

How was Wallace led to the Discovery of Natural Selection?

THE reviewer of Osborn's "From the Greeks to Darwin" (*antea* p. 362) says that Marshall quotes the fact of Wallace's being led "to the discovery of natural selection as he lay ill of intermittent fever at Ternate," and refers one to the abridged form of the "Life and Letters of Charles Darwin" for this statement. Having only the original edition in three volumes, from the year 1887, at my disposal, wherein I cannot find it, I would draw attention to my having published the fact as far back as 1870 ("Charles Darwin and Alfred Russel Wallace. Ihre ersten Publicationen über die Entstehung der Arten, nebst einer Skizze ihres Lebens und einem Verzeichniss ihrer Schriften." Erlangen, E. Besold, Svo, pp. xxiii. and 56, on page xviii.) The remarks to be found there are based upon a letter of Mr. Wallace's dated November 22, 1869, and now before me, a passage of which runs thus:—

"The paper No. 9 ['on the law which has regulated the introduction of new species' A.N.H. 1855] should be read along with No. 19 ['on the tendency of varieties to depart indefinitely from the original type' P.L.S. 1858]. When I wrote it I was firmly convinced of the derivative origin of species, but had not arrived at an idea of the process. When I wrote No. 19 at Ternate [in the year 1858] I did not [know] what were Mr. Darwin's views or the nature of the work he was engaged on, except generally that it was on 'Variation.' I hit upon the idea of 'Natural Selection' (though I did not give it that name) while shivering under the cold fit of ague, and I was led to it by Malthus' views on population applied to animals. As soon as my ague fit was over I sat down, wrote out the article, copied it, and sent it off by the next post to Mr. Darwin. It was printed without my knowledge, and of course without any correction of proofs. I should, of course, like this fact to be stated."

This I did in my pamphlet of 1870 on the page quoted, and on page 39, and I hope Dr. Wallace will forgive me for now making known the whole of his highly interesting statement *in his own words*. Of course I am not sure whether he did not tell or write the same to some one else, though I am not aware that it has been published.

Ordinary mortals dream nonsense in their fits of fever, a philosopher of Dr. Wallace's standing conceives original ideas!

A. B. MEYER.

Zoological Museum, Dresden, August 19.

THE letter to Prof. Newton, published in the abridged "Life of Darwin," was written in 1887. I had entirely forgotten that I had written on the same subject to Dr. Meyer in 1869, or that he had published anything in reference to it. That letter probably contained my earliest statement on the subject, and it agrees substantially with my later statements.—A. R. WALLACE.

A Problem in Thermodynamics.

SIEMENS taught us how, by using the heat of the gases escaping from a furnace to heat the gas and air before entering the furnace, we could obtain temperatures limited only by the fire-resisting quality of the materials of which the furnace is constructed. Now, it occurred to me whether on the same principle very low temperatures might not be reached. My idea is this: If compressed air is expanded to atmospheric pressure, the gas does work in overcoming the resistance of the atmosphere, and is cooled to a corresponding amount.

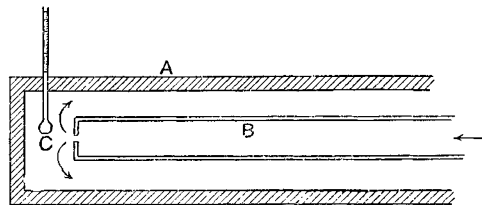
Suppose, for instance, the gas is compressed to 1/100 of its volume, then 1 cubic metre would perform, in expanding against the atmospheric pressure of 1 kil. per 1 square centimetre, or 10,000 kilos per square metre, an amount of work equal to $10,000 \times 0.99 = 9900$ kilgr.-metres, and absorb $\frac{9900}{424}$ units of heat.

Now, 1 cubic metre of air weighs 1.24 kil., and, having a specific heat of 0.24, the temperature of the expanded air would be lower 78° than before expanding.

Now suppose A is a tube of a material impervious to heat—that is, a perfect non-conductor—and B a tube made of a perfect conductor of heat; the tube A being closed at one end, and B having a small opening in the end.

Now, if a continuous supply of compressed air is kept up in

tube B, this air will come down in temperature, and, passing along between A and B, cools the compressed air before it expands.



I should be glad if any of your readers could give me the theoretical minimum of temperature produced at c.

Essen-Ruhr, Germany.

E. BLASS.

A Remarkable Flight of Birds.

ON September 30, 1894, about 3 p.m., I was observing the sun through an 8-inch telescope. I noticed some dark figures of birds passing, like shadows, across the sun. I was using a dark glass, and the birds were, consequently, only visible when seen against the bright solar disc. The silhouettes of the birds were very sharply and clearly cut. Every few seconds a bird would emerge from the darkness, pass slowly across the sun and disappear on the other side. I watched them for over ten minutes without any decrease in their numbers. The whole number of birds must have been enormous, otherwise it would have been impossible for some of them to have passed as frequently as they did between my telescope and the sun. The birds were flying in a southerly direction, and were quite invisible to the naked eye. I was, therefore, unable to determine their distance, but should think they must have been two or three miles away, for the telescope was in focus for the birds and sun at the same time. I do not know what birds they were. Comparing the spread of their wings with the solar disc, I should say their wings subtended an angle of about two minutes. The place from which I observed them was Shere, a village between Guildford and Dorking. I am told that such a flight of birds has not before been recorded in this country, and have been urged to publish an account in the hope that other astronomers, who may have seen a similar thing, may be led to mention the fact. Shere, Guildford.

R. A. BRAY.

THE IPSWICH MEETING OF THE BRITISH ASSOCIATION.

IN our last article we gave a general outline of the local arrangements for the Meeting. The programme, as a whole, is now fairly complete. A slight alteration has been made with reference to the soirées; the first will be given by the Ipswich Scientific Society and the Suffolk Institute of Archæology jointly, and the second by the Mayor of Ipswich (Mr. J. H. Bartlet). The fitting up of the Section Rooms is proceeding rapidly, and arrangements are being made for the darkening of those in which a lantern will be used. In the case of Sections A and B, which meet in the same building, only the room allotted to Section B will be fitted up with dark blinds and a lantern screen, and the Sections will be asked to exchange rooms on days when papers requiring lantern illustration are read in Section A. The same arrangement will be made as to Sections D and K, which meet in the two rooms at the Masonic Hall. For the President's address in these Sections, the Lyceum Theatre, which is a short distance off, will be placed at the disposal of the Sectional Committees, as the Masonic Hall rooms may be hardly large enough to contain all those who would probably wish to be present on these particular occasions. For a similar reason, Section G, which meets in the Co-operative Hall, will be asked to allow the President's addresses in Sections A and B to be delivered there. A spacious room adjoining the main