

before the end of the Devonian. The forms of this species are extremely variable, and Professor Gosselet gives in this Memoir the results of the study of numerous specimens, more particularly of those occurring in the Schistes de Barvaux, where it is especially numerous and well preserved. Detailed measurements of a large series of forms show important variations in nearly every feature of the shell, and the only constant character is the simple form of the ribs on the lateral or winged portions. The extreme variations are connected by a crowd of intermediate forms, and in the course of development the same individual may pass from one variation to another. There can, therefore, be no question of varieties in a zoological sense, and the author arranges the various modifications into groups of forms, six in number, taking as a basis the proportion of the width of the shell to the length of the smaller valve.

The author considers that *S. Verneuli* finally dies out without giving rise to another species; for though there are no extreme differences between it and such forms as *S. attenuatus* and *S. Mosquensis*, there are yet no intermediate forms known to indicate that it may be the ancestor of these latter. Discussing the question of the name properly belonging to the species, that of Murchison is adopted as, on the whole, having the best claim to priority.

To all interested in the question of Brachiopodal development the carefully worked out details in this Memoir, and its abundant illustrations, should prove highly valuable.

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ORIGIN OF THE YOUNGER RED ROCKS.

SIR,—In his very interesting paper “On the Younger Red Rocks,” my friend Dr. Irving professes himself to be more than ever convinced that the pebbly sandstones of the Bunter—such as those in Notts—consist of Triassic *sandbanks* deposited in narrow *tidal seas*, and “the great pebble beds of the Warwickshire and Budleigh Salterton type” are “the ‘Chesil banks’ which formed the shore equivalents of the sandbanks.”

This is a matter on which I thirst for knowledge. Will he then kindly inform me: (1) What instances of marine conglomerates are known in which length, breadth, and thickness are simultaneously great? As a rule, the zone on which ordinary shingle is deposited extends but a few feet vertically above high-water mark, and not many below it. These conditions must somewhat restrict its breadth, and still more its thickness. Banks of the “Chesil” type can attain a greater thickness (though I should be glad to hear of one which came near or exceeded 100 feet), but they are comparatively narrow. (Mr. H. B. Woodward states that the Chesil Bank at Portland is 200 yards wide and 42 feet high.) But, according to Mr. Ussher and other authorities, the length and breadth of the area occupied by the Budleigh Salterton pebble bed is measured by miles, and its thickness varies from 50 to nearly 100 feet. The pebble beds in

the Midland Bunter must have extended over a large part of Staffordshire (interruptions being mainly due to faulting or denudation), and they have trespassed on the neighbouring counties. Their thickness sometimes come near, if it does not exceed, 300 feet, and very commonly is over 100 feet.

(2) How he explains the fact that the Triassic pebble beds present such a close resemblance to the *nagelfluh* of the Alps, and to the great gravel beds of later date which overspread the lowlands on both sides of that chain, while it is difficult to find a parallel for them among beds undoubtedly marine. I asked these questions in 1890, but failed to obtain an answer, so I am obliged to repeat them.

T. G. BONNEY.

THE SOUTHERN DRIFT.

SIR,—In this month's number of the Journal of the Anthropological Institute, p. 45, there is a statement of so misleading a character that I cannot pass it over without comment. In speaking of the Hill Gravels of Berkshire, Mr. O. A. Shrubsole says: "this extensive deposit is composed of the Southern Drift of Phillips and Prestwich." Anyone reading this would suppose, as he gives precedence to the late Prof. Phillips, to whom I have not referred in my paper on the Southern Drift, that I had failed to make acknowledgment to him as the discoverer thereof. Such an impression I am anxious at once to remove. I was at a loss to conceive what foundation Mr. Shrubsole had for this statement. On turning to Phillips' "Geology of Oxford and the Valley of the Thames, 1871," I find at p. 460 the following paragraphs. After speaking of the "operation of a great flood, a deluge, coming from the north, north-west, and north-east," bringing down spoil of the Midland Counties into the Valley of the Thames at Maidenhead and at Kensington, Prof. Phillips says: "Looking at the distribution of foreign drift in the country under review, we find evidence of abundant currents from the north, which brought plenty of gravels on the western side, but no Boulder-clay; and plenty of Boulder-clay with some gravels on the eastern side; while in the middle space there are traces of currents from the south transporting *flints and Sarsen-stones*." A diagram accompanies this description showing flints and *Sarsen-stones* from the Chalk hills, apparently of Kent, striking in between the north-west and north-east drifts. He then goes on to say how this might have been effected by the agency of ice. This is all that Professor Phillips says of the composition of this drift. He does not even mention the term "southern" in the text; but in the index attached to the word "drift" are the words "northern" and "southern," used, I presume, merely as antithesis. No definition of age nor superposition is attempted; and, be it observed, surrounded as the Thames basin is on all sides by Chalk ranges, flints and *Sarsen-stones* are of themselves no sufficient evidence of direction of their source.

Long before I had formulated my ideas respecting the Southern Drift, I often had the opportunity of discussing with my old friend