

pointing out to Mr. Ward, when first he used the term *felsidolerite* (a term of barbarous etymology and self-contradictory), that the more acid lavas of the Lake District agreed with some porphyrites, and only differed from andesites by slight mineral changes.

Again, the rhombic pyroxene which I have described in the Eycott Hill rock (which I leave among the *basalts*) differs in some respects from that described by Messrs. Cross, Iddings, and Teall, and (as may be seen from my paper) is more nearly related to the mineral which occurs in certain peridotites and serpentines. As I went in 1876 to examine a rock containing it, I presume it was pretty well known some years previously. It was, however, very natural that Mr. Ward should overlook this mineral—indeed, a characteristic specimen may not have occurred in the slide or slides which he examined. This, however, seems so obvious a truism that the only motive which I can understand in Mr. Rutley's letter is to hint obliquely that I have not done Mr. Ward full justice. This I maintain is not warranted by anything in my paper. No one can regard the memory of Mr. Clifton Ward more highly than I do, for I continue to regret him as a near and dear friend, no less than I esteemed him as a geologist. But I did not and do not consider that I was bound to preface my paper by some apologetic remarks for venturing to correct slightly and add a little to what he had written on the subject. If we, whose lives are spared, are not to endeavour in our humble way to advance knowledge, for what are we living?

T. G. BONNEY.

SUBTERRANEAN CONTOURING ON GEOLOGICAL MAPS.

SIR,—In the May Number of the GEOLOGICAL MAGAZINE, received yesterday, a Correspondent of yours asks me: "How the position for contours [of Rock Beds] may be accurately ascertained at depths far removed from observation, amongst highly contorted or disturbed strata?" Really I know of no method but digging; yet it sometimes happens even in such extreme cases that an opinion of more or less value can be formed by means of a careful instrumental survey.

What I maintain is that contour lines enable any such opinion to be expressed clearly and precisely, of course the mode of expression does not by its clearness and precision increase the certainty or truthfulness of what is expressed. Nor are opinions necessarily valueless because not certain beyond a doubt; nor are all opinions to be called mere "fancies." If geological maps could literally only give what has actually been observed on the surface, they would generally be barren indeed and leave almost as much to the intelligence of the reader as the unexplored ground does. The aim of geological surveys is to ascertain the probabilities in regard to what is hidden; and in easy cases and thorough surveys the probabilities perceived by the geologist are sometimes practically as good as certainties; in many other cases the indication with precision of the probable, not "merely possible, position" of a rock bed under cover

is of the highest importance. Clearly the degree of probability ought not to be exaggerated. But precision is in itself no such exaggeration.

The very necessity of stating an opinion precisely and definitely, if at all, and of making it correspond throughout with all the surface facts so far as they have been observed, is a great incentive to careful thoroughness; and the work specially required for underground contours, both in the field and in the office, gives much more than ordinary value to a geological survey.

He asks further: "Whether I would advise the use of distinct plans on which to record the positions of the contours at the various depths, when ascertained." Of course the various depths can have each but one contour line, and naturally I would not recommend a separate map for every contour line; but perhaps some misprint or other slip has concealed the drift of the question.

NORTHAMPTON, MASS., 20 May, 1885.

• BENJ. SMITH LYMAN.

THE CLASSIFICATION OF THE JURASSIC SYSTEM.

SIR,—I had not intended to trouble you with any further remarks on this subject, but as part of Dr. Blanford's last letter has been repeated in the June Number of the *MAGAZINE* for the sake of correcting a typographical error, I may be allowed to answer the paragraph so reprinted.

I think Dr. Blanford fails to apprehend the object of my reference to the Lower Calcareous Grit. It was this,—he proposes to place the Coral Rag in the upper division and the Oxford Clay in the middle division of the Jurassic system; I reply that the lower member of the Coral Rag is so closely connected by its fossils with the Oxford Clay, that it would be unphilosophical to draw such an important line of separation below it.

I do not see what the Callovian has to do with this argument, but I had certainly not forgotten its existence, for I happened to mention it in my first letter (*GEOL. MAG.* 1884, p. 525) as forming the base of the Oxfordian.

My argument is simply this, that there is a greater palæontological change in passing from the Cornbrash to the Oxfordian than there is between the Oxfordian and the Coral Rag. The question of the lithological change is not worth further discussion; I quite admit that the point should be decided on palæontological grounds, but I do not agree with Dr. Blanford's method of handling the facts.

June 6th, 1885.

A. J. JUKES-BROWNE.

THE DARWIN MEMORIAL STATUE, the execution of which, in white marble, had been entrusted to Mr. Boehm, was unveiled by H. R. H. the Prince of Wales at the British Museum (Natural History), Cromwell Road, on Tuesday, 9th June, 1885, when Prof. Huxley, President of the Royal Society, delivered an address on behalf of the Darwin Memorial Committee, and handed over the care of the statue to the Trustees. The Prince of Wales replied on their behalf. The Archbishop of Canterbury and many other of the Trustees were present, and a large assemblage of scientific men and friends and admirers of Darwin filled the Great Hall.