

If the order of the tints is recurrent, it is only using another word for the same fact to say that it is circular; and it is possible so to arrange the colours of the spectrum in a circle that any two tints which are opposite to each other shall be complementaries.

Grassmann's results are purely theoretical, but they coincide in a great degree with the experimental determinations of Helmholtz, and of Clerk Maxwell. In such experiments the method is to mix two or more coloured lights by letting them fall on the same spot of white paper. Mixture of colouring stuffs will not give the same result.

I now come to some remarks of my own on the theory of complementaries.

If colours are so arranged on the circumference of a circle that every tint has its complementary opposite to it, as has been done by Newton and by Grassmann after him, any two tints which are 180° apart are complementaries, and any two tints which are 360° apart coincide. If then the theory of the octave is true, of two tints which are 360° apart, the number of vibrations in a second (or the frequency of vibrations) of one is twice that of the other. It might be expected that the ratio of the frequency of vibrations between any two tints which are 180° apart, would be the square root of this; or, in other words, that when the frequency of the vibrations of any colour is known, that of its complementary might be found by multiplying or dividing, as the case may be, by the square root of two.

To put this in another form: If we so arrange the tints, from red to its octave, where purple turns red again, round the 360° of a circle, that any two tints separated by equal areas shall have their frequencies of vibration in equal ratios; then, as the frequencies of vibration of the two reds which are separated by 360° stand to each other in the ratio of 2 to 1, the frequencies of vibration of any two tints which are separated by 180° will be to each other in the ratio of the square root of 2 to 1. Now if the theory of a chromatic octave be true, the pair of tints which are 360° apart are exactly alike, and we might expect those which are 180° apart to be complementary to each other.

But this is not the case.

The ratios of the wave-lengths and of the frequencies of vibration (which, of course, are in the ratios of the reciprocals of the wave-lengths), corresponding to various tints, have been determined with great accuracy by Prof. Clerk Maxwell (*Philosophical Transactions*, 1860), by means of an interference-spectrum. The numbers in the following table, which are given on his authority, are the numbers of wave-lengths in the retardations; each colour is written in the same line with its complementary. In the case of bluish-green, blue, and indigo, I take the middle one of three places in the same colour.

Red	36.40	Bluish green	48.30
Orange	39.80	Blue	51.80
Yellow	41.40	Indigo	54.70

If the frequency of vibration of the colours in the second column were to that of their complementaries in the first, in the ratio of the square root of 2 to 1, the numbers would be—

Bluish green	51.47
Blue	56.28
Indigo	58.54

Thus the frequencies as observed were considerably less than as calculated from the hypothesis. The differences are all on the one side, and are much too great to be the result of any accidental error. The complementary tints in the foregoing table are not precisely opposite, but approach each other by the green side of the circle; and if from the portions of the circle from red to yellow, and from bluish green to indigo, any two tints are taken which stand exactly opposite, so that their frequencies of vibration are in the ratio of 1 and the square root of 2, their union will not give pure white, but white with a blue tinge.

But does this disprove the hypothesis that the true complementaries are those tints whereof the frequencies of vibration are in the ratio of 1 and the square root of 2? I think not.

Complementaries are usually understood to be tints, which by combination form a colour sensibly identical with that of sunshine. But is this correct? The solar spectrum is not pure, in consequence of the great number of absorption lines towards the violet end. That of the electric light, on the contrary, is free from absorption lines, and, in consequence of their absence, the electric light is sensibly bluer than that of the sun. If now the colour of the electric light, instead of that of sunshine, were taken as the true white, it appears probable that experiment

would show the frequencies of vibration in any colour and its complementary to be in the ratio of 1 and the square root of 2.

There are some remarks on this subject in the 2nd vol. of my work on "Habit and Intelligence," of which book you inserted a notice by Mr. Wallace on 15th Nov. and 2nd Dec., 1869, but it is more thoroughly thought out in this letter.

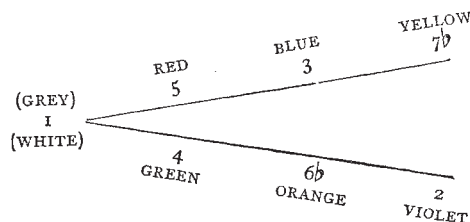
JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, April 16

M. RADAU, in his "Acoustique," says:—"The disdain with which most musicians repel the invasion of their domain by the exact sciences is to a certain extent justified." I venture to think it is very much justified, since little has been accomplished in aid of a technical theory of music by scientific men from Pythagoras down to Helmholtz. The highest service the mathematicians have rendered was to assist in destroying the application of their own theories by establishing the universally received system of "equal temperament." Now that the "effects" of colours are falling under the manipulation of mathematicians, could not the learned who are occupying your columns with the old discussion on "ratios" condescend to receive some warning from the history of "speculative music"?

One of your correspondents asks for a "white sound"! Seeing that a complementary colour completes the numerical value of the white rays more or less as the inversion of any musical interval completes the octave, is it unscientific to assume that the white ray must be the analogue of the monochord? Allow me to assume that it is so, and that white and black are complementaries, as M. Chevreul admits. Let me also assume that they are the two extremes of *light and shade*, including many gradations—many octaves—of intermediate shades of grey.

Taking any normal gradation of light and shade, and calling it grey or white, as the generator, the primary colours and their complementaries correspond to the harmonics, thus:—



The following series of figures

I	2	3b	4	4b	5	6b	6	7b	7	I
...
I	7b	6	6b	5	5b	4	3	3b	2	2b

represent what musicians call a table of inversions in the octave. It must be understood that in the system of inversions of numbers I employ, what is meant by a number and its inversion are the distinct notes in the scale the two numbers represent. For instance, 1 to 5 is C to G in ascending, and the inversion is 1 to 4, C to F—always in ascending, and counting from the generator No. 1.

Hence the following table of abstract intervals and their inversions, produced in regular order from the generator:—

Unison	Minor	Major	Minor	Major
I	5th	3rd	3rd	7th
White or Grey	Red.	Indigo.	Blue.	Yellow.
...
I	4th	6th	6th	2nd

White } Green. Chrome. Orange. Violet. Lavender. Green+Red (brown Octave)

Musically speaking, the generator No. 1, as the root of a natural dissonant chord 1, 5, 3, 7b, becomes No. 5 of the scale, or dominant of the key, the tonic of which is four degrees higher.

Consequently, if there be any analogy at all between sound and light, or between musical intervals and colours, the key-note of the spectrum would be *green*—the ray of medium refrangibility—and four degrees higher than the dominant or generator *white*. In modern views of harmony, I may remark it is not the concord or triad, but the dissonance, which is the basis of the technical theory.

Collecting, then, the abstract intervals given above, and con-

sidering them as representing separate notes, and arranging them in regular order, counting the original generator as No. 5, we get the following scale—major, minor, and chromatic—of F \sharp , or green:—

(Tonic)											
5	6 \flat	6	7 \flat	7	1	2	3 \flat	3	4	4 \sharp	5
C	D \flat	D	E \flat	E	F	G	A \flat	A	B \flat	B	C
Grey.	Laven- der.	Violet.	Indigo.	*Blue.	Green.	Red.	Orange.	Chrome.	Yel- low.	Lemon.	White.

* Indigo is, I think, a misnomer: it should be purple between blue and violet.

On the same system it is easy to construct an enharmonic scale on the principle employed by M. Chevreul. The double flats and sharps sometimes give ternary compounds. For example, 4 $\sharp\sharp$ equals green + red + red, and its inversion 5 $\flat\flat$ would give red + green + green. Some of the neutral greys, olives, slates, browns, &c., which would not appear in a table so constructed and calculated at a normal pitch, are produced by lowering the diapason.

From the above very brief explanation of the system of inversions, the following results may be suggested:—

1. That a table of colours of all gradations, with their complementaries, may be musically expressed in numerical notation with the greatest exactitude.

2. That, contrary to scientific opinion, it does not follow that because the red ray has the lowest degree of refrangibility, &c. &c., or perhaps because it happens to be at the bottom of the series of prismatic colours, it should necessarily be the initial note on the tonic of a scale.

3. Even if the red ray be the tonic, it does not follow that the scale of the spectrum should be *major*, as is too frequently given in elementary works on optics. By the system of inversions of numbers here presented, the scale of the spectrum appears, by disjoining the conjunct tetrachords, to consist of one tetrachord major and one minor, corresponding to the descending minor scale in use, of F \sharp minor, supposing C \sharp to represent the normal pitch of the dominant No. 5 corresponding to any given intensity of white light. Moreover, one conjunct tetrachord of the spectrum appears in *ascending* and one in *descending*, both tetrachords *converging* on the tonic.

4. If the analogy be true so far, there is only one colorific key. Modulation through a series of colorific keys, as in *modern* music, is impracticable. The reasons I have not space to explain.

J. G.

MR. SEDLEY TAYLOR has, it seems to me, written his criticism on my letter published in NATURE, Feb. 10th, far too hastily. I do not compare the diameter of the rings with one another, but *their cubes*, otherwise we should be led in establishing the musical

analogy to the absurd equation $\sqrt[3]{\frac{1}{2}} = \frac{1}{2}$. It would perhaps have been better to have said, that the ratios of the spheres described on the diameters of the rings, taken successively from red to violet, two and two together, the 1st to the 2nd and the 2nd to the 3rd, &c., give a series of fractions identical with those expressing the relative lengths of the musical chords from D to C, ascending and taken in like manner. As Mr. Taylor doubts Prof. Zannotti's accuracy, I will quote the following passage from Biot's "Precis Elémentaire de Physique," 3rd Ed., Vol. ii. Paris, 1824, p. 400, *et seq.* Speaking of Newton, "Il mesura les diamètres des anneaux simples de même ordre, dans la partie intérieure et dans la partie extérieure de leur périmètre, et en les considérant successivement aux limites des diverses couleurs du spectre a commencé par le violet extrême. Suivant sa méthode constante, il prit soin de lier ces résultats par une loi mathématique qui les représentât avec une suffisante exactitude. Il trouva ainsi que les diamètres, soit intérieurs, soit extérieurs, étaient sensiblement entre eux comme les racines cubiques des nombres $\frac{3}{2}, \frac{1}{2}, \frac{2}{3}, \frac{3}{4}, \frac{4}{5}, \frac{5}{6}, \frac{6}{7}, \frac{7}{8}, \frac{8}{9}, \frac{9}{10}, \frac{10}{11}, \frac{11}{12}, \frac{12}{13}, \frac{13}{14}, \frac{14}{15}, \frac{15}{16}, \frac{16}{17}, \frac{17}{18}, \frac{18}{19}, \frac{19}{20}, \frac{20}{21}, \frac{21}{22}, \frac{22}{23}, \frac{23}{24}, \frac{24}{25}, \frac{25}{26}, \frac{26}{27}, \frac{27}{28}, \frac{28}{29}, \frac{29}{30}, \frac{30}{31}, \frac{31}{32}, \frac{32}{33}, \frac{33}{34}, \frac{34}{35}, \frac{35}{36}, \frac{36}{37}, \frac{37}{38}, \frac{38}{39}, \frac{39}{40}, \frac{40}{41}, \frac{41}{42}, \frac{42}{43}, \frac{43}{44}, \frac{44}{45}, \frac{45}{46}, \frac{46}{47}, \frac{47}{48}, \frac{48}{49}, \frac{49}{50}, \frac{50}{51}, \frac{51}{52}, \frac{52}{53}, \frac{53}{54}, \frac{54}{55}, \frac{55}{56}, \frac{56}{57}, \frac{57}{58}, \frac{58}{59}, \frac{59}{60}, \frac{60}{61}, \frac{61}{62}, \frac{62}{63}, \frac{63}{64}, \frac{64}{65}, \frac{65}{66}, \frac{66}{67}, \frac{67}{68}, \frac{68}{69}, \frac{69}{70}, \frac{70}{71}, \frac{71}{72}, \frac{72}{73}, \frac{73}{74}, \frac{74}{75}, \frac{75}{76}, \frac{76}{77}, \frac{77}{78}, \frac{78}{79}, \frac{79}{80}, \frac{80}{81}, \frac{81}{82}, \frac{82}{83}, \frac{83}{84}, \frac{84}{85}, \frac{85}{86}, \frac{86}{87}, \frac{87}{88}, \frac{88}{89}, \frac{89}{90}, \frac{90}{91}, \frac{91}{92}, \frac{92}{93}, \frac{93}{94}, \frac{94}{95}, \frac{95}{96}, \frac{96}{97}, \frac{97}{98}, \frac{98}{99}, \frac{99}{100}$, lesquels représentent les longueurs que doit avoir une corde de musique pour produire toutes les notes d'une gamme mineure; c'est-à-dire, que si l'on représente par 1 le diamètre intérieur d'un certain anneau, lors qu'il est formé par les rayons rouges qui composent la partie la plus extrême du spectre, $\sqrt[3]{\frac{1}{2}}$ exprimera le diamètre intérieur du même anneau, quand il sera formé par les rayons qui sont la limite du rouge et de l'orange, et ainsi de suite jusqu'à $\sqrt[3]{\frac{1}{2}}$ qui représentera le diamètre intérieur du même anneau quand il sera formé par les derniers rayons violets pris à l'autre extrémité du spectre."

I can only add, that if Mr. Taylor doubts also the accuracy of M. Biot, he can easily refer to Newton's own treatise on colour. Rome, March 16

W. S. OKELY

The Barlow Lens

* I HAVE found the addition of a double concave lens to my telescope and microscope of so much service that I am anxious to call the attention of your readers to this simple application for increasing and improving the working power both of telescopes and microscopes. The application consists in the introduction of a biconcave lens in the adapter, which holds the eye-piece of the telescope, at a distance of two or three inches from the field-lens; as the focal length of the instrument is thereby increased, it is necessary to adjust the distance of the lens from the eye-piece according to the length of the adapter, so that the latter still admits of being drawn out sufficiently for focussing. A friend procured me several lenses of different powers at the ridiculous price of a shilling a-piece from an optician and spectacle-maker at Brighton, which answer admirably.

The chief advantage obtained by the use of this lens is the great increase of magnifying power without a corresponding loss of light. This is a great desideratum in looking at a planet, but it is equally important in separating double stars. With a low eye-piece of 60, my refractor (one of Cook's with a 3 $\frac{1}{2}$ in. object glass, and the addition of the Barlow lens) shows the Companion of Rigel beautifully.

I first became aware of this useful application many years ago, in reading Admiral W. H. Smyth's "Cycle of Celestial Objects." In page 343, vol. i., he states: "On receiving it, I directed the telescope upon a watch-plate fixed on a distant chimney, which quickly proved the power of the lens in enlargement, with scarcely any obscuration of light. While the image expanded under each progressive eye-piece, I was surprised at the additional advantage of its simultaneously flattening the whole field of view; and though the magnifying power became double on distant objects, the apparent magnitude of the spider-lines diminished in an equal ratio: a property which, with all powers above three hundred, is of considerable benefit to operations upon close double stars, and the finer micrometric desiderata. I afterwards raised the discs of the Satellites of Jupiter, and examined several double stars, with equal facility and advantage, the definition being quite distinct, and the stray light rather subdued than increased. After a little practice, I came to the conclusion that the achromatic concave lens will render the instrument to which it shall be applied equal to two telescopes for particular cases; for if a set of observations be made *with* it and another set *without* it, the errors of vision will be in some degree neutralised, or even done away with."

In spite of this strong recommendation I never gave it a trial until a few weeks ago, when a paper in the Polish language by Prof. Piotrowsky passed through my hands. It remains to this

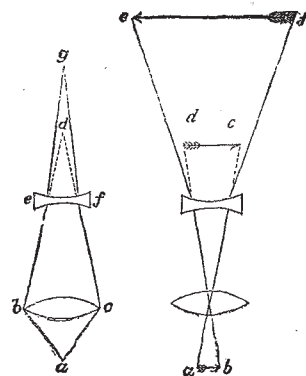


Fig. 1. *b c*, object glass; *c f*, Barlow lens; *d g*, foci of *b c*, with and without Barlow lens.

day a sealed book to me, but the two annexed figures taken from it leave no doubt in my mind that the paper treats on the same subject of which Admiral Smyth speaks so favourably. The result of my own trial made me regret having foregone for many years an advantage which I have every reason to congratulate myself on now possessing; but this circumstance it is also which induced me to ask for a small corner in NATURE for these remarks, when other more interesting subjects are less pressing than usual.

Walham Grove

F. d'A.