

in Norfolk. He detailed the several papers which he had read at the Meetings of the British Association at Nottingham, Brighton, and Bradford, in proof of the existence of a Forest-bed in Norfolk and Suffolk, which he called the Anglo-Belgian basin, of a succession of growths of forests, and of alternate elevations and depressions which have taken place in that region, and argued thence by analogy the extreme probability that such existed in the Carboniferous epoch. He relied upon the fact that such coal-deposits would be at a workable depth, in consequence of the elevation of the land at Hunstanton about 1700 feet above its original position at Yarmouth; and this upheaval of the chalk afforded a complete refutation of Prof. Hull's statement, founded on the supposed dip of the slaty rocks of the Harwich boring, namely, that an old Palæozoic land-surface extended from Harwich over all the Eastern Counties into Scandinavia, as represented in his Map, appended to the Report of the Coal Commission, and that this old high and dry surface was incapable of coal-growth. Mr. Gunn submitted that, if the southerly dip of the Harwich slaty rock extended in a northerly direction, it must have been reached at the Norwich boring, which was sunk considerably lower than that at Harwich, and did not pierce through the Gault. Mr. Gunn dwelt especially upon this as the most serious objection to the prospect of reaching coal at Hunstanton, or rather a Carboniferous bed, an opinion expressed so strongly by the Professor at the meeting at Brighton. Mr. Gunn also referred to the evidence of local subterranean movements in proof of the proximity of disturbances acting upon what he regarded as a thin envelope of Tertiary and Secondary deposits, probably not exceeding 1000 feet, and perhaps much less. He referred to the evidence of boulders, which he hoped to adduce on a future occasion.

2.—“On the Geology of Nottingham.” By the Rev. A. Irving, B.A., F.G.S. Part I.

CORRESPONDENCE.

MR. JAMES GEIKIE ON SCOTCH AND ENGLISH DRIFTS.

SIR,—Those who have read Mr. J. Geikie's papers in Vols. VIII. and IX. of the *GEOL. MAG.* will perceive that in his new work called “The Great Ice Age” he has changed his opinion concerning the correlation of Scotch and English Drifts. Instead of lumping together the three great drifts of the N.W. of England as varieties of “Till,” he now agrees with the opinion I published in the *GEOL. MAG.* for Sept. 1872 (Vol. IX.), namely, that “the eskers of Ireland and kames of Scotland were piled up during some part of the [N.W. of England] middle sand and gravel period,” and that the upper or brick clay is the representative of the Scotch shelly clay. This change of opinion has, I fancy, enabled Mr. Geikie to correlate more readily the drifts of this country with those of Sweden and America. Mr. Kinahan, in the last Number of the *GEOL. MAG.*, does not believe in the existence of a decided upper Boulder-clay in Ireland. In the

N.W. of England it is a strikingly *original* formation, as proved by the freshness of the numerous and generally parallel striæ on the included stones. It is, however, confined to low levels.

D. MACKINTOSH.

FURTHER REMARKS ON VULCANICITY.

SIR,—The statement of your footnote at p. 127 of my Reply to Mr. Scrope (*GEOL. MAG.* for March, 1874), in which you say that “Mr. Mallet in this and other passages certainly misapprehends Mr. Scrope,” does not seem to me justified by the facts, and I beg of you to permit me to explain why.

The objections made by me to the supposition of a thin crust and a great liquid nucleus are alike applied by me to the hypothesis of Hopkins, of a thin crust covering his subterranean liquid lava lakes. I have throughout my reply linked these together. Whether, therefore, Mr. Scrope now continues to adhere to the former notion, as I am warranted in taking from the tenor of almost all his writings,—or whether, as in the papers in the *GEOL. MAG.* Vols. V. and VI., to which you direct attention, he more recently is disposed to abandon the moribund thin crust and liquid nucleus theory, and to fall back upon the thin crust and fiery lakes of Hopkins (for the views in the papers above referred to seem to me neither more nor less than that),—I have not, as I believe,—certainly not consciously,—misapprehended Mr. Scrope. The difficulties of Hopkins’ notion, as adopted by Mr. Scrope, are just as great as those of the older one of thin crust and liquid nucleus, with this additional disadvantage,—that whereas the latter is an assumption that may stand alone, the former (Hopkins’ or Mr. Scrope’s) is an hypothesis as arbitrary, and which cannot be admitted at all without admitting another hypothesis previously, upon which it must depend.

10th March, 1874.

ROBERT MALLET.

ORIGIN OF THE FLEET.

SIR,—I can assure Col. Greenwood that I have read his valuable book more than once, and each time with considerable profit to myself; still I cannot agree with all his conclusions. My paper on the origin of the Lagoon called the Fleet is not as explicit as I would wish, as I intended to point out that the sea-banks are due to the principal currents, which are usually the incoming tidal currents. In some places, as off the S.E. coast of Ireland, the prevailing winds and incoming tidal currents act conjointly; but west of Carnsore point [the S.E. headland of Ireland] there are cross-tides, apparently due to the curl round the Salter Islands: and here we find, at the lagoon called Ballyteige mudlands, that the beach travels from Crossfarnoge Point towards the W.N.W., although the prevailing winds are from the S.W. and S.S.W.; while on the coast of Mayo, between Killiney Bay and Clew Bay [a coast apparently most favourable to the formation of bars and lagoons], the beach has no regular set, but goes sometimes north, at other times south, or inland or seaward, although there are on this coast nearly constant winds from between the south and west, generally a few points off S.W. From what I