships of the form. It has seemed to me that these difficulties might be overcome by the recognition of a new category, for which the name "idiomorph" suggested itself. This name may be objectionable on account of the term idiomorphic, used in crystallography, and it is probable that someone can think of a better. If it is accepted, it may be abbreviated to "id.," as "var." is written for variety, and "subsp." for subspecies.

To illustrate the different methods, we may take certain bats of the genus Chilonycteris, found in the Greater Antilles, using the facts recently published by Mr. Rehn.

C. macleayii group.

	i. (Species).	ii. (Sub-species.)	iii. (Idiomorphs).
Jamaica ...	*C. grisea*, Gosse.	*C. macleayii grisea.*	*C.* (*macleayii* id.) *grisea.*
Cuba ...	*C. macleayii*, Gray.	*C. macleayii.*	*C. macleayii.*
Haiti ...	*C. fuliginosa*, Gray.	*C. macleayii fuliginosa.*	*C.* (*macleayii* id.) *fuliginosa.*
Porto Rico	*C. inflata*, Rehn.	*C. macleayii inflata.*	*C.* (*macleayii* id.) *inflata.*

C. parnellii group.

Jamaica ...	*C. parnellii*, Gray.	*C. parnellii.*	*C. parnellii.*
Cuba ...	*C. boothi*, Gundlach.	*C. parnellii boothi.*	*C.* (*parnellii* id. *boothi.*
Haiti ...	*C.* ?[1]	*C. parnellii?*	*C.* (*parnellii* id.)?
Porto Rico	*C. portoricensis*, Miller.	*C. parnellii portoricensis.*	*C.* (*parnellii* id.) *portoricensis.*

[1] Doubtless exists, but not yet discovered.

The proper name of the idiomorph would be a binomial, the name of the superspecies being inserted when advisable, just as subgeneric names are inserted, within brackets.

T. D. A. COCKERELL.

Colorado Springs, Colorado, U.S.A., May 7.

Graphic Methods in an Educational Course in Mechanics.

I AM glad to have succeeded in calling forth some correspondence on this subject. But since I have evidently failed to make my views clear, may I briefly restate my contention?

By an educational course in mechanics, I mean a course intended to teach a beginner the principles of mechanics; a course that will leave him properly equipped for more technical work.

By "analytical methods" I mean those methods in which we resolve forces and take moments about axes. Working diagrams and plotted curves (as is quite clearly implied or stated in my former letter) would accompany such work, and would not come under the head of graphic statics.

By "graphic methods" I mean those methods that depend on accurate drawing only, there being no calculation; methods in which "resolution" is replaced by the drawing of force polygons, and "taking moments" by the drawing of funicular polygons.

I advocated the exclusive use of the former methods in bringing the beginner up to the desired point at which there would no longer be danger of confusion of ideas as to principles. Such methods demand the use of simple equations and of a little elementary trigonometry.

Mr. Milne, I see, agrees with me in the main. I cannot, however, agree with him in his view that the employment of analytical methods implies that the teaching is not to be experimental, or leads to impressing on the pupil the idea that "statics is practically useless." Surely he would find "*resolving*" and "*taking moments*" more practical than drawing polygons of forces and funicular polygons in introducing a beginner to the action of machines, to matters of friction, to the nature of bending moments and shearing forces, to the torsion of shafts, and, indeed, to most of the problems of practical mechanics.

Even in the case of "statics of structures," if we limit ourselves (as I do here) to such a range as will be sufficient to make the principles clear, there is much to be said for the analytical "*method of sections*"; and if this be employed there is less temptation to present to the beginner the *un*practical "weightless frame, loaded at the joints only." However, in this branch of mechanics, graphic methods must be employed sooner or later when the learner passes beyond the simpler forms of structures.

Mr. Trotter has quite misunderstood me! He speaks much about (or against?) mathematics; says that my pupils should "emerge as mathematicians"; and refers (deprecatingly?) to "wranglers."

I cannot see that the employment, with beginners, of the methods of *resolution* and *taking moments* would produce a race of wranglers, any more than that the employment of graphic methods would produce a race of geometricians or artists.

Further, he considers me as opposed to the use of diagrams, and as preferring formulæ to explanations given in "quite ordinary language"; and he asks (indignantly?) whether I "would deny the use of a piece of string on a globe to explain great circle sailing?" I may state briefly that I am not a mathematician, that I am fond of diagrams, that I delight in simple language, and that I would give two pieces of string to any pupil who had serious aims in view. I do not think that the above were quite reasonable deductions from my letter.

I do not wish in my turn to misunderstand Mr. Trotter. But I gather from the second paragraph of his letter (vol. lxx. p. 81) that he claims the use of "quite ordinary language" as the prerogative of those teachers who use graphic methods in preference to the analytical methods of "resolution" and "taking moments"? I gather also, from the last paragraph, that, in his opinion, *to resolve forces* and *to take moments about axes* "confuse learners of statics"; and that these analytical methods are a failure when applied to dynamics? Certainly there is here a real difference of opinion between Mr. Trotter and myself.

Devonport, May 28. W. LARDEN.

THE graphic methods are the complement of the analytical, and a mind brought up on either to the exclusion of the other is but half trained. I agree with Mr. Milne that the best results are obtained when the two methods are used side by side. But there is another and potent reason for including graphic methods in an elementary course; they can to a great extent be used at an earlier stage and before the student has proceeded far in his mathematical training. The triangle of forces is practically the only principle involved, and if this is satisfactorily taught, so that in any practical application the student can write out clearly an explanation of his diagram showing what the different lines represent, he will then proceed naturally to the analytical methods of *resolving* and *taking moments*. But he will never abandon the graphic methods, which should now be developed simultaneously with the analytical. His mastery of the two, with the analytical, as I think, resting on the graphic, will give him greater resourcefulness than he would be likely to obtain from an exclusive use of one method.

I want to see the study of mechanics, even in its elementary stages, brought into closer union with practical requirements, and the barrier which usually separates theoretical from applied mechanics to a considerable extent removed. The inclusion of graphic methods tends to prevent the discussion of fantastical problems invented by the mathematician from usurping the consideration of the more practical kinds required by the engineer. In the elementary work it is not usual to take account of the internal forces which are called into play when any solid is in equilibrium under external forces. I think that the stresses induced in a bar of no appreciable weight by forces applied at its extremities should be considered at a very early stage, and then the student may work easy problems on the equilibrium of simple frames. Of course these problems are all more or less idealised, but they will serve to show him that he is at work upon something of practical value, and he will not fail to grasp and appreciate it. W. J. DOBBS.

East Putney, May 27.

The Drumming of the Snipe.

IT is disputed whether the snipe's drumming—a curious noise, suggestive of a miniature threshing machine—is made by the bird with its wings or by its tail, or by both wings and tail. Some recent observations incline me strongly to believe that the tail plays at any rate the more important part. During the performance the bird flies at a great height round and round in a wide sweeping circle. At intervals he makes a sudden and rapid descent, holding his wings partly flexed and his tail spread to its full extent. The outermost tail feather on either side points outward at a greater angle than those adjoining it, so that when the bird is watched through a good field glass daylight shows between it and the next; and, if I am right in my view, the drumming sound is due to the rush of air against this isolated feather. The snipe's tail feathers seem so puny that it is at first difficult to believe that they can produce so great a result. But if an outer one be taken—it is slightly scimitar-shaped with the outer web much reduced—and swung rapidly through the air, the drumming noise may be distinctly heard, though it seems but a very faint echo of the loud throbbing hum that startles one when it suddenly descends from an ethereal height, and the small bird is descried, hardly more than a speck to the naked eye, circling round in wild career, and now and then swooping headlong downwards and thrilling the air with his weird music. F. W. HEADLEY.

Haileybury.