

that Fouqué & Lévy do not directly challenge them. It will, however, be noticed that the papers of Dr. Sorby and Professor Hartley, published as they are in the records of Societies such as the Royal, Chemical, Microscopical, and Mineralogical, and in the Reports of the British Association (and published, moreover, quite independently, and for different objects of research), might very well not have all come under the notice of MM. Daubrée, Fouqué, and Lévy; and that indeed seems to have been the case.

Speaking for myself, I certainly should not have seen most of the above papers had not the authors most generously sent me reprints.

I am certainly in a great difficulty. One of the minerals relied on by the Geological Survey to prove pneumatolytic action at temperatures above the critical temperature of water is topaz. But, in Professor Hartley's paper on Fluid Cavities to the Chemical Society in 1877, one section is entitled "On the Probable Temperature incident to the formation of Topaz," and one conclusion arrived at is that topaz sometimes crystallises under and sometimes over the C.T. of water.

The petrologists dismiss all the evidence relied on by the chemists for ascertaining the temperatures of rock-formation. But there is this fact to be borne in mind, that while the chemists have minutely studied separate minerals, the petrologists have taken a wider view of rocks and magmas.

The following example will serve to show how widely eminent petrologists and chemists differ as to probable temperatures. Professor Hartley, in discussing the formation of negative cavities in quartz, observes:—"The mineral is crystallised at a high temperature, say 150° C." (on Fluid Cavities).¹ The theory adopted by the Geological Surveyors often necessitates a temperature exceeding 365° C.

Since the publication of the Cornish Memoirs I have for the first time understood the irritation that my unfortunate little papers have naturally caused. St. Paul hits the position off exactly: "If I know not the meaning of the voice, I shall be unto him that speaketh a barbarian; and he that speaketh shall be a barbarian unto me."

I can assure my geological friends that for very many reasons I most deeply regret ever having published outside the provinces anything on the subjects of either Petrology or Ripplemark; as both subjects have led to a vast amount of genuine misunderstanding and discomfort, and I may add of mystification; and they are not worth it.

A. R. HUNT.

ORIGIN OF THE SUDBURY NICKEL ORES.

SIR,—In Professor Coleman's interesting restatement of what he regards as "incontrovertible proof" of the igneous origin of the Sudbury nickel ores, he makes the safe assumption that I had not seen the long announced second part of his monograph (Report of the Bureau of Mines, Ontario, vol. xiv, No. 3). It would be inexcusable for anyone to discuss the Sudbury mining field without

¹ Reprint, p. 8.

careful consideration of any work that Professor Coleman had published on it. I am not aware that his second report was available when I prepared my address. Its issue was reported in *Economic Geology* for June, 1906; but as the paper is not included in the Annual List of Literature received by the Geological Society for either 1905 or 1906, I presume *Economic Geology* was supplied with an advanced copy. This view is supported by the fact that Professor Coleman, in a paper in the *Journal of Geology* for last month, published six months after the manuscript of my address had to be in the hands of the printer, refers to his report as "recently distributed." I notice, moreover, that there is no reference to it in Messrs. Campbell & Knight's paper on the Microstructure of the Nickeliferous Pyrrhotites, which was received in this country after my address had been delivered. As the report was apparently inaccessible to American authors, it is not surprising that it was not available on this side of the Atlantic.¹

My opinion as to the origin of the Sudbury ores is not so emphatic as Professor Coleman's article would suggest. The opinion which he quotes was introduced by the words "appear to have been," and the next sentence continues the same expression of doubt—"if Dickson's facts be right," etc. Without having been in the field, I should be sorry to express a final opinion on either side. But so far as I am capable of judging from the literature, the igneous origin of the ores is not yet established, and is faced by greater difficulties than the alternative theory. Messrs. Campbell & Knight, in their recent paper in *Economic Geology* (June, 1907), also conclude that "Dickson has a weight of evidence to prove that his specimens are of secondary aqueous origin" (p. 351). They claim that (p. 365) in all the chief mining fields of nickeliferous pyrrhotite the mode of origin of the ores was the same, and that the basic rocks with which the ore bodies are associated were first formed, then fractured, and then "ore-bearing solutions came in and replaced the rock-matter wholly or in part by pyrrhotite. Later on the pyrrhotite, etc., was also strained and broken, and the deposition of pentlandite and chalcopyrite followed." Hence I am not the only one who is not yet convinced that the igneous origin of these ores is "the correct view." J. W. GREGORY.

GEOLOGICAL DEPARTMENT,
UNIVERSITY, GLASGOW.
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KITCHEN-MIDDENS IN NORTH CORNWALL.

SIR,—In Mr. B. B. Woodward's interesting paper in the *GEOLOGICAL MAGAZINE* (January and February, 1908) on "The Drift and Underlying Deposits at Newquay," he mentions kitchen-middens and cooking-sites as occurring towards the upper parts of the deposit of sand (Fig. 1, p. 15, January). It may be interesting to note the similar occurrence in the Trevoise district further to the north-east of many such kitchen-middens and cooking-sites. In October, 1901

¹ I am informed (Feb. 17th) that Professor Coleman's report has not even yet been received at the Geological Society's Library.