course would destroy and obliterate these delicate forms. There must be a steady set of wind, enough and not too much of it, and the air must be saturated with moisture in a certain state of molecular division. Some of these data might, perhaps, with the resources of a modern laboratory, be settled by experiment. If the experiment succeeded it would be an extremely pretty one.

F. POLLOCK
C. C. COLLIER

Woodtown, Harabridge, South Devon, January 2

Krukenberg's Chromatological Speculations

MY attention has been lately called to a recent publication of Dr. C. F. W. Krukenberg, entitled "Grundzüge einer Vergleichenden Physiologie der Farbstoffe und der Farben," in which some remarks and misstatements occur relative to my work, which in self-defence I feel I am not justified in letting pass without comment.[1]

(1) With regard to his observations on the colouring matter obtained by me from the integument of certain invertebrates, and which I called "*dermochrome*," I do not see why I should have left it unpublished because three-quarters of a year before he had found that "lipochromes" were widely distributed in the animal kingdom. I found that lutein and hæmatoporphyrin occurred together in a peculiar combination, and said so. I suppose the offence lies in the name "lutein." This word must now, according to Krukenberg, be got rid of, because he has chosen to call it "lipochrome." Perhaps, after all, "lutein" is more appropriate, as it does not mean fat pigment; for this pigment occurs where there is no fat, *e.g.* it is not derived from fat in the *Corpora lutea*. Krukenberg bases his conclusions mainly on the reactions of the solid pigment with nitric and sulphuric acid and iodine, but I hope to have something to say on this point before long.

(2) Krukenberg maintains that the chlorophyll of cantharides is due to that in the intestines of these beetles. He committed himself to this theory at an early stage of his investigations, *before he knew of Pocklington's observations;* but after seeing the abstract of my paper read at the meeting of the British Association at Southport in 1883, in which I called attention to Pocklington's work, he makes it appear that he knew all about it long ago, which is not fair. Now since Pocklington and I obtained chlorophyll from the *elytræ* of these beetles, I do not think the above theory can be accepted, except it can be proved by Krukenberg that the intestine ramifies through the *elytræ*.

(3) Krukenberg says that *I* "*assume*" that the chlorophyll spectrum seen by me in the integument of the larva of *Pieris rapæ* is due to chlorophyll in that situation, whereas it is really due to chlorophyll-holding masses in the intestine. I never did "assume" anything of the sort. I said distinctly at Southport that it was due to food chlorophyll in the intestine, as could easily be proved, for on emptying the intestine the chlorophyll band could no longer be seen. This must be a wilful misrepresentation, as he acquired the knowledge of Pocklington's work from the same abstract in which my explanation occurs.

(4) He further says that my knowledge of the literature of the subject must be great when I assume that he has confused "*anthea green*" and "*diatom yellow*," whereas I said distinctly "it would appear, according to Geddes" (see Geddes' paper in NATURE, vol. xxv. p. 303) that he had confused them. I may, however, now observe that his supposition that the colouring matter of the yellow cells of *Anthea* is what he calls a "hepatochromate" can easily be disproved; all that is necessary is to add a little caustic potash or caustic soda to its alcoholic solution when the colouring matter becomes completely altered; for this reason any deductions drawn from Krukenberg's "saponification method" in this case are of little value.

(5) Krukenberg says he had found "chlorophyll-like stuffs" in the livers of animals before I had done so. I am sure this statement is open to question, as his spectra are not accurate representations of what is seen in solutions of *enterochlorophyll*. In most cases only one or two bands are shown by him, and the other proofs brought forward by me are not given in the accompanying text. If his own test for a true chlorophyll be accepted, I can, and hope shortly to, show that animal chlorophyll is a true chlorophyll, and can be obtained in the crystallised state, and the crystals are the same as those obtained by Dr. Hansen, an abstract of whose work will be found in this journal (vol. xxx. p. 224).

(6) It is further suggested that the darkening of the bands in solutions of "echinochrome" (a pigment whose spectrum I have lately described) produced by adding sulphide of ammonium, is caused by precipitation of certain ingredients. This is not the case. The same appearance is produced by stannous chloride and other reducing agents. I have, however, lately succeeded in isolating this pigment, and can confirm my former results. I hope to publish shortly an account of the spectra of its solutions.

(7) Krukenberg makes it appear that I have said that the green gland of the crayfish contains hæmoglobin. I never said so. The statement was this: "In the green gland of one crayfish a band was detected which, I think, was due to reduced *hæmatin*, but it was absent in the second specimen examined." Perhaps Krukenberg thinks that hæmoglobin and hæmatin are the same.

(8) I am made responsible for the statement that the eye of the house-fly contains hæmoglobin; I never said so, nor can I agree with Krukenberg that it gives no band. It gives a band at D, and is not similar to the pigment of the eye of Cephalopods, which he assumes to be the case.

I leave the inferences to be deduced from the above statements to others; but I must protest against Krukenberg's treatment of my work. It is at least satisfactory to know that my experience is not unique, as other English, German, French, and Italian workers receive an equally fair treatment by Dr. Krukenberg.

Wolverhampton, Dec. 23, 1884 C. A. MACMUNN

Our Future Clocks and Watches

I WOULD suggest, as a modification of "R. B.'s" suggestion in NATURE (p. 80), that the striking of the clocks on the twenty-four system might be varied at each quarter of the day, so as to indicate the time without so much striking. Thus, 1 (a.m.) to 6 might be indicated by the usual method; 7 could be indicated by two strokes, a pause, and one stroke; 8, by two strokes, a pause, and two strokes; and so on to 12; 13, by three strokes, a pause, and one stroke; and so on to 18; 19, by four strokes, a pause, and one stroke; and so on to 24, which being thus indicated by only ten strokes would require less effort to count, and make less noise than by the old system. Dials might be modified in the same way. Instead of twelve there would be only six divisions around the dial, and the quarter of the day could be indicated by a small wheel revolving behind a peep-hole, or by a third hand (which could be very short) revolving once a day over four divisions or quadrants, marked on the dial near the axis. People, however, would seldom or never need to look at this. Thus would be done away all the objections urged by Harmer. The hour-markings are only conventional signs any way, and it does not make any especial difference in what way the hours are indicated if people would only accustom themselves to the use of the twenty-four hour system in speaking and writing.

H. H. CLAYTON

Ann Arbor, Michigan, December 20, 1884

MODE OF RECKONING TIME AMONGST VARIOUS PEOPLES

THE recent Prime Meridian Conference at Washington has attracted attention to the methods employed at various periods, and amongst peoples in different stages of civilisation, to reckon time. Dr. Robert Schram, on October 24, read an interesting paper on this subject before the Geographical Society of Vienna, in which he dealt chiefly with the Chinese, Hindoos, and the Jews. The three units of measurement given by Nature herself are the rotation of the earth on its own axis, the revolution of the moon in its orbit, and that of the earth around the sun; these are wholly independent of each other, and neither is an aliquot part of the others. But from the earliest times efforts have been made to connect these units; there is the attempt to balance all three, which gives the luni-solar year, or those to connect the day with the course of the sun or of the moon, from which we get the solar or lunar year. In the earliest times the most complicated of these, the luni-solar year, in which it

[1] The papers in which my observations on the subjects referred to were published are:—*Proc. Roy. Soc.*, 1883 (No. 226); *Proc. Birmingham Philos. Soc.*, vol. iii., 1883; and *Brit. Assoc. Reports*, 1883.