over, the heights of the several plants are different, though all were sown at the same time. Differences in the comparative rates of ontogeny of the individuals and in the rates and degrees of development of the different characters are generally ascribed either to differences of environment or to "individual variation", i.e. to causes inherent in the individual which may be unknown or partly known, such as differences in gametic composition. The differences of individual environment of the plants under consideration must be very slight and no more than those of the different members of parallel lineages found where they grew and died together in the same geological bed; and the differences of rate and degree in the development of their ontogenetic characters is probably mainly due to individual variation. Similarly the differences of rate and degree exhibited by the trends of parallel lineages are probably due in a large measure to "individual variation" of the lineages, i.e. to causes inherent in the individual lineages. But here our analogy fails; for the ontogeny of an individual is largely a recapitulation of ancestral, and at least a repetition of parental, characters, while the evolution of a lineage is a march along new lines.

Again, if the relation of a species to its genus is similar to that of the individual to its species, then, to be logical, we must name our bundle of parallel lineages as a genus, and each lineage as a species; the arbitrary points on a lineage, which we have been accustomed to name as species, or as mutations in Waagen's sense, must be termed merely phylogenetic stages in the life-history of a species; and the nodes of Dr. Trueman's supposed plexus must be described as hybrids. Without advocating or deprecating such a course, I would simply moot its desirability.

New species may be regarded as arising either as hybrids at the nodes near the base of the plexus, or as new offshoots from a persistent radical. This supposed basal plexus is represented in the illustration just given by the chance crossings and wanderings of the stems before they find the string up which they ultimately climbed. Suppose that, when such crossing stems touched, they could fuse and produce at that point a hybrid shoot, this would illustrate the origin of a new species produced as above suggested.

W. D. Lang.

Chelsea.
16th August, 1924.

TYPE-SPECIMENS.

Sir,—The Governing Body of the Imperial College of Science and Technology having decided that it is undesirable to retain type-specimens of fossils in a palaeontological collection used for teaching purposes, it may be of interest to record the transfer of the very few specimens concerned to the several museums chosen as most appropriate in each case.

1. To the British Museum (Natural History)—holotype of

Drybrookia cubitalis H. Bolton, *Quart. Journ. Geol. Soc.*, vol. lxxx, 1924, pp. 17–21, pl. iii.

2. To the Museum of Practical Geology, Jermyn St.—(*a*) holotype of *Euidothyris holcophora* S. Buckman, from the *scissum* zone, near Sherborne, Dorset, figured in "Brachiopoda of Namyan beds" (*Pal. Indica*), pl. xx, fig. 32; (*b*) a specimen of *Nuttainia hibernica* Portlock, apparently that figured in "Geology of Londonderry," pl. v, fig. 2 [=*Terataspis* (*Paralichas*) *kildarensis* Reed]. This is one of many specimens which originally served as a "Demonstration Series" in the days when the Royal School of Mines was intimately associated with the Geological Survey, and eventually formed the nucleus of the present teaching collection of the Imperial College.

3. To the Bristol Museum of Science and Art—holotype of *Ammonites comptoni* S. P. Pratt, *Ann. and Mag. Nat. Hist.*, viii, 1841, pp. 163–5, pl. iv, fig. i, recently re-figured in *Type Ammonites*, vol. v, pt. 45, April, 1924, no. cdlxxxv. The history of this specimen cannot be traced, but its identity with Pratt's figure, first noticed by Mr. Buckman, is unquestionable. It now lies in the Museum which contains Pratt's other Christian Malford types.

A. Morley Davies.

Imperial College, S.W. 7.
29th August, 1924.

THE NOMENCLATURE OF ROCKS

Sir,—Will you allow me a little space in which to reply to Mr. Sargent's letter in the August number? With no ulterior motive, but with the apparently vain hope of causing an error to be rectified, I have made certain definite criticisms of his use of the rock-name "spilite". It would be more to the point and certainly of greater interest if Mr. Sargent would discuss the facts, instead of indulging in baseless accusations of misrepresentation. His letter was evidently calculated to convey the impression to your readers that the title of the paper under discussion had been deliberately misquoted in order to gain a point at his expense. In point of fact I quoted the title printed on the cover of the "separata" of the paper, and used as the page heading, rather than the lengthy title of the paper itself. The reference as given was quite unambiguous, and Mr. Sargent is merely side-tracking the issue which, I venture to believe, has been clearly stated in my previous communications. The opinions of other petrologists interested in rock nomenclature would be of value.

A. K. Wells.

King's College, London.
1st October, 1924.