

It is hardly possible to exaggerate the importance of the question now under discussion in *NATURE*, "What was Kant's view of Space?" A mistake there is simply fatal. I therefore rejoice to find the columns of that paper are so generously thrown open to those who, like myself, are not primarily concerned with physical science. But this question, like all others in philosophy, has a proclivity to indefinite expansion, and unless its discussion be rigidly restricted to the main issue involved in it, the conductors of *NATURE* will have to ostracise it. Their space is not an infinite form, but a quantum to be carefully economised. It is, for example, an unwarrantable waste of that commodity to make Hegel the exponent of Kant on a point where Hegel taught that Kant was wrong.

It is fortunate for our interests, as students of Kant, that Mr. Lewes, while committing the strange oversight of criticising Kant's Intuition from Hegel's standpoint, in his last letter (*NATURE*, Jan. 27) enables us to *dénouer* the main issue from the mass of questions which entangle it. He evidently, if tacitly, slights the plank I threw to him, viz., that Thought, in its ultimate relation to Intuition, borrows, or has reflected on it, the forms peculiar to Sense. What are Kant's *Begriff vom Raume, Begriff der Zeit*, but this? (With these expressions, compare the following:—Also ist die ursprüngliche Vorstellung vom Raume Anschauung *à priori* und nicht Begriff. *Transac. Æsth.* s. 3, 4.) This reflection of form is not what Mr. Lewes is after. He maintains that, according to Kant, "the activity of mind is threefold—Intuitive Thought, Conceptive or Discursive Thought, and Regulative Thought." (Is not Regulative Thought discursive?) So, then, the main issue between Mr. Lewes and (I think) Professor Croom Robertson on the one hand, and Professors Sylvester and Huxley, Mr. W. H. S. Monck, and myself on the other, is plainly this. *Did Kant mean to teach that man has Intuitive Thought, i.e., Intellectual Intuition?* Now that I must be understood emphatically to deny; and in the event of the shortcomings of better men than myself, I hold myself prepared to establish the negative of that question, understanding by Thought the *genus* of which Understanding and Reason are *species*.

Ilford, Jan. 31

C. M. INGLEBY

Dust and Disease

THE extremely important discoveries brought to light by Professor Tyndall will call forth great exertions on the part of thinking persons to carry his plans into operation, and I have no doubt, when due precautions are taken to sift infected air as it passes into the lungs of those whose duties take them where contagion abounds, we shall have the happiest results.

So great will be the tide of interest in this direction, that I am anxious to cast into it a theory I have long held, in hopes that it may drift in some one's way to be turned to use; I commend it to the travelling portion of your readers especially.

Whilst travelling in some very unhealthy parts of Africa, more particularly amongst the marshes bordering on the Shire and Zambesi rivers, it was often necessary to camp at night just where the canoe happened to be moored when daylight failed us. Reeds, rushes, and mud were never many feet off, and the accumulation of scum, decaying vegetation, &c., lodged in the sedge, made the situation as delightful to mosquitoes as it was trying to the constitution of the European.

Still, with all this, as long as it was possible to rig up a mosquito curtain, I am convinced that really less danger existed in thus sleeping in the midst of miasma than in other places where less of it was supposed to be present, but where the traveller felt no necessity to stretch this thin covering over him.

I have in this way done canoe journeys of twenty to twenty-five days in length without a day's illness from fever, and I could instance similar experiences on the part of others.

Now the reason I assign is this: the mosquito curtain is to miasma, what the Professor's cotton-wool respirator is to the poison of scarlatina, we will say.

The curtain, after being used once or twice, saturated with dew, folded up whilst damp and crammed into the limited space generally provided for it in the safest place, becomes just so much affected by this treatment that each thread loses its smooth glaze, and is soon fluffy and fuzzy for want of a better expression.

The little honeycomb holes in the fine "net" are now a series of small six-sided sieves, each covered over with the fine filaments of cotton which have got disturbed and frayed up. Dew, falling upon a surface of this kind, quickly turns it into an exquisitely

fine strainer—in fact almost a film of water—through which all the air has to pass which is breathed by the person reposing beneath it.

Now, it is an old notion that the miasma which produces the bilious remittent fever (the pest of this part of Africa in question) and various other diseases of the tropics, cannot pass across water.

I believe that acting upon this theory, the Admiralty provides that boats' crews shall sleep in their boats anchored off shore in malarious rivers. However, be this as it may, I have a strong belief that the "wet sieve" *does* stop the poison in some way or other, and that it is a great safeguard to the voyager in these places.

The whole subject of miasm is in the dark; it is lawless as a cause of disease; it baffles the most astute, but the day may be coming when such hints as these of Prof. Tyndall's shall fit into an organised attack upon it, and we shall be able to overcome it in a measure.

A curtain, properly made, and taken care of with that instinct which alone is begotten by the buzz of mosquitoes, is perhaps the most valuable possession a man can have against deadly attacks in the night whilst men are asleep: were its merits studied more, we should not find men stuffing their companions so perpetually with quinine, to the keeping up an unhealthy tone by this abuse alone, and to the confusion of this most invaluable medicine when it is really called in to do its duty upon the fever-stricken patient.

Chatham, Jan. 24

HORACE WALLER, F.R.C.S.

Scenery of England and Wales

THE willingness you have hitherto shown to give authors an opportunity of defending themselves against being misunderstood, induces me to hope that you will allow me to disclaim being the author of certain statements, and to deny the truth of other statements, on which an anonymous reviewer in your last number mainly founds the charge of boldness he brings against me for writing the work entitled "Scenery of England and Wales," &c.

In one part of the review *I am made to say* that I "purposely refrained from reading;" in another it is assumed that my reading has "consisted mainly of the recent journals and magazines;" and further on it is asserted that I wrote the book "without reading."

The facts are, that for many years I devoted more or less time to reading on the subject of Denudation, and that, as stated in the Preface, until lately I purposely refrained from "reading *very much* (a distinct thing from not reading) lest a bias should be given to my opinions."

My reason for not quoting the remarks of the late Principal Forbes on the glaciers of Norway, was not, as implied by your reviewer, because I underrated the *denuding power of glaciers*, but because Forbes said very little on the subject.

Mrs. Somerville's estimate of the velocity of the Rhone may be incorrect, and perhaps, likewise, her statement that the declivity of the river is 1 foot in 2,620; but this is no reason why your reviewer should leave the reader to suppose that I misquoted Mrs. Somerville. In other parts of the work I have referred to the velocities of many currents besides the one off the southern promontory of Shetland.

The argument against denudation by currents, derived from the non-displacement of *barnacles*, would, I think, never be brought forward by any one acquainted with the fact that sea-waves often remove stones and large blocks while barnacles in the immediate neighbourhood are left undisturbed—that waves and currents, by their insinuating, undermining, overturning, and removing action, can carry on the work of denudation within a few inches of an unabraded rock-surface—and that a certain amount of resistance to be overcome is necessary to enable all denuding agents to produce effects which can be immediately perceived. On the western shore of Morecambe Bay, sea-waves and currents detach and remove fragments of limestone rock by a lateral process, while the brink of the unremoved mass of rock retains its glacial polish; and many other instances illustrative of this subject might be stated.

The fact that for more than twenty years I have confined my observations to England and Wales, and devoted nearly my whole time to visiting, revisiting, and studying every part of the country, is no reason why I should not have ventured to write a work on the Scenery of England and Wales in connection with Denudation. The country stands almost alone as regards the variety and importance of its geological phenomena, including

surface-features and types of scenery. My work is not confined, as your reviewer asserts, to a defence of marine denudation, for more than a third of it is devoted to the consideration of the real or assumed effects of atmospheric agents; and instead of being put forward in a self-confident spirit, as your reviewer would likewise lead the reader to suppose, I have stated in the Preface that "my object will be gained if I have said enough to stimulate the geologist and intelligent tourist to further observation."

D. MACKINTOSH

"Correlation of Colour and Music."

ANALOGIES between tone and tint are a tempting subject; and sound and light have enough admittedly in common to make it rash to say that the connection may not extend to their effects on the ear and eye; but that your correspondents (Jan. 13th and 20th) are seeking for unity in a direction in which it is not to be found, seems to me to be rendered pretty certain by the very evidence to which one of them, Dr. de Chaumont, appeals (Jan. 20th); I mean by that of "the researches of Helmholtz and others."

I have often wondered at the small attention paid to the general law which these researches have established. Even M. Jamin, in his *Cours de Physique*, dismisses Newton's theory of compound colours as "empirical," and apparently of no significance. It is as much and as little empirical as the Newtonian astronomy; both consist of general laws applied by means of particular constants: the evidence for the laws is in both cases equally inductive, and the determination of the constants equally empirical.

Stated without reference to the geometrical and dynamical analogies which I suspect have had something to do with obscuring its significance and tainting it with "empiricism," the fundamental law of composition of colour is this:—

Of any four colours whatever, either there is one which may be matched by a compound of the other three, or there are two which may be compounded so as to match a compound of the other two.

It is obvious that if negative values of an ingredient can be admitted, these alternative cases are the same; and the geometrical and dynamical analogies depend on the fact that, if addition of vectors is substituted for composition of colours, the proposition remains true, becoming in fact a very elementary one. And it follows that all colours may be co-ordinated, by means of three independent variables, with reference to any three colours whatever.

Accordingly, when differently coloured lights reach the eye together, the combination produces a single resultant colour varying according to the proportions of the ingredients, and completely superseding them; whereas, when two sounds of different pitch are sounded together, we still hear both: and, though we hear certain other tones besides, these other tones have each a pitch determined by the pitches, but independent of the intensities, of the original sounds.

The truth is, that the ear and eye deal with impressions in totally different manners. The ear deals with a complex musical sound exactly as a system of resonators does; it sensibly decomposes the sound into certain simple tones, just as the complex harmonic motion which produced the sound is theoretically decomposed by Fourier's theorem into the simple harmonic motions which would produce the simple tones. In order to understand the manner in which the eye deals with a compound colour, we must turn our attention to that particular unidimensional series of colours which constitutes the spectrum. As this is what your correspondents have done, the issue will be all the closer.

By the law above stated, all the colours of the spectrum might be co-ordinated with reference to any three colours chosen in the spectrum or out of it. But it has been ascertained by Mr. Maxwell* (to whose labours we are chiefly indebted in England for what we know of the composition of colours) that there are three colours in the spectrum to which all the rest stand in relations giving these the character of "primary" colours. They are the particular red, green, and blue, whose wave-lengths are, in Fraunhofer's measure, respectively 2328, 1914, 1717: and they divide the spectrum into three parts, in each of which every colour, it appears, may be matched by a compound of the two (out of these three) between which it lies. This is very accurately the case between 2328 and 1914 and between 1914 and 1717: it is much less accurately the case on the red side of 2328

and on the violet side of 1717; but in this region observation is difficult, and various eyes variable; and it seems probable that, as Mr. Maxwell infers, every colour in the spectrum, and therefore every colour in nature, is, as felt by us, a compound of three elementary sensations of colour excited separately by those three rays.

Now it must be observed that this result does tend to justify so much of the anticipations mentioned by Mr. Barrett as Sir John Herschel and Mr. Grove had long ago committed themselves to: it shows that the spectrum, like the musical scale, does in a manner return into itself. Beyond this the analogy fails.

In the first place there are, in music, no fixed tones with reference to which other tones possess any general properties at all; much less the property of being matched by combinations of them. In relations of tone, the constant quantities are not constant tones, but constant intervals between tones. Still, no doubt, if the three primary colours stood (as Dr. de Chaumont seems to think they do) in the arithmetical relations of tonic, fourth, and fifth, the fact would be as remarkable as two numerical coincidences could make it. But the case is not so. The ratio 2328:1914 (or 1'211) corresponds not to the interval of a fourth, but to an interval about two-ninths of the way from a minor third (6:5 or 1'2) to a major third (5:4 or 1'25); and the ratio 2328:1717 corresponds not to the interval of a fifth, but to an interval about a third of the way from a natural fourth (4:3 or 1'333) to a sharp fourth (45:32 or 1'406); intervals which, I presume, one can make nothing of.

Mr. Barrett's principal argument depends upon Prof. Listing's demarcation of the colours answering to the names red, orange, &c.: much too vague a basis, I should have thought, for exact inference, even if Mr. Barrett had not been obliged to sacrifice a boundary to obtain his most important interval; moreover the correspondences in Table IV. are somewhat exaggerated for orange and yellow by errors of computation. Mr. Barrett does certainly get a good fourth, fifth, and sixth; but these coincidences seem to me to offer a simpler and more effectual key than that which he has applied to the lock. What this key is will be evident on substituting for the numbers in Table I. or II., the reciprocals of the same numbers. Take Table I. and divide ten millions by each of the numbers. The results, with a column of differences, are as follows:—

1382'3	162'8
1545'1	162'6
1707'7	162'5
1870'2	162'7
2032'9	162'5
2195'4	162'5
2357'9	162'9
2520'8	

I suppose this speaks for itself. Professor Listing has simply divided his spectrum into seven equal parts upon some scale which varies inversely as the wave-length. Such a scale would of course be furnished by comparative rapidities of vibration; but it is no use guessing. Whatever led him to this particular measure, it is evident what his measure virtually was, and it nearly corresponds with the ratios of the musical scale because these approximately form a "harmonic" progression.

The other suggested analogies are less definite. "The juxtaposition of two colours nearly alike is *bad*," but surely not what would be called *discordant*, except for the sake of finding an analogy between colour and music. The fact probably depends upon the extreme sensitiveness of the eye to the effect called *relief*; a sensitiveness shared in a different degree by the ear, but shared also by all modes of feeling, even the least material, as men count materiality. The best results of juxtaposition are generally those given by complementary colours: but the relation between complementary colours is one which depends partly on relief and partly on the laws of composition above stated, and has nothing corresponding to it in music. But indeed I am surprised that anybody should even look for an analogy between the effect of *simultaneous* sounds and the effect of *contiguous*, not *coincident*, colours.

For these reasons I venture to think it is only by the unphilosophical restriction of the word *physical*, which excludes biological relations, that "harmony in colour and music" can be said to "have a common physical basis."

With regard to the coloured bands within the rainbow, it is not doubtless without solid reasons that Mr. Grove can have decided against identifying the phenomenon he describes with

* Phil. Trans. 1860, pp. 57–84. On the theory of compound colours and the relations of the colours of the spectrum.