7.

SOME EXPERIMENTS ON COLOUR.

[*Nature*, III. pp. 234—237, 264, 265; 1871.]

THE theory of colour perception, although in England it has not yet made its way into the text-books, still less into the popular works on science, is fully established with regard to many important points. It is known that our perception of colour is threefold, that is, that any colour may be regarded as made up of definite quantities of three primary colours, the exact nature of which is, however, still uncertain. More strictly stated, the fundamental fact in the doctrine of colour is that, between any four colours whatever given as well in quantity as in quality, there exists what mathematicians call a linear relation, that is, that either a mixture of two of them (in proper proportions) can be found identical, so far as the eye is able to judge, with a mixture of the other two, or else that one of them can be matched by a mixture of the other three. There are various optical contrivances by which the mixture spoken of may be effected. In the year 1857, Mr Maxwell published an account of some experiments with the colour top undertaken to test the theory. From six coloured papers, black, white, red, green, yellow, and blue, discs of two sizes were prepared, which were then slit along a radius so as to admit of being slipped one over the other. Any five out of the six being taken, a match or colour equation between them is possible. For instance, if yellow be excluded, the other five must be arranged so that a mixture of red, green, and blue is matched with a mixture of black and white. The large discs of the three colours are taken and slipped on to each other, and similarly the small discs of black and white. When the small discs are placed over the others and the whole made to rotate rapidly on any kind of spinning machine, the colours are blended, those of the large discs and those of the small, each into a uniform tint.

By adjustment of the discs an arrangement may be found after repeated trials, such that the colour of the inner circle is exactly the same both in tint and luminosity with that of the outer rim. The quantities of each colour exposed may then be read off on a graduated circle, and the result recorded. For instance (the circle being divided into 192 parts), eighty-two parts red mixed with fifty-six green and fifty-four blue, match thirty-seven parts white mixed with 155 black. In this way Maxwell observed the colour equations between each set of five, in all six sets, formed by leaving out in turn each of the six colours. Moreover, for greater accuracy each set was observed six times, and the mean taken. But according to the theory these six final equations are not all independent of each other, but if any two of them are supposed known, the others can be found by a simple calculation. Accordingly, the comparison of the calculated and observed equations furnishes a test of the theory; but in practice, in order to ensure greater accuracy, instead of founding the calculations on two of the actually observed equations chosen arbitrarily, it is preferable to combine all the observations into two equations, which may then be made the basis of calculation. In this way, a system of equations is found necessarily consistent with itself, and agreeing as nearly as possible with the actually observed equations. A comparison of the two sets gives evidence as to the truth of the theory according to which the calculations are made, or if this be considered beyond doubt, tests the accuracy of the observations. In Maxwell's experiments the average difference between the calculated and observed systems amounted to ·77 divisions of which the circle carried 100. So good an agreement is regarded by him as a confirmation of the whole theory; but it seems to me, I confess, that only a very limited part of it is concerned. The axioms, in virtue of which it is permitted to combine the colour equations in the manner required for the calculations, are only such as the following:—If colours which match are mixed with colours which match, the results will match. It is difficult to imagine any theory of colour which will not include them. What proves the threefold character of colour—the most important part of the doctrine—is simply the fact that with any five coloured papers *whatever* a match can be made, while with less than five it cannot (except in certain particular cases). In regard to this point the value of the quantitative experiments is rather that they show of what sort of accuracy the eye is capable in this kind of observation. Those to whom the subject is new may think at first that if colour be threefold a match ought to be possible between any *four* colours. And so it is possible if there is no other limitation; but in experiments with revolving discs we are subject to a limitation, being obliged to fill up the whole circumference somehow. The difficulty will clear itself up, when it is remembered that one of the five colours may be black, so that with any *four* colours and *black* a match can be made with revolving discs.

It was rather for my own satisfaction than with the hope of adding anything new to a subject already so fully and ably treated by Maxwell, that I commenced a repetition of his experiments. The colours used were, roughly speaking, the same as his, as was also the general plan of the observations. The agreement of the calculated and directly observed equations was very good, the average error being only ·24 divisions, of which the complete circle contained ninety-six, or one-third of the corresponding average error in Mr Maxwell's Table. A second set of observations and calculations made after a year's interval with a different set of colours gave about the same result. I am inclined to attribute the considerably greater accuracy of my observations rather to an excellent perception of minute differences of colour (to which I have always found my eyes very sensitive) than to greater care in conducting the experiments. One precaution, however, I have found so important as to be worth mentioning. Unless the small discs are very accurately cut and centred, a coloured rim appears on rotation between the two uniform tints to be compared and adjusted to identity, which is exceedingly distracting to the eye, and interferes much with the accuracy of the comparison. One set of observations made with the same care, and apparently as satisfactory as any of the others, puzzled me for some time on account of the great discrepancies with the others which it exhibited. I have no doubt that the cause lay in the different character of the light on the day in question, which came from the unusually blue sky which sometimes accompanies a high wind. On the other days the light came principally from clouds. I have had no opportunity of confirming this opinion by a repetition of the experiment with a sky of the same degree of blueness, but that the disagreement was not the result of unusually large errors of observation, is, I think, to be inferred from the fact that the observations under the blue sky were as consistent among themselves as any of the other sets. As the point is of some interest, I give the figures in full.

July 23, blue sky.

Black.	White.	Red.	Green.	Yellow.	Blue.	
0	+ 30	+ 122	+ 40	− 77	− 115	obs.
0	+ 32·2	+ 120·8	+ 39·1	− 78·8	− 113·2	calcd.
+ 94	0	− 132	− 60	+ 55	+ 43	obs.
+ 91·6	0	− 133·5	− 58·5	+ 54·4	+ 45·9	calcd.
− 138	− 54	0	+ 24	+ 50	+ 118	obs.
− 138·3	− 53·7	0	+ 23·1	+ 49·5	+ 119·5	calcd.
+ 92	+ 50	+ 50	0	− 66	− 126	obs.
+ 94·1	+ 49·5	+ 48·5	0	− 65·2	− 126·7	calcd.
− 154	− 38	+ 86	+ 52	0	+ 54	obs.
− 154·6	− 37·5	+ 84·6	+ 53·1	0	+ 54·3	calcd.
+ 139	+ 18	− 128	− 64	+ 35	0	obs.
+ 138·5	+ 19·7	− 127·5	− 64·5	+ 33·9	0	calcd.

The numbers read off for the big discs are written with the sign + prefixed, and those corresponding to the little discs with –. Thus the first line may be read:—30 parts white together with 122 red and 40 green, match 77 yellow and 115 blue. The upper line of each pair represents the actual observation, and the second is the theoretical equation calculated from two in the manner described. The average difference between the two sets of numbers which may be taken as a measure of the inaccuracy of the observations amounts to 1·1. A similar table, formed from the observations of July 20 (cloudy), and which agreed very well with the results of other days, is as follows*:—

Black.	White.	Red.	Green.	Yellow.	Blue.
0	+ 30	+ 117	+ 45	– 79	– 113
0	31·1	116·2	44·8	79·9	112·2
+ 90	0	– 128	– 64	+ 56	+ 46
85·9	0	128·4	63·5	57·0	49·0
– 136	– 56	0	+ 22	+ 52	+ 118
137	55	0	22·3	50	119·6
+ 100	+ 50	+ 42	0	– 64	– 128
99·2	51	41·9	0	65	127·1
+ 135	+ 21	– 123	– 69	+ 36	0
135·7	21·5	122·7	69·3	34·8	0
– 152	– 40	+ 80	+ 56	0	+ 56
152·6	39·5	81	56	0	55

The average error is here ·95, showing only a trifling better agreement than the former set, so that the blue sky observations are nearly as self-consistent as those made with cloud-light. Moreover, the agreement is itself very good, being decidedly better than Maxwell's, though his calculations refer to a *mean* of six sets of observations.

While therefore there is no reason to distrust the results of July 23 any more than of July 20, the differences between them are much greater than can be ascribed to errors of observation. It will be found that they relate principally to the quantities of red, the numbers under that head being considerably greater for the case of the blue light from the sky. I am not aware whether the difference of sky and cloud light has ever been made the subject of direct investigation, but it would seem a fair inference that it must consist mainly in a relative deficiency of the red rays. If this be so, as I have other grounds for suspecting, the light of the sky would be similar in composition to that of dilute solutions of copper, which acquire their light

* These calculations were made by means of Prof. Everett's Proportion table, which seems admirably adapted to work of this sort.

blue tint by a partial suppression of the extreme red*. There is no doubt that the colour equations are dependent on the character of the light, as may easily be proved by taking an observation looking all the time through a layer of coloured liquid. It is not, however, the most brilliantly coloured solutions that cause the most disturbance, for anything like a complete stoppage of all the rays which are capable of exciting one of the primary colour sensations would affect both the mixtures to be compared in nearly the same manner, putting the observer in fact very much into the position of a colour-blind person. Those liquids will be most efficient which have a different action on parts of the spectrum allied in colour. For instance, an aqueous infusion of litmus has a strongly marked action on the yellow ray, stopping it with great energy, even in rather dilute solutions. It is easy to trace the effect of looking through this on most of the colour equations. Consider, for example, the fifth equation of July 20 (that from which the blue is absent) wherein red and green are matched against black, white, and yellow. The red and green will for the most part escape absorption, but the white and yellow will be shorn of a part of their yellow rays. The match supposed to have been adjusted without the litmus must evidently be spoiled; the red and green mixture becoming strongly yellow in comparison with the other. In order to restore equivalence the yellow must be considerably increased. On trial I found that 124 black + 19 white + 49 yellow matched 121 red + 71 green.

It is only the impurity of the colours on the discs that prevents the effect being still more strongly marked, for with the pure colours of the spectrum the most violent alterations are possible. When a match is made between the simple yellow and that compounded of pure red and green, almost any coloured liquid acts unequally on the two parts and destroys the balance. The simple yellow, of course, retains its colour under any absorbing influence, and can only be changed in luminosity. Chloride of copper extinguishes the red component of the compound yellow, which accordingly becomes green. Litmus would leave the compound colour nearly unchanged, while it extinguishes the simple yellow. It is needless to multiply instances.

Before leaving the compound yellow, of whose very existence many are incredulous, I will mention an easy way of obtaining it, which is the more desirable as the use of the pure spectral colours is not very convenient. In

* Direct observations, made since the above was written, show that there is no *peculiar* deficiency at the red end of the spectrum, but a general falling off as the refrangibility diminishes from one end to the other. If lights from sky and cloud are of equal intensity at the line C in the red, the first will be somewhere about twice as bright as the other at b in the green. This is for a well-developed blue light taken from the zenith; but, even with a large allowance, enough difference remains to account for the discrepancies in the two sets of colour disc observations. I have lately found from theory that the power of very small particles to scatter the rays belonging to different parts of the spectrum varies as the inverse fourth power of the wave-length.

order to isolate the red and green rays of the spectrum by means of absorption, the first thing is to find a liquid capable of removing the intermediate yellow and orange. With this object we may fall back on the alkaline solution of litmus, whose opacity to the yellow, and particularly to the orange, rays is so marked. The next step is to remove the blue and bluish green, for which nothing is more convenient than the chromate of potash. A mixture of these two liquids in proper proportions, easily found by trial, isolates the green and extreme red rays with considerable perfection, and exhibits in a high degree the phenomenon of Dichromatism. According to the thickness traversed by the light the red or the green predominates, and there is no difficulty with a given thickness in arranging the strength of the solution so as to give a full compound yellow. It is worth notice in confirmation of the opinion expressed as to the character of the sky-blue, that when a cloud seen through the liquid appears a full yellow, or even orange, the former, if at all intense, acquires a decided green colour. A window backed by well-lighted clouds, when looked at across a room through the liquid and a prism, has a very splendid appearance, the red being isolated on one side, and the green on the other; while the intermediate space, where the two overlap, shows the compound yellow in great perfection. Another liquid, in some respects preferable, which answers the same purpose, may be made by mixing chloride of chromium and bichromate of potash. Through either of them the sodium flame is invisible, though they may easily be made to correspond with it in colour very closely. I tried to obtain a liquid capable of isolating the pure yellow ray, but only with partial success. The best was a mixture of bichromate and permanganate of potash with a salt of copper (sulphate or chloride). The first removes the blue and violet, the second the green, and the third the red, and thus the yellow is isolated in considerable purity. This liquid is very unstable. The comparison of the simple and compound yellow (which nearly matched) was interesting. One was transparent to the sodium flame, the other completely opaque to it. When the two are brought together so that the light has to traverse both, almost complete darkness results, even when the brightest clouds are used. I should mention that it is only when the light is strong that any of these liquids give yellow in full perfection; otherwise the colour is more nearly described as brown, which is, in fact, identical with a dark yellow or orange. The best natural yellows, such as chrome, are partly simple and partly compound, returning all the light which falls upon them except the blue and violet. It is clear that neither a purely simple nor a purely compound yellow can rival them in brilliancy.

Impartial observers, unprejudiced by the results of mixing pigments, or, on the other hand, by experiments on the spectrum, see, so far as I can make out, no connection between the four principal colours—red, yellow, green, and blue. It seems to them quite as absurd that yellow should be compounded of red and green, as it most unquestionably is, as that green should be a compound of blue and yellow, though many have accepted the latter alternative on the authority of painters, and some have even worked themselves into the belief that it is only necessary to look at the colours in order to recognise the compound nature of green. My own prejudice would be on the other side, the result of experiments on the compound yellow, which is seen so easily to pass into green on the one side or red on the other. The most impartial opinion that I can form is that there is no real *resemblance* between any of the four, and if this be so it is certainly a most remarkable, if not unaccountable, fact. The difficulty is not so much that we are unable to analysė the compound sensation, as to explain why our inability is limited to yellow (and white). For everyone, I imagine, sees in purple a resemblance to its components red and blue, and can trace the primary colours in a mixture of green and blue. Sir John Herschel even thinks that our inability to resolve yellow leaves it doubtful whether our vision is trichromic or tetrachromic, but this seems to me to be going much too far. Surely the fact that the most saturated yellow can be compounded of red and green, deprives it of any right to stand in the same rank with them as primary colours, however little resemblance it may bear to them and blue. Besides, if yellow is to be considered primary, why not also white, which is quite as distinct a sensation as any of the others? Undoubtedly there is much that is still obscure in the mutual relations of the colours—why, for instance, as mentioned by Sir John Herschel, a dark yellow or orange suggests its character so little as to be called by a new name (brown), while a dark blue is blue still. But difficulties such as these should make us all the more determined to build our theories of colour on the solid ground that normal vision is threefold, and that the three primary elements of colour correspond nearly with red, green, and blue.

Yellow.

It was not from any experiments of my own, but on the authority of Helmholtz, that I asserted [supra, p. 84] the identity of brown with a dark yellow or orange. He found that the pure red and yellow of the spectrum gave the various shades of brown when seen by the side of more brilliantly lighted white surfaces. (*Physiologische Optik*, p. 281.) There is therefore nothing in the nature of the colour to exclude complete saturation, although it may well happen that most of the browns we ordinarily see fall somewhat short of it.

In *Nature* of Jan. 26, Mr Munro calls attention to the great brilliancy and saturation of many natural yellows as accounting for the difficulty of resolving them into their components. It is, no doubt, quite true that a full yellow could not be compounded of such reds and greens as we come across in daily life, but it is equally certain that a drab or dilute yellow could be; and yet no one recognises the fact by his unaided senses, or thinks it anything but strange and unlikely when told of it. And after all can it properly be said that natural yellows are more saturated than other colours? That they approach more nearly the corresponding tints in the spectrum is admitted; but is that test a fair one? It seems to me that the homogeneous yellow itself must be considered as dilute when brought into comparison with the nearly primary red and green.

I have another difficulty in accepting Mr Munro's explanation. A suitable mixture of any red, green or blue will give a neutral grey. All four come within our every day experience; but such a result seemed to Goethe, soon after Newton proved it, a paradox of paradoxes; and I believe to unsophisticated minds it seems so still.

Mr Munro has ingeniously shown from the colour equations that there is no more primary blue in my blue disc than about $2\frac{1}{2}$ as much as in the red *plus* $1\frac{1}{3}$ as much as in the green—a conclusion which seems somewhat startling. In choosing the coloured papers and cards for the discs, I had great difficulty in finding a green that was even tolerably good, and the one that I finally used reflected large quantities of blue light. I had some thought of trying a green silk disc, which was of a much better colour, but feared errors depending on the different character of the surface.

It is not hard to see a reason for the comparative scarcity of good greens. To obtain a good red orange or yellow by means of absorption, all that is necessary is to cut away the spectrum above a certain point; for a good blue, the rays standing below a given one in refrangibility must be got rid of; but in order to isolate a green in anything like purity, the absorbing agent must hit off *two* points of the spectrum, removing all below one point and all above the other. The result is, that while nearly saturated yellows and reds abound—the scarlet of the geranium is almost perfect—hardly a good green is to be met with. The best I know is a mixture, prepared by adding bichromate of potash to a strong solution of sulphate of copper. The addition of a little chloride of chromium to remove the yellow more effectually is perhaps an improvement.

[1899. Further experiments upon the subject of this paper are described in *Nature*, vol. XXV. pp. 64—66, 1882.]