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Physiological Optics and Physical Geometry

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Argument

Hermann von Helmholtz's distinction between "pure intuitive" and "physical" geometry must be counted as the most influential of his many contributions to the philosophy of science. In a series of papers from the 1860s and 70s, Helmholtz argued against Kant's claim that our knowledge of Euclidean geometry was an *a priori* condition for empirical knowledge. He claimed that geometrical propositions could be meaningful only if they were taken to concern the behaviors of physical bodies used in measurement, from which it followed that it was posterior to our acquaintance with this behavior. This paper argues that Helmholtz's understanding of geometry was fundamentally shaped by his work in sense-physiology, above all on the continuum of colors. For in the course of that research, Helmholtz was forced to realize that the color-space had no inherent metrical structure. The latter was a product of axiomatic definitions of color-addition and the empirical results of such additions. Helmholtz's development of these views is explained with detailed reference to the competing work of the mathematician Hermann Grassmann and that of the young James Clerk Maxwell. It is this separation between 1) essential properties of a continuum, 2) supplementary axioms concerning distance-measurement, and 3) the behaviors of the physical apparatus used to realize the axioms, which is definitive of Helmholtz's arguments concerning geometry.

Introduction

In the decades about the turn of the twentieth century, few topics were more popular among scientifically inclined philosophers and philosophically inclined scientists than the Helmholtz-Lie *Raumproblem*. In its philosophical guise, this problem challenged neo-Kantian philosophers and scientists to account for the advent of non-Euclidean geometries, and to explain whether, and how, Euclidean geometry could be said to be privileged, that is, why the space of intuition is, as was generally assumed, Euclidean, and not pseudo-spherical. And one is hard put to find a popular treatise on the subject which fails to consider the analogy between space proper and "spaces" of sensations, i.e. on the possibility of organizing colors, tones, or tactilia in manifolds, whose spatial structure derives from additive combinations of primitive sensibilia.[1] The favored example is inevitably that of colorimetry, and this with good

[1] Examples of better-known authors would include (Carnap 1922; Poincaré 1909; Russell 1996; Weyl 1927).

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reason: it was the only one of these phenomenological spaces to have been mapped out in any detail. Indeed, it is fair to say that the successes achieved in this one case by Helmholtz, Maxwell, Hering and others held out the promise of similar analyses of the other senses. Even though this promise was never truly fulfilled, this detail did little to detract from the charm of the notion. For what epistemological proposition could be more enticing to such men of science than the suggestion that the entire field of their perceptions was structured like a well-ruled graph?

To take just one example, in his *Philosophie der Mathematik und Naturwissenschaft*, Hermann Weyl connected his discussion of the various possible geometries to that of relativity theory with short excurses on Hermann Grassmann's vector calculus, and on the application of projective geometries to the continuum of colors. He concluded the latter passage by remarking that, "It is important for the philosophical discussion of geometry that there exists along with space another, quite different domain of givens – the colours – which form a continuum allowing of geometrical treatment" (Weyl 1927, 56). The modern reader may well suppose that, for such authors, the notion of a color-geometry was a mere diversion. But Weyl most certainly knew that the connection between the *Raumproblem* and colorimetry had been there from the beginning, for Helmholtz had done critical work in both areas.[2] Helmholtz himself emphasized the connections between color-theory and geometry, pointing out that when he first began his geometrical investigations "two examples were available to me in physiological optics itself of other severally variable [*mehrfach veränderliche*] manifolds which can be portrayed spatially; namely, the system of colors, which Riemann cites, and the measuring out [*Ausmessung*] of the visual field by the eye [*Augenmass*]" (Helmholtz 1868a, 619). In this physiological work, he was forced to consider the connections between measurement operations and spatial metrics, which connection, as is well-known, formed the crux of his later works on "physical geometry." In making this claim, I do not mean to suggest that *Riemann*'s work in analytical geometry departed from colorimetric considerations – only that Helmholtz's metrological interpretation of it did so. But since that interpretation dominated the discussion of the *Raumproblem* for several decades, having no little impact on Einstein's reflections in the early part of this century, the case at hand reveals an unexpected influence of nineteenth-century physiology into the core of twentieth-century physics.

Retinal Images

Before considering Helmholtz's papers on color perception in detail, I will touch briefly on the second example of a physiological manifold to which Helmholtz refers

[2] The same cannot be said for Carnap. In a footnote to his discussion of the spatial arrangement of colors in his thesis, he claims that such an "application of formal space to non-spatial objects is absent from the literature" (Carnap 1922, 80). This remark is hard to square with the long list of Helmholtz's works appearing in his bibliography.

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in the above quotation: the *Ausmessung* of the visual field by the *Augenmass*. The reference is to the work Helmholtz published in two papers from 1863 on eye motions.[3] The point of these papers was to give an explanation of Donders' Law, which stated that to each direction of the principal axis of the eye (to each direction in which one can look directly) there corresponded a unique angle of torsion, i.e. a unique degree of rotation of the eyeball about the axis determined by this line of sight. Helmholtz explained this phenomenon by arguing that it was obviously beneficial if the image projected on the retina by an object retain a rigid connection (*einen festen Zusammenhang*) among its points as the center of vision was moved about the object. This criterion was easily fulfilled when the motions were slight, or when they lay along "meridians" passing through the primary position (that in which the eye looks straight ahead). In the case of large motions that did not follow meridians, however, it could not be met, thus some distortion was inevitable. The angles of torsion which Donders' Law predicts are precisely those, Helmholtz argued, which result in a minimum of such distortions when the eye sweeps the visual field. Simply put, we do our best to preserve the geometrical relationships holding between the points of the image on our retina as we scan.

It is not hard to see how this research connects up to one major strand of Helmholtz's arguments concerning the status of geometry. As is well known, Helmholtz sets out in those papers (Helmholtz 1868a; Helmholtz 1868b; Helmholtz 1870; Helmholtz 1878a; Helmholtz 1878b) to define the conditions under which measurements in space can be carried out at all. Since, he argues, measurements depend essentially on our being able to establish relationships of congruence between bodies in space, we must postulate that these bodies preserve rigid connections between their various points independently of their locations and orientations. This property was not fulfilled in the case of "the *Ausmessung* of the visual field by the *Augenmass*," for an after-image (corresponding here to a displaced rigid body, or measuring instrument) which under some motions would appear congruent to another image in the visual field, would fail to be so in others. This difference depended on the initial position of the eye, as well as on the path it had to take to get to its final one. Visual afterimages do not, in other words, fulfill the condition of free mobility that Helmholtz set on his rigid bodies:

> This means in other words that the congruence of two spatial images is not dependent on their situations, or, all parts of space are congruent to one another if one disregards their delimitation [*wenn von ihrer Begrenzung abgesehen wird*], just as all parts of the same spherical surface are congruent with regard to the curvature of the surface, if one disregards their delimitation.

[3] (Helmholtz 1863a; Helmholtz 1863b). These papers are discussed in (Lenoir 1993; Turner 1994; Turner 1996).

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> The visual field displays a more limited mobility of retinal images on the retina. I have dealt with the particular consequences which ensure for the estimation of distances by means of the eye [*Augenmass*] in my physiological optics. (Helmholtz 1868a, 624)[4]

In fact, there are notable similarities among Helmholtz's methods in "Über die Tatsachen, die der Geometrie zum Grunde Liegen" and "Über die normalen Bewegungen des menschlichen Auges." In both cases, Helmholtz postulates that internal relations between a system of points remain constant (in the geometrical case) or are held to a minimum (in the optical one) as the system undergoes infinitesimal displacements; in both papers, he goes on to derive expressions for all possible transformations of the co-ordinates of the system that are consistent with his respective postulates.

This connection can help us understand the interpretation that Helmholtz gave of his geometrical arguments up to and including the last of his papers on the subject, "Über den Ursprung und Sinn der geometrischen Sätze; Antwort gegen Herrn Professor Land" (Helmholtz 1878b). There Helmholtz argues against his neo-Kantian opponent in a purely "idealistic" voice, asking the latter to concede that appearances in space must have two sort of causes, "topogeneous" and "hylogeneous moments." When we talk about bodies moving about in space, we are always talking about an interplay between both sorts of moments or causes: that is to say, any description of the behavior of material bodies in space involves reference to connections between material properties, such as colors and tactilia, and spatial locations. An image on the retina is composed of stimulations of color receptors at particular points (Lotzian *Lokalzeichen*), and a moving image may, or may not, preserve its internal relations as the eye sweeps its field. Similarly the combinations of material and spatial properties making up a *Raumgebilde* may or may not preserve their structure as we move them (they might, after all, simply disappear, the object simply evaporate, the moment we touched it).

This aspect of Helmholtz's physiological work gives us a partial insight into his thinking. It reveals both the mathematical source of some of that work, as well as the sort of models Helmholtz had in mind when proposing methods for "visualising" non-Euclidean geometry. But it does not get to the origin of Helmholtz's most important insight, which concerned the interrelation of intuitions, measurement and axiomatic systems. It was in this regard that "colour-geometry," to use Weyl's phrase, played a critical role. To make this point clear, I shall begin by considering the various possible interpretations one can offer of Helmholtz's four seminal papers in geometry. For even a cursory consideration of the arguments in his geometrical papers leaves the reader wondering *in what sense* Helmholtz thinks geometry is an empirical science.

There are several possible alternatives. Most simply, his position might be that space can be "curved" in many different ways. We cannot be sure what this intrinsic

[4] Robert DiSalle touches on this point in DiSalle 1993, 511.

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curvature might be without carrying out empirical investigations. A more sophisticated argument might be that, although a number of geometries are consistent with physical space, only one can be accommodated to physical theory. A further possibility is that geometry is in itself a form of physical theory, in that it describes not the properties of space itself, but only those of bodies in space. Lastly, Helmholtz's point may be that geometry is neither a physical science, nor a science concerned with space itself, but rather a science of measurement, which is empirical because it is necessarily tied up with the physical systems used in measurement. I will be urging the last interpretation in what follows. However it must be conceded that Helmholtz does not systematically distinguish between the above alternatives, and that he makes numerous statements which, on their own, support each of them. So we must begin by looking at the main lines of argument of the four papers in question.

Here we find two quite distinct lines of argument: a positive "transcendental" (Torretti 1978, 168) argument dealing with the physical bases of measurement systems; and a negative one concerning the intuitive imaginability of alternative geometries. The first of these is advanced in the two 1868 papers. The second argument is the main concern of the 1870 paper, and the last paper, from 1878, combines the two. This development is largely the result of Helmholtz's overlooking, in 1868, the possibility of a pseudo-spherical geometry. In the two papers of that year,[5] Helmholtz had argued that geometry presupposed that it made sense to speak of relations of congruence between arbitrarily distant, and arbitrarily situated forms in space (*räumliche Gebilde*). Axioms and theorems speak of line segments of equal (or greater, or lesser) length, and postulates enjoin us to construct the latter. They thereby presuppose that we can ascertain whether two line segments stand in these relations. This supposition entails that it be possible to compare such forms with one another, and therefore that one can make such comparisons by means of translating and rotating rigid bodies. In consequence, geometry presupposes a factual basis, namely that bodies and motions suited to the measurements operations presupposed by the axioms are in fact available. In 1868b, Helmholtz derived a unique expression defining distance in those metrics compatible with these conditions, and showed that this expression agreed with that of Riemann for the Euclidean metric. Shortly after announcing this result, however, he realized that his analysis did not disallow pseudo-spherical metrics. He concluded that geometry was empirical in a second, stronger sense. Euclidean and pseudo-spherical geometries disagree with regard to the behaviors of rigid bodies; alternatively, for the same phenomena, they disagree on what counts as a rigid body. The arguments concerning the visualizability of non-Euclidean geometries are, in the papers of 1870 and 1878, aimed at showing that there is no logical or intuitive impediment to imagining movements of rigid bodies,

[5] The first paper (1868a) is falsely dated 1866 in Helmholtz's *Wissenschaftliche Abhandlungen*. This error is generally reproduced in the literature, and often leads commentators to overlook the fact that the argumentation of these two papers is quite distinct from that of the subsequent two.

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or, in the 1878 paper, "physically equivalent magnitudes," which are in accordance with a pseudo-spherical metric.

The second of these arguments obviously builds on the first. In the first, Helmholtz maintains that geometrical propositions *have meaning* if and only if certain contingent facts obtain. In the second, he argues that when two competing geometrical systems depend on distinct sets of such facts, asking which geometry is valid will be equivalent to asking which set of facts is the case. The first step has been called a "transcendental" one, because it argues that if (mathematical) language concerning distances in space is to make sense, then certain possibilities must be realized. The second seems more straightforward: if neither logic nor intuition can decide between two exclusive possibilities, then it is a contingent and empirical question as to which obtains.

There are two fundamental objections to Helmholtz's claims. First, as Helmholtz himself acknowledged, it is hard to see how we could know whether the facts at the basis of geometry obtained without assuming it in the first place. If we had to determine whether or not a body which had been rotated was rigid, we could not do so without assuming that the question made sense in the first place. Practically, this means that the mere coincidence of two bodies at various points in space does not entitle us to call them *rigid*; furthermore, if we do choose to do so, we are appealing tacitly to the idea that they have not suffered change due to forces, in other words that we have a notion of lengths underlying and independent of the bodies in question. So "rigidity" depends on "equal length" conceptually, not the other way round. This calls the first of the arguments into question (cf. Wahsner 1994, 250–251). Second, any set of bodies whose behaviors we take as supporting one or the other geometrical candidate can, under suitable transformations of mechanical laws, be redescribed as non-rigid, or as subject to the actions of forces. This criticism, which Helmholtz also anticipated, is targeted at the second argument. Taken together, these two objections undergird Poincaré's conventionalist critique. Given that the objections are good ones, and that Helmholtz had raised them both, why did he not see them as calling his project into question?

A common answer to this question is that Helmholtz was hindered from seeing their pertinence by his commitment to a physiological conception of space (Hatfield 1990, 220ff.; DiSalle 1993, 514). Because he failed to grasp the constitutive role of spatial intuition in Kant's epistemology, he assimilated the latter's remarks concerning the necessity of geometrical knowledge to claims about the character of subjective experience (Hatfield ibid.; DiSalle ibid., 505–506). On this reading, it is the alternative visualizations which do the work for Helmholtz, for they were to demonstrate that Kant was wrong in thinking that experience had to conform to geometry. This deficiency is reflected in both of the arguments described above. For in both cases, Helmholtz is mistaken in thinking that he can establish concepts like "rigid body" or "physically equivalent magnitudes" without appeal to an *a priori* framework. A different line is taken by Gregor Schiemmann (Schiemann 1997, 219ff.), who has argued recently that Helmholtz's investigations into geometry are fundamentally determined by a "mechanistic world-view." On Schiemann's account,

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Helmholtz is blind to the second of the above-mentioned objections because he believes that geometrical properties can be derived from mechanical ones.[6] Since, however, one cannot appeal to prior knowledge of mechanical laws in order to select an appropriate geometry, the claim that one or the other geometry is empirically valid cannot be maintained.

As there are numerous passages to support both views, it would be senseless to argue that Helmholtz is entirely innocent of these charges. Nevertheless, both the psychologistic and mechanistic readings pay too little attention to the centrality of measurement in Helmholtz's papers.[7] For Helmholtz, his professional interest in the physiology of the senses notwithstanding, characterizes geometry repeatedly as a "science of spatial *measurement*." And his discussions of the relations between alternative geometries and mechanics stress geometry's role in making the quantitative description of spatial objects and events possible. How is this explicit emphasis borne out in the structure of Helmholtz's arguments?

Let us begin with the so-called "transcendental" argument of the first two papers. Geometry is defined there as a science of spatial measurement. By this, Helmholtz does not mean, however, that geometrical propositions are statements about inherent properties of space. He seems instead to complicate the relations between 1) axiomatic systems and 2) the spatial continuum, by introducing a third element 3) the measurement operation. The possibility of carrying out 3) in practice is a condition for the significant application of the propositions of 1) to the elements of 2). More precisely, the availability of 3) is the empirical condition under which measurements which satisfy the axioms of 1) can be applied to objects situated in 2). For if there were no such operations, distinct spatial objects might intuitively appear to satisfy relations such as "longer" or "shorter," or "congruent," but there would be no guarantee that these relations would be found to hold when the bodies in question were superimposed on one another.

In his popular lecture of 1870, Helmholtz emphasizes the possibility of imagining both pseudo-spherical and Euclidean universes, arguing that the behaviors of the objects used for carrying out measurements (our most rigid bodies) can be consistently imagined as being either Euclidean or pseudo-spherical. Nevertheless, he admits in conclusion that a "stringent Kantian" might choose to hold to Euclidean

[6] "Instead of recognising that one cannot derive the Euclidean character of real space from the validity of the mechanical law of inertia, [Helmholtz] commits just this fallacy" (Schiemann 1997, 233). But Schiemann's claim rests on an apparent misinterpretation of the passage he refers to (Helmholtz, 1870, 29). Helmholtz argues there that we are free to regard the world in which we live as having a pseudo-spherical geometry. This would entail our reformulating the law of inertia (among others), from which Helmholtz infers that there is an essential connection between geometrical and physical principles. Schiemann takes Helmholtz to be arguing that because we know that the law of inertia is true, we can infer that Euclidean geometry must be true, because only it is compatible with that law. Thus, according to Schiemann, Helmholtz "tacitly assumes that it could only come to such a change [in the law of inertia] in the unreal case that our measuring instruments were not subject to the same curvature [of space] as the objects to be measured."

[7] Carrier and Wahsner address this topic in greater detail.

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geometry in the face of such evidence, and would thereby demand that the objects involved in measurement operations conform to requirements that derive "analytically" (purely logically) from the axioms he held to. This possibility exists precisely because it is only once we have supplemented geometrical propositions with physical ones, such as the principle of inertia, that they gain "a real content, that can be confirmed or rejected by experience" (Helmholtz 1870, 30). Finally, in his 1878 paper, Helmholtz mobilizes this latter insight by arguing that the sufficiency of a method of measuring must in the last instance be evaluated relative to a concept of "physically equivalent magnitudes" derived from physical considerations: physically equivalent magnitudes are ones in which the same processes can exist or take place in equal periods of time. Helmholtz does not specify how such equal periods of time are to be determined.

In contrast to the argument of the first two papers, which claimed that Euclidean geometry was the unique logical consequence of requirements placed on measurement,[8] the latter argument departs from the assumption that measurement must define and quantify those spatial magnitudes that are presupposed by physical laws. In both cases, however, Helmholtz's claim is that in order that certain metrological concepts – equal length, physically equivalent magnitude – have a determinate meaning, a measurement procedure must be at hand.

In other words, what is characteristic of Helmholtz's critique is 1) the decoupling of the topological properties of space from its metrical ones, and 2) the claim that the metrical properties are the product of measurement operations. As I argue in the following section, Helmholtz had encountered theoretical and practical difficulties in his research on color which were exactly analogous. To see the relation between the two research programs, I will begin by giving a description of the work on color, after which I shall return to the later papers on geometry. This first section draws heavily on a number of recent studies on Helmholtz's work in physiological optics, but differs from them in an important respect (Kremer 1993, Sherman 1981, Turner 1994). Since previous authors have been concerned with physiological optics per se, they have understandably identified only in passing, if at all, those aspects of that work which are related to geometry. Most important of these is the emergence of the distinction between measuring *sensations*, and measuring their causes. A preliminary statement of this difficulty may serve to keep the main line of argument clear. The result of the research program described in the following was the "trivariance" or three-receptor theory of color perception, which says that color impressions are produced by the actions of three fundamental color receptors. These receptors were assumed by both Maxwell, and later Helmholtz, to respond differently to light of varying wavelengths. The reason that colors can be organized in a plane, or space, is that one can regard these receptors as the physical grounds of three independent variables defining a continuum. But the three-dimensionality of the space can be

[8] Thus that it followed, in the language of the third paper, "analytically" from these requirements.

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captured by other variables, such as color-tone, brightness, and hue. Just as physical space can be *described* by any set of three linearly independent vectors, the same holds true of the color-space. But unlike physical space, the color-space has an "objective" ground,[9] for one set of such vectors – those physically realized in the receptors – is presumably the unique cause of the space's existence. Furthermore, the responses of these receptors to physical light presumably give the color-space a uniquely correct metrical structure.

A central difficulty that emerged in Helmholtz's and Maxwell's work after 1855, however, revolved around the following difficulty: they were convinced that the above account was correct, but they had no way of identifying the primitive receptors, let alone specifying exactly their response curves. Later research (not described in this paper) centered on developing methods to do this. But both in 1855, and in the first edition of the *Handbuch* in 1860, the claim that one had identified the "empirical" structure of the color-space was untenable. One way to make this point simply is as follows: assume that you accept the trivariance theorem. You also accept that the excitations of the three receptors in response to light of a given wavelength can be quantified, because it is a physical process. Does it follow from this that the subjective impressions of the three fundamental color-sensations can be compared quantitatively in the same way?[10] There is no inherent quantitative relation between pure impressions of red and green. Thus any mapping of the colors onto a spatial structure will simply ascribe to them conventional metrical relations, in specifying what counts as "equal brightness" and how the "quantities" of these sensations are to be mapped onto quantities of light. In his last paper on geometry, Helmholtz criticized the notion that we could have intuitions of equal spatial magnitudes (or that, if we did, they had any privileged significance) on just these grounds: it is only the connection of those spatial magnitudes selected as equivalent by means of a measurement procedure to spatio-temporal processes that carries objective meaning.

In order to see how these difficulties emerged, I discuss four publications in detail: Helmholtz's 1852 "Über die Theorie der zusammengesetzten Farben"; Grassmann's 1853 "Zur Theorie der Farbenmischung"; Maxwell's 1855 "Experiments on Colour"; and Helmholtz's 1855 "Über die Zusammensetzung von Spectralfarben." Although my subject is Helmholtz, the work he drew on in this period was not his alone: a key role was played by the mathematician, Hermann Grassmann, as well as by the young James Clerk Maxwell. It was only considerably later that Helmholtz adequately acknowledged the influence of the former on his work, mainly in the

[9] The point is merely that we do not believe that there is a single "correct" set of basis vectors that describe physical space. I am not, quite obviously, going to speculate about the ontological basis of physical space in this paper.

[10] The problem of specifying a procedure for quantifying sensation led to an entire research program, founded by Fechner, in "psychophysical" measurement. Whatever the merits of that later work, in the 1850s the results were not yet in. On the contrary, the problems encountered there were instrumental in bringing the latter field into being.

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second edition of the *Handbuch*, as well as in his late work on measurement, *Zählen und Messen*. Maxwell was never given his full due. In any event, there can be little doubt the formulation of the problems which, in my view, were then transferred by Helmholtz to geometry proper, was essentially determined by the two other men. Since all four papers were attempts at analyzing and developing the mixture-table, or color-wheel, that Newton had described in his *Opticks*, I begin by outlining Newton's theory, and then discuss each of the four papers in chronological sequence. I then return to the connections between the color-space (which is what it had become by the end of this period) and Helmholtz's geometrical papers.

Newton's Barycentric Color Wheel

The "barycentric" color wheel was presented in Newton's *Opticks* as a calculating device, whose function was to permit one, "In a mixture of Primary Colours, the Quantity and Quality of each being given, to know the Colour of the Compound" (Newton 1931, 154). The colors of the spectrum were arrayed around the circumference of the wheel (fig. 1) in arcs of seemingly arbitrary length at each of whose middle-points a load-point (*p*, *q*, *r*, . . .) was inscribed.

Newton explained that in order to locate the color resulting from mixing two or more spectral colors one had simply to locate that point in the wheel which lay at the

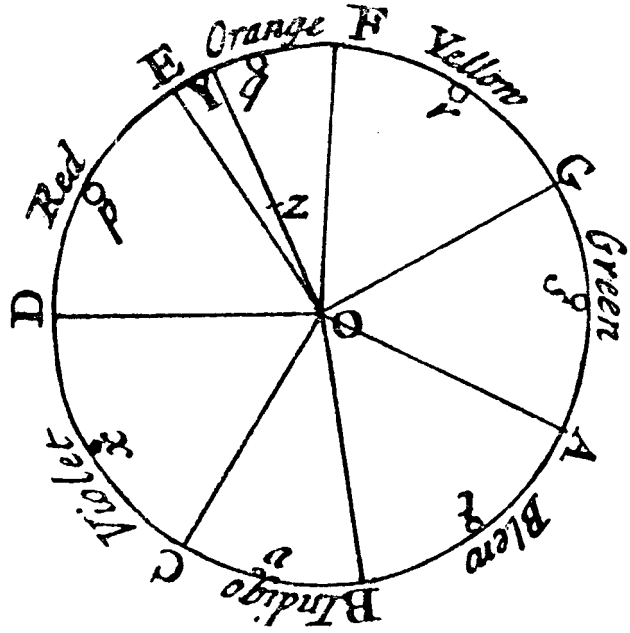


Fig. 1. Newton's Color Wheel.

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"Center of Gravity" (ibid., 155) of the several colors. That is, one imagined a weight placed at the appropriate load-point of each of the colors in the mixture, and determined the point z in the circle that would lie at their center of gravity. The weights of the components were to be determined by "the Number of Rays" of each spectral color entering into the mixture. One could then find the color of the latter by extending a line from the center $\boldsymbol{O}$ through z. The distance of z from $\boldsymbol{O}$ would "be proportional to the Fulness or Intenseness of the Colour, that is, to its distance from Whiteness" (ibid., 155–56).

Newton did not think that the spectral colors arrayed around the circumference of his wheel were properties of light itself.[11] But he assigned different statuses to the rays evoking these colors: white and (presumably) mixed colors, which are always paler than their spectral counterparts, do not stand in a one-to-one relation with a kind of ray, as does the sensation of red to a "Rubrifick" ray,[12] and this difference is encoded in the form of the color-wheel, and in the accompanying directions for its use. By selecting points on the circumference, one determines the locations of points on the interior. But no line connecting interior points ever intersects the exterior, and each chord passes through only interior points, which geometrical relations reflect the fact that mixed colors cannot yield pure ones, and that no pure colors mix to form another one, respectively.

Newton did not explain how he had arrived at this exact form for the wheel, nor what he meant by "the Number of Rays" that each color in the mixture was to have. He also did not explain what, if anything, the wheel might represent, that is, whether it was anything beyond a device that allowed one to arrive at an answer to a particular question.

Helmholtz's 1852 "Über die Theorie der zusammengesetzten Farben" (Helmholtz 1852b)

Newton's theory distinguished between pure and mixed colors, but it did not have an equivalent to the "primary" colors which formed the basis of color-tables in the industrial/arts tradition, and which had played a central role in the theories of Young and Brewster.[13] Helmholtz, in his *Einladungsschrift* of 1852, divided the primary-color theories into three groups, "explicitly fram[ing] his investigation as a test of these agnostic (painterly), physical (Brewster), and physiological (Young) views of the primaries" (Kremer 1993, 228). These theories asserted,

[11] "Colours . . . in the rays are nothing but their Dispositions to propagate this or that Motion into the sensorium, and in the Sensorium they are sensations of those Motions under the Forms of Colours" (ibid., 125).

[12] "The Rays in that mixture do not lose or alter their several colorific qualities, but by all their various Kinds of Actions mix'd in the Sensorium, beget a sensation of a middling Colour . . ." (ibid., 158).

[13] Accounts of the theories of Brewster and Young can be found in (Kremer 1993; Sherman 1981).

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> 1) either that the primary colours are those out of which all possible others may be composed;
> 2) or, as with Mayer and Brewster, that the primary colours correspond to three objective kinds of light;
> 3) or that they correspond to three distinct primary sensations of the optic nerve fibres, out of which the other colours sensations are composed, as with Thomas Young. (Helmholtz 1852b, 7)

The question Helmholtz set out to answer was, in other words, the following: Where does color-mixture take place? These various theories made room for a wide variety of experimental results, and one may take it that Helmholtz's aim was to decide among them as best he could, as opposed to discrediting them all. But even this reading may credit Helmholtz's rhetoric too much, for he, in the opening paragraph of the paper, makes quite clear which option he prefers:

> Light rays of various wavelengths and colours differ in their physiological action from tones of various vibration periods and musical pitch essentially in that each pair of rays, acting simultaneously on the same nerve fibre, call forth a simple sensation, in which even the most practised sense-organ cannot distinguish the individual combined elements, ... This unification of the impression of two distinct colours in a single new colour impression is obviously a purely physiological phenomenon, and depends only on the characteristic reactions of the optic nerve. (Ibid., 3)

Whereas a practiced ear could distinguish the component simple tones in a complex chord, the same was not true of complex colors; and whatever the number of fundamental colors, the bulk of those we sense are the results of *physiological* mixture. As Newton before him, Helmholtz took spectral light-rays to be simultaneously of "differing wavelength and colour."[14] For Newton, fundamental light-rays of a given refrangeability and color were such that – whatever their phenomenological qualities – they could not be decomposed by physical means, e.g. prismatically. A primary-color theory suggests, however, that some of the colors around Newton's wheel are themselves "mixed." And so the questions arose: What is mixed? And how? Brewster's claim had been that the light itself was mixed, and that prismatic decomposition was not the last word: he had decomposed prismatically "pure" light with filters. Helmholtz rejoined in a simultaneous article against Brewster that he had been unable to repeat these experiments (Helmholtz 1852c). Such a separation could not be achieved if the spectral light had been adequately purified (and that using methods depending only on refractive properties). Of course this could not rule out the possibility that some third method (neither refractive nor

[14] "Es bleibt also der von Newton angenommene Zusammenhang der Schwingungsdauer oder Brechbarkeit mit der Farbe unverändert bestehen" (Helmholtz 1852a, 600).

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filtrative) would not reveal, say, spectral yellow light to be mixed. But it would have to be shown that the lights in question were indeed pure in respect of their wavelengths if such a positive conclusion concerning their physical composition were to be drawn (ibid., 44). It followed that, if anything could be meaningfully said to be mixed, it would have to be the sensations. Nonetheless the variables subject to experimental control could only be the wavelengths and the intensities of pure spectral light, for there was no way of operating on the sensations directly.

Although he had designed an apparatus which afforded him control of both light intensity and color, in 1852, Helmholtz was able to replicate Newton's results only partially: some chromatometric aspects of the color-wheel were maintained (red and yellow were shown to yield an orange which was barely distinguishable from spectral orange), but the overall topology of the wheel, that is to say the generality of Newton's barycentric equations, could not be maintained, for Helmholtz could not find more than a few of the complementary pairs (pairs of spectral lights which mixed to yield white) that Newton's wheel predicted. The problem was not that, as he claimed in his next paper on the subject, he had been misled by "peculiar physiological properties of the human eye" (Helmholtz 1855, 46).The method he had used had not allowed him to produce enough spectral light, nor indeed to adequately control its intensity. Helmholtz mixed his spectral lights by superimposing two spectra, and adjusting their relative widths. The quantities of lights entering into the mixtures could be only roughly adjusted, and certainly could not be measured. But this difficulty only concealed a further one, which was to become critically important later: in what sense could one's attaching measures, however imprecise, to the spectral lights be seen as equivalent to measuring phenomenological colors?

Response to Helmholtz's 1852 paper was critical, which is not surprising when one considers his largely negative conclusions. Brewster's theory, Young's theory, indeed Newton's theory were all wrong; and although it was possible to mix a large number of pigment-colors from only a small number of pigments, the same did not hold for the lights which conveyed their colors to the eye. Helmholtz himself could not have been satisfied with his results, which contradicted common sense. After all, even pigments mixed more predictably and from a smaller number of primaries than Helmholtz's results suggested light did. But pigments are also easier to measure, whereas both the wavelength and quantity of spectral light are not. So in the following years, armed with a far more sophisticated apparatus, Helmholtz returned to the fray (cf. Kremer 1993, 232). But in the interim, the mathematical basis for his research had been radically changed.

Grassmann's 1853 "Zur Theorie der Farbenmischung"

Probably the most important stimulus to this further work was a paper published by Hermann Grassmann in 1853 on the "Theory of Colour-mixture." Today recognized as the founder of vector analysis, Grassmann was also an accomplished philologist,

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publishing a translation and dictionary of the Rig-Veda, as well as on phonology.[15] Grassmann's paper applied the calculus of his neglected 1844 *Lineale Ausdehnungslehre* (Grassmann 1878) to the structure of Newton's color wheel, introducing four principles of color-mixture which were used as formal axioms to derive the geometrical properties of the wheel. The wheel became, on this analysis, a two-dimensional cut through a three-dimensional color-space, whose dimensions were, in modern terminology, the hue, brightness, and saturation of the various colors. But this description does not do justice to Grassmann's insight, for the most remarkable feature of the *Ausdehnungslehre* was that it described spaces as the products of *operations* with fundamental *additive magnitudes* (thus the term *lineal*) (cf. Beutelspacher 1996, 5).

According to Grassmann's analysis, each color impression was completely determined by three magnitudes, representing the color-tone (the hue), its quantity (brightness), and the quantity or intensity of admixed White (the saturation) (Grassmann 1853, 70). Spectral colors retained the special role they had in Newton's analysis: only they had a direct relationship to wavelength, and all other colors were composites of them. This property had an exact mathematical correlate in Grassmann's axiomatized system: the value of the third magnitude (the saturation) of the pure spectral colors was always zero, i.e. the intensity of a spectral color was given entirely by the second magnitude, representing the brightness of that color, whereas the intensity of a mixture was distributed between the magnitudes for brightness and saturation.[16] A mixture of two (or more) spectral colors is located, as with Newton, at a point on the interior of the circle which represents the "centre of gravity" of the colors in question. But on Grassmann's wheel, the distances of that point from a) the center and b) the periphery represent its brightness and "quantity of admixed white," respectively. Thus Grassmann used his two-dimensional surface to represent three independent quantities, where the third quantity – absolute brightness – was a function of the brightnesses of the colors in the mix. He summarized these relations in a single fundamental proposition:

> From this it follows that, if one conceives the entire mass as unified in the centre of gravity, in which case one calls the centre of gravity loaded with such a weight the *geometric sum* of the individual points loaded with their weights, then every light-impression can be represented exactly, in accordance with its three moments, by a given point loaded with a given weight. The direction from the centre in which this point lies, or, just as well, the point at which this direction intersects the circumference, represents the colour-tone. The weight of the point represents its total intensity. The product of this intensity and the distance from the centre represents the intensity of the colour, and the

[15] In the 1870s, Grassmann published a paper paralleling the one discussed here, in which he attempted a planar arrangement of phonemes by means of his *Ausdehnungslehre*. The *Ausdehnungslehre* itself was modeled on Leibniz's "universal characteristic."

[16] This relation was secured by his fourth principle, on which more in the following.

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product of the intensity with the distance from the circumference represents the quantity of admixed white. (Ibid., 82)

For Grassmann, the location of a point within the circle specifies the color-tone and the *ratio* of brightness to saturation, and the absolute intensity of the mixture in question is given by a distinct value, represented as a weight. The location of a given mixed color would be determined by means of the weighted vector sum of the colors entering in the mixture (a "geometric sum" in the terminology of the *Ausdehnungslehre*). The mixture-law can be described by means of a two-dimensional representation only because his fourth law – that the intensity of a mixture (the combined brightness and whiteness) equals the sum of the intensities of its components – made those values linearly dependent.[17] Although he presented this analysis as a pious defense of Newton's doctrine, Grassmann achieved with it a level of generality which was at best implicit in Newton's own account. The truth of his axioms conceded, Grassmann could prove formally that Helmholtz's results had to be wrong. This proof is of particular interest because it depended only on his axiom of continuity (each color would change continuously as its brightness, spectral color or saturation was changed) and on the assumption that the spectral colors could be arranged in a closed loop about the wheel within which the point representing white was enclosed – in other words, Grassmann argued from purely topological characteristics of the color-plane that there had to be more complementaries than Helmholtz had been able to find.

Two points stand out: 1) Spectral colors are distinguished within the mathematical representation only in their having no saturation, that is, only in their third co-ordinate being zero; 2) Grassmann's third law, which states that the actual composition of a given color is irrelevant to its effects in a given mixture (mathematically, that all sets of vectors summing to a given vector are equivalent as regards the contribution of that vector to further sums) suggests that the choice of those colors which serve as a basis for a given portion of the color wheel is – mathematically – irrelevant to the laws of color addition which hold on the wheel, i.e. that the additive properties holding within the wheel can effectively be dissociated from any one set of vectors spanning it (that was indeed the strength of the vector-calculus Grassmann had developed). Of course each one of the infinite set of spectral colors was still assumed to be a physically unmixed primary, even though the space of all color-impressions was definable by means of only three variable quantities, as

[17] "dass die gesamte Lichtintensität der Mischung die Summe sei aus den Intensitäten der gemischten Lichter. Hierbei verstehe ich unter der gesamten Lichtintensität die Summe aus der Intensität der Farbe, wie ich sie oben festgestellt habe, und aus der Intensität des beigemischten Weiss . . ." (Grassmann 1853, 82). That is to say: the mixture law is composed of a function mapping four variables (the colour-tones and -brightnesses of the components) onto two (the colour-tone and -brightness of the mixture) and a second function mapping two independent variables (the intensities of the components) onto one (the intensity of the mixture). Newton's wheel did not clearly distinguish these relationships.

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was the one operation – mixture – on this space. For the fact that the vectors added only positively meant that none of the spectral colors could be synthesized (since any addition of colors of distinct tones would decrease the saturation, that is increase the whiteness, of the mix), from which it followed that only the full complement of spectral colors sufficed to span the color plane under the single additive operation which Grassmann allowed.

We can illustrate the exceptional role of the primary colors by means of a naive question: What lies outside Newton's and Grassmann's color-wheel? On their intended interpretation, the answer is: nothing, for the points on the circumference represent colors of maximal tone-intensity and minimal whiteness; these points lie at the limit of possible perceptions. At the same time, however, the planar representation and the mathematics that describe it do permit a hypothetical answer: the points outside the color wheel would represent colors of still greater tone-intensity, with negative whiteness (super-saturated colors, in current parlance). Such colors could provide a basis for a space greater than that of the color-wheel, a space within which the spectral colors themselves could be additively synthesized. This is, as we shall see, what Maxwell in fact proposed when he reanimated Young's theory, to which Helmholtz also eventually subscribed. So it is worth inquiring what speaks (or spoke) against this move.

Newton, as we saw, ascribed his spectral colors a completely different status from those mixed out of them. They stand for physically unmixed lights, and, at the same time, they are the colors out of which the other colors are combined. In other words they name spectral lights directly, and these lights function in turn as simples. The colors within Grassmann's and Newton's wheels have neither of these properties. They are, we might say, equivocal: each of them may be physically decomposed, and each allows of different decompositions; furthermore such a decomposition can (although it need not) be specified by means of other color-terms, which may themselves be equivocal in just this sense. None of these authors consider that the primitive elements of sensation-mixtures might have *no* unique correlate in what Helmholtz called the "objective" properties of light. Even if Helmholtz was prepared in 1852 to consider the question of whether, say, the sensation of pure spectral yellow is "mixed," he still assumes that the fundamental elements of that mixture will be colors, i.e., that these elements will themselves be sensations that denote pure spectral lights. And so even Grassmann, whose mathematical analysis opens up the possibility of abandoning this assumption, still argues, at the end of his paper, that the first question to settle is how the "homogeneous colours" are in fact laid out around the circumference on Newton's wheel (Grassmann 1853, 83–84). Helmholtz, as we shall see in the next section, did not quite agree with this: in his 1855 paper on color-mixture, he assumes that determining the *shape* of Newton's wheel is just as important. He does not, however, doubt that this can be settled "objectively," i.e., by determining the relationships holding between what he too calls "homogeneous colours."

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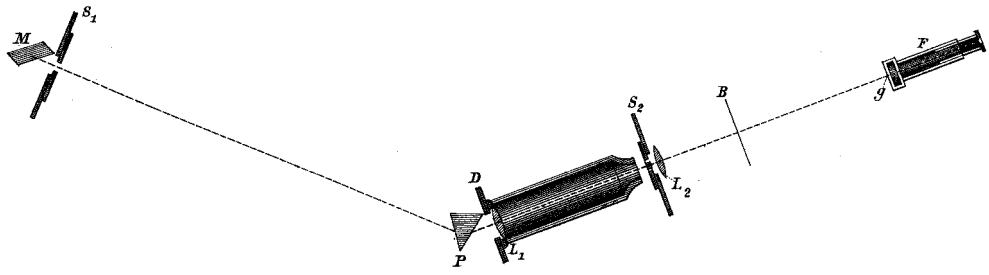


Fig. 2. Helmholtz's color mixture apparatus from 1855.

Helmholtz's 1855 "Über die Zusammensetzung von Spectralfarben" (Helmholtz 1855)

What Helmholtz needed to achieve this latter goal was what the French physicist Léon Foucault graciously offered him: "a simple method for combining in any proportion all kinds of simple rays, determined by their position in the spectrum, with the same rigour and ease as when mixing material colours."[18] Foucault's method allowed one to project two pure spectral colors onto a large surface, while screening out other light sources. The mixture could be observed directly, instead of being viewed through a telescope. More importantly, however, it allowed precise control of the quantities of the spectral lights entering into the mixture.[19] With this new set-up (fig. 2) Helmholtz could overcome the problems on which his earlier research had foundered, and which had let him to reject Newton's planar representation of colors.

His apparatus worked as follows. A beam of light from the heliostat M is projected through the diaphragm S_1 (which blocks out superfluous light), is diffracted into a spectrum by the prism P, after which it is focused by the objective lens L_1 on the screen S_2, on which two adjustable slits filter out pure spectral light from different regions of the spectrum. The two beams of light emanating from S_2 are then focused by L_2 on the imaging screen B. Helmholtz was justifiably proud of the screen S_2 (fig. 3), which was depicted not only in the article, but in both editions of the *Handbuch*.

The small white rectangle at the center of the screen represents the region on which the spectrum was projected. The two diagonally mounted plates were moved by means of the screws d and δ, so as to set the slits l and λ anywhere along the spectrum. The widths of the slits were adjustable by means of the screws m and μ. Thus the four screws provided control of both essential magnitudes of the light in the mixture: spectral color, and quantity (intensity) of the light.

[18] (Foucault 1853a) reprinted in (Foucault 1878, 52). German translation: (Foucault 1853b).

[19] Foucault seems to have exaggerated the claim cited above, for his version of the screen described in the following did not allow the experimenter to vary the intensities of the lights after the screen had been put into place (cf. the editor's comments on Foucault's apparatus in Foucault 1878, 52–53). This would have made it prohibitively difficult to calibrate the intensities as Helmholtz did.

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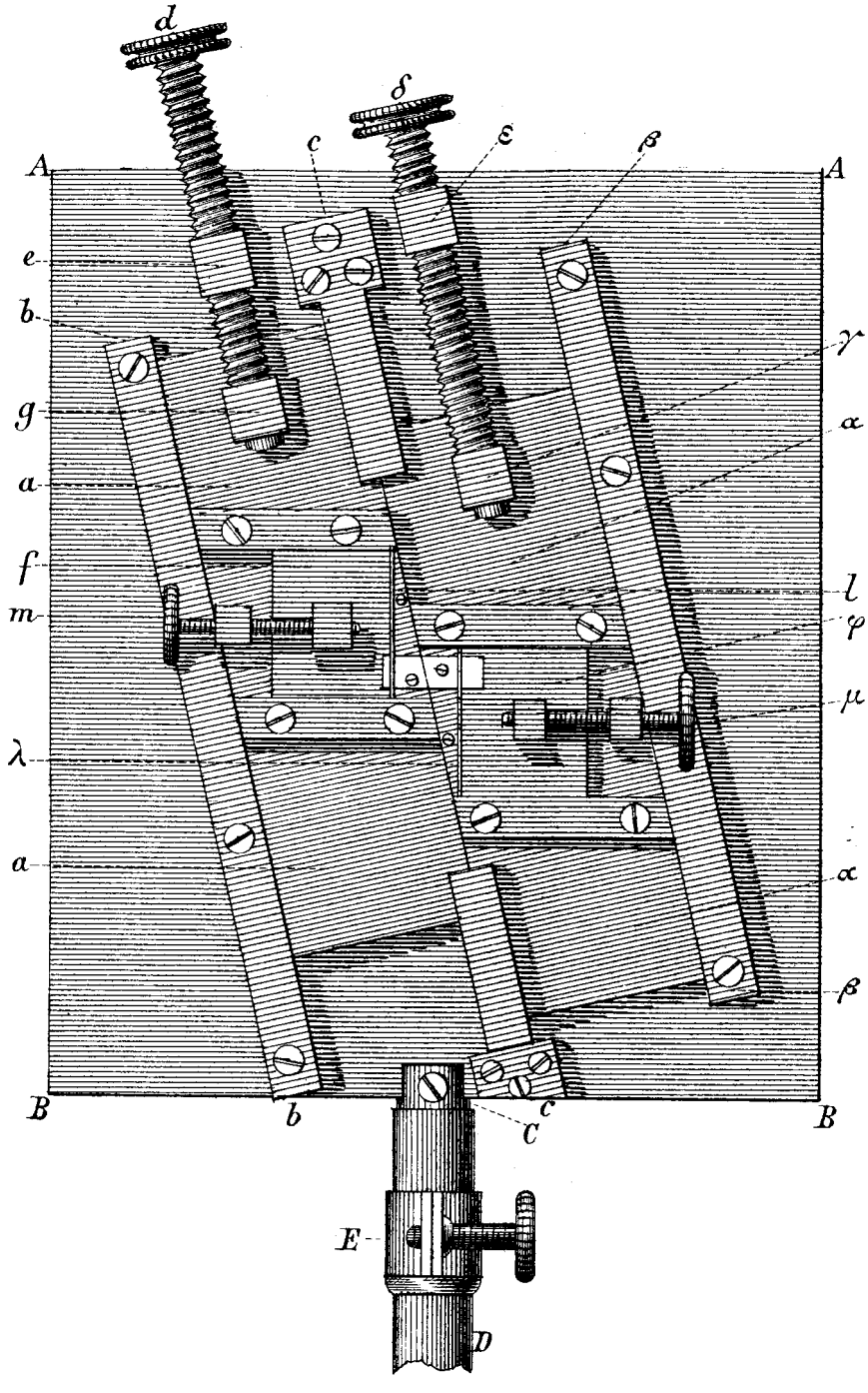


Fig. 3. The screen for parceling out quantities of colored light.

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In order to quantify the former, Helmholtz removed the projection screen, and viewed S_2 through the telescope F, to the front of which he had attached a vertically striated glass plate. This produced a series of spectra on both sides of the slit, whose apparent separation he measured against a millimeter scale etched on the back of S_2. This distance reflected the diffraction of the light by the striae on the glass plate, and was thus a function of its wavelength. By measuring this value for a number of Fraunhofer lines and comparing them to Fraunhofer's published measurements of their wavelengths, he was able the translate the diffraction measurements into wavelength values. The widths of the slits he measured microscopically once he had achieved a mixture yielding a proper white.

As I explained above, Grassmann had argued that from his first two principles – that all mixed colors can be represented as a mixture of a spectral color with white, and that a mixture changes continuously when one or the other of its components changes continuously – it followed that each spectral color would have a complementary. This argument presupposed that the spectral colors themselves could be arranged in a continuous loop, i.e. the circumference of the color-wheel.[20] Since his proof rested on assumptions of continuity and ordering only, it did not show *which* pairs of colors would be complementary, nor in what proportions such pairs would have to be combined. Helmholtz's improvement to Foucault's apparatus lay in his being able to control both the locations and the widths of the slits on his plate – thus the color-tones and their intensities – continuously while the apparatus was in place. This enabled him to isolate many more complementary pairs than he had been able to do in 1853. These pairs were of particular interest precisely because they were assumed to lie on opposite sides of the color-wheel, of which white was the colorless center, thus their positions could define – empirically – its shape.

It was the *shape* of the curve, however, which lay at the root of Helmholtz's disagreement with Grassmann on the significance of the latter's fourth principle. That principle stipulated that the intensity of a mixture of lights be equal to sum of the intensities of the lights composing it. But, Helmholtz observed,

> This fourth principle can be applied in three essentially different ways, depending on the method one uses to lay down the intensity. First, one could call the light-intensity of distinct colours equal when they appear equally bright to the eye . . . . Second, one could declare the determination of the measurement-units of differently coloured light to be

[20] Grassmann's proof (Grassmann 1853, 73–75) is too intricate to give in full. In essence, he proves that 1) Each color A has a color A' "opposite" it on the color-wheel, in that colors on either side of A' produce distinctly *colored* sets of mixtures when mixed with A in varying intensities; and, on the assumption that A has no complementary, this property of A' leads to a contradiction, for 2) If one considers two colors B and C infinitely close to A' on either side of it, it follows that a) the sets of mixtures of B and C with A must differ only infinitesimally, although b) A' was defined in 1) in such a way that the members of the two sets consist of distinct color tones. Since a) and b) contradict one another, A' must produce white when mixed in some proportion to A.

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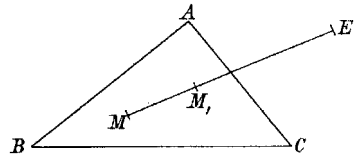


Fig. 4. Locating colors on the mixture plane.

> arbitrary, and take this fundamental principle to mean that there was a way of determining the units according to which the light-intensity of the mixture was always equal to the sum of the mixed lights. . . . The third way is this: one says that one has found a method for determining the intensities of different coloured lights, and one demands that the fundamental principle be correct for this particular way of reckoning intensities as well. This is how Grassmann proceeded. (Helmholtz 1855, 67–69)

In a detailed scholium to the second interpretation of Grassmann's principle, Helmholtz showed that the shape of the curve bounding the color-field depends in large measure upon arbitrary assumptions. In choosing any three colors (mixed or unmixed) and specifying their locations and units of brightness, one would of necessity specify the location of every color that might be composed out of them. By extension, one would specify the location of all further colors, including the spectral ones. Assume we have chosen *A*, *B*, and *C* as our basis colors, and have determined the proportions – expressed in terms of arbitrary units for the intensities of the three colors – in which they mix to yield *M* (fig. 4). Then a color *E* lying outside the triangle *ABC* (that is, which cannot be mixed out of them directly) can be located, and its unit of intensity defined, by finding a further color M_1 lying inside of *ABC*, and which in turn is produced by mixing *M* with *E*. *E*'s location and brightness are functions of those of *M* and M_1, and their locations and brightnesses depend only on those stipulated for *A*, *B*, and *C*. Thus *E*'s location and brightness also depend only on the latter specifications.

Depending on the three colors chosen, the locations assigned them on the plane, and the methods for measuring their intensities, the other colors could be distributed in many different ways. Each color-plane so determined could be mapped, by means of linear transformations, onto the others, while no one of them could be seen as *the* color plane. In each case, the spectral colors, although still lying at the boundary of this field, would also be arrayed in accordance with the choice of *A*, *B*, and *C*, and the units of their intensity. As I remarked above, the spectral colors were represented, in the mathematics of Grassmann's representation, no differently from the mixed ones (all were vectors, or *additive Grösse*); however, the method he described for constructing the wheel assumed that they were laid down first. Helmholtz here identified the possibility, although he did not make use of it, of reversing Grassmann's procedure, and thereby determining the locations of spectral colors on the basis of stipulated locations for mixed ones.

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Thus according to the second interpretation of Grassmann's fourth principle, the shape of the color wheel, and the distribution of colors within it depended essentially on conventions of measurement: in modern terminology, all these color planes had the same affine structure, and the various possible arrangements represented nothing more than various choices of basis colors and their units. Nevertheless, Helmholtz did not conclude that each such plane was equally objectively valid. The first and third interpretations of Grassmann's fourth principle *did* allow one to fix the shape and distribution, albeit on quite different assumptions: in the first case, one would say that two colors were equally bright when they appeared to be so, quite independently of their physical characteristics, and one would then derive the color-plane implied by that stipulation; in the third case, one would stipulate the conventions of measurement and the spectral curve which were to hold, while altering the arrangement of the colors to conform with these stipulations. Grassmann had defined the intensity of the spectral lights to be such that equal quantities of complementary lights mixed to form white light, from which it would follow that all spectral colors would be equidistant from a white center.[21] But his method could lead to unforeseen and unintuitive results. In fact, Grassmann's assumption that the spectral colors formed a loop could only be saved by admitting mixed colors to the circumference of the wheel, for there are no spectral colors "between" violet and red.[22] Perhaps more seriously, on Grassmann's approach, the notion of perceived brightness would in turn have to be sacrificed to brightness as defined in accordance with the above definition. And, finally, the arrangement[23] of the colors around the periphery of the plane would depend on these stipulations as well.[24] Grassmann's approach, in Helmholtz's opinion, saved the mathematics at the expense of the phenomena. Such a color-plane could serve, as it had for Newton, as a means of calculation, but its structure would correspond directly neither to the objective structure of physical light, nor to the subjectively experienced, intuitive relations holding among color-sensations.

Helmholtz opted for the first interpretation of Grassmann's principle: define spectral colors as equally intense when they are perceived as being so, and let the mixed colors fall where they might on the plane. Projecting two complementary lights on his screen, he adjusted the slits until the mixture showed white. After measuring the width of the slit of the (apparently) brighter light, he placed a rod in the path of the lights and narrowed this slit until the two colored shadows thrown by

[21] In fact, this requirement could be met for up to at most five spectral colors, so the resulting curve would not be perfectly circular.

[22] In (Grassmann 1877) he accepted this result, but felt his method basically untouched by this development.

[23] Not their order, but their proportions on the circumference: certain ranges of colors would occupy more or less of the circumference.

[24] He complained in 1878 that his demonstration "must have [contained] significant conceptual difficulties, since even Helmholtz did not fully understand it" (Grassmann 1877, 218). He, in turn, does not seem to have understood Helmholtz fully. As I discuss below, the dispute and misunderstandings derived from their quite different interpretations of the nature of the color-space itself, that is, of what it in fact represents.

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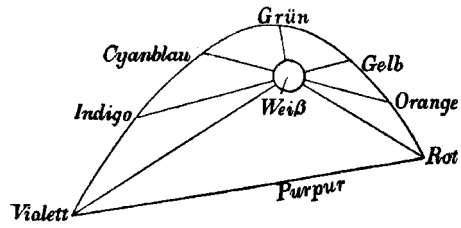


Fig. 5. Helmholtz's "objective" color wheel.

the rod appeared equally dark. He then measured the slit at its new setting. He set the ratio of the intensities needed to produce white to the ratio of the slit's widths in the two settings. So he found that, green-yellow being apparently brighter than violet, the slit-width for green-yellow at which the two colors showed white was one-tenth the width at which they threw equally dark shadows. Thus the ratio of the brightness (intensity) of spectral violet to spectral green-yellow was 1:10, and the "distance" of green-yellow from white would be one-tenth that of violet from white. Proceeding in this manner with the other three pairs of complementary colors he had found, he defined the relative weights of each pair, and supplemented these numerical relations with informal observations of mixtures that were not white (red with equally bright green was found to give a reddish orange, etc.) (Helmholtz 1855, 64). These results he graphed in a flattened color-wheel (fig. 5). Since Helmholtz, in violation of Grassmann's expectation, had been unable to find a spectral purple, he filled in this portion of the spectrum with a line of purplish colors produced by the various mixtures of red with violet. They served as ersatz complementaries to green. The remainder of the curve he traced roughly in accordance with the weights determined in the slit-adjustment experiment.

Maxwell's 1855 "Experiments on Colour" (Maxwell 1855)

Helmholtz was not the only one to read Grassmann's paper. It may well be that Riemann had done so as well. And Maxwell drew on it in the research he presented to the Royal Society of Edinburgh in March 1855 in a paper entitled "Experiments on Colour." Here Maxwell used Grassmann's principles to construct a normal color-plane, which he then compared with ones for red-blind subjects. He offered the differences between the two as evidence for a trichromatic theory, and thereby both explained the source of Grassmann's three independent variables and opened the way for regarding the set of colors as making up a manifold in Riemann's sense of the term. The paper had three parts: first, Maxwell described in detail a method for situating colors on the plane determined by three arbitrarily chosen basis-colors; second, he advanced a somewhat improved version of Young's trichromatic theory, suggesting that every color was the product of three elementary nervous processes; last, he produced evidence for this claim by comparing the colors-planes described by

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normal subjects with those arrived at by the color-blind. So Maxwell committed himself first to a version of Young's trivariance theory, which Helmholtz had rejected in 1852, and simply did not mention in his 1855 paper.

Whatever the theoretical overlap between the two men's work, Maxwell had a decided advantage over Helmholtz in one respect: he used a low-tech color-top (which he called a "chromatic teetotum") instead of the high-tech light-mixing apparatus available to established researchers like Foucault and Helmholtz. Maxwell's teetotum allowed him to produce incomparably more – and in a certain sense more precise – data than Helmholtz had been able to do. By altering the areas occupied on the top by different color samples, Maxwell could generate accurate numerical values for the "quantity" of each color going into the mixture. Furthermore he did not compare *colored* mixtures to one another, but to the central region of his top, on which separate black and white papers were set to produce a distinct shade of white/grey. If, say, three colors could be arrayed on the wheel in such a proportion that they showed a colorless grey, then it would be possible to set the Black and White papers in such a proportion that they showed the same shade of grey. The areas of all the papers (read off a scale on the circumference) would then constitute a color-equation, e.g.

$$\text{a)}\quad .37V + .27U + .36EG = .28W + .72\ Bk.$$

Maxwell chose Vermilion (V), Ultramarine (U), and Emerald Green (EG) as his basic colors, emphasizing that they differed in hue from those commonly thought to be "primary" colors, indeed that their choice had been in large measure arbitrary, that they were nothing more than "kinds of paint."[25] The value for white obtained when these colors were mixed, he then normalized by a factor of $3.57 = 1/0.28$, using this value to adjust the coefficients of all other equations he arrived at. When multiplied with the raw weights of a), this factor yields the quantities of V, U and EG which would be required to yield one brightness-unit of W. So, for example, in order to locate Pale Chrome (PC) on the plane, he took the following equation, in which PC has been substituted for the basic color V, and the proportions of U, EG and W have been adjusted to yield a match between two greys – the one resulting from the colored swatches, the other from the black and white ones.

$$\text{b)}\quad .33PC + .55U + .12EG = .37W + .63Bk.$$

Multiplying through by the normalizing factor 3.57, one gets:

$$\text{c)}\quad xPC + .55U + .12EG = 1.32W$$

[25] "The reason for this deviation from the received system is, that the colours on the discs do not represent primary colours at all, but are simply specimens of different kinds of paint, and the choice of these was determined solely by the power of forming the requisite variety of combinations" (Maxwell 1855, 276).

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Maxwell then solved for x = 1.32 − (.55 + .12) = .65, calling this result the "corrected value of .32PC" (Maxwell 1855, 280).[26] It specified uniquely the location of Pale Chrome on the plane.

The need for this adjustment is obvious from the equations a) and b). The swatches V, U, and EG mix to form a darker white than do PC, U, and EG, as is reflected in the lower value for W (.28 as opposed to .37). Thus the effective brightness of the swatch of PC used must be greater than that of the swatch of V used in the basic color equation a). Normalizing its brightness to that of V means increasing its coefficient accordingly. This adjustment was a direct application of Grassmann's fourth principle (the brightness of the mixed color must equal the sum of the brightnesses of the component colors), which serves to emphasize just how much Maxwell's approach assumed the basic correctness of that mathematical analysis. In essence, Maxwell chose the second of Helmholtz's interpretations of Grassmann's fourth principle: selecting three arbitrary colors, whose brightness units were implicitly determined in the choice of the "kinds of paint" representing them, he constructed the color plane by defining the locations and brightness units of all other colors as functions of those six posited values. This procedure can be visualized as follows. Imagine that the colors form a roughly conic solid, whose axis runs from a pure white apex to a pure black point at its base. Cuts perpendicular to the axis represent color planes of constant saturation radiating from a grey point to a circumference along which lie the spectral colors. Maxwell's procedure begins by selecting three arbitrary points in this solid, which together define a plane cutting through it. He then determined the positions of these three points on that plane relative to the axis of the solid (equation 1). Using the color equations resulting from measurements with the top, he then situated other colors to construct a color-plane which, like Grassmann's was a projection of the surface of the cone onto that plane.

The mere fact that three colors sufficed to determine the shape of the plane suggested that three fundamental colors would indeed be sufficient to generate all the others in a manner consistent with Grassmann's analysis, but for one niggling point: no pure spectral colors could be synthesized – the purity of the mixed colors was always inferior to that of the spectral colors. And even if one ignored that fact, it remained the case that, as Helmholtz argued, the shape of the color-plane (whose contour was the spectral color curve) depended on the choice of the three basis colors used in a particular experimental set-up, along with their postulated units of brightness. So one would not, without further refinements, be able to figure out *which* three colors were most like, i.e. closest to, the true primaries on the color plane by carrying out measurements of this sort. Maxwell disposed of these difficulties by taking two radical steps: first, he argued that the three true "primaries," whatever they were, *did not lie on the color-plane*; second, he compared the measurements he had obtained for the bulk of his subjects (where there was very good agreement) with

[26] Maxwell means .33PC.

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those of the color-blind subjects, and used these results to "locate" the elementary red-sensation.

What previous advocates of Young's theory had failed to understand, Maxwell argued, was that the three fundamental retinal processes, which Young had claimed corresponded to the three sensations of red, green, and violet, were not identical either with those three color-sensations, nor were they directly connected to "the lengths of the undulations of the various rays of light" (Maxwell 1855, 284). Each of these three "systems of nerves . . . acts not, as some have thought, by conveying to the mind the knowledge of the undulation of light, or of its periodic time, but simply by being *more* or *less* capable only of increase or diminution . . ." (ibid., 283). Every light-ray whose wavelength falls within the visible range acts on all three systems simultaneously, even if it is a pure spectral ray. The range of colors which we normally perceive can be represented as a figure inscribed within the triangle of possible "mixtures" of these three elementary color-sensations (fig. 6, the circle is Newton's wheel; the vertices of the triangle are Maxwell's elementary, or "pure" sensations).

Nonetheless, the exact location of the new "pure sensations" (ibid., 295) could not be described by means of observations of colors lying within the circle, and that for two reasons, the one mathematical, the other physical. 1) Any number of non-collinear triplets of points lying outside the circle could form a basis for it (that is, any three that enclosed it), so any three such points might represent the three elementary sensations. In the terms of Grassmann's vector analysis, one could not draw conclusions about the vectors – thus the system of co-ordinates – which formed the

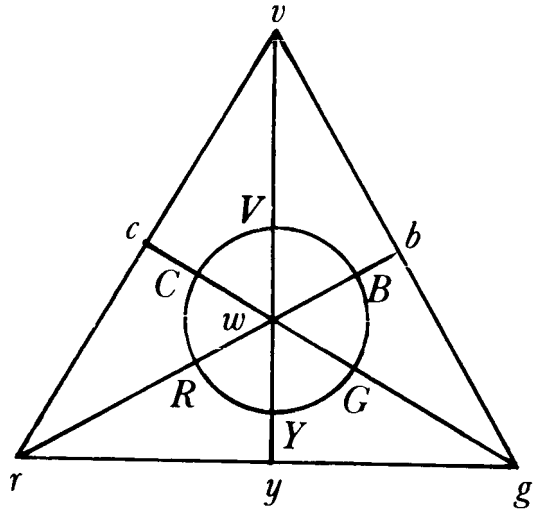


Fig. 6. Maxwell's extended color-plane.

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physical basis of the color-plane by considering its structural properties alone.[27] 2) If we had access to the physical correlates of the colors lying outside the wheel, but inside the triangle, we might overcome this difficulty. But the colors within the wheel were all and only those held to have a physical cause, that is to say, whose ultimate correlate was posited outside the mind and body of the subject. And Maxwell and Helmholtz could alter only *such* causes directly. The difficulties encountered in manipulating and measuring the elements of the color-plane had been up until this point largely technical and definitional. But on the new theory, these elements had dropped out of sight for the subject, and slipped from the experimenter's grasp.[28]

By the publication of the first edition of the *Handbuch der physiologischen Optik* in 1860, Helmholtz had also converted, without giving much credit to Maxwell, to exactly this view. His interest in the exact nature of the elementary sensations, and in the mathematical description of the color-space to which they gave rise, continued until his death: the second edition of the *Handbuch* contains an extension of the trivariance theory, in which the elementary sensations are explicitly identified as mathematical functions realized in the conductive nerve tissue (Helmholtz 1911, 341–343); and Helmholtz attempted, in a series of papers in the 1890s, to determine the "metric" of the color-space (Helmholtz 1891a; Helmholtz 1891b; Helmholtz 1891c). This sequence of experiments, and the theoretical difficulties they engendered, had a deep influence both on Helmholtz's epistemology and on his later conception of geometry. So let us return to these connections.

Geometry

In setting out a mathematical method for describing color mixture, and supplying at least preliminary physical and physiological interpretations of that method, Grassmann's, Helmholtz's, and Maxwell's papers formed the basis on which all subsequent work on colorimetry drew. As such, they mark a watershed in the history of sense-physiological research. But all three men were equally well interested in the more general problems raised by this work, which centered on the correct methodology for determining the structure of a perceptual continuum. In the course of this investigation, it had emerged that the identity of this structure resulted from an interaction between definitions of "line" and "distance" on the one hand, and procedures and conventions implementing these definitions on the other. Such an interaction arises because phenomenal colors are not, in themselves, magnitudes. For

[27] Each set of three vectors corresponded to an affine transformation of the color-space, and the significant topological and metrical relations of that space were invariant under those transformations.

[28] I am deliberately omitting the second innovation in Maxwell's approach: his use of color-blind subjects. This did make it possible to draw conclusions about the elementary red-sensation – precisely because such individuals have, on the Young-Maxwell-Helmholtz theory, a two-dimensional color-field. The introduction of other, pathological, subjects into the research program changed it profoundly. To do that change justice would require a longer treatment, and one with a different focus.

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they cannot be added and subtracted to one another. In consequence, the *phenomenal* color-plane has no inherent metrical characteristics. Such characteristics are always relative to methods and conventions for determining equal lengths within it, thus to some external referents of those lengths. Since this dissociation of metrical and topological properties is definitive for both Helmholtz's and Riemann's interpretations of the latter's theory of manifolds, it is unsurprising that the deliberations of our color theorists anticipate the methods and arguments employed in the later debates on the nature of another perceptual continuum, namely physical space itself. This point can be made evident by considering Helmholtz's analysis of Grassmann's fourth principle, and comparing the distinct possible interpretations he identifies there with his views – and the views he ascribed to his antagonists – in the later papers on geometry.

Considered formally, Grassmann's principle stated that the total intensity of a color (the brightness of the spectral color and the brightness of the admixed white) is equal to the sum of the total intensities of the colors out of which it is mixed. It is only on the strength of this principle that a color-*plane* can encode all the relevant relations between the three color-variables. It does so by making the brightness of white light a linear function of spectral brightness, and thereby permitting a projection of a three-dimensional surface onto the plane (cf. Sherman 1981, 100). Helmholtz's analysis of this principle showed, in essence, that sets of possible color-planes were equivalent if they could be regarded as projections of the same surface. Grassmann had assumed that the surface in question was a cone, whose apex was white at a unit intensity, and whose base was a circle with the pure spectral colors radiating from a black center to unit intensities at the circumference. The group of possible projections was therefore made up of elliptical curves (Helmholtz 1855, 69). Color-planes arrived at by means of empirical measurements which did not belong to this family were incompatible with it, because they could not be projections of the same three-dimensional surface. Helmholtz thus objected that Grassmann's *a priori* insistence on a plane of a particular type could not be justified.

Of course, given a suitable adjustment in one's measurement procedure for the colors entering into a mixture, *any* planar arrangement consistent with the phenomenal continuity of the colors could be constructed. What Helmholtz objected to in Grassmann's paper was that the procedure he had proposed for determining quantities of color could be assumed to agree with a particular disposition of colors on the plane, or groups of linear transformations thereof. There is some irony in this, since Grassmann – at this point the only person on the planet with a firm grasp of vector algebra – would certainly have got the point. For what Helmholtz had shown was that planes which were linear transformations of each other could be regarded as empirically equivalent, whereas those which were not – those in which the affine structure of the space was not preserved – could not be so regarded. Since a line on the color plane is defined by two colors lying on it, which in turn can be mixed to yield the other colors on the line, a transformation which destroyed these

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relationships would entail either a change in Grassmann's axioms, or in the specification of the measurement procedure, if it were to retain empirical validity.

In any event, Helmholtz was convinced that *he* had discovered something important: that the organization of the continuum of colors was induced by the combination of four factors. The first of these was a definition of a straight line as a set of colors, any one of which can be mixed out of two others in the set. The second was a definition of distance: a color's location relative to two others on a line is given by the ratio of the quantities of the latter entering into the mixture. The third was a definition of unit quantities, and the fourth was a method for measuring the quantities. As he observed in the 1868 "Tatsächlichen Grundlagen der Geometrie," in which he compares the determination of distances in physical space by means of the displacement of rigid bodies to the definition of distance on the color-plane, it was not a foregone conclusion that one would find a method that was consistent with these definitions. After all, he had failed to do just this in 1852. Furthermore, once one had discovered such a method, it was a matter for investigation to determine what arrangement the colors would take on that method (Helmholtz 1868a, 616).

There are two aspects of this analysis that are of particular significance. The first is the availability of an *appropriate* measurement method. The second is the *empirical equivalence* of groups of color planes. Before Grassmann's seminal paper, a host of planar and spatial arrangements of phenomenal colors had been proposed, many of which were intended to encode mixture relations (for instance of dyes, paints, threads in tapestries). Both the dimensionality and the continuity of the color-"space" were well known. In his paper, Grassmann defined an axiomatic system that described how to tie such a spatial (in the reduced case, planar) arrangement to numerical values derived from a measurement procedure. But the definitions of brightness, color-tone and saturation he gives do not presuppose a single physical interpretation. Much in the manner of modern measurement axioms, they define an ideal set of relations between colors, which may or may not be realizable in practice. Helmholtz's criticism of Grassmann was perhaps redundant, but it identified an important point. Treating colors as *magnitudes* is contingent on being able to manipulate them quantitatively, and the mere fact that they exhibit the topological characteristics of a spatial continuum is no guarantee that one will be able to do so in practice.

The second point concerns the groups of empirically equivalent planes resulting from modifying the placement and units of basis color. In his discussion of Grassmann's principle, Helmholtz distinguished sharply between selections of basis colors and units which *arbitrarily* determined the shape of the spectral curve (thus the arrangement of all colors on the plane), and the method he had used, which had made these determinations with reference to the equal brightness of pure spectral colors "as they appear to the eye." But Helmholtz weakened this claim in the successive editions of the *Handbuch*. We have already seen why. The procedures used for constructing the plane cannot uniquely determine a single "correct" one, for the shape of the plane is always dependent on certain initial conventions – the choice of basis colors and their brightness units. As I mentioned earlier, these various planes can

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be imagined as projections of a solid on variously angled planes. Alternatively, as Helmholtz explained in the 2nd edition of the *Handbuch*, "if one imagines the geometric colour-plane as represented on an elastic plate, which one can stretch evenly in every direction, one would get a similar array of form-changes (*Gestaltveränderungen*), but each of these colour-planes would remain correct" (Helmholtz 1896, 338).

He goes on to ask, "what remains constant in these changes?" Here there are two possible answers: 1) Even as brightness units are redefined conventionally, the affine structure of the plane remains the same; that is to say, given two distinct pairs of colors, the color lying at the intersection of the two lines connecting each is always the same. Here there is no question of one or the other projection being empirically correct. 2) Alternatively, if brightness units and standard colors are simply *defined* in terms of objective properties of light, then a unique plane and arrangement of colors can be singled out. Helmholtz's (and later Maxwell's) curve of spectral colors was empirical in one respect: names of colors were replaced by wavelength values. Brightness was still subjective, however. A fully objective curve in the sense of modern chromaticity measurement replaces brightness by luminous flux. Here there is no more question of varying the curve, because the intensive magnitudes of color phenomena have been completely replaced by extensive ones from another theory. If one sticks to the phenomena, however, the shape of the curve will necessarily depend on metrological conventions.

These alternatives represent two different ways of understanding what is "objective" in the structure of phenomenal experience. From the point of view of 1), there are relationships between colors which hold independently of our knowledge of the absolute ground of these relations. Some of these – continuity, dimensionality – are inherent to the phenomena. Others – their organization in "lines" determined by mixtures – are the product of axioms stipulating that they be so arranged, and the result of carrying out the mixtures prescribed by those axioms. Finally, properties such as the distance separating the colors (what is to count as equal distance between colors) are determined by conventions for measuring quantities of the latter.

From the other point of view, all of these characteristics can be given a unique objective ground in terms of the physical characteristics of their underlying physiology, for instance in terms of exact response curves correlating excitations of elementary receptors to the wavelength and amplitude of physical light. This kind of objectivity removes any dependence of the color plane on conventional procedures, but only at the cost of replacing our terms for the phenomena with terms belonging to another physical theory (green becomes a wavelength of light, brightness a measure of radiation). The imperfectly realized quantitative relations between phenomenal colors take on an objective meaning either when they are tied to objective phenomena by means of measurement operations, or when their physiological basis has been exhaustively (and mathematically) elucidated. Considered on its own, the continuum of colors carries objective meaning only by virtue of its

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being "a system of differences," to use Helmholtz's paraphrase of Riemann's conception of a manifold (Helmholtz 1870, 16).

Readers familiar with Helmholtz's epistemological writings will be reminded of a shift in his understanding of his sign- or symbol-theory of meaning. In his 1852 "Über das Sehen des Menschen," sensations are compared to signs because, for example, colors do not resemble their physical causes, but merely denote them. But later formulations of this theory (beginning with the first edition of the *Handbuch*) extend it radically. On Helmholtz's new conception, it is only differences or changes in signs which carry objective information about the world. This change is, I suspect, due in large measure to the research covered in this paper, which was conducted during the same period. On the trivariance theorem, which Helmholtz soon adopted from Maxwell, elementary sensations have *no* objective referent, for they cannot be stimulated on their own. Furthermore it is only with regard to their differential relations (those reflected in their affine structure) that they convey objective information, at least so long as one confines oneself to the perspective of the subject. From this subjective perspective, all connections to "external" phenomena are mediated by measurement operations, and therefore dependent on our being able to physically realize the definitions of these operations.

I think that a number of connections to his work on the epistemology of geometry are fairly evident as well. In my introductory discussion, I argued that Helmholtz's papers on geometry go through two phases. At first convinced that the definitions of rigidity, free mobility, and rotation (coupled with the assumptions of the continuity and the unboundedness of the spatial continuum) were sufficient to determine a unique metric, Helmholtz argued that 1) the truth of Euclidean geometry was equivalent to 2) the meaningfulness of spatial measurements in general. And since 2) was equivalent to 3) the physical realization of the definition of a freely movable rigid body, it followed that 1) and 3) implied one another. Euclidean geometry is true because a particular kind of measurement can be carried out in the spatial continuum. It is, Helmholtz mistakenly thought, the only way of assigning objective meaning to spatial "differences," because it is entailed by the requirements placed on the measurement procedure, which was to permit the comparison of the spatial difference between any two pairs of points. In the second phase, Helmholtz admitted that his analysis did not uniquely determine which of a family of geometries was valid. In the papers of 1870 and 1878, he emphasized the compossibility of Euclidean and pseudo-spherical measurement systems, while admitting that an absolute decision between the two could only be made in the light of mechanical assumptions and an (operational) definition of length with respect to time. However, he continued to reject as untenable the assumption that Euclidean geometry could claim *a priori* validity.

The peculiar mixture of transcendental and empiricist elements in the first phase of Helmholtz's work on geometry can be better understood if we look at his experiences in colorimetry. For both in Grassmann's paper and Helmholtz's comments on it, we find a clear distinction between intuitive properties of the color

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continuum, axioms defining metrical relations, and the empirical procedures realizing these axioms. There is no sense in asking whether colors are, in and of themselves, arranged in Newton's wheel or in Helmholtz's. Intuitively, they have continuity and dimensionality as inherent properties, but nothing else. But it is equally clear that Grassmann's axioms of color-mixture can only with difficulty be viewed as empirical in origin. They impose a structure on color-phenomena that at best refers to relations between the causes of colors, and these relations are hidden from us. Thus the proposition that yellow lies "between" green and red gains its meaning only in the context of a mixture law. Furthermore, that procedures for measuring and mixing colors in accordance with such a law can be developed (that we can identify, manipulate and measure the causes of colors) is obviously guaranteed neither by the axioms nor the phenomena. The possibility that colors might fail to be a metrical space was, from 1850 to 1860, a live one.

Once Helmholtz realized, however, that the mere requirement that every distance in space be comparable with every other did not force a Euclidean metric, his argumentation took a new turn. No longer content with the claim that the application of geometry to experience presupposed certain behaviors among bodies, he now argued that the results of spatial measurements were meaningful only in so far as they could be related to propositions of physics. Since general physical laws require for their formulation spatial units relating physical processes to time, it is the responsibility of geometry to codify the relations holding between such "physically equivalent" spatial magnitudes. He termed such a geometry a "physical geometry."

Helmholtz gave a detailed development of this argument in his last paper on the subject (Helmholtz 1878b). Here, the entire discussion parallels the metrological analysis of the color-space. The paper presents the same argument in two modes: a realistic and an idealistic one. Realistically, the question of what geometry should count as objectively valid is characterized as being equivalent to determining "the objective equivalence of the real substrates of spatial magnitudes, which [equivalence] shows itself in the unfolding of physical relations and processes" ("*welche sich im Ablauf physischer Verhältnisse und Vorgänge bewährt*") (ibid., 641). From the idealistic point of view, since this objective equivalence cannot be checked against underlying relations among these substrates, it can only be arrived at by means of measurement operations. The argument thus replicates the dilemma Helmholtz faced in determining the true structure of the color-space: the latter had such a structure in the event that a complete physical reduction could be accomplished; however in the absence of such a reduction, dependence on measurement procedures and conventions was unavoidable.

Helmholtz calls those elements of (noumenal) reality that are responsible for our observing an object at a given location at a given time "topogeneous factors." These factors interact with "hylogeneous factors" (the causes of material sensations) to produce appearances of movable bodies in space. Physical geometry presupposes the existence of law-like behaviors of such bodies, thus in turn regular connections

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between the topogeneous and hylogeneous factors.[29] From the realistic point of view, we want a physical geometry to tell us which groups of topogeneous factors give rise to physically equivalent spatial magnitudes, in that they interact with the material ("hylogeneous") factors in a law-like fashion. Such a geometry would be objectively valid in the case that the relations of equivalence it ascribed to the spatial magnitudes was preserved when mapped onto the topogeneous factors.[30] The problem is that we have no access to the topogeneous factors. In consequence, we have to use physical procedures to establish these relationships of equivalence. If space is transcendental in the sense that we could never know its noumenal ground, then its "real" metrical structure is also unknowable. We then have no choice but to give it one by settling on a physical means of determining these equivalence relations.

At the conclusion of this paper, Helmholtz concedes that there might be a "direct" action of topogeneous moments on our intuition that would make reliance on physical measurement unnecessary:

> This does not yet justify our declaring a geometry based on transcendental intuition to be impossible. One could for instance assume that an intuition of the equality of two physical magnitudes could be induced directly by the action of the topogeneous factors on our consciousness without physical measurement, thus that certain aggregates of topogeneous moments also be equivalent with regards to an immediately perceivable psychical effect. The whole of Euclidean geometry can be derived from the formula defining the distance between two points as a function of their orthogonal co-ordinates. Let us assume that the intensity of this psychic effect, whose equality appears as the equality of the distance between two points in the imagination, depends in the same way on some three functions of the topogeneous moments of each point as the distance in *Euclidean* space depends on the three co-ordinates of each such point. Then the system of pure geometry of such a consciousness would fulfil Euclid's axioms, whatever the relations between the topogeneous moments of the real world and their physical equivalence. It is clear that in this case the question regarding the agreement between the psychical and physical geometry of spatial magnitudes could not be decided by the form of intuition alone. And if it turned out that there was such an agreement, this would have to be considered a natural law, or, as I called it in my popular lectures, a pre-established harmony between the world of representation and the real world, just as the coincidence of the straight line described by a light ray and that formed by a string pulled tight depends on a natural law. (Helmholtz 1878b, 658–659)

If the topogeneous factors produced in us a direct intuition of equal distances, such that we were able to declare spatial magnitudes equal without recourse to a measurement procedure using movable bodies, the resultant geometry could at best

[29] See Hyder 1999 for a detailed account of the structure of this argument.

[30] Cf. DiSalle, 517: "So the laws which our mind represents as the structure of space are just the natural laws governing the topogeneous factors." Strictly speaking the laws in question are those governing the interactions of the topogeneous and hylogeneous factors.

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agree accidentally with its physical counterpart. Helmholtz fails to consider the objection that, in the event that his physical geometry disagreed with such an intuitive one, they would also disagree with regard to what constituted "the same physical processes," which relation was to ground the definition of physically equivalent magnitudes. For what counted as an inertial motion in the one framework would be a forced motion in the other. This indifference reflects an early instance of a "constructive axiomatic" approach, in which the operational definition of certain basic terms of a physical theory is taken as unavoidable (cf. Carrier 1994, 280).

Helmholtz's conviction that metrological procedures are the epistemological basis of metrical relations, and that, indeed, intuitive relations between spatial magnitudes have no privileged significance is a view that was forced on him during the work on colorimetry, and this source is clearly reflected in the passage at hand. Corresponding to a direct intuition of spatial distances is the direct intuition of "distances" between colors,[31] i.e. statements such as "This light is twice as bright as that one" or "These two colors differ from each other just as much as those two." Such statements refer to phenomenal colors as if the latter stood in numerical relations to one another independently of conventional determinations (once again, choice of basis colors, brightness units, measurement procedures). But we do not, as a matter of fact, have the ability to intuit precise mathematical relations between colors, nor, for that matter, is it clear what such intuitions could *mean*, if they were not given some sort of reference. A theatrical lighting designer, or a painter, might develop and make use of such a capacity, but only because he was concerned with mixing lights and paints. Helmholtz's example is supposed to be an analogous reductio ad absurdum. We are invited to consider a functional dependence holding between the "intensity of this psychic effect, whose equality appears as the equality of the distance between two points in the imagination" and the topogeneous factors, i.e. the real causes of the representations of spatial locations. A person who possessed this remarkable ability would be able to intuit the relative distances between pairs of points without relying on a physical process of measurement. Helmholtz's objection is that such relations are empty until they are used to describe the motions of bodies. At this point, they may come into conflict with those relations established by measurements with rigid bodies. Furthermore, this counterexample is undermined, in Helmholtz's view, by the fact that we simply do not have the ability to intuit distances in physical space.[32]

Given this reliance on the physiological analogy, it may seem surprising that Helmholtz *denies* that such a direct functional dependence could have any privileged meaning. For in the case of colorimetry, the aim was to know just this: how are the colors to be arranged spatially in such a way that each can be viewed as a value of a function whose arguments are physical properties of light? And the answer to this

[31] Conversely, in the paper at hand, distances between spatial points are described as "intensities," the estimation of which was at the heart of Helmholtz's analysis of Grassmann's fourth principle.

[32] At Helmholtz 1878b, 650, he argues that such a pure intuition of spatial magnitudes would have to be completely exact.

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question could be uniquely and objectively given once a complete knowledge of the underlying mechanisms was at hand. Helmholtz does not draw the same conclusion with regard to geometry. For he agrees with Kant that the three-dimensional spatial continuum is a transcendental condition of experience, and therefore that we have no access to the underlying topogeneous factors. This sort of an objective grounding in the "real" structure of space is impossible. There remains only the alternative he had identified in colorimetry once he had realized that the exact nature of the underlying physiological processes were, for the foreseeable future, beyond access: that the quantitative relations holding among part of a continuum gain their objective significance by their connection to measurement operations; and these measurement procedures confer such significance on the magnitudes only because they connect them to a larger domain of phenomena. In the case of colorimetry, this had meant that the arrangement of the colors on the plane was dependent on the choice of basis colors, the mixture law, the units of brightness, etc. And in the case of geometry, it meant that spatial magnitudes could be called equivalent only when they were connected by means of measurement procedures to physical processes taking place in time. In both cases, the "system of differences" constituting the manifold takes on determinate metrical properties only when both measurement procedures and a larger set of phenomena are involved.

Conclusion

How then, should we understand this terminal position? In what sense did Helmholtz take geometry to be empirical in this last paper? In my introductory remarks, I outlined two positions he might have adopted: 1) geometry is empirical in that it is a species of mechanics (Helmholtz as mechanist); or 2) geometry is empirical in that it is learned by observing the behaviors of physical objects (Helmholtz as physiologist). I would argue instead that Helmholtz came to the position that geometry is empirical in that 1) it does not describe inherent properties of the spatial continuum; and 2) it is objectively indeterminate until supplemented with mechanical propositions. From this it followed for him that 3) an operational description of spatial measurement procedures had to be specified, which would make use of rigid bodies (and clocks, although Helmholtz did not go into this), where both the concepts of rigidity and equal length of time are physically, as opposed to theoretically, specified. If the formulation of physical principles demands that concepts of length be operationally defined, and it is furthermore impossible to determine length relationships *a priori*, it follows that some sort of operational basis must be imposed on the continuum. As we have seen, the upshot of Helmholtz's argument in the 1878 paper is that once we have ruled out the direct quantification of the causes of our intuitions of spatial differences, geometrical propositions remain meaningful in one sense only, namely as propositions concerning the behaviors of instruments used to define groups of physically equivalent magnitudes. These groups,

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in turn, take on objective significance only once we are concerned with describing relations between time, matter, and space. On this reading, Helmholtz is above all a metrologist, in that he views geometry as an operationally implemented system of measurement. The exact form that this system takes will depend on the physical environment which experimental scientists inhabit.

I should remark in closing that this connection between Helmholtz' work in physiological optics and his argument concerning physical geometry permits us to characterize his relations to Kant's philosophy quite exactly. In the Introduction to his *Erhaltung der Kraft*, Helmholtz had subscribed to the view that the task of natural science was to tie back the changeable in experience to changes in the position of matter in space and time. In other words, he endorsed wholeheartedly the Kantian project of codifying experience by connecting intensive magnitudes (in Kant's sense) to extensive ones, namely space and time. Since the latter form the ground of geometry and mathematics, this undertaking is necessary and sufficient to mathematize experience. Helmholtz's work on color-research was wholly consistent with this plan: Tying colors and their relations back to objective statements about spatio-temporal properties of light was a typical piece of research in the neo-Kantian tradition of sense-physiology as carried out by Müller, Herbart, and Lotze, to name a few of Helmholtz's predecessors. In the course of this research, Helmholtz, as well as Grassmann and Maxwell, was forced to reflect on the concept of a phenomenal continuum and the conditions under which "distances" in such a continuum could be defined. This research then boomeranged, for Helmholtz then used it to undermine a key assumption of the Kantian project.

The assumption in question was that whereas intensive magnitudes were not fully mathematizable, the extensive magnitudes of time and space were so. Thus Kant had seen it as the task of what he called "Phoronomy" to provide a "construction" of the intensive magnitude of speed in terms of the intuitions of space and time: the Phoronomy of his Metaphysical Foundations gives a recipe for adding and subtracting speeds by treating them as line segments;[33] in other words, it assumes that the sciences of the pure extensive magnitudes are certain and *a priori*. One way of understanding Helmholtz's work – certainly that best suited to the long passage quoted above – is as an argument asserting that our pure intuitions of extensive magnitudes cannot possibly do the reductive job assigned to them. For just as the system of difference that comprises the color-space is bereft of metrical relations until measurement conventions are imposed on it, the same holds for space itself, according to Helmholtz. These conventions can only be implemented by using material objects in space, and the relations thus singled out are in turn not objectively significant until they are supplemented with laws describing the motions of bodies. For Helmholtz, space has no metrical characteristics, thus no geometry at all, unless the first of these

[33] In the Phoronomy, Kant represents speeds as directed line segment, and shows how this process presupposes a (relatively) unmoving empirical space in which a body is in motion.

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can be achieved. And even in the case envisaged above, where an intuitive metric is permitted, the latter has no special claim to legitimacy.[34]

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[34] See Hyder 2001 for a discussion of Helmholtz's relation to Kant, and of the similarities between Helmholtz's views and those of Einstein, as well as Hyder 2002.

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