



XX. On a systematic arrangement of colours

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It is added that this discovery is of great moment, as it removes an *anomaly*, which hitherto remained, against the general law in the congelation of liquids, (alluding no doubt to the congelation of *alcohol*;) viz. that all liquids become solid at a certain temperature.

That *alcohol* itself, at present the measure of low temperatures, would become solid as well as other liquids, provided a sufficient degree of cold were produced, no person, I should think, ever doubted.

It is a well known fact, respecting the congelation of the mineral acids, that the most concentrated state of them is by no means that in which they resist the greatest cold without freezing. It is possible this may be the case with respect to *alcohol*, but this does not appear very probable.

If it be an ascertained fact, that a temperature sufficiently cold can be produced by art to fix or freeze *alcohol*, or *spirit of wine*, of any strength, we must in future look for some other measure of low temperatures than liquids; and of course, in such a case, a thermometer of *metallic construction* would naturally present itself*.

I have merely made these cursory remarks, being engaged at this time in other pursuits, for the reason assigned above; and when the fact is clearly and accurately established, I shall adopt it in the stead of the one I at present consider myself bound to adhere to.

Oxford, July 6, 1813.

RD. WALKER.

XX. On a Systematic Arrangement of Colours. By
THOMAS FORSTER, Esq.

To Mr. Tilloch.

SIR, AMONG the desiderata of philosophy may be included the want of a systematic arrangement of colours†, with specific names for each, whereby the numerous combinations and shades of colour, which appear on the surfaces of bodies, may be expressed with greater precision than they can be at present with our imperfect and indefinite names. I was first induced to think on this subject from the great difficulty that I experienced from time to time in conveying on paper an adequate idea of the various and dissimilar tints

* I allude to the contraction or elongation of metallic wire according to changes of temperature. See a paper on this subject, Phil. Mag. Aug. 1810, page 119.

† In future, perhaps, some theory of smells may be formed by repeated experiments with compounds of them. Something like what Haller is said to have had in view.

displayed by the clouds and the haze on different occasions, which were occasioned by the refractive powers of the nubiform particles of water, and which I wished to register in my Meteorological Journal. The terms in common use, such as red, yellow, blue, green, orange, purple, &c. were not sufficiently definite, for of all these there are numerous varieties. That any nomenclature should be constructed which shall precisely define every combination and shade of colour is almost impossible, since the varieties and approximations of one colour towards another are infinite, as they depend on the proportions of mixtures, the quantities of which we may suppose capable of being varied infinitely; but still a more perfect set of names might be arranged than has as yet been done. I am surprised that scientific persons, but botanists in particular, have not before this attempted something of the kind. How different is the red of the flower of the peony from that of the papaver rhœas! How almost contrasted does the brilliant red of the scarlet lychnis appear to the red of the papaver orientale called the monk's-hood poppy! Who is there who cannot discover much difference in the colours of the flowers of the spring crocus, of the field ranunculus, and of the evening primrose, and are not these termed yellow flowers? What distinction between the blue of the sonchus cœruleus from that of the field hyacinth!

The colour we call green has nearly as many varieties: we hear of grass-green, apple-green, &c. but these terms do not express the numerous kinds of green observable in different leaves and other natural productions. The word brown appears still more various; it seems to have become the common name for all unknown and mixed corruptions of colour*.

To rectify the present imperfect descriptions of flowers, and other natural and artificial productions, by a more accurate nomenclature for colours, is a desirable object; but what is the best mode of forming such a nomenclature becomes a different question.

If the colours of many wild flowers could be relied on as standards, from not being found to vary much in different situations and at different periods, we might have a nomenclature by reference to them: but this would be objectionable, in as much as one principal use of the specific names being that of enabling botanists to describe the tints

* In superadding the terms pink, lake, scarlet, orange, &c. we have not done much towards a perfect nomenclature, as there are varieties describable by these names,

of flowers, they must in this case describe the colour of many by reference to that of others, which must be presumed to be known. In this case, therefore, the nomenclature could only have reference to well known plants, and would be quite useless in countries where the said plants were not either indigenous or commonly cultivated.

As all the numerous tints in nature may be said to consist in the combinations and shades of the primitive colours; that is, in the proportions of the various mixtures, and the intensity produced by the degree of light; the most accurate method would be to investigate these said proportions, and make a nomenclature which should have reference to them: but as this would be a work of great labour, it would be advisable if we could construct some more easy and familiar nomenclature. I merely suggest these hints at present, which I hope may be improved upon in future.

Yours, &c.

Clapton, June 30, 1813.

THOMAS FORSTER.

XXI. *Mr. BAKEWELL in Reply to Mr. FAREY, on the Great Derbyshire Fault.*

To Mr. Tilloch.

SIR,—IN the Philosophical Magazine for July 1812, some queries were addressed by me to Mr. John Farey, respecting what he has denominated the great Derbyshire Fault, requesting him to favour the public with some proofs of its existence. The last number of your Magazine contains a letter from Mr. F. purporting to be a reply to mine, but omitting entirely the proofs which he was requested to produce. I therefore take the liberty of refreshing his memory, with a second request to have a short and intelligible answer to these queries, as they regard the determination of a most important question in the Geology of England. That your readers may have a more distinct view of the subject, I shall briefly observe, that by the "great Derbyshire fault" is meant a rent or fracture of the earth's surface, which has torn the island from Nottingham to near Macclesfield in Cheshire; and from thence in a line northwards into Yorkshire or Lancashire. According to Mr. Farey, the discoverer of this fault, the strata on the north side of it are elevated from a vast depth, and the strata on the south terminate against this rent or fault, or are cut off by it. To use a familiar illustration of faults given by Dr.

Kidd