College and Columbia University. The editorial board is announced to be the same as last year, including Professors Cassius Jackson Keyser and David Eugene Smith, of Columbia University; Professor Raymond Clare Archibald, of Brown University; Professor Louis Charles Karpinski, of the University of Michigan; Professor Lao Genevra Simons, of Hunter College, and Professor Gino Loria, of the University of Genoa. "Notes and Queries" will again be edited by Professor Archibald. It is also expected to print hitherto unpublished manuscripts now in university and private libraries in this country.

DISCUSSION

AGE OF FITCHBURG GRANITE

WHEN the Tufts group visited the quarries at Fitchburg last spring they were particularly asked to keep an eye out for radio-active minerals which one can recognize by the cracks which radiate from them. A number of specimens were obtained, but they were mostly allanite.

One piece, however, the spinthariscope showed to be much more radio-active and proved to be uraninite. It was only about four millimeters in diameter, but I was able to dig out about 30 milligrams and send to Dr. Friedrich Hecht, 38 Währingerstrasse, Vienna, Austria, who was able to make on 18 milligrams by micro-chemical methods the accompanying analysis. I am sending this to SCIENCE for a number of reasons.

MICROANALYSIS OF FITCHBURG URANINITE

Used for total analysis : 18 mg.
Used for determination of H_2O: 6 mg.

	Per cent.
Insoluble residue	27.39
(of which SiO_2)	(25.48)
PbO	2.72
(Pb)	(2.52)
$Fe_2O_3 + Al_2O_3$	not determined
Rare earths	1.85
ThO_2	3.86
(Th)	(3.39)
U_3O_8	59.19
(U)	(50.20)
CaO	0.84
MgO	0.15
H_2O (−110°)	0.27
H_2O (110–300°)	2.90
Loss on ignition	....
Alkalies	present, but not determined

$Pb/U + 0.25.Th = 0.049_4$

Analysts: F. Hecht and Edith Kroupa.

There is so little Th that using the factor 0.36 instead of 0.25 will make no appreciable difference. Allowing for AcD and ThD we would get an age:

$15{,}600 \log(1 + (RaG = 2.37)/(U = 50)) = 366$ million years

In the first place, I should like to call attention to the fact that with micro-chemical methods a geologically useful analysis can be made on such small quantities, and I think Dr. Hecht would be glad to arrange for such analyses by coworkers for others, and by no means solely on radioactive minerals. Methods have been developed for a variety of compounds.

In the second place the analysis indicates that this granite is much older than I had expected (360 million years) and akin to that at Branchville, Conn., from which a uraninite has been analyzed by Hillebrand (N.R.C. Bulletin 80, p. 341). This, however, agrees with what Professor Berkey had thought and shows what important help may be given by such research.

Finally, as one swallow does not make a summer it would be desirable to have analyses made of other such minerals, and I would like to call attention to the possibility of so doing even from such small quantities. It would be very interesting to have analyses from New Hampshire granites, and such minerals should be found in the quarries north of Keene.

ALFRED C. LANE,
Chairman, N.R.C. Committee on the Measurement of Geological Time

"EXPANSION AND CONTRACTION" OF CHROMATOPHORES

SUMNER,[1] in a recent article in this journal, contends that a large majority of investigators interested in changes in color in vertebrates hold that the movement of pigment granules in the chromatophores is not due to "expansion or contraction" of the chromatophores, but that they continue to use this expression in spite of the fact that its meaning is not at all in accord with their view. To remedy this incongruous situation, he proposes to substitute "chromatosome" (pigment body) for "chromatophore." He says: "My suggestion is that we continue to employ the terms 'expansion' and 'contraction,' since something obviously does expand and contract, but that we credit these movements to the things that actually do expand and contract, namely, the pigment-masses within the cells."

It is well known that the colored substance in the chromatophores in the vertebrates is in the form of numerous discrete granules and that these granules move on definite paths through the cytoplasm out into the various branches of the chromatophores under certain conditions, and on the same paths back into

[1] F. B. Sumner, "Why Do We Persist in Talking about the 'Expansion' and 'Contraction' of Chromatophores?" SCIENCE, 78: 283–4, 1933.

the central part under other conditions. The motor mechanism involved in this movement is doubtless located in the colorless cytoplasm through which the granules move. It certainly is not in the colored substance itself. In other words, while it is evident that the pigment masses (chromatosomes) change enormously in form, there is no evidence indicating that they *per se* change in size, *i.e.*, expand and contract, and that the change is due to processes within them. It seems to me therefore that the phrase "expansion and contraction of these masses" (chromatosomes) describes the phenomena in question but little, if any, more accurately than the phrase "expansion and contraction of chromatophores."

Under the conditions which induce movement of the pigment granules out into the branches of the chromatophores they become distributed through a relatively large space, and under those which induce movement in the opposite direction they become concentrated in a relatively small space, and their function is obviously specifically associated with the space they occupy, *i.e.*, the extent of this distribution. I would therefore suggest the phrase "distribution and aggregation of pigment granules" (melanin, xanthine, *et al.*) in place of "expansion and contraction of chromatophores" or "chromatosomes." In a paper[2] and in lectures on the subject, I have used the verbs "spread out" and "aggregate" to designate respectively movement out into the branches and in the opposite direction. These phrases express precisely what occurs, and until more is known about the mechanics of the processes involved, I see no need for any others.

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SLICKENSIDES

IN the fireclays and other underclays beneath the beds of coal in the Pennsylvanian system of the United States, the countless millions of slickensides command the interest of the geologist, who studies them, and provoke the question as to their origin. Many of them, perhaps most of them, are but a fraction of an inch in length and in no wise involve the overlying or underlying beds. Neither does any one of them cut the whole of the clay bed in which they exist. They must have been formed by the material on one side of each of these minute fractures slipping past the material on the opposite side. But even so, the motive force is not evident; neither is the manner in which it operated, because these small fracture surfaces extend in all directions and dip at any angle, even in the same mass of clay.

Where the slickenside surface involves the whole of a bed or cuts across two or more beds no such problem exists. The slickenside surface was produced by the pressure of one side of the fractured surface against the other side as the two parts moved away from each other along the faulted surface. Such slickenside surfaces are most easily produced in plastic clays and clay shales and most difficultly formed in pure quartz sandstones, as in Wisconsin, where the friction had to be sufficiently great to fuse the quartz grains into a natural enamel, but enamel which still preserved its striae.

Because of the problem involved in the origin of these slickensides in the fireclays and underclays, it was with keen interest that, in 1929, slickensides in the making were observed in the delta of the Mississippi River off the mouth of South Pass. Mud lumps are numerous here. They are shoved up to the surface and a few feet above it rather suddenly, instantly to be attacked by the waves and worn down to gulf level. One such mud lump, tied to west jetty-end by a sand spit or sand bar, was being undercut by the waves on the gulf side. As the waves cut back, the unsupported overlying mass of plastic clay slumped irregularly back into the gulf, producing the most beautiful slickensides imaginable. Of course these slickenside surfaces, at the moment of their development, dipped steeply downward, but the undercutting of a partly slumped mass could produce a new set of slickenside surfaces intersecting the earlier set at any angle whatsoever, just as these surfaces do in the fireclays and underclays.

The process of development of these slickensides in the mud lumps of the Gulf of Mexico suggests that at least some of the slickensides in the fireclays and underclays could have been formed by the waves in the Pennsylvanian swamps attacking the material of the soil in which the coal vegetation grew. The process could not produce slickensides in those fireclays and underclays in which the roots and root impressions of the coal vegetation still extend downward into the clay, but it would be interesting and instructive to observe whether or not clays penetrated by plant roots have slickenside surfaces intersecting at all angles; whether or not they have slickensides of steeply inclined surfaces only; or whether or not the clays penetrated by roots have fewer slickenside surfaces than have the clays free from such roots and root impressions.

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[2] S. O. Mast, "Changes in Shade, Color and Pattern in Fishes, and Their Bearing on the Problems of Adaptation and Behavior, with Especial Reference to the Flounders Paralichthys and Ancylopsetta," *Bulletin of the Bureau of Fisheries*, 34: 177–238, 1914. Albert Kuntz, "The Histological Basis of Adaptive Shades and Colors in the Flounder *Paralichthys albiguttus*," *Bulletin of the Bureau of Fisheries*, 35: 1–28, 1915.