gold, and subsequently the entry of mining companies into the field, a great deal of geological research has been carried out."

It seems strange that Dr. Pulfrey should have overlooked the pioneer work of Combe, whose report and map laid the foundations of the geology and petrology of the area ; for although the full report and map were not printed, they were lodged in the Kenya Secretariat (and subsequently in the Mines Office ?), Nairobi, so that all official geologists have had access to them, and condensed statements, by Combe, giving a fair amount of detail were published several years before 1931. The references are :—

(1) " Kavirondo, Kenya," *Ann. Rept. Geol. Surv. Uganda* for 1927, p. 15.

(2) " Kavirondo, Kenya," *ibid.*, for 1928, p. 9.

(3) " The Green-Grey Rocks of North Kavirondo," *ibid.*, for 1929, p. 18.

At the end of the second of the publications listed above the following will be found :—

" In a further search for mineral deposits, the belt of intruded rocks of the Karagwe-Ankolean System along the edge of the Maseno intrusion should be prospected in an easterly direction adjacent to the Edzawa River in Northern Maragoli and Nyangori."

This advice was taken and resulted, as is now widely known, in the discovery of the Kakamega goldfield.

With regard to works relevant in this connection published after 1931 but before the appearance of Dr. Pulfrey's paper under reference, but not therein referred to, the following should be noted :—

" The North Kavirondo Area," by A. D. Combe, *Mem. II Geol. Surv. Uganda* (" The Geology of South-West Ankole . . ."), Appendix II (" The Distribution of the Rocks of the Karagwe-Ankolean System "), 1932, p. 218.

" A Contribution to the Study of the Geology of Kavirondo," by K. A. Davies, *Bull. No.* 2, *Geol. Surv. Uganda*, 1935, p. 30.

E. J. WAYLAND.

GEOLOGICAL SURVEY OFFICE,
ENTEBBE, UGANDA.
27th March, 1936.

THE BASAL COMPLEX IN JAMAICA.—A REPLY.

SIR,—I have read with much interest Dr. C. T. Trechmann's paper in the June number (pp. 251–267) on the Basal Complex in Jamaica. As he takes a quite different view of the age of the Jamaica schists and granodiorite from that expressed in my 1929 paper (*Q.J.G.S.*, lxxxv, 440–492) I wish to say at once that I am not convinced by the evidence, particularly as to the age of the grano-

diorite, which he asserts to be intrusive into the Tertiary " White Limestone " and to be not earlier than Oligocene in age. The acceptance of his interpretations of the exposures that he describes would still leave us with a body of other evidence which cannot be reconciled with his conclusions. Unfortunately, I am much handicapped in discussing his paper, because the described exposures are some 4,000 miles away and personal examination of them cannot be made.

It is a pity that the author has not fortified his evidence by a petrological description of some of the more important rocks he mentions, but in his paper he has generously offered to allow an investigation of his rock-specimens by anyone interested, an offer of which I intend to avail myself, and, as I am at present preparing a detailed account of the geology of the Kingston District for the Geological Society of London, I hope to include a discussion of his views in it after I have studied his specimens.

Meanwhile, I will comment on his interpretation of two of the sections described in his paper. On page 259 he describes the junction of the granodiorite with the Purple Conglomerate in the River Wagwater as an intrusion, whereas I have mapped it as a high-angled thrust traceable for some miles. As the Purple Conglomerate itself contains, in places, an abundance of granodiorite pebbles identical in character with the plutonic rock—I can supply Dr. Trechmann with precise localities where they can be found—it is very unlikely that the granodiorite is intrusive into this formation. In his account the author states that the conglomerate is " crushed to a green and purple mass for several feet away from the granite " and that a little farther away it is normal and uncrushed. This local crushing at the junction supports the view that my mapping of the boundary as a thrust is correct, and, in the absence of any photograph or sketch of the exposure, I suggest to the author that he might consider whether the granitic material in the crushed conglomerate at the junction might not have been torn out of the granodiorite along the zone of thrusting.

Another piece of evidence adduced by the author is the supposed thermal alteration of the White Limestone in Green Bay below the Lazaretto (p. 261). It is unfortunate that he failed to find the well foliated green hornblende-schists and amphibolites which occur on the road at a higher level than the Lazaretto itself. These schists lie immediately under the limestone in the core of an anticline or dome, and microscopic examination by Mr. Higham (page 486 of our paper) showed that they bear traces of an older foliation which is crossed by newer, more dominant planes. They also furnish evidence of movement at a still later period when the rock was fractured and the thread-like fissures filled with quartz. These schists are similar to the green Westphalia Schists of the Complex, about 18 miles away, where they are associated with other altered rocks, including marbles.

But although the author did not see these schists he discovered the very interesting section in the cliff below the Lazaretto shown in his text-fig. 3. The succession there is read by him as (4) normal White Limestone underlain by (3) a few inches of friable broken-up yellowish dolomitized limestone, which in its turn overlies (2) a grey and streaky or white marble 1–2 feet thick. This marble passes down into (1) a " hornfelsed and darkened limestone, veined and fractured ". The author considers that all these beds belong to the White Limestone formation, and that the lower beds have been thermally metamorphosed by " probably some intrusion not far away ". On the other hand, my own interpretation of his text-figure is that the base of the White Limestone should be drawn below the few inches of dolomitized limestone, and that the marble and hornfelsed beds below it are members of the Basal Complex on which the limestone rests unconformably, as it does on the hornblende-schists near by.

How otherwise is the presence of the hornblende-schists to be explained ? The White Limestone in this area forms a gentle dome with dips of 25° to 30°, and has never been subjected to severe earth-movement, yet in the core of the dome immediately underlying the normal limestone we find thoroughly foliated schists which give evidence of two periods of dynamo-metamorphism and a much longer geological history.

I consider that the author has failed to prove his case for the Tertiary age of the granodiorite, and, in my opinion, can never hope to do so. He himself notes (p. 263) that the White Limestone can be found within a few inches of this great plutonic mass with no more change than a little dolomitization, although there is a wide zone of hornfelsing not far away. He offers the explanation (without giving any proof) that the hornfelsed rocks have fallen into or become pendent in the granodiorite, while the unaltered limestone is " at the outer contact " ! My own view is of course that the plutonic rock was already unroofed when the White Limestone was deposited and that both it and the hornfelsed beds are of much earlier age and are part of the Basal Complex.

C. A. Matley.

10 Milverton Terrace,
Leamington Spa.
5th June, 1936.

NOMENCLATURE OF CONGLOMERATES.

Sir,—In a study entitled " An attempt at the Correlation of the Ancient Schistose Formations of Peninsular India ", the first part of which is now in the press (*Memoirs Geological Survey of India*, lxx), I have had occasion to discuss the nomenclature of conglomerates, and as this may prove of interest to the readers of your