THE IDEA OF CONTRASTED DIFFERENTIATION.

SIR,—The *Reply* by Dr. Nockolds (3) to my paper (2) shows that confusion still remains on the essential point at issue—the validity of his idea of contrasted differentiation. The attempt to weaken the force of my adverse criticism avoids the issue and does not carry us any further. Nockolds originally wrote (1, p. 34): " There is clear evidence that differentiation in intercrustal magma basins yields two contrasted magma types, acid and basic." Having no knowledge of this " clear evidence ", I asked what it was (2, p. 235), but none is given in the *Reply*. The observed association of contrasted magma-types is knowledge we share; it is no more evidence of contrasted differentiation than the observed association of toys and stockings on Christmas morning is evidence of the reality of Santa Claus.

Nockolds writes (3, p. 530): " If, therefore, the acid and basic portions in, say, a composite minor intrusion come from a common source, I see nothing wrong in saying that they have differentiated from that source." Granting the *if*, there is, of course, nothing wrong. But that all important *if* cannot be granted until there is positive evidence that the acid and basic portions did come from a common source. Theoretically, it is just as possible that they did not come from a common source, or that they did so only in part. Of the very large number of theoretical possibilities which can be imagined, the three schematically outlined below are sufficiently representative for the present discussion. Not merely one, but all of these must be tested by the evidence available.

Pre-existing Acid Rock
↑
I.—MAGMA → Heat
I.—MAGMA → Basic Magma or Rock

{ Acid Magma generated by partial or complete *Fusion*.

Pre-existing Quartzite
↑
II.—MAGMA → Alk-aluminous Emanations
II.—MAGMA → Basic Magma or Rock

{ Acid Rock or Magma generated by *Transfusion*.

III.—MAGMA → Residual Acid Magma
III.—MAGMA → Basic Rock

{ Acid Magma generated by *Crystallization Differentiation* with separation of liquid and solid phases.

I.—A process which is difficult to distinguish from II, of which it is a limiting case. See C. Sorochinsky (4).

II.—A particular instance of a kind of syntexis in which the amount of energy and the compositions of both the emanations and the pre-existing rocks affected by them may vary widely. For examples of evidence see Doris L. Reynolds (5) and A. Holmes (6) and the papers referred to therein. The position is far from being as hopeless as Nockolds makes out, when he writes (3, p. 535) of "a process of gaseous distillation in which rocks are acted upon by unspecified emanations (in an unspecified manner) coming from an unknown source at unknown depths". When manifest effects cannot be explained by prevailing conceptions, what can be more scientific than to attempt to discover the unrecognized factors? Many of us are engaged in research directed to this end; we seek to determine the nature of the emanations responsible for observed effects in particular cases. To trace their immediate source is not usually difficult, but it would be an unwarranted assumption to pretend that the immediate source is the ultimate source. To the physicist, cosmic rays present a similar incentive to research: these rays, which are perplexing realities, derive their energy from some unknown ultimate source in the depths of space. Emanations, equally real, derive their energy from some unknown ultimate source in the depths of the earth.

III.—A process of crystallization differentiation followed by separation, which is advocated by Nockolds to account for the association of acid and basic rocks. I do not deny that there is a contrast in composition between the acid material in, say, a quartz-dolerite or tholeiite and the crystal-mesh through which it is distributed. What I pointed out was the mechanical impossibility of bringing about separation except by the application of powerful stress. I have no objection to the idea of contrasted crystallization differentiation where the rocks themselves afford evidence of the operation of stress. See N. L. Bowen and J. F. Schairer (7, p. 395). Nor do I object to the idea of contrasted differentiation where there is evidence of separation by some process of, or akin to, gaseous transfer—though, as indicated above, this is the basis of a different hypothesis. Whether the interstitial material is 5 per cent or 15 per cent makes no essential difference to the mechanical difficulties. It is easy to pour 15 per cent of water into sand, but it is impossible to get it out again except by evaporation or the application of stresses equivalent in their effects to those of dynamic metamorphism.

Nockolds writes (3, p. 533): "If the final residuum of augite-andesites can ooze into vesicles when it only forms a very small proportion of the mass, there seems to be no reason why the acid residuum we are considering cannot also move quite readily." Again, the argument depends on the all important *if*. How can one know that the residuum oozed into "vesicles"? Actually there are all gradations between xenoliths, transfused xenoliths, ocelli, and "vesicles" occupied by felsic material. See J. S. Flett (8) and R. Campbell, T. C. Day, and A. G. Stenhouse (9). What evidence there is points to dispersion *from* the felsic spots rather than to concentration *into* the felsic spots.

Nockolds' suggestion that my attitude is "strangely inconsistent" (3, p. 532), is a further symptom of the persistent confusion between evidence and interpretation. Any interpretation, hypothesis, idea, postulate, or supposition may, and should, be dismissed or modified when it is found to be at variance with evidence. Evidence of one kind, if it be genuine, cannot be at variance with evidence of another kind. Arising out of this point, I should like to say that I took conscious pains to write my paper (2) with particular care, having in mind that those who live in glass houses should not throw stones. I must therefore insist that statements which may be true of *crystallization differentiation* are not necessarily true of *differentiation*; that statements which may be true of *ultimate source* are not necessarily true of *immediate source*; and that statements which may be true of *interpretation* are not necessarily true of *evidence*.

The essential point of my paper was to demonstrate that the idea of contrasted crystallization differentiation, as hitherto developed, has not been shown to be valid. That idea, therefore, should not be used as if it were an established principle of petrology. Many petrological papers are marred by the failure to distinguish between interpretation and evidence, and if this discussion directs attention to the necessity of avoiding what has become a lamentably widespread departure from scientific method it should go far towards closing our ranks against future controversy.

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1.—S. R. NOCKOLDS. "The Production of Normal Rock Types by Contamination and their Bearing on Petrogenesis," GEOL. MAG., LXXI, 1934, 31–9.
2.—A. HOLMES. "The Idea of Contrasted Differentiation," GEOL. MAG., LXXIII, 1936, 228–238.
3.—S. R. NOCKOLDS. "The Idea of Contrasted Differentiation: A Reply," GEOL. MAG., LXXIII, 1936, 529–535.
4.—C. SOROCHINSKY. "Étude pétrographique de l'édifice volcanique du Kahusi et du Biega (Kivu). (Mission géol. Com. Nat. de Kivu)," *Mém. Inst. géol. Univ. Louvain*, ix (vi), 1934, 98 pp.
5.—DORIS L. REYNOLDS. "Demonstrations in petrogenesis from Kiloran Bay, Colonsay. I: The transfusion of quartzite," *Min. Mag.*, xxiv, 1936, 367–407.
6.—A. HOLMES. "Transfusion of quartz xenoliths in alkali basic and ultrabasic lavas, South-West Uganda," *Min. Mag.*, xxiv, 1936, 408–421.
7.—N. L. BOWEN and J. F. SCHAIRER. "The Problem of the Intrusion of Dunite in the Light of the Olivine Diagram," *Rep. XVI Internat. Geol. Cong. Washington, 1933*, 1936, 391–396.
8.—J. S. FLETT *in* "The Geology of Caithness," *Mem. Geol. Surv. Scotland*, 1913, 113.
9.—R. CAMPBELL, T. C. DAY, and A. G. STENHOUSE. "The Braefoot Outer Sill, Fife. Part I: The Braefoot Promontory," *Trans. Edin. Geol. Soc.*, xii, 1931, 360.