

ing, but the rest stuck to it. We believe that African bees would follow a person almost any distance, but it seems hardly probable that that kind of bee found in the northern part of Africa would be likely to store such large quantities of honey, and can only account for it in this way: as they swarmed, the different swarms clustered in the under side of the cliff, and although they were separate colonies, their combs might be built adjoining each other. Doubtless, many of us still remember Mr. Benton's *Apis dorsata*, when he was getting specimens of them from *Apis Dorsata* rock in Ceylon, where he found a large number of colonies all clustered on the under side of a cliff. Although their combs were very large, and some of them close together, yet there were about 14 colonies. In a country where the bees have to get on the under side of cliffs in the absence of any other place, it is not unreasonable to suppose that a large number of colonies might be clustered together. A friend, writing us from India, stated that he saw a large number of colonies so clustered, and he termed it a small village of bee colonies attached to the under side of a cliff. We should be glad indeed if Mr. Wood would give us any further information in regard to this matter. It is certainly very interesting to us.

For THE CANADIAN BEE JOURNAL.

Chloroform to Prevent Increase.

WE are always learning, and particularly so, if we are willing to look into new ideas and methods. The man that has learned it all, and the man that is full of good ideas he will not impart to others is generally one and the same man. Although we must have every respect for the bee-keeper who does not care to write articles for the public press from inability to put good practical thoughts on paper, or disinclination—these very good practical men I may say, however, are always willing to answer any question fully when conversing with them. We are all anxious to get at a system largely non-swarmling, and be able to use it at will. I have been particularly anxious to do this, as I believe a great deal of our winter losses would after certain seasons be reduced if there were less swarming permitted. I am not ashamed to say I allowed myself to be led into testing and inducing others to test the system of

prevention of increase by swarming, by chloroforming bees, and induced the Experimental Union to undertake the work. Now let it be understood every experiment conducted is not good work, only if it prove a success. It may be just as good if it prove a failure, as if it does others need not spend time or money over the matter. It was in such a spirit willing that the results should decide—that the matter was taken up—a spirit in which to make a just and fair test any experiment should be conducted. Your correspondent, who does not sign his name (probably he is ashamed to do it) says the agreement was to supply the chloroform. I think not—the method was simply given in which the experiment was to be conducted. Your correspondent should state facts, not fiction. We had very few respond as willing to take part—probably the majority felt sure that it would not prevent swarming, and did not care to convince others, only one or two reported, as the season was a very poor one with those experimenting—the results were duly reported to the Experimental Union, and so few reported, and so indefinitely, the bee journals were not afflicted with the report. If your correspondent will apply for the report to the Union, I will be happy to send it to him with more interesting matter printed with it. Perhaps it would have been better to have undertaken something else in the line of experimentation, as few believed it would prevent swarming, but our funds were small, and the experience of the committee smaller than it is now. I may say our efforts this year are meeting with a very hearty response, and some of the sections have already been returned—we have a few more to spare. As far as the personal attack on myself is concerned, of course I cannot deny that I feel it; it, however, casts a reflection upon the judgment of not only all the bee papers in America, but the British and Norwegian journals, and all the agricultural papers in Canada; two of the leading ones I withdrew from owing to pressure of work. Under the circumstances, and being in such good company, I will try and bear my share of the burden with resignation, knowing, Mr Editor, that you will be kind and just enough to grant this space in your valuable journal. I have no faith in prevention of increase by chloroform. If you wish it, I will tell you why.

Yours, &c.,

R. F. HOLTERMANN.

Brantford, July 9, 1891.

The above article was received too late to be inserted in our issue of the 15th of July, in which our apology to Mr.