point of the German authors* cited by Dr. Baldamus, I will here adduce the following passages :—

"Quant au genre Coucou (*Cuculus*) et notamment à l'espèce type du genre, notre Coucou Chanteur (*C. canorus*), on sait quelle étonnante diversité offre la coloration de son œuf, toujours de forme ovée, diversité telle que nous nous abstiendrons d'en aborder la description détaillée."—DES MURS, *Traité Général d'Oologie Ornithologique*, &c. Paris : 1860, p. 219.

"Ces œufs sont très-petits relativement à la taille de l'oiseau, et varient beaucoup pour la couleur. Ils sont ou cendrés, ou roussâtres, ou verdâtres, ou bleuâtres avec des taches petites et grandes, rares ou nombreuses, d'un cendré foncé, vineuses, olivâtres ou brunes, avec quelques points et parfois des traits déliés noirâtres. Nous en possédons deux du blanc le plus pur, et un autre d'une seule teinte bleu-verdâtre, pris dans un nid de Stapazin."—DEGLAND et GERBE, *Ornithologie Européenne*, &c. Paris : 1867, vol. i. p. 163.

I produce this testimony as to *facts* with the greater confidence, because the *opinions* of the witnesses differ from my own, and not one of them, so far as I can gather from their works, was acquainted with Dr. Baldamus's essay.

2 and 3. "Were these [sixteen varieties of eggs] seen to be deposited by the bird, or how were they identified as those of the cuckow? . . . . Is there not room for error here?"

The evidence on which the eggs in question were referred to the Cuckow has been printed in full by Dr. Baldamus and the translator of his essay. To repeat it here would occupy much space and, I think, be unnecessary. It is of much the same kind as the evidence with regard to most Cuckows' eggs. I will freely grant that it might be more satisfactory—if it were so my former paper would never have been written, since naturalists must then have at once accepted the theory. But, on the other hand, I have a right to ask this : If the eggs in question were not Cuckows', what birds laid them? Surely not those in whose nests they were found, because it is a fact which most oologists will confirm, that when birds lay larger eggs than usual the colouring is commonly less deep, and though exceptions may occasionally be found, yet here we have sixteen which are at the same time larger than usual, and of a colour at least as deep, supposing them to belong to the nest-owners. *Sixteen* cases are too many to be exceptional, but this is the number only of the specimens *figured* by Dr. Baldamus; upwards of *sixty* are more or less fully *described* by him.

4. "How then is this process effected?"

In answer to this, Mr. Sterland quotes a very brief summary of my own explanation, to which I have nothing now to add.

5 to 9. The next five questions, for brevity's sake, I will not repeat. They are very pertinent, but are far more easily asked than answered, for they open a wide field of speculation and investigation, since all the hitherto unexplained phenomena of "Dimorphism," "Trimorphism," and "Polymorphism," seem to enter here. But with respect to one of the questions (No. 6), I submit that even if there were no other instance satisfying the conditions imposed by Mr. Sterland than that which I alleged, it is no true argument against the truth of what I advanced. But I think there is an indication of it in other species bearing very directly on the point. Take the Blackcap Warbler and the Tree-Pipit. The eggs of the first are well known to present at least two very different appearances, and those of the second are still more variable. Since Mr. Sterland will not allow that my Eagles fulfil his conditions (and of course he has a perfect right to do so), perhaps he will permit me to bring forward these birds. I have some reason for believing that the same hen Blackcap constantly lays eggs of similar colour. Do the birds of this species hatched from eggs with reddish shells lay eggs of the same character, or brownish ones, and *vice versâ*? If of the same character, we have such an example as is required. If of the other colour, it becomes a case in some measure of "Alternate Generation," but still reducible to a law. That there should be no law at all seems to me at least unlikely, though I fear its discovery is hard.

Certain facts of Dimorphism and Polymorphism are known, but I have not met with any attempted explanation of the phenomena even in such decided and remarkable cases as those of the Malayan Butterflies given by Mr. Wallace (Trans. Linn. Soc. vol. xxv. pp. 5-11). Why the different forms of one species of *Papilio* inhabiting the same district remain distinct is perhaps more unaccountable than that the different forms of Cuckows' eggs should be preserved, for it does not seem to me unlikely that the colour of the egg and the maternal instincts should depend upon the *hen* bird; in which case, granting the hereditariness (if I may make such a word) of the qualities already specified, I think there would be no difficulty.

10. A full reply to Mr. Sterland's last question would lead me to anticipate much that I intend to say when you again permit me to trespass on your readers' forbearance. Consequently, I must defer it until I come to the consideration of "Cuckows' Dupes."

ALFRED NEWTON

Cambridge, Dec. 11, 1869

* They are Naumann, Thienemann, Brehm, Gloger, and Von Homeyer. Unfortunately, Dr. Baldamus does not refer to the passages in their writings wherein this opinion is expressed; and as most of these writings are somewhat voluminous, I have not always been able to find what are the passages meant. I presume that Mr. Sterland has been more fortunate, for he would scarcely doubt the assertions without knowing what they were, and I should be much indebted to him if he will tell me where they occur—indeed, I am uncertain which of the Brehms and which of the Von Homeyers is intended.

By way of postscript of my letter of the 11th of December (for the delay in publication of which I am in no way accountable*), permit me to offer a few remarks on the communications of Mr. Dresser and Mr. Cecil Smith which have since appeared.

Mr. Dresser says (p. 218) that he "cannot quite agree with Professor Newton that Cuckows' eggs *as a rule* are subject to great variety." I am not aware that I had made such an assertion. The nearest approach to it that I can find is my statement (p. 74), that "it has long been notorious to oologists that eggs of the Cuckow (*i.e.* of the Common Cuckow of Europe —the only species I had mentioned) are subject to very great variety," and in proof thereof I have since furnished some other (and, I think, satisfactory) evidence. Mr. Dresser himself has also brought two or three additional examples which confirm my statement. For the knowledge of these I am much obliged to him, as well as for stating the result of his own experience in support of my supposition that the eggs of the same hen Cuckow resemble each other.

Mr. Cecil Smith (p. 242) seems to me to be as unfortunate in his interpretation of my remarks as he was in that of Dr. Baldamus's (p. 75, note). I feel sure that I have not "pruned and paved" down the doctor's theory so "that there is but little of the original left." To the facts alleged by that naturalist I have taken no exception—on the contrary, I have borne witness (pp. 74, 75) to their general truth; and in the attempt to offer a reasonable explanation of them, I am certain that my "manipulation" is not open to any such charge as that made by Mr. Smith. My "cautious and limited statement" is not different from that of the doctor, nor does "it entirely sweep away" a single assertion of his as to matters of fact. Mr. Smith, apparently, thinks because I have referred to the number of Cuckows' eggs yearly found in nests of the Hedge-Sparrow in this country, without ever bearing any resemblance to the eggs of that bird—a fact, of course, fully admitted by him—that I must thereby deny the single exceptional case adduced from Germany by Dr. Baldamus; but I have never maintained, because no likeness is to be traced in a great many instances, that none was ever perceptible, and accordingly there is no "issue of fact" between the doctor and myself. I must take the liberty of adding, that Mr. Smith, having, as I before showed, misunderstood Dr. Baldamus, has now misunderstood me; and this being the case, it is perhaps needless for me to take up more of your space.

ALFRED NEWTON

January 3, 1870

The Veined Structure of Glaciers

I THINK there is no one point in connection with glaciers more interesting than their veined structure, or one upon which so much has been written that remains equally unsettled. The difference of opinion about it between the authors who have published most upon the subject are not less remarkable than the phenomenon itself: no two are agreed, except in considering it as a constitutional feature.

Professor Agassiz maintains (*Atlantic Monthly*, Dec. 1863) that the horizontal layers of pure ice which are formed between the beds of snow from which a glacier is born, constitute many of the identical veins or plates of pure ice which pervade the glacier when it is in full life and activity; and attributes the inclination which they make, in the latter case, to their former horizontal position, to the contortion, bending, or folding, to

* [The delay in the publication of Prof. Newton's letter is owing to an oversight. It was received prior to the communications of Mr. Dresser and Mr. Cecil Smith, printed in our eighth and ninth numbers.—ED.]