

one of the lines in the Shannon Basin, making six in all, have been deflected, five of them considerably and one through an angle of 45 degrees, so as profoundly to modify the interpretation. The two "rather generalized" have been generalized to such effect that the arrow which should pass down the *east* side of the Slieve Bloom Mountains is carried down the *west*. The figure on p. 190 shows Wright's arrows by the full lines and Professor Gregory's version by broken lines. The superposition was done by photographic projections from a lantern slide in use here.

These may seem small matters, but it would strike at the very root of our confidence in scientific statement of fact if an author were permitted without protest to take any liberties he might choose with the work of another author and describe the result as "after W. B. Wright" or "added from the map of Mr. Wright".

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THE NOMENCLATURE OF PETROLOGY.

SIR,—Dr. Arthur Holmes, in his useful little book with the above title, disapproves of the term *syenoid*, which I have used as a contraction of feldspathoid-syenite, giving as his reason that the suffix *-oid* has been used in other senses, as in granitoid, trachytoid, pegmatoid (Evans), and dacitoid (Lacroix). The matter is a very trivial one, but in the present involved condition of petrographic nomenclature no proposal for simplification should be dismissed without fair consideration, and the very examples that Dr. Holmes quotes show that he has not considered the matter fully. Of the four terms that he quotes, the last two are of later introduction than mine, so that I might at least claim the right of priority. This is true, too, of Lacroix's revival of "basanitoid"; and I cannot recall any other instance of a rock name in current use that ends in *-oid*. Then granitoid and trachytoid are adjectives, and should correctly be written granitoidal and trachytoidal, just like conchoidal and saccharoidal. But a more important consideration is just that *every* familiar suffix is used in various senses, and the ubiquitous *-ite*, for example, serves for rocks, minerals, fossils, meteorites, alloys, chemicals, official drugs, patent medicines, and a great variety of commercial products. I think, then, that Dr. Holmes' objection is not a very well-reasoned one.

The reason why it seems desirable to have a single word in place of the double-barrelled "feldspathoid-syenite" is pretty obvious. In the first place these rocks are quite as distinct from the syenites as are—let us say—the monzonites, and they have therefore an equal claim to a distinctive name. In the second place, nepheline-syenite is long enough already, and when further mineralogical qualifications have to be added the name becomes unwieldy. A dreadful example of this is Dr. H. A. Brouwer's "ægirienamphiboolbiotietnepheliensyenietporphyr".

If another name is called for, as, I think, it is, the form *syenoid* (derived from *felspathoid syenite*, but it can also be interpreted literally as “*syenite-like*”) has an advantage over an entirely new name, inasmuch as it preserves the connexion with *syenite* and so imposes no fresh burden on the memory. Also it is only one of a whole series of terms which are all constructed on the same principle; and finally it is brief and euphonious.

I grant that the matter is a trivial one, yet I am convinced that in the judicious use of prefixes and suffixes we shall find the best solution of the difficulties of nomenclature. If this is “*perversity*”, then I am guilty and unrepentant.

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“THE RELATIVE AGE OF CONCRETIONS.”

SIR,—There is one paragraph in the interesting and suggestive paper by Mr. W. A. Richardson (in the *MARCH GEOLOGICAL MAGAZINE*) over which I feel constrained to join issue with the writer. In the course of arguments in favour of subsequent formation of the concretions surrounded by “*conformable*” lines of stratification, and against the hypothesis that “*consolidation pressure*” produces such effects, he quite rightly assumes that the latter would require the occurrence of similar “*conformity*” around fossils and other contemporaneous objects offering special resistance. And then he states (p. 118) “*But it certainly is not*”. My experience (and surely that of every worker in the Chalk) would compel me to emend that sentence by omission of its last word. It is hard to find a specimen of *Micraster* or *Echinocorys* in the nodular parts of the *planus*-zone that is not considerably damaged by “*slickensiding*”, precisely similar to that affecting the nodules themselves. While fully prepared to believe that these particular nodules are truly “*subsequent*” (although the fossils cannot be so), I cannot accept this particular argument. It is always unsafe to be “*certain*” about a negative.

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