

Hill House, contain nearly all the testacea now living in our rivers, and none of those extinct in Britain, and no bones of mammals, proves them to be much newer than the neighbouring deposits containing older forms of life.

Again, the principal object of the essay seems to be to demonstrate the Post-glacial age of the valleys in the south-east of England, and especially that of the Thames. That demonstration has altogether eluded my grasp. An appeal to the author's elaborate maps, in the rooms of the Geological Society, supplies proof that is directly subversive of his theory, The whole question lies in a nutshell. Do you, or do you not, find Boulder-clay *in* the basins drained by the rivers of which he writes? Is it present in those of the Roding and Blackwater? A glance at Mr. Wood's map of the area drained by the former, shows that he recognizes that it is so found. In reference to the latter river I have to correct a mistake. Mr. Wood wrote to me for proof of its occurrence in the basin of Blackwater; and, unfortunately, without dreaming that my hurried note would be quoted in print, instead of referring to my note book, I ran my finger up an affluent of the Blackwater, instead of the main stream, and wrote Ingatestone and Mountnessing,—a mistake that Mr. Wood has italicised and noted with a mark of admiration. I ought to have written Witham Station. So far, indeed, as Mr. Wood's maps go, the Boulder-clay occupies any level, irrespective of inequality of surface, and therefore they prove that the hill and valley system "was sketched out" before the deposit of the overlying Boulder-clay. Of course, in many places, the Boulder-clay has been denuded by the present streams, and areas of London clay, of variable extent, have been exposed. If Mr. Wood restricts the term valley to the hollow in the immediate vicinity of a stream, and does not mean the area below a line drawn from one watershed to another, he is merely disputing about terms. If the excavation of the Thames Valley, using the term in the latter sense, took place in Post-glacial times, the deposits contained in it must also be Post-glacial, and the evidence of fossils characteristic of Pliocene mammals in France and Italy, is useless in classification. To say the least, no evidence has yet been adduced in support of this hypothesis, that is based merely on a belief that the entire valley-system of the South-East of England originated in centres of arc-like or curvilinear disturbance."

W. BOYD DAWKINS.

11TH NOVEMBER, 1867.

DR. A. VON KOENEN, ON THE BELGIAN TERTIARIES.

To the Editor of the GEOLOGICAL MAGAZINE.

SIR,—In the November number of the GEOLOGICAL MAGAZINE, M. von Koenen, in dissenting from my way of viewing the Belgian and East Anglian Kainozoic formations, represents me in a manner to which I may reasonably object. My paper having been published in the Journal of the Geological Society I should be sorry should its members be misled.

M. von Koenen starts with what is calculated to produce an erroneous impression. At page 504 he says, "Mr. G. A. has published a number of observations made at Antwerp *during his short stay.*" Of himself he says, by way of contrast, "I have visited Antwerp on five separate occasions, in three different years;" but had he read my paper with more attention than he has, he would have seen that I had been there repeatedly; and what is more to the point, that I had seen the sections at Edeghem in 1861 (p. 234), when the extent of open work was much more favourable for geological observations than in 1865.

He complains (p. 505) that I do not follow the divisions of any of the authors who have described those beds; if this means that I have not used such terms as Oligocene, Miocene (ГЕОЛ. МАГ., p. 507), it is true, but it was not from ignorance; rather from an old conviction that such a system of nomenclature was based in vague, mistaken, and theoretical views. He is incorrect, however, when he states that I have disregarded old names. I took, what I still consider to be, the natural division of the Belgic Kainozoic beds—that of M. Dumont and M. Nyst. The natural system in geology and palæontology is that which describes old sea-beds and their contents, according to the guidance which the naturalist and hydrographer have derived from the dredge and sounding lead; in place of this, the artificial systematists have endeavoured to set up what are merely convenient Museum arrangements.

Some of M. V. Koenen's sentences are contradictions rather than objections, p. 505, "Barton Clay does not correspond in age with Rupel Clay." Waiting for better evidence to the contrary, it seems to me that the approximation of the purely marine clays of Rupelmonde to those of Barton is closer than that which can be established between any two deep sea mud-beds of the English and Belgic Nummulitic formations. It may be, and must be, that a freshwater formation in one place is the equivalent of a purely deep-sea series in another, the Physical Geologist may some day arrive at their arrangements, but not so the Cabinet Conchologist. Another short phrase used by M. Von Koenen at p. 505, is also calculated to mislead: "The Tertiary beds of Cassel, Luithorst, Freden, and Diekholz, which he puts into the upper Kainozoic, are coeval with the Grafenberg and Sternberg Sandstones which he puts into the Tongrien." Put in this way, it certainly represents me as writing nonsense, but I wrote nothing of the kind. The reference given is to a note, in which I state "that the map of the Crag Sea has been drawn so as to include the Upper Kainozoic formation near Cassel, etc." There is an extension of sea-bed thus far into Hesse with the following fauna:—

Solen ensis, *Maetra triangula*, *Corbula nucleus*, *C. revoluta*, *C. cuspidata*, *Tellina distorta*, *Astarte incrassata*, *Cyprina Islandica*, *Venus plicata*? *Cardium papillosum*, *Isocardia cor.*, *Arca diluvii*, *A. noæ*, *Nucula sulcata*, *N. margaritacea*, *N. minuta*, *Calyptrea vulgaris*, *Bulla utricula*, *B. ovulata*, *B. lignaria*, *B. Lajonkairieana*, *B. acuminata*, *Eulima subulata*, *E. nitida*, *Natica castanea*, *Turritella communis*, *T.*

carinifera, *Siliquaria anguina*, *Cerithium vulgatum*, *Lima perversum*, *L. trilineatum*, *Buccinum macula*, *Pleurotoma rugulosum*, *Mitra eburnea*, *M. plicatula*, *Dentalium strangulatum*.

There is a sufficiency of Lusitanian features in this assemblage to make it referable to that older condition of the North Sea known as the Crag Period; instead of associating such a fauna with that of Sternberg and Grafenberg, one object of my paper was to show that there was no blending.

The mystification as to Cassel arises from the same cause as it did at the Bolderberg; there is an admixture of fossils, but it is purely accidental, owing to the lowest beds of one series (Kainozoic) having been superimposed upon the uppermost beds of another (Tongrien). This last has not been misunderstood by M. D'Orbigny (see Von Koenen, p. 505), in whose geological scheme it is the latest and uppermost marine assemblage of the great Nummulitic Period, and of its Germanic sea area.

M. D'Orbigny's only misconception consists in his placing his "Tongrien" as a "sous-étage" of the "Falunien." Into this he was misled by the German authors. It is an error which may be turned to good account by others, as showing how unsafe it is to methodise from a bag of fossils gathered from the remanic beds of one locality.

Yours truly,

ROBERT GODWIN-AUSTEN.

CHILWORTH MANOR, GUILDFORD,
November 19th, 1867.

MR. WHITAKER ON "SUBAËRIAL DENUDATION."

To the Editor of the GEOLOGICAL MAGAZINE.

DEAR SIR.—I most unwillingly request of you to allow me space to reply to some observations of my colleague, Mr. Whitaker, contained in his paper "On Subaërial Denudation," published in the number for October last; and calculated to convey a very erroneous impression of my views on this subject. Owing to a variety of circumstances, I had not read this paper, nor was I aware that any personal allusion to myself was contained therein, until a friend called my attention to the passage a few days since. In that passage I find myself represented (p. 453) as "*a strong believer in the sea, and nothing but the sea,*" as objecting to reasoning on logical principles, and the writer concludes with the following:—"One should not be surprised at the advocates of the marine formation of valleys and escarpments looking down on logic, and scorning syllogisms, . . . unless *they follow and overcome those prejudices* which contracted views of nature and magnified opinions of the experience of man may have begotten," etc. What may be the meaning of "following" and "overcoming" a prejudice, is a question which may well be left to those who alone are conversant with logical reasoning.

If my critic had only taken the trouble to refer to my paper in the GEOLOGICAL MAGAZINE (Vol. III, p. 474) on "The Denudation of the Valleys of Lancashire," and to another paper to which refer-