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## **Abstract**

In this thesis I examine what the geometry of visual experience is. This is a question which to some has had an a priori answer, and to some its relevance to philosophy has been considered questionable. I argue that the question is of philosophical concern partly because it generates an interesting form of the argument from illusion against Direct Realism, and partly because it concerns the phenomenal character of visual experience. I consider and offer objections to a number of arguments, both a priori and empirical, for the conclusion that the geometry of visual experience is not the same as the geometry of the physical environment. Three proposals are argued against: that visual experience underdetermines a geometry; that the geometry is of the spherical non-Euclidean type; and that the geometry is of the hyperbolic non-Euclidean type.

# **The Geometry of Visual Experience**

**PhD Thesis**  
**University of Durham**

**Phillip Meadows**

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**2007**



**- 2 JAN 2008**

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## **Chapter 1 – Introduction**

What is the geometry of visual experience? This is the central question that will be investigated in this thesis. It is a question with a considerable intellectual pedigree – Euclid, Newton, Berkeley, Reid, and P. F. Strawson, amongst others, all addressed it in one form or another. However, as we shall see, the understanding of the question underwent a significant conceptual change in the late Nineteenth and early Twentieth century, after the development of non-Euclidean geometries. Both Euclid and Newton, in their respective *Optics* can be thought of as attempting to characterise vision by utilising the conceptual resources of axiomatic geometry. Such characterisations are historical antecedents of the question as it is to be understood here, but they are not directly relevant to a modern interpretation of the question, for two different reasons.

Firstly, Euclid and Newton do not address the question of what is the correct characterisation of visual *experience*. The question, as it is to be understood in this thesis is one that concerns the *phenomenal character* of visual experiences. George Berkeley, as we will come to see, had much to say about visual experience, spatial properties and geometry in his *Essay Towards a New Theory of Vision*, although much of it was negative in character. Thomas Reid was perhaps the first thinker to say something substantive and positive about the geometry of visual experience that addresses the issue of what the phenomenal character of visual experience is. With Reid there is another development toward the modern reading of the question: we begin to see the development of the idea that the geometrical description of visual experience radically and systematically diverges from the geometrical description of the physical objects vision can put us into perceptual contact with. The development of Reid's ideas put him very close to the development of a non-Euclidean geometry; however, as an answer to the modern form of the question, Reid's theory required the development of the idea that a space can be given a distinctive characterisation in terms of its curvature that does not appeal to regions outside the space.

The question as it is to be understood in this thesis raises concerns about the spatial properties of visual experience, but it does so in a way that is different from a



number of other, related questions about the spatial properties of visual experience. There is, for instance, a venerable debate about whether visual experience is three-dimensional or two-dimensional. This was one of the ways in which both Berkeley and Reid, and later William James in his *The Principles of Psychology*, were interested in the question of the geometry of visual experience. This concern has also been addressed more recently by O'Shaughnessy, Lowe and Smith.<sup>1</sup> The concern about the number of dimensions of visual experience is often taken up in connection with the perspectival shapes that are exhibited by the objects in the environment surrounding, and including, our physical bodies. The concern about the geometry of visual experience that will be explored here, whilst related to this more traditional debate and even drawing on it, takes as its object a different aspect of the spatial character of visual experience. Consequently, I will not engage directly with the literature covering that debate, except insofar as it becomes necessary in the course of addressing the question of the geometry of visual experience.

The more traditional debate about the dimensionality of visual experience takes place under the assumption that the geometry of visual experience is Euclidean in character – i.e. it has no intrinsic curvature; and the parallel postulate holds. This assumption is, however, tacit in many contemporary discussions of the spatial properties of visual experience and rarely commented on, especially within the context of philosophical problems about perception. The question that will be dealt with here is whether this tacit assumption is itself correct; whether visual experience is correctly described by one of the various possible non-Euclidean geometries. Since the development of non-Euclidean geometries, philosophers and psychologists, especially those from the middle of the Twentieth century, have developed arguments for two different kinds of non-Euclidean geometries for visual experience. More rarely, contemporary philosophers and psychologists have attempted to defend a Euclidean geometry for visual experience.

In this thesis I will follow the latter course and attempt to defend a Euclidean geometry for visual experience. However, my defence will be largely negative in character: I will attempt to show that the extant arguments that have been offered for a non-Euclidean geometry can be rejected and that, where appropriate, the data

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<sup>1</sup> See Brian O'Shaughnessy. *The Will*. 2 vols. (Cambridge: Cambridge University Press, 1980); E. J. Lowe. "The Topology of Visual Appearance." *Erkenntnis* 25 (1986): 271-274; and A. D. Smith. "Space and Sight" *Mind* 109 (2000): 491-492.

supporting them can be explained in a way consistent with a Euclidean geometry. The caveat 'where appropriate' is significant, as it highlights a difference in approaches to the question that have been taken by those philosophers and psychologists who have addressed the question. In a survey paper on the state of the debate in the 1970's, Patrick Suppes wrote:

Philosophers of past times have claimed that the answer to the question, Is visual space Euclidean?, can be answered by *a priori* or purely philosophical methods. Today such a view is presumably held only in remote philosophical backwaters.<sup>2</sup>

One interesting quite recent development is that current work on Reid's classic discussion of the geometry of visual experience has attempted to provide an answer by such *a priori* methods. I will argue that such *a priori* arguments are not convincing for the kind of reason that Suppes has in mind: that the question surely has an empirical character. However, Suppes himself appears to have had in mind a different philosopher whose methods, if they are to be counted as *a priori*, were importantly distinct from the contemporary arguments inspired by Reid: P. F. Strawson. I will argue that Strawson's arguments cannot be dismissed in the manner Suppes suggests they should be.

Suppes' own sympathies lie with an empirical approach to the question that was taken by psychologists from the middle of the Twentieth century through to the time of his own article. These psychologists generated data that suggests a non-Euclidean geometry for visual experience. There have, however, been a number of objections put to these theories on account of their method, if they are construed as claims about visual *experience*. I argue that these objections are not convincing, but then develop a way to account for the data in a way that is consistent with a Euclidean geometry. To this extent I agree with Suppes that the empirical approach is relevant, indeed it cannot be ignored without being accused of doing armchair theorising. However, without philosophical reflection on the metaphysical and methodological presuppositions of the empirical approach we are liable to fall into error. The danger is particularly acute in the present case because the question focuses on the

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<sup>2</sup> Patrick Suppes. "Is Visual Space Euclidean?" *Synthese* 35 (1977): 397.

phenomenal character of visual experience – the mental notoriously resists straightforward and philosophically unsophisticated investigation by empirical methods.

In addition to the general interest of this question, and the concern about the appropriateness of certain methods of enquiry into it, there is a further philosophical concern that this question relates to. Suppes highlights a way in which the question may be considered to be empirical, but in a derivative or indirect way. Such a view could be supported if there were conceptual reasons for thinking that the geometry of visual experience must be whatever the geometry of physical space is, where the latter issue is clearly an empirical issue. Suppes says “to some extent this must be the view of many laymen who accept that to a high degree of approximation physical space is Euclidean, and therefore automatically hold that visual space is Euclidean.”<sup>3</sup> The difficulty for such a position lies in providing such conceptual arguments about perception. There is, however, a form of argument that argues in the opposite direction: if the geometry of visual experience is not the same as the geometry of physical space, then we must conclude something about perceptual experience – that it is always indirect. This argument is known as the argument from illusion; one of my concerns in this thesis will be to avoid the difficulties presented by such an argument by rebutting the arguments for a non-Euclidean geometry.

In the remainder of this chapter I will provide a brief introduction to non-Euclidean geometries, before going on to motivate the philosophical concern about the directness of perception in more detail by looking first at the argument from illusion in its most general form. I will then turn to a particular instance of the argument from illusion – the form of the argument we get upon reflection on the spatial properties of visual experience. I will then contrast two versions of the argument from illusion, as based on spatial properties. The two versions will be, on the one hand, the traditional form, and, on the other, the form of the argument from illusion that arises from reflection on the geometry of visual experience. I will argue that some of the traditional responses to the traditional form of the argument from illusion based on spatial properties are ineffectual against the argument based on concerns about the geometry of visual experience. I will then turn to more general

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<sup>3</sup> Loc. Cit.

concerns about the very possibility of a ‘geometry’ of visual experience, concerns that originate in the work of Berkeley.

## **Euclidean and Non-Euclidean Geometries**

To describe a geometry as ‘non-Euclidean’ is fundamentally contrastive – it just means that the geometry in question deviates from Euclidean geometry in some way. So, for example, ‘absolute geometry’ does not contain the parallel postulate or any form of its denial, and is thus ‘non-Euclidean’. The number of non-Euclidean geometries is, therefore, quite large. Philosophers who have attempted to provide an answer to the question of what the geometry of visual experience is typically limit the scope of the inquiry by concerning themselves with just one of three possible geometries: Euclidean geometry or one of two non-Euclidean geometries, hyperbolic geometry and spherical geometry. These three geometries are unique in that they all have a constant curvature. To limit the present discussion to a manageable size and to deal comprehensively with the arguments already extant in the literature, I will also only concern myself with Euclidean geometry and the two non-Euclidean geometries of constant curvature.

The two non-Euclidean geometries of constant curvature that we will be concerned with here are themselves unique in that they can be obtained from Euclidean geometry by replacing the parallel postulate in each case with a different form of its denial. The parallel postulate in Euclidean geometry holds that for any straight line  $l$  and any point  $p$  not on that line, there is one and only one straight line passing through  $p$  that does not intersect, or is parallel to, line  $l$ . There are two ways this could be denied. Firstly, by asserting that there is *more than one* straight line through point  $p$  that does not intersect line  $l$  – this is the postulate found in hyperbolic geometry. The second way of denying the Euclidean parallel postulate is to assert that there are *no* straight lines through point  $p$  that do not intersect line  $l$  – this is the postulate found in spherical geometry.

This difference in the parallel postulate for each geometry results in each geometry consisting of different theorems that can be derived from the initial axioms or postulates. One fairly well known difference concerns the angular sums of figures,



such as triangles, in each geometry. In Euclidean geometry, as we are all taught quite early on at school, the angular sum of triangles is always equal to 180 degrees. In hyperbolic geometry the angular sum of triangles can be less than 180 degrees; in spherical geometry the angular sum of triangles can be greater than 180 degrees. These differences, together with others that will be discussed within the context of the arguments for each particular geometry, provide a means of testing for the geometry of visual experience. We can inquire as to which of the sets of relations specified by each geometry match the relations which are to be found between or are instantiated by the elements of our visual experience.

Each of the geometries is a set of logically related sentences that specifies a total description of a space. Each of these spaces, however, is incommensurate with the other spaces with different curvatures. Patrick Heelan expresses this admirably in terms of the shapes that are possible in a given locality within a space:

In any geometrical space, there is associated with every place (such as an extended neighbourhood around a point) a definite repertory of constructible shapes, that is, of shapes that could be constructed there consonant with the geometry. Different places in general have different repertories of constructible shapes except for constant curvature spaces, where these repertories are everywhere the same. The shapes in these repertories, moreover, are characteristic of the geometry of the space, so that Euclidean repertories contain only Euclidean figures, non-Euclidean repertories contain only non-Euclidean figures. No Euclidean shape is at the same time a non-Euclidean shape of similar description in a space of equal dimensions; consequently there is no overlap between the repertories of Euclidean three-dimensional (3D) shapes and repertories of 3D non-Euclidean shapes.... The conclusion is that there are no common elements with common geometrical descriptions in the repertories of 3D Euclidean and non-Euclidean shapes.<sup>4</sup>

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<sup>4</sup> Patrick Heelan. *Space Perception and the Philosophy of Science* (Berkeley and Los Angeles: University of California Press, 1983), pp. 47-48.

The crucial point being made here is that Euclidean space and each of the non-Euclidean spaces under discussion are incommensurate. Whatever similarities or formal relationships there may be between Euclidean shapes and non-Euclidean shapes, an object cannot be at once Euclidean and non-Euclidean. Nor, if we are substantialists about space, can a space be at once Euclidean and non-Euclidean, or indeed at once hyperbolic and spherical. This way of thinking about geometry is robustly Realist and anti-conventionalist. As we shall see in chapter 2, this has not always been accepted. However, for the present purposes I will not question it.

The importance of this view about the incommensurability of spaces with different geometries is that if, on investigation, it turns out that the geometry of visual experience is other than the geometry of the space in which the objects in our environment exist, then we will be in a position to run a form of the argument from illusion against the claim that we perceive objects *directly*. Moreover, the incommensurability of spaces with a different geometry makes one particular form of this argument much harder to respond to than has been traditionally thought. I will now turn to the philosophical background to this issue: the argument from illusion.

### **Direct Realism and the Argument From Illusion**

The argument from illusion is one of a group of similar arguments that have been put forward against the position in the philosophy of perception known as Direct Realism. This position is a Realist position in that it affirms the mind-independent existence of the objects perceived in the world, such as tables and chairs. Within the philosophy of perception, the purpose of these arguments is to show that our perceptual awareness of the world, and the objects in it, is not direct. The alternative proposal offered after this denial is that perceptual awareness of the world is always indirect, but in a way that requires some clarification; this position is known as Indirect Realism. To this extent the argument does not, of itself, contest the Realism of the Direct Realist.

We will not have a clear understanding of the philosophical position that the argument from illusion is meant to put pressure on until we have some understanding of what it means to say that perception is direct. An account of what this means is typically offered by contrasting it with perception that is always indirect. However,

the sense of ‘indirect’ here is not what it might ordinarily be taken to be in such locutions as ‘perceive indirectly’, and so some clarification is required.

We are commonly aware, by means of perception, of objects such as football players on a field in such a way that might reasonably naturally be described as indirect. For instance, I could be watching them on television. In such a case I would be aware of the football players indirectly – my awareness of them would somehow be mediated by my awareness of the television. This idea is most commonly expressed in terms of the relation of ‘in virtue of’: we are aware of one object in virtue of our awareness of another, immediate object. Here are some formulations of this idea, first from Frank Jackson in his *Perception: A Representative Theory*, and second from A. D. Smith in his *The Problem of Perception*:

That is, we often see things in virtue of seeing other things.

Now for our definition:  $x$  is a *mediate object of (visual) perception* (for  $S$  at  $t$ ) iff  $S$  sees  $x$  at  $t$ , and there is a  $y$  such that ( $x \neq y$  and)  $S$  sees  $x$  in virtue of seeing  $y$ .<sup>5</sup>

Here we have an example of one thing being perceived in virtue of something else being perceived. When one thing is thus perceived *in virtue of* some distinct item being perceived, we can say that perception of the latter mediates perception of the former, and that this former object is not the *immediate*, but only the *indirect, object* of perception.<sup>6</sup>

The instance of indirect perception of footballers on a television screen, however, is not an adequate articulation of what the Indirect Realist is claiming when he asserts that perception is indirect. This is because the possibility of such kinds of indirect perception is just one of the features of perception that requires explanation: it highlights a distinction that the Direct Realist will and must accept. In these kinds of cases the footballers, or whatever is indirectly perceived, are objects we ordinarily,

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<sup>5</sup> Frank Jackson. *Perception: A Representative Theory*, (Cambridge: Cambridge University Press, 1977), pp. 19-20

<sup>6</sup> A. D. Smith. *The Problem of Perception* (Cambridge, Mass.: Harvard University Press, 2002), p.6

pre-theoretically take ourselves to be aware of. Moreover, this is also the case with the television, or whatever objects we may quite naturally say we perceive other objects in virtue of: they are objects that we pre-theoretically take ourselves to be aware of. However, Indirect Realism holds that all of the objects that we may pre-theoretically take ourselves to be aware of are themselves *always* perceived indirectly, in virtue of some kind of object that we do *not* ordinarily, pre-theoretically take ourselves to be aware of. Indirect Realism is therefore an error theory: it holds that we are systematically mistaken in our commonsense assertions about perceptual awareness.

The exact characterisation of this class of objects (the class that the Indirect Realist claims always mediates our perceptual awareness of the object we pre-theoretically take ourselves to be directly perceptually aware of) varies amongst Indirect Realists. For some it is a class of non-physical, peculiarly mental objects; for others it is a class of physical, but internal objects, such as brain states or retinal excitation; for still others it is a class of surfaces of the physical objects that surround the perceiver. What they all have in common is the claim that the objects that mediate perceptual awareness of the objects we pre-theoretically take ourselves to be directly aware of are radically different from those latter objects.

So much by way of a characterisation of the two positions that are at issue in the philosophy of perception. Let us now look at the argument from illusion itself, which is one of the arguments that are commonly put forward to put pressure on Direct Realism. This argument has been much discussed throughout the history of philosophy and has been given a variety of formulations. For my purposes here I will follow the exposition of the argument as it is found in A. D. Smith's *The Problem of Perception*. My reason for this is that the account of the argument offered there is both clear and thorough, but also highlights the seriousness of the challenge offered by the argument from illusion to Direct Realism.

The argument from illusion is just one of a class of arguments against Direct Realism that all share a common characteristic. They all highlight some feature that whatever we are immediately perceptually aware of possesses and which the object we might pre-theoretically take ourselves to be immediately aware of does not possess. Conversely, these arguments may highlight some feature that the object we might pre-theoretically take ourselves to be immediately aware of possesses, but that whatever we are immediately perceptually aware of does not possess. By an application of

Leibniz's Law, the objects we are immediately perceptually aware of cannot be the objects we pre-theoretically take ourselves to be immediately aware of.

Such observations about the disparity of properties can be used to construct this kind of argument against Direct Realism because it is a position that can be construed as making a claim about the identity of the objects we are perceptually immediately aware of and those objects we pre-theoretically take ourselves to be aware of. Here is how Smith puts the point:

Direct Realism must be understood as making an identity claim: that the immediate object of awareness in standard perceptual situations is a normal physical object – in other words, that it is *identical* to some such object.<sup>7</sup>

By 'normal object' Smith just means the objects we pre-theoretically take ourselves to be directly aware of. The application of Leibniz's Law implies the denial of the identity claim made by the Direct Realist.

The argument from illusion is just one member class of this general class of argument that employs Leibniz's Law to this effect. It is differentiated from the others in terms of the kinds of features that are observed to be disparate between whatever the immediate object of perceptual awareness is and the objects we pre-theoretically take ourselves to be directly aware of in perception. Smith offers an exhaustive list of all the possible kinds of such disparities; this list therefore exhausts all the possible kinds of argument that can employ Leibniz's Law in the way indicated above:

Concerning the immediate object of awareness and the normal object:  
(a) one possesses a genuine attribute that the other lacks; (b) one bears a genuine relation to another item that the other does not; (c) one exists at a place where the other does not; (d) one exists at a time where the other does not; (e) one exists and the other does not.<sup>8</sup>

It is (a) that the argument from illusion concerns itself with, but it is quite reasonable to ask what is meant by a 'genuine attribute'; what are these features or

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<sup>7</sup> A. D. Smith. *The Problem of Perception* (Cambridge, Mass.: Harvard University Press, 2002), p. 8

<sup>8</sup> Loc. Cit.

attributes that the argument from illusion concerns itself with? We can at the very least provide a negative account. Firstly, the use of the adjective ‘genuine’ as applied to ‘attribute’ is intended to exclude intensionally specified attributes that Leibniz’s Law does not apply to. So, John’s believing the tomato he sees to be black, together with the truth that the tomato is red and not black, does not provide us with the materials for a valid application of Leibniz’s Law.

The other members of the list exclude from (a) concerns about existence, whether temporally and spatially indexed, or not. It also excludes relational properties, and this exclusion requires some comment. The kind of relation that Smith has in mind here to exclude from (a) are relations of dependence, such as the dependence of objects of immediate perceptual awareness on the brain in a way that cannot be asserted of the objects we pre-theoretically take ourselves to be aware of in perception, unless we give up Realism. It would be surprising to exclude from (a) the spatial relations that the objects of immediate perceptual awareness bear to each other.

There is some difficulty about specifying the features that the argument from illusion concerns itself with, as not all properties will want to be considered relevant. Examples include things feeling wet, or feeling slimy, or looking broken – surely we do not want wet, slimy or broken sense data. I will not concern myself with the difficulties in specifying how to exclude such cases, although Smith offers some considerations supporting the need to draw some distinction between those which are and those which are not relevant to the argument from illusion<sup>9</sup>; for now we can characterise them positively in the following rough sort of way: they are the properties that might be thought proper to each sense. In the case of sound they may include the properties of relative distance; direction; pitch; timbre; and loudness. In the case of vision they may include various spatial properties, such as shape and relative size; hue; saturation; and brightness. It is in respect of these kinds of properties that the argument from illusion asserts a discrepancy between the properties of what we are immediately perceptually aware of and the properties of the objects we pre-theoretically take ourselves to be perceptually aware of.

The argument itself has four stages. The first is simply the observation that illusions can occur – things can appear other than they are. The second stage is a move that has come to be known as the ‘sense datum inference’: it turns on the claim

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<sup>9</sup> A. D. Smith. *The Problem of Perception* (Cambridge, Mass.: Harvard University Press, 2002), p. 49

that when something appears to have a feature it does not, we are then immediately aware of something that does have that feature. The third stage of the argument is the application of Leibniz's Law – there is an object that we are aware of which possesses a property that is not possessed by the object we pre-theoretically take ourselves to be aware of. So, the object we are immediately aware of cannot be identical to the object we pre-theoretically take ourselves to be aware of. The fourth and final stage of the argument is commonly called the 'generalising step'. Up to this point in the argument, the conclusion of non-identity is restricted only to cases where illusion is actually occurring – however, the generalising stage consists of offering reasons for thinking that the conclusion of non-identity must also hold in cases of perception that are not occurrences of illusion.

The form of the argument from illusion that I have just outlined may be called the general version of the argument from illusion. The reason for calling it this is that the first premise is quite general. The argument is not reliant on the contingencies of illusions concerning particular properties: any genuine case of illusion will be sufficient to get the general version of the argument from illusion running. The generality of the present version of the argument has consequences for the kind of strategy that is needed to respond to it. Once a case of illusion has been accepted, the challenge that the general version of the argument presents to the Direct Realist is to make sense of it. The further stages of the argument aim to show that the only way to do so is to reject Direct Realism. Defending Direct Realism against this argument therefore requires, at least as an opening manoeuvre, providing reasons for rejecting one of these further stages.

Attempts to resist the argument from illusion most commonly focus upon the second stage, the sense datum inference. In particular, proponents of Direct Realism put forward reasons for rejecting the principle on which it turns, which asserts that in illusory experiences there is some object of awareness that has the illusory property. There are two popular suggestions for how we can reject this principle, each corresponding to different theories of perception: Intentionalism and Adverbialism. A brief discussion of these theories will be useful for illustrating why there is something genuinely challenging about the argument from illusion.

Intentionalism rejects the principle that in cases of perceptual illusion there must be something that possesses the illusory property on the grounds that, as a general truth about representation, when something is represented as F there need not

be something that is F. Consider the case of belief discussed earlier, the case of John's believing the tomato he sees to be black, where the tomato is red and not black. The belief case, I said, does not provide us with the materials for a valid application of Leibniz's Law; this is because the property of blackness occurs as part of the representational content of the belief. Perception is, according to the Intentionalists, a form of representation and illusion a form of misrepresentation. Misrepresentations, although they are non-veridical, nonetheless do not require there to be something that has the property given in the representational content.

This strategy of the Intentionalists for rejecting the principle underpinning the sense-datum inference rests on the entirely questionable assumption that the illusory properties are merely part of the representational content of the experience. If the Intentionalist accepts this assumption then they will be hard pressed to say what it is that distinguishes perceptual states from other kinds of intentional mental states. It seems sensible that one of the differences between believing a particular tomato to be black and seeing it as black is that the property of blackness is not merely intentionally present in the latter case. Intentionalists argue that it is not part of the notion of representation that there need be anything that has the represented property; this response is effectively to argue that there is something about the nature of *perceptual* modes of representation that justifies the principle that underpins the sense-datum inference.<sup>10</sup>

The second strategy for rejecting the principle underpinning the sense-datum inference comes from Adverbialism. Adverbialism concedes that illusory properties can be present in a perceptual experience, and not be merely intentionally present. Nonetheless, it also proposes to deny that there need be any object of awareness that possesses these properties. Adverbialists do so by claiming that the properties are modes of the experience itself, rather than properties of the object of the experience. Reflecting this, the proper way to express the properties of experience is using adverbs: instead of describing the experience of being aware of something that is black we describe the experience as an instance of sensing blackly.

There is a debate about whether the Adverbial theory has the resources to adequately deal with the structural complexity that is to be found in perceptual

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<sup>10</sup> Howard Robinson pursues this line of objection in H. Robinson. *Perception* (London: Routledge, 1994): pp. 163-174. Similarly, A. D. Smith argues in the same vein in A. D. Smith. *The Problem of Perception* (Cambridge, Mass.: Harvard University Press, 2002): pp. 40-50.



experience. Jackson suggests that it would not be able to adequately deal with a single experience of, say, a red square to the left of a green triangle. Some device is needed to reflect the fact that the adverbs ‘redly’ and ‘squarely’ should go together and ‘greenly’ and ‘triangularly’ should go together, and in such a way that also allows us to articulate the spatial relations present in the experience. Jackson suggests that this cannot be done without introducing objects of experience. Michael Tye has attempted to show that it is possible for the Adverbialist to express this complexity.<sup>11</sup>

However, as Smith has argued, even if one accepts the Adverbialist’s point that such properties are in experience, but one is not compelled to introduce objects of experience to be able to even acknowledge their presence, the argument from illusion will not have been adequately dealt with. This is because it remains true that in cases of illusion there are non-intentional properties in experience that are not properties of the objects we pre-theoretically take ourselves to be aware of. Acknowledging the Adverbialist’s point means that we are not *forced* to accept the conclusion of the argument, but we have not been shown that we can *explain* the presence of these properties without introducing some sort of intermediate object of awareness. It remains possible that either the experience or the property itself could be such an intermediate object. The challenge posed to the Direct Realist is, as Smith observes, to *show* that the possessor of the property, or even the property itself, is not an object of awareness. The argument on this view is not to be construed as a deductive argument for Indirect Realism, but an abductive argument for it.

As Smith argues, to show that the properties are not objects of awareness requires giving an adequate analysis of perception on which this turns out to be the case:

It is, however, perhaps just possible that we shall be able to develop an analysis of perception according to which such qualities do not function *as immediate objects of awareness*.<sup>12</sup>

Such a theory of perception would explain why the non-intentional properties in experience, or experience itself, are not to be counted as intermediary objects of

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11 For Jackson’s arguments see Frank Jackson. *Perception: A Representative Theory*. Cambridge: Cambridge University Press, 1977, chapter 3. For Tye’s response, see M. Tye. “The Adverbial Approach to Visual Experience” *Philosophical Review* 93 (1984): 195-225.

<sup>12</sup> *Ibid.*, p. 55

awareness. It is not my purpose here to examine or develop any such substantive analysis of perception, but to explore the question of the geometry of visual experience. The significance of the preceding discussion is that, insofar as one is sensitive to the challenge posed by the argument from illusion, there is a philosophically interesting form of the argument from illusion that is based on the geometry of visual experience.

### **The Relation of the Question of the Geometry of Visual Experience to the Traditional Debate About Spatial Properties**

In the preceding section I outlined the general structure of the argument from illusion. It should be clear that if the geometry of visual experience diverges from the geometry of the objects we pre-theoretically take ourselves to be aware of, i.e. tables, clouds, and so on, we will be in a position to run an argument from illusion, but a particular version based on the geometry of visual experience. Above, we considered the argument in its general form. The first premise was quite general: things can appear other than they are. As was said, the way to block the argument in its general form involves giving an analysis of perception on which it turns out that the properties identified as belonging to visual experience that generate cases of illusion are not to be counted as being properties of objects of awareness, or counted as themselves objects of awareness. As I have said, my purpose here is not to explore this line of inquiry. The discussion above just serves to provide some motivation into the inquiry into the geometry of visual experience by highlighting the way in which Direct Realism would be challenged if the geometry of visual experience is not the geometry of physical space. I will now proceed to argue that there is a philosophically interesting version of the argument from illusion, which takes the overall geometrical structure of visual experience as the set of properties that are illusory.

As I said at the outset, it is nothing new to attempt to get the argument from illusion up and running by observing a disparity between the spatial properties of the objects we pre-theoretically take ourselves to be aware of and the spatial properties of the immediate objects of visual awareness. As David Armstrong has put it “we have been confronted *ad nauseam* with the penny that looks elliptical when viewed

obliquely.”<sup>13</sup> However, some philosophers have attempted to block such a specific version of the argument from illusion at a more local level than has been suggested is needed for the general form of the argument. This is a version of the argument that is not based on the general possibility of illusion, but is based in particular on spatial properties.

What is interesting is that the argument from illusion based on the geometry of visual experience, if it can be made, will be philosophically more challenging than this ‘traditional’ form. To show this, I will highlight some of the contrasts between what I will call the traditional form of the argument from illusion concerning spatial properties, and the argument from illusion that arises from reflection on the geometry of visual experience.

In traditional discussions, when concerns about the spatial properties of visual experience arise in connection with the argument from illusion, it is common to find a number of claims made about the spatial properties of our visual experiences that, if the answer to the question about the geometry of visual experience turns out to be a non-Euclidean geometry, are quite simply false. Clearing up some of these issues will therefore be of some merit on its own, but there is more at issue than this. If these traditional observations are false because a mistaken geometry has been assumed for the description of the experiences, then the local ways of blocking the argument from illusion based on spatial properties are misguided. Moreover, these local ways of blocking the traditional form of the argument are ineffective against a form of the argument from illusion based on a difference in the geometry of visual experience and the geometry of the space in which objects in our surrounding environment are arrayed. This cuts out one of the options available to the Direct Realist to respond to the argument from illusion based on spatial properties.

To illustrate what I mean by this last point I will rehearse some of the usual ways in which the traditional form of the argument from illusion based on spatial properties is set up. This argument is based on assertions that are typically made about the apparent shapes of external objects, and which has been a staple of philosophical discussion of perception for a long time: perspectival properties. It is commonly asserted, as one of a number of possible means of getting the argument from illusion going, that thin cylindrical or ‘circular’ objects like coins or plates appear elliptical.

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<sup>13</sup> D.M. Armstrong. *Perception and the Physical World* (London: Routledge and Keegan Paul, 1961), p. 11

when viewed in an orientation other than orthogonal to the line of sight. Even though the properties under discussion are thought of as perspectival properties, the justification for this claim is an appeal to the phenomenal character of the relevant visual experiences. The idea is that anyone with even a rough understanding of the difference between a circle and an ellipse who reflects on their visual experience will recognize that there is some sense in which the predicate ‘elliptical’ applies in such situations.

The next step is to argue that there is something that one is aware of in such a situation that is elliptical. At this point Direct Realism is under fire because the argument from illusion is up and running. However, the thing to notice is that early on this argument tacitly concedes that the geometry of visual experience is Euclidean. What is being contended is that a concept from Euclidean geometry, ‘being an ellipse’, needs to be used to adequately describe a visual experience of some external object that is not an ellipse – but which nonetheless has a Euclidean shape. If the arguments under consideration in this thesis that favour one of the non-Euclidean geometries turn out to be successful, then this way of motivating the argument from illusion has been misguided all along. This is because the shapes of the elements phenomenologically available in the visual field are radically different from what has commonly been asserted – they are shapes that could not possibly obtain in a Euclidean space. However, this is not an observation that particularly favours the Direct Realist, because it now turns out that a number of the ways in which the Direct Realist can respond to the traditional form of this argument cannot straightforwardly be applied to the new problem.

At its most general level, one way in which Direct Realists typically respond to this traditional line of argument concerning perspectival properties is to try to find some way of reconciling the applicability of the predicate ‘is elliptical’ to visual experience with the fact that the coin, or perhaps its surface, is not actually an ellipse. Moreover, this reconciliation needs to be effected in such a way that the proposed property of being elliptical can quite properly be construed as a feature of the external object. To illustrate how this kind of response goes, and so to illustrate why it is ineffectual against the version concerning non-Euclidean geometrical properties, I will discuss this argument as it appears variously in the works of J.L. Austin, C.W.K. Mundle, D.M. Armstrong and more recently by proponents of the ‘enactive’ theory of perception, such as Alva Noë.

The argument I loosely described above, concerning circular or cylindrical objects looking elliptical, is sometimes formulated more precisely as turning on a contrast between the single, real shape of an object and the multiple, variable ways in which it appears from different points of view. Here is how Mundle expresses it:

Since the penny retains a specific shape, size and colouring, and since the corresponding sense-data vary so widely in their shapes, sizes and colours, it is impossible to identify most of the sense-data with the surface of the penny. The same surface of the same physical object cannot simultaneously be round *and* elliptical....<sup>14</sup>

Restricting ourselves just to shape properties, we can articulate a precise form of this argument:

- 1) Physical objects, or their surfaces, have a single real shape.
- 2) Often, under a variety of circumstances, whatever we are immediately aware of in visual experience has a different shape from the real shape of the physical object or its surface.
- 3) At least in this range of cases, the physical object cannot be what we are immediately aware of in visual experience.

One kind of response to this argument involves taking exception to the use of the word 'real' in this context. This general kind of approach was taken by Austin in his *Sense and Sensibilia*. He argued that 'real' is a quite normal word with a fixed meaning that has currency in everyday, non-technical use. 'Real', he asserts, is a word that is 'substantive hungry'. Where it is possible to meaningfully say 'This is pink' without knowing what it is, this is not so in the case of the word 'real'. 'This is real' only acquires a definite sense when we understand what kind of a thing it is supposed to be a real instance of. This is because, as Austin points out, 'one and the same object may be both a real *x* and not a real *y*.'<sup>15</sup> The function of 'real', Austin claims, is to mark out a distinction between those things that are real instances of a kind and those that are not, but it is the negative use that determines the meaning of the positive use.

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<sup>14</sup> C. W. K. Mundle. *Perception: Facts and Theories* (Oxford: Oxford University Press, 1971), p.28

<sup>15</sup> J. L. Austin. *Sense and Sensibilia* (Oxford: Oxford University Press, 1962), p. 69

It is what Austin calls a ‘trouser word’ – it is the ways in which a thing could be not a real x that gives the sense of ‘is a real x’:

But with ‘real’ (as we noted earlier) it is the *negative* use that wears the trousers. That is, a definite sense attaches to the assertion that something is real, a real such-and-such, only in the light of a specific way in which it might be, or might have been, *not* real. ‘A real duck’ differs from the simple ‘a duck’ only in that it is used to exclude various ways of being not a real duck – but a dummy, a toy, a picture, a decoy, etc.; and moreover I don’t know *just* how to take the assertion that it’s a real duck unless I know *just* what, on that particular occasion, the speaker has in mind to exclude.<sup>16</sup>

For Austin, it is just not clear what sense in general can be attached to the ‘real shape’ of physical objects. This is because ‘real shape’ only has content in relation to its negative use – the use employed in premise (2). Austin thinks that the case of the coin masks the complexities involved here, because we have a single word to describe it. However, this is not always the case:

But coins in fact are rather special cases. For one thing their outlines are well defined and highly stable, and for another they have a *known* and a *nameable* shape. But there are plenty of things of which this is not true. What is the real shape of a cloud? And if it be objected, as I dare say it could be, that a cloud is not a ‘material thing’ and so not the kind of thing which has to have a real shape, consider this case: what is the real shape of a cat? Does its real shape change whenever it moves? If not, in what posture *is* its real shape on display?... It is pretty obvious that there is *no* answer to these questions – no rules according to which, no procedure by which, answers are to be determined. Of course, there are plenty of shapes which the cat definitely is not –

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<sup>16</sup> Ibid., p. 70

cylindrical, for instance. But only a desperate man would toy with the idea of ascertaining the cat's real shape 'by elimination'.<sup>17</sup>

One point to note is that if there is any problem here at all regarding the real shape of cats, the problem applies to all spatial objects. The problem regarding change of shape over time is in principle just as applicable to the coin as it is to the cat. If I dent a coin, or heat it up, it changes its shape: the fact that in the case of the cat its change of shape is partly the result of its own agency is irrelevant here. All this example shows is that if there is to be a meaningful sense to 'real shape' it must be temporally indexed.

The idea behind Austin's discussion seems to be that if the suggested contrast between real shape and non-real shape cannot be intelligibly be made, the conclusion (3) cannot be intelligible either. The main problem with Austin's attempt to solve this problem is that he focuses on just the one use of the word 'real' – the use exemplified by the contrasting locutions 'is a real x' and 'is not a real x'. C. W. K. Mundle, in his discussion of the issue of real shape, identifies two further uses of 'real'. The first of these is what he terms the 'existential' sense of 'real', together with its contrary 'unreal'. This is where to assert that a given thing is real, one asserts that it exists; conversely, to assert that a thing is unreal is to assert that it does not exist:

In another common use of "real", "so and so is real" means that so and so exists, and "unreal" means non-existent.<sup>18</sup>

The second use of 'real' that Mundle identifies is the one most relevant to the argument at hand – and also offers a way of blocking the argument. This is the common use of 'real' that is contrasted with 'apparent', when used 'to describe a perceptible property'.<sup>19</sup> So, to speak of a 'real shape' just is to mark out the contrast with the shape property an object may appear to have but in fact does not have. It marks out precisely the contrast easily expressible for simple geometrical shapes: the contrast that Austin was so sceptical about drawing for more complex geometrical shapes in the last passage just quoted from him.

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<sup>17</sup> Ibid., p. 67

<sup>18</sup> C. W. K. Mundle. *Perception: Facts and Theories* (Oxford: Oxford University Press, 1971), p.79

<sup>19</sup> Loc. Cit.

Even though Mundle is prepared to admit that there is a perfectly legitimate and intelligible contrast that is expressed by the use of 'real' as applied to shape properties in premises (1) and (2), he thinks that this does not mean that the argument goes through. This is because if 'real shape' gets its sense by contrast with how the external object appears when in orientations other than orthogonal to the line of sight, then 'real shape' in premise (1) just means the shape the object appears to have when viewed in an orientation orthogonal to the line of sight. In this case, Mundle argues, premise (2) will just be an unsurprising consequence of premise (1). The conclusion of the argument is no longer forced upon us, because an object being a shape that appears circular from one point of view is compatible with the object being a shape that appears elliptical from another.

When, in the premise, the penny is said to have 'a single real shape', what is meant by "real shape" is the shape the penny looks(ph) when viewed from some point normal to one of its surfaces. (Note that "normal to" here means 'perpendicular to'.) In that case it is a mere tautology to add that the shapes the penny looks(ph) from substantially different angles are not, in *this* sense of "real shape" its real shape. It is obviously invalid to infer from this that anything is not real in some *other* sense of the "real", e.g. the existential sense. But this is the conclusion that is drawn from the argument in question... that what *exists* in my field of view and is visibly elliptical is not the surface of the penny and must be something else. This conclusion is drawn as a result of failing to distinguish different uses of "real".<sup>20</sup>

There is nothing about contrasting the 'real' shape of an object with its apparent shape that forces us to conclude that the apparent shape cannot be a property of the external object. The final thing that is needed to deal with this particular form of the argument is to show that it is possible to construe properties like 'appears elliptical from x', as picking out genuine properties of external objects. Various philosophers, most notably the advocates of the 'enactive' theory of perception in recent times, have pursued this aim by claiming that awareness of apparent properties

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<sup>20</sup> Ibid., p. 80



is awareness of the projective properties of an object. In Mundle's argument, the contrast between 'real' and 'apparent' shape properties appears to be one of convention. The real shape is picked out as the shape the external object appears to have from some particular point of view: that where the object stands in a certain orientation to the line of sight. However, nothing particular turns on this, in fact contemporary discussions of this point sometimes forego discussing 'real' shape properties at all. After all, the surface of a coin *is* (roughly) a circle and *not* an ellipse.

One very clear proposed explanation of the legitimacy of construing properties like 'appears elliptical from x' as a genuine property of external objects comes from Armstrong in his *Perception and the Physical World*. The idea is that what is picked out by these locutions is a certain projective property that the external object legitimately has. Armstrong illustrates this by means of the notion of a "'square' shape":

Suppose that there were an open grille with squares like graph paper set up perpendicularly at a short, but fixed, distance in front of our eyes. Suppose further that lines are drawn from the perceived object to our eyes. These lines will form a pattern on the grille of a certain size and shape. Let us call what is projected on the grille the 'square' size and shape of the object perceived. Now a thing's 'square' size is perfectly objective.... It is a function of the object's size and shape, together with the spatial relations that the object has to our body....

If we view a round penny from an oblique angle it is geometrically necessary that its 'square' shape is elliptical. What is more, an object's 'square' size and shape is something that we can and do actually *observe*, just as much as we can observe the *intrinsic* shape and size of the object. The fantasy of the grille is just a device to make the conception of 'square' size and shape more vivid. We can, and do, perceive what is the projected size and shape of a perceived object onto a perpendicular cross-section of the space before our face.<sup>21</sup>

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<sup>21</sup> D.M. Armstrong. *Perception and the Physical World* (London: Routledge and Keegan Paul, 1961), p. 13

This is a reasonably clear attempt to construe apparent spatial properties as perfectly good objective features of the external objects of awareness. It is a proposal echoed by proponents of the ‘enactive’ account of perception, such as Alva Noë in his *Action In Perception*:

P-properties – the apparent shape and size of objects – are perfectly “real” or “objective.” Indeed, the relation of P-shape and P-size to shape and size can be given by the most precise mathematical laws.... Importantly, in order to characterize P-properties, there is no need to refer to sensations or feelings.<sup>22</sup>

We are now in a position to see why the issue of the geometry of visual experience generates a new and interesting form of the argument from illusion, based on spatial properties. If any of the arguments to be considered in this thesis for a non-Euclidean geometry for visual experience are correct, then this presents a problem for this kind of attempt to construe apparent properties as spatial properties of external objects. This is because in the traditional form of the argument the real properties of the object are, *ex hypothesi*, Euclidean properties; as are the apparent properties. What was needed to be shown in that case was that the apparent properties just were not the contraries within Euclidean geometry of the real Euclidean property. If all apparent shape properties turn out to be non-Euclidean properties then the situation is not so simple, because they will all be incompatible with *any* Euclidean real property. This provides a much harder version of the argument from illusion for spatial properties than has previously been appreciated – largely due to an unreflective acceptance that visual experience is Euclidean. In chapter 3 I will take up this issue in more detail and argue that a number of proposals to evade this difficulty are not effective.

Another way of dealing with the traditional argument, which the issue of the geometry of visual experience has a bearing on, is one offered by Smith in his *The Problem of Perception*. Smith’s strategy is to deny that external objects appear to have any shape other than they actually have; he insists that the proper description of the shape of the penny *must* involve how it appears oriented to the observer. The

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<sup>22</sup> Alva Noë. *Action in Perception* (Cambridge, MA: MIT Press, 2004), p 83

correct description of the coin in orientations other than orthogonal to the line of sight, according to Smith, is that it looks round and tilted away from you:

... the suggestion that pennies, for example, look elliptical when seen from most angles is simply not true – they look round – and in no sense... is the look of such a tilted penny an illusion. Such a penny (usually) looks just the way it is: round and *tilted away from you*.<sup>23</sup>

Smith wants to claim that when shape constancy is, or even merely could be, in operation in our visual experience, we can just drop completely the predicate ‘appears elliptical’ in favour of ‘appears round and tilted’ as a description of the immediate object of our awareness. In doing so, we can avoid any temptation to conclude that it is not the external object. However, with the version of the argument from illusion where the apparent properties are non-Euclidean properties and the real properties are Euclidean this strategy will not be effective. This is because even if we grant that in such cases the object of immediate awareness appears turned away from the observer, this will not eliminate the incompatibility of it appearing a non-Euclidean object turned away from the observer, which the external object, *ex hypothesi*, is not.

So, I have given examples of two kinds of responses to the traditional argument from illusion concerning shape properties, and I have argued that these will not be effective against a version of the argument from illusion that takes apparent shape properties of objects to be shape properties from a geometry different from the geometry of the space in which the external object are arrayed. This, I suggest, means that the issue of the geometry of visual experience is of some philosophical interest.

### **Physical Space and Geometry**

In both the traditional version of the argument and the new version based on the geometry of visual experience, the thing that gets the challenge to Direct Realism going is the discrepancy between the correct geometrical descriptions of the visual experience, on the one hand, and the physical objects, on the other. There are

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<sup>23</sup> A. D. Smith. *The Problem of Perception* (Cambridge, Mass.: Harvard University Press, 2002), p. 172

therefore two variables that could be changed to get this discrepancy: the description of the experience and the description of the physical objects. I have said that in the traditional form of the argument from illusion based on spatial properties there is a tacit assumption that both of these descriptions are fundamentally of the same sort: Euclidean. In the traditional form of the argument it is the distribution of Euclidean predicates to the elements of visual experience that is incompatible with the distribution of Euclidean predicates to the elements of physical objects.

In the new version of the argument, the challenge to Direct Realism arises from a discrepancy in the *kind*, rather than the distribution, of predicates ascribed to the elements of visual experience and the elements of physical objects. In the preceding discussion, I illustrated the possible discrepancy by holding fixed the assumption of the traditional argument that the geometry of physical objects is Euclidean. However, this is not the only way to generate the new version of the argument from illusion; all that is needed is the disparity between the two variables. So, by holding fixed the geometry of visual experience we could plausibly still generate the argument by arguing for a different geometry for physical space. It has been suggested that the Theory of Relativity provides support for such a move on the side of the geometry of physical space; here is a quote from James Hopkins:

On interpretations which are common, plausible, and scientifically useful, the geometry of space according to Einstein's theory is not in general Euclidean.<sup>24</sup>

Hopkins himself uses this consideration, together with others that we will come to in chapter 2, to motivate the concern about perception:

It seems that science has given reason for thinking that Euclidean geometry is false, that physical space may most accurately be described by a non-Euclidean geometry. Yet examples lead us to suppose that the only space we can imagine, picture, or visualise, is one described by Euclidean geometry. But the space it seems we must

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<sup>24</sup> James Hopkins. "Visual Geometry" *The Philosophical Review* 82 (1973): 6

picture as Euclidean is the same space as that which, on scientific grounds, is judged non-Euclidean.<sup>25</sup>

So, it would seem that the philosophical interest surrounding the issue of Direct Realism requires looking at both of these variables: the geometry of physical space and the geometry of visual experience. In the absence of a convincingly argued answer to both the question about the geometry of visual experience *and* the question about the geometry of physical space we will not even be in a position to run the new form of the argument from illusion based on spatial properties. However, it is obviously not possible to provide a comprehensive treatment of both sides of this issue here. The aim of this thesis is strictly to look at the cases that have been put forward for the various different geometries for visual experience in question here. However, most philosophers who have been concerned with the question of Direct Realism in connection with the geometry of visual experience *have* assumed that the geometry of physical space is Euclidean. It is, therefore, worth outlining a number of possible considerations that may motivate this claim about physical space, even in the face of what has been said above about the Theory of Relativity.

The first point to note is that in non-Euclidean spaces of constant curvature there are small regions surrounding points that answer to Euclidean descriptions. This is sometimes expressed by saying that such spaces are locally Euclidean, but globally non-Euclidean. This applies to the case of physical space: the phenomena that suggest a non-Euclidean geometry for physical space occur over very large distances, such as those between stars. It is possible, then, to claim that despite being globally non-Euclidean, physical space is locally Euclidean. In visual perception we are typically aware of local physical phenomena, such as tables, clouds and expanses of sky.<sup>26</sup> In this way, it could be claimed that if it could be shown that the geometry of visual experience is Euclidean it would be legitimate to claim that there is no disparity between this geometry and the geometry of local physical objects.

This looks, at first pass, like a way to maintain that there is some sense that can be made of the claim that physical space is Euclidean, despite the claims of Physics. However, this line of reasoning is open to the following kind of objection:

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<sup>25</sup> Ibid., p. 7

<sup>26</sup> There is, of course, considerable room for objection here – what are we to make of our visual awareness of the sun, which is no small object; or our awareness of the distances between stars?

surely it is impossible for a space to be at once globally non-Euclidean and locally Euclidean. Hopkins offers an argument for this claim, which is based on the relations of parts of a space to the whole:

The situation alluded to here is not that one geometry is true of small regions while another, inconsistent with it, is true of large regions. This could not be the case. Large regions are composed of small regions; and if one spatial region is Euclidean, and another adjoining region is Euclidean, then the larger region composed of the adjoining regions must also be Euclidean. So if we regard large regions as non-Euclidean, we cannot regard the small regions composing them as Euclidean.<sup>27</sup>

Hopkins goes on to claim that what it means to say that a globally non-Euclidean space can be locally Euclidean is just that the local phenomena can be described by Euclidean geometry within an acceptable degree of accuracy.

One possible response to this argument of Hopkins' is to point out that it relies on the questionable assumption that whatever the geometry of some whole space is, the geometry of its parts must be the same, and vice versa. Now, if this is true, it is not so in virtue of some general principle of mereology. Nonetheless, Hopkins' point does seem intuitively plausible, especially if we accept Patrick Heelan's point, discussed at the outset of this chapter, that for every given point within a space, the only figures constructible at that point are those given by the geometry of the whole space. In this case it looks like we are committed to accepting Hopkins' point.

There is at least one possible line of response to this, which is to argue that the plausibility of Hopkins' claim turns on a view of space as a substratum that is independent of spatial objects. The plausibility of Hopkins' point seems to come from the thought that local 'regions' of space are parts of a larger substratum, and that the geometrical character of the parts are, in this case, determined by the geometrical character of the whole substratum. However, if we just think of space as a system of relations between physical objects, then it is less clear that we must think that the relations obtaining between the elements in one set of objects, those that are

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<sup>27</sup> Ibid., p.21

reasonably small and near to us, are determined by the relations obtaining between the elements in a larger set, containing the original set. This could be because the relations depend in some way on the absence or presence of some elements in the set. As Patrick Suppes observes, such contextually determined relations are common in physics:

Consider, for example, the corresponding situation with bodies that attract each other by gravitation. The introduction of a third body makes all the difference to the motions of the two original bodies and it would be considered bizarre for the situation to be otherwise. This also applies to electromagnetic forces, mechanical forces of impact, etc. Contextual effects are the order of the day in physics, and the relevant physical theories are built to take account of such effects.<sup>28</sup>

My purpose here is not so much to defend the position that I have outlined above; it is after all, quite counterintuitive. However, it at least illustrates a way of claiming that, whatever the deliverances of physics may be, those physical objects, which we pre-theoretically take ourselves to be aware of in perception, answer to a Euclidean geometrical description.

There is another, less committing, line of argument which may diminish the apparent need for a definitive answer to the question of the geometry of physical space, within the context of the concern about Direct Realism. Let us grant that something like what the Theory of Relativity claims is true: i.e., that the physical world is globally non-Euclidean, whatever the details turn out to be. It will still be true that, to an acceptable degree of accuracy, we can describe local phenomena using Euclidean geometry. Now if we add to this the quite plausible assumption that our inability to perceive the actual way things are results from the fact that there are limits to human visual acuity, then we should not expect the geometry of the experiences of local phenomena to diverge in a radical and noticeable way from the roughly accurate geometrical characterisation of the physical objects. So, for example, if a physical triangle about the size of the musical instrument can be correctly said to have internal angles that add up to 180 degrees; within a very small margin of error that cannot be

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<sup>28</sup> Patrick Suppes. "Is Visual Space Euclidean?" *Synthese* 35 (1977): 416

noticed due to the limitations on human visual acuity, then we should not expect the geometry of visual experience to ascribe an angular sum to such a local object that is, upon reflection, noticeably greater or less than 180 degrees.

This gives us a way of testing, independently of any knowledge of what the correct geometry of physical objects is, whether there is a divergence of the spatial properties of physical objects and the spatial properties of visual experience. If there is no divergence, then the geometry of visual experience *should be* roughly Euclidean. This offers two approaches to the question of the geometry of visual experience, both of which will be followed here. First, we can investigate whether there are any arguments favouring a Euclidean geometry for visual experience and assess how successful they are. Second, we can see whether there are any countervailing arguments, which favour other geometries for visual experience. In this way, we can fruitfully look at the arguments for a geometry of visual experience without any prior commitments in respect of the truth of the Theory of Relativity.

### **The Possibility of a Geometry of Visual Experience**

Before turning to the various arguments for different geometries, it is important to look at one crucial assumption of the whole debate about the geometry of visual experience. This is the assumption that it is possible to give a geometrical description of visual experience. Considering this assumption will also provide further historical background to the question at hand.

As was emphasised at the beginning, throughout the history of philosophy a number of thinkers have pursued the question of the geometry of visual experience. Prior to the development and acceptance of non-Euclidean geometries, however, the precise question we are concerned with in this thesis could not be raised with much clarity. Despite this, it is interesting that a number of philosophers of the Eighteenth and Nineteenth Centuries who explicitly discussed the geometrical characterisation of visual experience in ways relevant to the present debate were aware of the attempts to show the impossibility of non-Euclidean geometries. Some ascribe to Thomas Reid the discovery of a non-Euclidean geometry prior to the work of Bolyai and Lobachevski. Reid, it seems, was aware of the (unsuccessful) early attempts to show that denying the parallel axiom resulted in incoherence. Immanuel Kant was also



aware of these attempts and in his discussion of space asserts that there is no *conceptual* contradiction to be found in geometrical propositions that are not compatible within a geometry:

That in such a [synthetic *a priori*] concept no contradiction must be contained is, to be sure, a necessary logical condition; but it is far from sufficient for the objective reality of the concept, i.e. for the possibility of such an object as is thought through the concept. Thus in the concept of a figure that is enclosed between two straight lines and their intersection contain no negation of a figure; rather the impossibility rests not on the concept in itself; but on its construction in space....<sup>29</sup>

As the final sentence suggests, for Kant the special status of Euclidean geometry is to be accounted for in a way that does not appeal to *logical* incoherence. This aspect of Kant's discussion and its relation to the issue of the geometry of visual experience will be taken up in more detail in chapter 2.

With the notable exception of George Berkeley, prior to the philosophers mentioned above, there was no investigation into the geometry of visual experience distinct from an investigation into optics, such as those investigations conducted by Euclid and by Newton. Berkeley's concern about the geometry of visual experience, or our idea of 'visible extension', takes on the character of asking whether visible extension is a proper 'object' of geometrical description. This question has been interpreted in the following way: could there possibly be a geometrical description of 'visible extension', or visual experience? This question is significant to the line of enquiry in this thesis, because if it can be shown that the correct answer is to deny the possibility of a geometrical description to visual experience then the new version of the argument from illusion based on spatial properties cannot even get started.

Berkeley, it is claimed, gave a negative answer to this question. As I will argue, I suspect that Berkeley is not really concerned with the question of the *possibility* of a geometrical description of visual experience. Norman Daniels, in his discussion of Berkeley's position, in Daniels' *Thomas Reid's 'Inquiry': the Geometry of Visibles and the Case for Realism* calls the negative answer to the question about the

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<sup>29</sup> I. Kant. *Critique of Pure Reason*, trans. P. Guyer and A. Wood. (Cambridge: Cambridge Univ. Press, 1998), p 323

possibility of a geometrical description of visual experience ‘the Strong Negative Thesis’. This thesis is to be contrasted with ‘the Weak Negative Thesis’, which denies just that geometry *usually* describes visual experience.<sup>30</sup> Now, I find it so implausible that anyone would even entertain the idea that geometry *usually* describes visual experience that I find it hard to credit anyone, let alone a philosopher of Berkeley’s abilities, with taking the proposal seriously enough to feel the need to argue against it. However that may be, as Daniels highlights, there is a question about how convincing the considerations Berkeley appeals to are against the very possibility that geometry could describe visual experience. In section 151 of his *An Essay Towards a New Theory of Vision*, Berkeley appeals to three considerations:

- (1) “visible extensions in themselves are little regarded.”
- (2) Visible extensions “have no settled and determinate greatness.”
- (3) “men measure altogether by the application of tangible extension.”

As Daniels correctly points out, none of these three considerations provides any convincing support for the Strong Negative Thesis, the claim that there could not possibly be a geometry of visual experience. Firstly, consideration (1) just seems irrelevant to this claim. Consideration (3) is only relevant to the possibility of a geometrical description of visual experience if it can also be shown that there is no metric that can possibly be assigned to visible experience. This looks at first pass like what (2) may be asserting, and read in such a way it seems highly questionable. If we look at Berkeley’s argument for this claim at section 60 of the *Essay* it becomes clear that either Berkeley made an egregious error in reasoning, or he was not claiming this. Section 60 just points out that a given visible extension can be present in experience when objects of different sizes are seen at different distances. As Daniels points out, this in fact implies that visible extensions have a determinate magnitude:

It should be obvious that Berkeley’s argument does not prove that visible extensions or figures do not have determinate magnitudes. In fact, Berkeley’s argument implies just the opposite. We do have a way of judging that the visible extensions are equivalent or we would not

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<sup>30</sup> Norman Daniels. *Thomas Reid’s ‘Inquiry’: the Geometry of Visibles and the Case for Realism* (Stanford, CA: Stanford University Press, 1989), p. 53

be able to associate the different tangible extensions with the *same* visible one.<sup>31</sup>

Now, it is possible that Berkeley did construe (2) along the lines of a denial of any possible metric for visible extension, and just made a mistake. However, this would not explain why considerations (1) and (3) seem so totally dislocated from the alleged target of the argument, the possibility of a geometrical description of visual experience. I suspect that in raising the issue of the ‘object’ of geometry, Berkeley is making a much more subtle point. This is that the fact that geometry is a science which takes ‘tangible extension’ as its subject matter is evidence for assigning ideas of tangible extension a special categorical status above ideas of visible extension. Putting the point in terms Berkeley may not have been happy with: tangible extension is the real extension of the objects of perceptual awareness; visible extension is just a representation of this real extension. Berkeley effectively says as much in section 152 of his *An Essay Towards a New Theory of Vision*, where he gives a positive characterisation of the general relationship between visible figures and tangible figures:

It is therefore plain that visible figures are of the same use in geometry that words are: and the one may as well be accounted the object of that science as the other, neither of them being otherwise concerned therein than as they represent or suggest to the mind the particular tangible figures connected with them.<sup>32</sup>

Berkeley does not appear to be saying anything about the possibility of giving a geometrical characterisation of visual experience; he seems to be trying to find reasons for ascribing a special status to our ideas of tangible extension, a status that can be withheld from ideas of visible extension. The grounds for denying it to the latter are given by the three considerations given above: these are just data about visual experience suggesting that it is tangible extension that is to be ascribed a special categorical status over visible extension.

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<sup>31</sup> Ibid., p. 54

<sup>32</sup> George Berkeley. *Philosophical Works: Including the Works on Vision* (London: J. M. Dent, 1996), p. 63

I have suggested that in these sections of the *Essay*, Berkeley was not seriously concerned with the question of whether a description of visual experience in geometrical terms could possibly be given. Rather, I have suggested, he was appealing to the fact that it is tangible extension that geometry is usually concerned with in order to show that tangible figure has a status comparable to revealing the 'real' shapes of the objects of perceptual awareness. If this is right, then even Berkeley did not deny that there could possibly be a geometry of visual experience. However, it is crucial to note that whether my interpretation of Berkeley is correct does not really matter for the purposes of this thesis. This is because if Berkeley *was* attempting to make such a denial, then the points raised by Daniels are perfectly valid against the proposed arguments for that position. Either way you turn, no good reason has yet been supplied for thinking that you cannot give a geometrical description of visual experience.

### **Methods of Enquiry and the Structure of the Thesis**

In what follows, I will look at various arguments that can be found for the various geometries for visual experience. As I outlined at the start of this chapter, throughout the history of this debate there have been a number of methods of enquiry used to establish the geometry of visual experience. Some contributors have employed straightforward introspection about visual experiences; some have employed arguments based on the shape of the eye; others have argued in an a priori manner from what are hoped to be minimal assumptions; still others have taken an empirical tack, running psychological experiments on subjects under controlled conditions. However, all of these approaches, if they are to be relevant to the question of visual experience, must have as their target a description of the phenomenal character of visual experience. For a number of these approaches, some work will be needed to show how the proposed arguments are directed towards this issue.

I will now turn to the main business of this thesis and consider these arguments, arranged according to which geometry they are put forward in support of. I will begin by looking at the arguments for a Euclidean geometry. In the chapters following this, I will look at a number of arguments for a spherical non-Euclidean

geometry. Finally, I will look at arguments and some experimental evidence for a hyperbolic non-Euclidean geometry.

## **Chapter 2 – Euclidean Geometry**

### **Introduction**

I have suggested that it is possible to motivate a version of the argument from illusion based on the geometry of visual experience, even in the absence of a commitment regarding the question of the particular geometry of physical space. This is because local objects, which are the objects of perceptual awareness, should satisfy a Euclidean description, to an acceptable degree of accuracy. A corollary of this is that anything identical to these local objects, such as the immediate objects of perceptual awareness, should also satisfy such a description to the same level of accuracy. This suggested two approaches: either investigate whether there are any arguments favouring a Euclidean geometry for visual experience, or see whether there are any countervailing arguments favouring other geometries for visual experience. In later chapters I will be following the latter of these two approaches. However, I will begin by looking at arguments for a Euclidean description for visual experience. In what follows, I will refer to the claim that there is a Euclidean description of visual experience as the ‘Euclidean thesis’.

There is very little explicit discussion of the Euclidean thesis in any of the literature, even by those who explicitly address the question of the geometry of visual experience. This is perhaps a little surprising, given that it seems to be an implicit assumption of the traditional forms of the argument from illusion based on spatial properties. What little discussion there is, especially in the latter half of the Twentieth Century is largely critical of the Euclidean thesis and is usually bound up with some of the alternatives that have been proposed.

There is a natural enough explanation for the absence of sustained discussion of the Euclidean thesis, which has to do with the history of discussions of this issue. Roughly speaking, prior to the discovery of non-Euclidean geometries, the Euclidean thesis was the only position that could be articulated. As such, there was no need to motivate the claim. After the discovery of non-Euclidean geometries, the theoretical possibilities of what the geometry of visual experience could be multiplied. In the initial period after the discovery it was still taken for granted by a number of

philosophers, such as Frege<sup>33</sup>, that the geometry of visual experience is Euclidean; this claim was used to account for the historical predominance of Euclidean geometry in mathematics. However, as there were now competing possible geometries for visual experience it eventually became apparent that reasons for accepting the Euclidean thesis would need to be supplied.

One of the few philosophers to attempt to motivate the Euclidean thesis after the discovery of non-Euclidean geometries is P. F. Strawson. His arguments emerge from an attempt to show that Kant's way of accounting for the peculiar status of geometrical propositions, whilst implausible as a general account, is nonetheless a plausible account of the nature of the geometry of visual experience. Some commentators on Strawson whose primary concern is the question about the geometry of visual experience, and not Kant scholarship, tend to be dismissive of Strawson's discussion. We have already encountered one such commentator in the previous chapter: Patrick Suppes. In his paper 'Is Visual Space Euclidean?' he is critical of Strawson for just ignoring the other possible geometries for visual experience and the wealth of empirical evidence supporting them. Suppes says:

From the standpoint of the large psychological literature I have surveyed, it is astounding to find Strawson asserting as a necessary proposition that phenomenal geometry is Euclidean.... The astounding feature of Strawson's view is the absence of any consideration that phenomenal geometry could be other than Euclidean and that it surely must be a matter, one way or another, of empirical investigation to determine what is the case. The qualifications he gives... do not bear on this matter but pertain rather to questions of idealization and of the nature of constructions, etc. The absence of any attempt to deal in any fashion whatsoever with the large theoretical and experimental literature on the nature of visual space is hard to understand.<sup>34</sup>

Whilst it is true that Strawson's discussion does not even mention the empirical evidence to which Suppes refers, this alone is not sufficient to dismiss his position. Moreover, Suppes' dismissal is unfair to Strawson — it does not even mention

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<sup>33</sup> G. Frege. *The Foundations of Arithmetic* (Oxford, 1950), p. 20

<sup>34</sup> Patrick Suppes. "Is Visual Space Euclidean?" *Synthese* 35 (1977): 415

Strawson's own arguments for his position. Whatever the eventual assessment of his arguments may be, the arguments are more substantive than Suppes seems prepared to allow, given the brevity with which he sums up Strawson's position, which lasts little more than the paragraph just quoted.

Consequently, I will spend the first part of this chapter articulating and evaluating Strawson's position. After discussing Strawson's position, I will turn to two authors whose discussions on the geometry of visual experience are historically and thematically related to the claim that visual experience is Euclidean, although they do not endorse this claim. The first of these authors is James Hopkins, who provides a critique of Strawson's position and in response proposes the view that visual experience has no determinate geometry. The second author is Hans Reichenbach, who offered arguments for the view that we can visualise non-Euclidean geometries. Reichenbach's arguments emerge from an analysis of the nature of the ability to 'visualise' figures.

### **Strawson's Defence of the Euclidean Thesis**

To understand Strawson's defence of the Euclidean thesis it is first necessary to understand the way in which Kant accounted for geometrical knowledge, and the reasons for his approach. Kant was concerned to reconcile what he saw as two features of geometrical knowledge. For Kant, 'geometrical knowledge' just meant Euclidean geometry. The two features of geometrical propositions he thought needed reconciling were:

- (1) The necessity of the axioms and theorems of Euclidean geometry.
- (2) That the truth of the axioms and theorems of Euclidean geometry could not be established by logic and was not just a matter of the definitions of their terms.

Strawson puts it like this:

He thought that Euclidean geometry applied to physical objects, to sense-given things in space. He was aware that the truth of its theorems was not guaranteed by logic and by explicit verbal definition. These



two considerations led him to say that it was a body of true synthetic propositions. On the other hand he attributed to the axioms and theorems a necessity inconsistent with their being merely empirical propositions.<sup>35</sup>

Kant thought that there was a potential problem if, in response to (2), we were forced to make the truth of geometrical axioms and theorems turn on empirical data. He thought that this would undermine (1): the non-logical necessity of those axioms and theorems. For this reason he postulated a non-empirical or ‘pure’ form of awareness, or ‘intuition’. This was then invoked to account for the necessary character of the propositions.

Subsequent developments in geometry suggest that Kant was wrong in his initial appraisal of the character of geometrical knowledge. This conception of geometry, which Strawson refers to as the ‘positivist’ account of geometry, has superseded Kant’s understanding and it offers two possible ways of looking at the propositions in a geometry. They are either to be taken as uninterpreted formulae in a calculus, or as interpreted formulae. Only when the formulae are given an interpretation do they express propositions about a set of objects and are truth apt. For the formulae to be interpreted, a domain must be specified. Typically, a geometry is given a physical interpretation – the domain specified is the set of points in physical space.

Now, Kant held that geometrical propositions are *at once* necessary and synthetic, in the sense of (2). However, the subsequent demonstration that non-Euclidean geometries are consistent, if Euclidean geometry is consistent, required a distinction to be drawn between the conditions under which a geometry is true of a domain of objects and a demonstration of that geometry’s logical consistency. Such a distinction opened up the possibility that the necessity of geometrical propositions is just logical necessity and only concerns uninterpreted formulae. Crucially, contra Kant, this kind of necessity is not threatened by the fact that it is contingent which set of objects that geometry is true of.

Here is Strawson’s characterisation of this position:

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<sup>35</sup> P. F. Strawson. *The Bounds of Sense: An Essay on Kant’s Critique of Pure Reason* (London: Methuen, 1966), p. 284

The problem does not exist, on this view, because in so far as there are necessary geometrical propositions, they are really truths of logic, only incidentally geometrical; while those propositions which are both synthetic and essentially geometrical are not necessary truths at all, but empirical hypotheses....<sup>36</sup>

The idea is that Kant introduced his notion of a non-empirical, or ‘pure’ form of awareness, or ‘intuition’, to characterise a necessity that was not a matter of logical necessity. However, it turns out that there is no reason to suppose that there is any other, non-logical kind of necessity attaching to geometrical propositions. Consequently, Kant’s talk of pure intuition is redundant.

Strawson accepts this general point; however, he suggests that there is some reason for thinking that there is a set of objects for which it is nonetheless not an empirical issue whether Euclidean geometry is the correct description of them. If there were such a set of objects, Strawson suggests, then it would perhaps be plausible to say that they are necessarily Euclidean. Strawson thinks that there is indeed such a set of figures: they are what he calls ‘phenomenal figures’.

Strawson appears to have two things in mind when he articulates what ‘phenomenal figures’ are, although he seems happy to run them together. Firstly, he includes figures that are delivered by what he calls ‘visual imagination’:

Kant said that it did not matter whether “construction of a [spatial] concept in pure intuition” took place with the aid of a figure drawn on paper *or simply in the imagination*. Now the visual imagination cannot supply us with physical figures. But it can supply us with what, for want of a better word, I will call *phenomenal* figures.<sup>37</sup>

Secondly, he includes amongst ‘phenomenal figures’ what he calls the ‘looks’ of physical objects:

The straight lines which are the objects of pure intuition are not... physical objects, or physical edges, which, when we see them, look

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<sup>36</sup> Ibid., p. 278.

<sup>37</sup> Ibid., p. 282

straight. They are rather the looks themselves which physical things have when, and in so far as, they look straight.<sup>38</sup>

All Strawson seems to mean by talking about ‘looks’ here is the phenomenal character of an experience – how things ‘appear’, in the phenomenological sense. The idea is that we systematise descriptions of visual appearances and we get a ‘phenomenal geometry’.

So, Strawson wants to claim that phenomenal geometry, the description of the appearances of objects and visually imaginable figures, is in some sense necessarily Euclidean. The character of this ‘necessity’ is that it is not an empirical issue whether Euclidean geometry is true of such figures. It is not obvious what Strawson means here; this is what he says:

If there can be such a thing as a system which is neither an uninterpreted calculus nor a physical geometry, but a phenomenal geometry, then it would be reasonable to say that it is, in a sense, independent of *empirical* intuition. So long as we can imagine spatially, we do not need, for phenomenal geometry, to check our results by reference to sense-given spatial objects.<sup>39</sup>

One thing that is certain from this passage is that Strawson thinks that the applicability of Euclidean geometry to phenomenal figures is independent of the way physical objects are. Later, Strawson articulates further what he means by explaining the necessity of Euclidean geometry for phenomenal figures in terms of its propositions being unfalsifiable:

Euclidean geometry may also be interpreted as a body of unfalsifiable propositions about phenomenal straight lines, triangles, circles, etc....<sup>40</sup>

So, what reason do we have for thinking that Euclidean geometry is unfalsifiable in respect of phenomenal figures? Strawson appeals to the fact that it

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<sup>38</sup> Loc. Cit.

<sup>39</sup> Ibid., p. 282

<sup>40</sup> Ibid., p. 286

seems impossible to visually imagine a figure that is contrary to Euclidean geometry. The example he chooses to elucidate this concerns the proposition from Euclidean geometry that not more than one straight line can be drawn between any two points: the parallel postulate. He says:

The natural way to satisfy ourselves of the truth of this axiom of phenomenal geometry is to consider an actual or an imagined figure. When we do this, it becomes evident that we cannot, either in imagination or on paper, give ourselves a picture such that we are prepared to say of it both that it shows two distinct straight lines and that it shows these lines as drawn through the same two points.<sup>41</sup>

This kind of consideration also struck Jonathan Bennett as being a plausible reason for thinking that we are ‘bound to regard spaces as Euclidean’.<sup>42</sup> The idea is that there is no possible phenomenal figure that would falsify the proposition that not more than one straight line can be drawn between any two points.

This part of Strawson’s account of the necessity he wants to ascribe to Euclidean geometry for phenomenal figures is not too difficult to swallow. It puts forward a piece of introspective evidence for the hypothesis that there is a set of figures, phenomenal figures, which Euclidean geometry is unfalsifiably true of. Problems emerge when we begin to consider his explanation of why there is this unfalsifiability, why it is impossible to visually imagine figures that contradict Euclidean geometry.

Strawson’s explanation of the impossibility is that the propositions are analytic, or ‘phenomenally analytic’; he claims they are ‘true solely in virtue of the meanings attached to the expressions they contain, but these meanings are essentially phenomenal, visual meanings, are essentially picturable meanings’<sup>43</sup>

This is where Strawson’s account is explicitly Kantian; he says:

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<sup>41</sup> Ibid., p. 283

<sup>42</sup> See Jonathan Bennett. *Kant’s Analytic* (Cambridge: Cambridge University Press, 1966): pp. 29-32.

<sup>43</sup> P. F. Strawson. *The Bounds of Sense: An Essay on Kant’s Critique of Pure Reason* (London: Methuen, 1966): p. 283.

Kant's phrase, "the construction of concepts in pure (i.e. non-empirical) intuition", does not seem at all a bad description of this essential method of exhibiting and elaborating the meanings of the expressions of phenomenal geometry.<sup>44</sup>

The idea is that propositions of Strawson's 'phenomenal geometry' are analytic. There is a difference between those pictures that can count as the meaning of 'two straight lines' and those pictures that can count as the meaning of 'two distinct lines both of which are drawn through the same two points'. This difference is somehow constitutive of having pictured what these expressions mean.

James Hopkins, in his article 'Visual Geometry', has provided a sustained critique of Strawson's position. Not surprisingly, he finds Strawson's account of the unfalsifiability of phenomenal geometry elusive. This is due mainly to the opacity of Strawson's notion of a 'phenomenal exhibition of meanings'. The most concrete idea that can be attached to this expression is the idea that a phenomenal figure, or mental image, or something, is the meaning of a term. If this is the most we can make of Strawson's claim, Hopkins argues, then it is not clear why the propositions of his phenomenal geometry are somehow necessary. This is by parity of reasoning with physical geometry: no exhibition of the meanings of the terms of a geometry given a physical interpretation, such that the propositions are all true, would be thought to justify the claim that that geometry is necessarily true of physical objects. Hopkins says:

For an exhibition of phenomenal figures, like one of physical objects, could naturally be taken to support no more than the claim that a certain geometry was contingently true of the exhibited objects.<sup>45</sup>

Hopkins observes that this is only a problem if you think that there is some sort of necessity attaching to phenomenal geometry, other than the kind of logical necessity of the interpreted formulae. If you give up on this, then there is no problem, which is precisely what Hopkins favours. He claims that '... there is no reason to

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<sup>44</sup> Loc. Cit.

<sup>45</sup> James Hopkins. "Visual Geometry." *The Philosophical Review* 82 (1973): p. 52

accept the assumption... that phenomenal propositions are necessarily true.’<sup>46</sup> Consequently, Hopkins argues that we should construe our inability to picture two lines along the same lines as a colour-blind person’s inability to picture anything red – as a contingent fact, dependent upon contingent features of the external world and our visual system.

Now, there is little doubt that Hopkins is right that what we can and cannot picture is dependent upon such contingencies. In this sense, what we can and cannot picture is not necessary – differences in the laws of nature or differences in evolutionary history would have resulted in different capacities. But this argument seems to totally bypass Strawson’s point.

If what Strawson meant when he claimed that these phenomenal propositions are necessary was that they would be true for humans in all possible worlds, including those with relevantly different laws of physics or human evolutionary developments, then Hopkins’ point would be a valid objection. Charges of anachronism aside, though, it is not clear that Strawson is committed to such a strong position. Strawson is certainly coy in his use of the term ‘necessary’, but it is clear from his discussion that it is the unfalsifiability of phenomenal propositions that distinguishes them from geometrical propositions that are given a physical interpretation. To be a valid response to Strawson’s argument, Hopkins’ objection would need to deny that there is any good reason for thinking that phenomenal propositions are *unfalsifiable*.

However, it is just not true that there is no reason to accept the unfalsifiability of phenomenal propositions. Our inability to picture explicitly non-Euclidean figures is precisely what led Strawson to formulate his position. Strawson’s appeal to Kantian construction in intuition appears to be just an attempt to offer an explanation of the unfalsifiability. What Hopkins’ discussion shows is that there is no reason to suppose that the unfalsifiability holds for all possible creatures with some visual apparatus. Strawson, however, is not committed to such a strong position.

In this respect Strawson’s position is different from the traditional position that has been ascribed to Kant, and different in a way that is directly relevant to the issue of Direct Realism. In accordance with the general drift of Kant’s transcendental arguments, it has commonly been held that Kant was attempting to articulate a position which maintains that the only *possible* form of outer intuition is Euclidean.

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<sup>46</sup> Ibid., p. 53

This translates to the stronger claim against which Hopkins' argument *is* valid – that visual experience must be Euclidean for all possible perceivers with a visual sense modality. It is also worth noting that this kind of position is of dubious use to the Direct Realist, despite the fact that there is no conflict between the local geometry of the world and the geometry of the visual experience. This is because the explanation of why the geometry of the experience is as it is, seems to be at odds with Direct Realism.

To see this, consider what a Direct Realist will want to claim about the geometry of an experience in a *visibly noticeable* spherical world. They will want to claim that the geometry of the experience is a spherical geometry. Moreover, it must be so because the geometry of the experience *depends upon* the geometry of the objects. This is quite general: they will want to claim that for any possible world in which Direct Realism is true, the geometry of the visual experience will match that of the geometry of the world. Moreover, they will insist that this match is not just coincidence – it holds because the geometry of the experience *depends upon* the geometry of the objects. To be a Direct Realist, it seems, it must be open to you to claim that the reason the geometry of the experience is as it is, has to do with the geometry of the external object. In the case of the stronger, transcendental claim such an explanation is redundant – the geometry of the experience is explained by the necessary conditions on the possibility of experience.

One final point should be made about Strawson's position. There is some question about what it means to claim that a geometry is unfalsifiable for a given domain, but to deny that this amounts to either a modal claim or a claim about the conditions for the possibility of experience. Surely the claim that a geometry is unfalsifiable for a given domain is just a Byzantine way of saying that the propositions are true for that domain. Imagine a creature whose visual experiences answer to a hyperbolic geometry. For such a creature, the propositions of hyperbolic geometry will be unfalsifiable for its phenomenal items. This is because, as was suggested is the case for us, there will be no experience the creature can have that will falsify the propositions of that geometry. But this is just because his experience is in fact (though contingently) answerable to a hyperbolic geometry. If this is right then the 'unfalsifiability' of a geometry for one's experience is only of epistemological significance to the creature trying to determine what the geometry of his experience is. It really adds nothing to the claim that the experience is contingently a given way. As

such, I will now drop the term ‘unfalsifiability’ and just speak of the truth of Euclidean geometry for our phenomenal figures, or the truth of phenomenal propositions.

### **Hopkins and the Indeterminacy of Visual Experience**

I have argued that while Hopkins is correct to assert that there is no reason to accept that phenomenal propositions are necessary, this does not compromise Strawson’s position. This is because the analogous claim, that we have no reason to accept the truth of phenomenal propositions, is just false – our inability to picture non-Euclidean figures counts as a reason for accepting Strawson’s conclusion. So, given that we have some reason for thinking that phenomenal propositions may be true, there are two possible critical responses available. Either you argue that we *are* able to picture non-Euclidean figures, or you offer an alternative explanation of our inability which does not imply the truth of phenomenal propositions. Hopkins adopts the latter strategy and offers one such alternative explanation.

Hopkins thinks that our inability to picture non-Euclidean figures is a consequence of distortions that occur as a result of the limitations of sight. These limitations in turn place restrictions on what we can visually imagine. Hopkins argues that the distortions endemic to vision make phenomenal figures indeterminate as to their geometrical properties. A consequence of this is that Strawson’s Euclidean phenomenal geometry cannot be true, as phenomenal figures have no determinate geometrical characteristics.

Hopkins thinks that the figures vision acquaints us with have no determinate geometry. Hopkins has a direct argument for this claim, which I will discuss shortly; the more immediate concern is to see how the idea that there are distortions involved in vision indirectly supports the claim to indeterminacy. First, though, we need some explanation of what it means to say that the geometry of vision is indeterminate. If it is to be a genuine rival to Strawson’s account and the Euclidean thesis, it must be a claim about the appearances of figures, in the phenomenological sense. What Hopkins appears to mean is that the figures vision acquaints us with can be equally well described by either Euclidean or a non-Euclidean geometry. The idea is that all



geometrical descriptions are underdetermined by visual appearances. Hopkins puts it like this:

The difference made by the assumption that a physical triangle is Euclidean as opposed to non-Euclidean is visually undetectable. It therefore looks just as much non-Euclidean as Euclidean.... So phenomenal geometry is not Euclidean. Rather it is neutral or indeterminate.<sup>47</sup>

This claim of indeterminacy is clearly at odds with the considerations that motivated Strawson's account. There it was taken that we can have visual appearances that are unambiguously Euclidean – for example, parallel straight visual lines that do not converge. Hopkins' position implies that this is a mistake; he thinks that our belief that such figures are unambiguously Euclidean (or indeed unambiguously non-Euclidean, if one wished to argue such) arises from a mistaken confidence in the acuity and precision of vision.

To show this, Hopkins appeals to the fact that there are restrictions involved in sight which produce distortions of the spatial properties of the physical objects that are seen. This distorting effect is underpinned by a simple principle that relates to sight: if a figure is to be seen, all of its elements must be simultaneously visible. Take a physical triangle about the size of the musical instrument of the same name; it can be seen without seeing all of it, but there is a perfectly straightforward sense in which to see the whole triangular figure, all of its elements must be seen contemporaneously.

Now, some spatial properties of the physical objects depend upon their scale: they may depend upon their lines having a certain thickness or a certain length relative to the other elements. But for some figures, in order for them to be visible all at once, their lines will need to be represented as thicker than they actually are, relative to their length. As Hopkins says:

Those characteristics required by considerations of scale may conflict with those needed for visibility.<sup>48</sup>

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<sup>47</sup> Ibid., p. 55

<sup>48</sup> Ibid., p. 57

The result of this principle is that for many physical figures to be visible, there must be some distortion of their spatial properties. This means that there are certain spatial relations that cannot be represented in vision.

So, how does Hopkins use this quite plausible feature of vision to block the natural supposition that we see Euclidean parallel lines? Here is Hopkins' statement of his argument:

Someone may, for example, think that he can picture Euclidean parallel straight lines. For simplicity, and to fix what is meant by a line, suppose he pictures such a pair of lines as could be drawn on a blackboard, a few feet apart and a few yards long, at the maximum ratio of length to thickness. Now it can be pointed out that his picture of these lines does not differ from one of lines which would meet if extended, say for a few miles. The picture does not exclude this possibility, so it does not show the lines as parallel. He may reply that he can regard the lines as extended; he can exclude the possibility that the lines he pictures would meet if extended, by picturing them as long as he likes. This is really the assertion that he can change the scale of his image to represent longer lines. But as the scale is changed, the picture ceases to show the disposition of *lines*.<sup>49</sup>

So, the argument seems to go:

- 1) We take the phenomenal lines to be parallel because they appear not to converge.
- 2) However, the phenomenal lines in question do not differ from phenomenal lines which would meet if they could be extended. In this sense the phenomenal lines are indeterminate.
- 3) We cannot resolve this indeterminacy by observing that the phenomenal lines could represent lines very far off, which in fact do not converge. This is because the length to width ratio of the phenomenal lines is not the same as that of the physical lines.

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<sup>49</sup> Loc Cit.

Premise (2) is just the claim that the phenomenal figures have no determinate geometry, but there is something telling about premise (3). Why would anyone want to resolve the indeterminacy Hopkins proposes by claiming that that phenomenal lines can be taken to represent lines far off? As I have said earlier, to be offering a genuine rival to the Euclidean thesis Hopkins must be claiming that the phenomenal figures themselves have no determinate geometrical description. However, premise (3) suggests that Hopkins thinks of the geometry of visual experience as related to how it represents the world to be. As I will argue shortly, this is essentially the weak point of Hopkins' argument, but we still need to see the whole of Hopkins' account of the evidence that convinced Strawson and Bennett to develop the position they both hold.

Hopkins' explanation of the data that led Strawson to his position is just a special case of this general point. The data concerned our inability to picture two distinct straight lines through two points. In the case of the lines on the blackboard, the indeterminacy arose because one and the same phenomenal picture could serve equally well as an inaccurate representation either of parallel lines or of lines converging very slightly. In the case Strawson considers, Hopkins' explanation is the same. When we have two distinct phenomenal lines through two points, we take one to be straight and the other to be curved. However, Hopkins argues, the phenomenal picture is what we would be aware of in seeing an intersecting curve and straight line close up, and also what we would be aware of in seeing a pair of non-Euclidean straight lines describing the two equally shortest distances between two very distant stars. In order to see such a figure, the lines and points would need to have their scale distorted to be visible. When this occurs, the phenomenal lines, Hopkins argues, no longer represent the situation accurately:

The only way to make anything visible here would be to thicken the lines. But then they would overlap before becoming large enough to be seen. Owing to the distortion required to make the lines visible, the only way to make two lines visible would be to bend one away from the other. Then one line would be and appear curved. Hence the only usable (visible) pictures fail to show two lines, or show one curved.....

So really there is no accurate picture of the situation described. Paths of the required ratio cannot be pictured. Because of their relative

thickness, the areas which can be pictured cannot mirror the disposition of lines; and in this case the particular form of distortion leaves no alternative but pictures easily interpreted as showing Euclidean lines. It is like the transformation of a delicate design painted over with a thick brush.<sup>50</sup>

So what can we make of Hopkins' argument that our inability to picture non-Euclidean figures can be explained in a way that does not imply that phenomenal propositions are Euclidean?

I think that it is not a viable alternative to Strawson's explanation. First, it should be noted that the sense of 'indeterminate' here has shifted. In the previous passages we have looked at from Hopkins, the idea was that phenomenal figures underdetermine a geometrical description. This is an instance of a theory underdetermined by evidence. What is operative in the second paragraph just quoted is the idea that the phenomenal figures can only ever be inaccurate or merely approximate representations of physical figures. The problem is it is just a non-sequitur to conclude from this that the phenomenal pictures *themselves* do not have a determinate geometrical description. An analogous case should illustrate the mistake: a particular shade of red can approximately represent a number of diverse shades of red, but this does not mean that it is indeterminate what that particular shade of red is.

If this is right then there is nothing in what Hopkins has said to justify the suggestion that the distortions inherent in seeing certain physical figures means that the phenomenal figures themselves have no determinate geometry. If there is no reason for thinking that phenomenal figures are indeterminate, then there is no reason to prefer Hopkins' account of the data to Strawson's. However, Hopkins also offers an argument for the indeterminacy of phenomenal figures that is independent of the discussion we have looked at so far. If this argument works, then Strawson's position must be wrong and we just need an explanation of why we are psychologically inclined to interpret the indeterminate phenomenal figures as Euclidean. To this end Hopkins could then place the emphasis on the last two sentences of the previous quote. He in fact gives such a psychological explanation of the proposed tendency to overlook the indeterminacy:

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<sup>50</sup> Ibid. pp. 62-63.

Partly the explanation is simple. Euclid's geometry is familiar and approximately true. We naturally describe in familiar terms, and where measurement is concerned we speak more or less imprecisely. We therefore naturally and correctly describe figures in Euclidean terms....<sup>51</sup>

So what is Hopkins' direct argument that phenomenal figures have no determinate geometry? It turns partly on his way of explicating how we should understand 'phenomenal figures'. He says:

The phenomenal triangle is Euclidean if the look of the physical one is; and this presumably is true if the physical triangle looks Euclidean.<sup>52</sup>

So we have:

- 1) If a physical figure looks Euclidean, the look of the physical figure will be Euclidean.
- 2) If the look of a physical figure is Euclidean, the phenomenal figure will be.

The idea here is that the geometry of the phenomenal figure depends upon the look of a physical thing, which in turn depends upon whether the physical thing looks that way or not. Hopkins thinks that it follows immediately from this that phenomenal geometry is not Euclidean, because no physical figures look Euclidean as opposed to non-Euclidean. Using triangles as an example, he says:

No physical triangle looks Euclidean as opposed to non-Euclidean. The difference made by the assumption that a physical triangle is Euclidean as opposed to non-Euclidean is visually undetectable. It therefore looks just as much non-Euclidean as Euclidean. Local observation and measurement fit equally with Euclidean and non-Euclidean assumptions; so it is not surprising that the looks of things fit both

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<sup>51</sup> Ibid. p. 55.

<sup>52</sup> Ibid. p 54.

equally.... So phenomenal geometry is not Euclidean. Rather it is neutral or indeterminate.<sup>53</sup>

So the argument for the indeterminacy of phenomenal figures can be put like this:

- 3) Take a physical figure – the difference made by the assumption that it is Euclidean as opposed to non-Euclidean is undetectable.
- 4) Therefore, the physical figure looks just as much non-Euclidean as Euclidean.
- 5) Therefore, the physical figure looks no determinate way.

By (5) and the conjunction of (1) and (2), we get the conclusion:

- 6) Phenomenal figures are indeterminate regarding their geometry.

I think this argument is problematic. Premise (3) speaks only about the geometrical properties of the physical figure and whether changes in its geometry would be detectable. It is not obvious why this should be taken to warrant any claim about the geometrical properties of the phenomenal character of the experience. So if (4), when speaking about the ‘looks’ of physical figures, is to be understood as a claim about the phenomenal character of the experience, then it is not clear that it is warranted by (3). Consequently, (5) would not be warranted either.

It could be responded that (4) is warranted because (3) means that whatever the phenomenal character of the experience is, it must be equally accurate as a representation of the geometrical character of the physical figure. This, it may be supposed, rules out both the Euclidean geometry and the non-Euclidean geometry of the physical figure, on the grounds that either one would be more accurate as a representation of the very properties they are instances of. But is there any reason to think that the experience *must* be equally accurate as a representation of the Euclidean case and the non-Euclidean case, as Hopkins supposes? There doesn’t seem to be anything in (3) that justifies this requirement. Accuracy for an experience is about the relationship between the properties of the experience and the properties of the

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<sup>53</sup> Ibid. p 55.

physical object; a relationship which is external to the experience – it is not something that shows up *in* the experience. For this reason the fact that the change in geometry supposed in (3) does not show up in experience does not mean that the experience must be equally accurate for both situations. It is quite possible that one experience is more accurate as a representation of the physical figure, but this difference in accuracy doesn't show up in experience.

The mistake is similar to that noted in the non-sequitur discussed earlier. Hopkins' discussion there slips from talking about physical lines and how they would need to be distorted to be visible (presumably by this he means 'capable of being seen'), to talking about the properties of 'visible' pictures, and then to talking about the properties of phenomenal pictures.

Hopkins also offers a parenthetical justification of his conjunction of premises (1) and (2). He thinks that unless we accept it, it would not be clear what it could mean to ascribe geometrical descriptions to phenomenal figures, or how it could be done.

This line of thought requires the geometry of phenomenal items to be tied, as in Strawson's account, to the geometry that things are seen or imagined to have. Otherwise, it is opaque what geometrical ascriptions to phenomenal items would mean, or how they could non-arbitrarily be made....<sup>54</sup>

I can see no good reason for accepting what Hopkins says on this point. Why should the correct description of the phenomenal character of an experience be tied to what that experience warrants us to conclude about the objects it is about? Moreover, is it really opaque to say of two elements of a visual experience that they are straight lines and that they converge? Consider the well worn example of seeing railway lines – there is one clear sense in which they appear straight and appear to converge, despite what we know about the actual disposition of the physical lines. This is, at some level, a description of the phenomenal character of the experience. There is nothing opaque or arbitrary in this, any more than saying that a white wall can appear red when bathed in red light. Strawson's evidence of our apparent inability to see

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<sup>54</sup> Loc Cit.

certain figures counts as a non-arbitrary way of arriving at a geometrical description of phenomenal figures. Moreover, as we shall see in subsequent chapters, there are other phenomenological considerations that recommend, in turn, a spherical geometry for visual experience and a hyperbolic geometry.

I think I have shown that Hopkins has offered no good reason for thinking that it is a mistake to interpret our inability to see certain kinds of figures as the result of their satisfying a Euclidean description. No good reason has been given to suppose that the inherent distortions involved in seeing a physical figure results in the experience having no determinate geometrical character. Moreover, irrespective of such constraints on seeing, no good independent argument has been given for thinking that visual experiences have no determinate geometrical description. It seems then that Strawson's position, which is basically the Euclidean thesis, is not threatened by any of the arguments Hopkins has fielded.

## **Reichenbach**

I will now turn to a discussion of the work of Hans Reichenbach, who has articulated something like a position on the geometry of visual experience. Like Strawson, Reichenbach takes his lead from some elements of Kant's work, although, like Hopkins, he is more critical of Kant's ideas. In chapters 9, 10 and 11 of his book *The Philosophy of Space and Time*, Reichenbach offers a critique of Kant's claim that there is a single fixed and determinate form that our awareness of spatial objects must take – i.e. that there is just one 'form' of pure (outer) intuition, the Euclidean form. Stated thus, Reichenbach's position sounds similar to Hopkins' position, but they differ in an important respect. There are two ways in which one could deny that there is a single fixed and determinate form of intuition. The first is to deny that intuition has *any* determinate form; the second is to assert that intuition can be 'informed' in multiple ways. Hopkins' claim involves the former way of denying Kant's claim, whereas Reichenbach's position denies it in the latter way.

In adapting Reichenbach's discussion to the purposes of a discussion about visual experience a few preliminary points need to be made. First, Reichenbach is primarily concerned with accounting for the consequence of the Theory of Relativity that the geometry of the physical world is non-Euclidean. It is for this reason that he



engages critically with Kant's idea that the necessity Kant believed needed to be ascribed to Euclidean geometry can be accounted for by appeal to the Euclidean form of outer intuition. Where Kant talks about 'intuition', Reichenbach discusses 'visualisation': 'visualisation' is just the word used to translate the same German word that, in Kant's writings, is commonly translated as 'intuition': '*Anschauung*'. So, I will be considering whether Reichenbach has convincing arguments for his claims about 'visualisation'. The second point to note returns to an observation made in connection with Strawson's discussion. 'Visualization', as it is used by Reichenbach, covers not only visual experiences, but is also used to cover cases of 'visual imagination'. Whatever may be said in connection with the latter, it is strictly the former that is relevant to this investigation. It will be necessary to make explicit how Reichenbach's discussion of visualizing Euclidean and non-Euclidean geometries can be adapted to formulate a position regarding the geometry of visual experience.

I shall begin by giving a general characterization of Reichenbach's position and how it relates to Kant's, before looking in detail at the support for his position. Reichenbach takes up Kant's idea that 'intuition' or, to use Reichenbach's term, 'visualisation' somehow has a normative function. It somehow prescribes which geometrical propositions we can and cannot assent to. In addition to this, Reichenbach claims, visualization has an image producing function. This seems to be the same thing that Strawson meant when he discussed figures delivered by 'visually imagining' them, although it is not exactly clear how Reichenbach intends this to be understood. At times Reichenbach writes as if to suggest that this involves some sort of awareness of an indistinct particular imagined figure – some sort of hallucinatory or purely intentional object. Nothing that follows stands or falls on this, however.

As was mentioned above in connection with Strawson's work, Kant appealed to this proposed 'normative function' to explain the necessity of (Euclidean) geometrical propositions, as he believed them to be. Given that subsequent developments in geometry revealed the necessity to be no more than the necessity of logical laws, the natural way to conceive of the role of the normative function is something weaker: something more like a constraint on what can be seen or imagined. This is the point from which Reichenbach starts. Given that the development of non-Euclidean geometries means that it is possible to conceive of non-Euclidean figures, the question Reichenbach raises is whether this normative function is fixed, as Kant

believed, so that we cannot see or imagine non-Euclidean figures. This is distinct from the question raised by Hopkins, and by those who argue directly for a non-Euclidean geometry for visual experience, in that Reichenbach focuses upon the issue of whether the limitations on what can be seen or imagined are *fixed*, whether as Euclidean or non-Euclidean.

As a first, rough characterization, Reichenbach's position is that what determines the geometrical character of what we can see or imagine is something that can be changed. He contends that the geometrical character of figures depends upon an antecedent and independent definition of congruence. Two figures are geometrically congruent if they have all the same spatial properties, except location and orientation. If we change this definition from Euclidean congruence, Reichenbach contends, we can come to see and imagine non-Euclidean relations.

Having said this, Reichenbach's position is complex and not tremendously clear; in particular, at times he seems to endorse evidence that has been taken to support the claim that our visual experiences are hyperbolic in character. At one point he discusses the familiar issue of how parallel rails appear to converge:

The sense impression of two rails is not that of parallelism, whereas we do recognize the rails as parallel in pure visualization.... The fact that two rails do not appear parallel, although they are parallel lines in an objective sense, proves nothing against the perceptual space.<sup>55</sup>

This much is fine, but what is interesting is the introduction of 'sense impressions' and what he has to say about them. In contrast to his views on visualization, as regards 'sense impressions' Reichenbach seems to think it is possible to give a geometrical description of their relations, and just refers to the work of Hillebrand and Blumenfeld on this point:

...we must ask whether there are any parallels at all in perceptual space.  
The answer to this question has been given long ago by psychologists.

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<sup>55</sup> Hans Reichenbach. *The Philosophy Of Space and Time* (New York: Dover Publications, 1958), p. 85

There are indeed parallels in perceptual space, but their form in an objective physical space is that of two slightly curved diverging lines.<sup>56</sup>

Hillebrand and Blumenfeld's work, to be discussed in depth in chapter 5, demonstrated a coordination of physical curves with judgments of parallelism, under certain experimental conditions, and has been taken as evidence for a hyperbolic geometry for visual experience.<sup>57</sup>

Reichenbach is, nonetheless, dismissive of the relevance of such evidence. He says, "All this is of course completely irrelevant for the problem of visualization."<sup>58</sup> The reason Reichenbach does not consider this a problem is because the coordination of physical curves with visual parallel lines leaves room for the claim that the visual parallel lines do not diverge – in short that the correspondence between visual elements and physical elements does not imply that their geometrical properties (such as being parallel) need correspond.

This is a point that has been articulated by Strawson, Hopkins and other commentators, and its consequences will receive a more comprehensive treatment in chapters 4 and 5. It certainly seems at first pass, though, that Reichenbach's point is sound. However, if it turns out that the work of Hillebrand and Blumenfeld does provide a good reason for thinking that the geometry of 'sense impressions' is determinately non-Euclidean, then this will be in conflict with Reichenbach's views on visualization. This is because if the problem of visualisation concerns whether we are somehow compelled to see, imagine or possibly even conceive of spatial relations only in the way specified by Euclidean geometry, then surely the fact (if it so turns out to be) that the relations between the elements of our sense impressions are those articulated by a hyperbolic non-Euclidean geometry will bear directly upon this problem. If our visual experiences are hyperbolic in character, and we can come to appreciate this, then there should be no reason to suppose that there are any *a priori* limitations on what geometrical relations we can see, imagine, or conceive. Moreover, it would indicate that there *are* limitations on what geometrical relations we can see – just not *a priori* limitations.

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<sup>56</sup> Loc Cit.

<sup>57</sup> See chapter 5, pp. 153-169.

<sup>58</sup> Loc Cit.

It is not entirely clear what Reichenbach intended the relation to be between 'sense impressions' and 'visualisation'. The fact that Reichenbach did not observe this potential point of conflict suggests that he was not entirely clear himself. His primary interest is with visualisation, which, as I have said, covers more issues than just the visual case: more than are relevant to this inquiry. Hopkins has interpreted Reichenbach's position on visualisation as serving also as an account of visual experience. The above unclarity of terms like 'sense impression' and 'visualisation' should just make us wary, as a point of exegetical charity, of ascribing such a position to him. However, adapting Reichenbach's position on 'visualisation' to serve as a position on the geometry of visual experience is philosophically interesting, as it represents a *prima facie* plausible account. As such, in what follows I will outline his discussion of 'visualisation' in some detail and show how such a position regarding visual experience can be constructed from it.

Reichenbach's position, as a position about visualization, is an adaptation of a conventionalist position regarding the geometry of physical space. Hopkins has provided a concise articulation of how Reichenbach's position on visualization is related to this. Regarding physical space, which geometrical description we arrive at when we investigate the relations between physical geometrical elements depends upon the results of measurements. Conventionalism begins with the observation that the relations such measurements establish can be interpreted in various ways:

In particular, the relations can yield one set of measurements and one geometry if the interval realized by a standard rod is taken as everywhere the same, other measurements and another geometry if the interval is taken to vary with the position and orientation of the rod. The differences in measurements will result in the relations' determining different sets of intervals congruent or equal. And with one set of intervals congruent the geometry will be Euclidean, with another, non-Euclidean.<sup>59</sup>

A bare statement of the indeterminacy that is articulated here is:

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<sup>59</sup> James Hopkins. "Visual Geometry." *The Philosophical Review* 82 (1973): 59

(a) It is possible to describe a world as Euclidean or non-Euclidean, depending upon which of its intervals are taken as congruent or equal.

This is very close to the conception of indeterminacy that it initially looked as if Hopkins himself was claiming for vision: that the figures underdetermine a geometrical description. However, where (a) differs from Hopkins' position is that it makes the further point that measurement together with a definition of congruence does determine a geometry. This gives it the following consequence:

(b) It is possible to describe a non-Euclidean world of rigid bodies as a Euclidean world of bodies changing dimensions with position and orientation, but in such a way that their coincidence relations stay constant; and vice versa.

However, (a) (and consequently (b)) is ambiguous between two claims. The first is an epistemological claim that we cannot be certain that a geometrical description is true of a set of spatial objects, on the basis of measurement and a definition of congruence. This is because we will have equally good reasons for accepting a different geometrical description, given a different definition of congruence. This means that although it is possible to describe a world as Euclidean or non-Euclidean, owing to formal properties of the sets of sentences, one description is nonetheless the true description. The second possible reading of (a) is that no geometrical description of the set of spatial objects is true. The support for this view comes from the observation that no description is favored above any other – they will each be equally confirmed by experimental evidence.

Reichenbach appears to endorse the stronger reading of (a) – that no geometrical description of the set of objects is true. He says:

Space as such is neither Euclidean or non-Euclidean... it becomes Euclidean if a certain definition of congruence is assumed for it... if a

different definition of congruence is introduced... space becomes non-Euclidean.<sup>60</sup>

Reichenbach's position regarding visualization is arrived at by arguing that principle (a) applies in respect of the figures furnished by visualization. A modified version of (a) for visualization would be:

(a\*) It is possible to describe the images provided by visualisation as Euclidean or non-Euclidean, depending upon which intervals are seen as congruent.

As with (a), (a\*) can be given a weaker and a stronger interpretation. The weaker interpretation leaves room for the possibility that as a matter of fact one description is true of the visualized images. The stronger interpretation is that no description is true. However, Reichenbach's position appears to involve a further claim – that when different intervals are seen as congruent, the relations that are seen change. It is not just that there are multiple possible, equally good descriptions of the visualized images. Reichenbach holds that which intervals are taken as congruent is a matter of definition, and this definition is 'projected' on or 'read into' the images produced by visualization. The character of the images is thus determinate in a sense, but is determined by the definition of congruence. Although it is determinate, it can nonetheless be changed by altering our definition of congruence. This kind of phenomena, where external factors affect the character of the experience, is not entirely uncommon in perceptual experience. There are many striking illusions featuring such changes, such as those found in cases of ambiguous figures, such as the duck/rabbit picture and the Necker cube. The existence of such phenomena at least lends some initial credibility to what might otherwise seem quite counterintuitive at first pass.

So, what substantive reasons are there for accepting this kind of view? I have indicated that there are reasons for being suspicious of this view, especially as regards visual experience. Hopkins is particularly sceptical about the suggestion; however,

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<sup>60</sup> Hans Reichenbach. *The Philosophy Of Space and Time* (New York: Dover Publications, 1958), pp. 56-57

this is partly due to his commitment to his own account of the geometry of visual experience:

... the change in visual congruence on which this account of non-Euclidean visualization pivots does not occur. No one in fact experiences a change of sight relevant to seeing or visualizing in non-Euclidean terms. It seems in consequence that those who claim non-Euclidean visualization do not actually accomplish it....

This conclusion might of course be refuted by the testimony of visualizers; but so far as I know, no testimony of any weight has been given.<sup>61</sup>

Hopkins here is restricting his discussion to the ability to see, whereas, as Reichenbach uses it, 'visualization' has a broader meaning. The reasons Reichenbach offers for thinking that (a\*) is true concern the nature of visualization. So, to do justice to Reichenbach, we need to look at those arguments.

The strong reading of thesis (a\*) depends upon the idea that the images produced by visualization are somehow neutral in respect of their geometrical description. If this claim to neutrality can be motivated then so too will (a\*). This is precisely Reichenbach's strategy in *The Philosophy of Space and Time*. Reichenbach attempts to show that the nature of visualization is such that the images produced by visualization are neutral, which for him means that it is not something internal to visualization that determines the character of the images produced. In particular, his strategy is to motivate his claim that geometrical propositions are not read off visualized images, but are instead read into them. If this can be demonstrated, then this will motivate the claim about the neutrality of the images.

This is precisely how Reichenbach introduces his idea. He begins by analyzing what it might mean to say that we cannot visualize non-Euclidean geometry. For Reichenbach, this means asking what the source of the compulsion found in the normative function of visualization is. One possibility is that the image producing function is the source of this compulsion – that the images produced by the image producing function are such that only Euclidean propositions can be read off them. If

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<sup>61</sup> James Hopkins. "Visual Geometry." *The Philosophical Review* 82 (1973): 61-62.

such images are constitutive of visual phenomenology, then this will be the general kind of position that would need to be adopted by anyone who wished to claim that it *is* possible to read the geometry of visual experiences off the visual phenomenology.

Reichenbach argues that the image producing function is not normative. He maintains that the results of the normative function are ‘read into’ the image rather than being ‘read off’ them. He offers two reasons for denying that the normative function of visualization has its basis in the image producing function. The first reason has to do with Kant’s idea that intuition is an essential part of geometrical proof. Reichenbach states this idea like this:

It seems that the source [of the normative function] is the image-producing function, because the image producing function is a necessary condition of the effectiveness of the normative function.<sup>62</sup>

If you thought that the images were essential to geometric proof, then the image producing function would be necessary for there to be the kind of compulsion that the normative function provides. However, the modern view of geometry has no need for such a claim, as non-Euclidean geometries have been shown to be logically consistent and Euclidean geometry has no privileged metaphysical status over any other. As such, the appeal to images in proof is redundant, which renders unnecessary an appeal to the image producing function to account for why visualization compels us to see or imagine in any particular way.

The second reason Reichenbach offers against the idea that visualisation is normative because of the character of the images it provides is that the images themselves are subject to the normative function. Just as we correct drawings which we may use to demonstrate the geometrical properties of figures, so we correct our own imaginative reflections. As an illustration of this, Reichenbach describes how a person attempted to figure out how many diagonals can be drawn from a given corner of a pentagon without the aid of drawings:

I immediately got the rash answer “five.” He was evidently in the phase of speaking offhand. Then followed a “no, one moment.” Now

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<sup>62</sup> Hans Reichenbach. *The Philosophy Of Space and Time* (New York: Dover Publications, 1958), p. 39



the image-producing function was employed, and after some reflection came the answer “three.” Here the image producing function had evidently furnished the wrong result. A “no” followed and after some moments the correct answer “two.” The normative function had intervened and corrected the images. It is not the case that we simply wait for images that will dictate the results to us.<sup>63</sup>

Whilst it is far from clear what one should make of this example, it is obvious that some manner of correction is being made. The suggestion is that the normative function corrects the visually imagined figures. However, this is where the breadth of cases that ‘visualisation’ covers begins to mask relevant differences. In the case of ‘visual imaginings’ Reichenbach’s point may seem plausible, although one can’t avoid the suspicion that the talk of imagined figures is inappropriately being thought of as involving a mode of phenomenal awareness that is analogous in some way to having a visual phenomenology. Here the reductivist claim that this kind of mental state can be unproblematically reduced to some belief state or judgment seems more compelling than it does in the case of the phenomenal character of visual experience.

In the case of visual experience, though, as opposed to the kind of ‘visual imagining’ case Reichenbach discusses, Reichenbach’s claim seems much less plausible. Consider the case of correcting a drawing; if we are actually trying to draw a pentagon and trying to draw the diagonals, we will correct it. Let us say that we draw one side longer than the others: we will correct it. If we draw too few sides, we will correct it, until we have a drawing that *is* a Euclidean pentagon. When we have a drawing that is a Euclidean pentagon, we can still ask whether it is a Euclidean pentagon, but this is just to say that we can correct our judgment – not correct the actual image. This point also holds for the elements of visual experience: it is not obvious that we can correct them in the same way we that can correct an image we are drawing. This is because as you draw the pentagon, you are drawing something that is *supposed to be* or *is intended to be* a pentagon. This is why the drawing can be corrected: this is why there is a norm. When you have a pentagon of which you are aware, be it a visual image or a physical drawing, there is no such intention. As such, there can be no correcting it relative to some intention.

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<sup>63</sup> Ibid., p. 40

It seems, therefore, that this second consideration of Reichenbach's does not offer a good reason for thinking that the character of visual images is not the source of some constraints on the correct geometrical description of those images. It is not obvious that in the case of visual images we do not read the geometrical description of those figures off them directly. The case of visual imaginings is not directly relevant. Moreover, Reichenbach's first reason for denying generally that visualisation is not normative on account of the image-producing function only defeats a reason for such a claim – it does not support it in any positive way.

The failure of Reichenbach to positively establish this point in respect of visual experience is the weak point in his argument for the claim that the geometrical character of figures is not fixed, when this is construed as a claim about visual experience. This is because we are not forced to search for the source of the compulsion of the normative function in tacit assumptions about the elements of the figure, as Reichenbach proceeds to do.

Reichenbach argues that we make tacit assumptions about the images delivered by visualisation. These tacit assumptions are what restrict which geometrical description of the images we are prepared to accept. One illustrative example of the way in which such tacit assumptions constrain what can correctly be said of a figure concerns whether all the elements must be coplanar:

We considered the theorem that a straight line intersecting one side of a triangle must also intersect another side of the triangle. Is this true? By no means; I can imagine a straight line descending in space and not situated in the same plane as the triangle; in this case it intersects one side only. This answer is certainly trivial – but often we do not notice how much we restrict a problem by tacit assumptions.<sup>64</sup>

When you make explicit these tacit assumptions in the initial formulation of the question, however, the number of compatible descriptions decreases. When all assumptions are made explicit, Reichenbach argues, there will only be one possible true description of the figure. This description will just be a part of a logically

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<sup>64</sup> Hans Reichenbach. *The Philosophy Of Space and Time* (New York: Dover Publications, 1958), p 41

consistent geometry. The source of the compulsion in visualisation, Reichenbach concludes is just an analogue of the compulsion we find in logical laws.

One of the tacit assumptions we make of images is that the elements comprising them are Euclidean elements; Euclidean lines, curves, etc. This, Reichenbach argues, is just because we are most familiar with this geometry. This allows Reichenbach to characterise the impossibility of seeing or visually imagining certain non-Euclidean figures, which impressed Strawson and Bennett, as a consequence of the assumption that the elements comprising the images produced by visualization are Euclidean.

Reichenbach argues that the question of whether we can see or imagine non-Euclidean figures should properly be formulated as a question of whether we can come to regard elements of a visual figure as such that we can unproblematically describe them by means of a non-Euclidean geometry. He claims that we can. As I have said, this general picture requires one to accept that the images produced by visualization are neutral in such a way that that it is plausible to say that which description of them we give is then fixed by *assumptions* about the elements. This is the significance of Reichenbach's attempt to motivate the idea that we do not simply read a description off images, but instead read descriptions into them. If I am right that there are no good reasons for accepting this claim, interpreted as a claim about visual experience, then we need not follow Reichenbach on this point. If this is right then it seems that Reichenbach's attempt to motivate claim (a\*) is not successful. This is in agreement with Hopkins' assessment that no adjustments of congruence can ever take place.

## **Conclusion**

My principle aim in this chapter has been to motivate the Euclidean Thesis. This was done by appealing to aspects of Strawson's discussion of the geometry of Euclidean phenomenal geometry. I have defended some of his claims about the geometry of visual experience against certain criticisms. I rejected Hopkins' criticisms of Strawson's claim that the propositions of Euclidean geometry are 'unfalsifiable' for visual experiences. Strawson's claim about the unfalsifiability of such propositions was shown to only have epistemic significance for the subject attempting to falsify

them. Really, Euclidean geometry is ‘unfalsifiable’ for visual experience just because it is true for visual experience.

My secondary aim in this chapter was to show that the two alternate ways of claiming that visual experience is indeterminate with respect to a geometry, as proposed by Hopkins and Reichenbach, can be blocked. In the arguments that are intended to establish their proposals, both authors beg the question against the possibility that the visual figures themselves have determinate geometrical properties.

The purpose of showing that these two proposals of indeterminacy can be blocked was necessary to make room for the possible truth of the Euclidean thesis. In subsequent chapters I will look at more fully developed arguments for geometries other than Euclid’s for visual experience, beginning in the next two chapters with the most serious contender from within the philosophical literature: spherical geometry.

## **Chapter 3 – Spherical Geometry – A Priori Arguments**

### **Introduction**

In this chapter and the next I will discuss the arguments to be found in the literature that support a spherical geometry for visual experience. This is, by a long way, the claim that has received the most attention from philosophers in recent times. Arguably, the first to articulate this claim was Thomas Reid in his *Inquiry Into the Human Mind on the Principles of Common Sense*. Independent, but related arguments for this claim were also offered by Richard Angell and Robert French in the 1970's and 1980's. These latter arguments generally appeal to salient features of the phenomenal character of visual experiences to justify a spherical geometry. More recently, however, Gideon Yaffe and James Van Cleve have offered reconstructions of Reid's original argument. In light of criticisms that have been made of Reid, these reconstructions all aim to extract from his discussion an argument which does not rely on empirical considerations, including those appealed to by French and Angell. Gordon Belot has also articulated an argument similar to Yaffe's, but without concerning himself with an exegesis of Reid.

In this chapter I will focus exclusively on the modern articulations of Reid's argument. The structure of this chapter is the following: I begin by giving an exposition of Reid's original discussion of the topic and highlighting areas of his discussion that people have traditionally criticised. This will put us in a position to adequately consider the arguments put forward by Yaffe, Van Cleve and Belot. In doing so I will principally assess these arguments on their own merits and only tangentially as interpretations of Reid's discussion. It will emerge from a critical discussion of these arguments that the attempt to purge any reliance on empirical considerations is not entirely successful. This naturally motivates an analysis, undertaken in chapter 4, of the arguments offered by Angell and French in order to assess how compelling such considerations are. In chapter 4 I will argue that, with one exception, the considerations they offer are not compelling as they stand.

## **Reid's Presentation of the Argument**

In his chapter of the *Inquiry* entitled “The Geometry of Visibles” Reid develops a geometrical description of ‘visibles’ that is remarkably close to a spherical geometry. Norman Daniels, among others, has argued that this section of the *Inquiry* entitles Reid to credit for the earliest discovery of a non-Euclidean geometry. Contrarily, Yaffe, Van Cleve and Edward Slowik have argued that there are good reasons for being more reserved about categorising Reid’s geometry of visibles as a genuine non-Euclidean geometry.<sup>65</sup> Those who wish to deny Reid such credit argue that he was just applying the Euclidean theorems that deal with Euclidean spheres to perspectival shape. This controversy is only partly relevant to the present discussion: what is not relevant is whether Reid genuinely discovered a non-Euclidean geometry. What is relevant here is that there are distinct claims that can be made about visual experience. The first is that visual experience is correctly described by a spherical geometry, with its spherical metric. The second is that visual experience is correctly described by the parts of Euclidean geometry that can be applied to the ‘perspectival’ shapes of external objects. What will be relevant to this discussion is whether it makes any difference to the issue of Direct Realism to assert one rather than the other.

To avoid prejudging any such relevant difference to the wider philosophical concern about Direct Realism I will initially present Reid’s discussion as neutrally as possible between the competing interpretations, relying heavily on Reid’s original text. The relevance of asserting either that the geometry of visibles is a genuine non-Euclidean geometry or that it is a part of Euclidean geometry applied to perspectival shape will be dealt with towards the end of this chapter, when I discuss some aspects of Gideon Yaffe’s reconstruction of Reid’s argument. My purpose in the first section of this chapter is to highlight the kinds of considerations that Reid brings to bear to demonstrate something that is crucial in his own discussion as well as mine, i.e., the discrepancy between the properties of visibles and the properties of the external objects that cause them:

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<sup>65</sup> For the most comprehensive discussion of this see E. Slowik. “Conventionalism in Reid’s ‘geometry of visibles’” *Studies in History and Philosophy of Science* 34 (2003): 467-489.

When the geometrician draws a diagram with the most perfect accuracy; when he keeps his eye fixed upon it, while he goes through a long process of reasoning, and demonstrates the relations of the several parts of his figure; he does not consider, that the visible figure presented to his eye, is only the representative of a tangible figure, upon which all his attention is fixed; he does not consider that these two figures have really different properties; and that what he demonstrates to be true of the one, is not true of the other.<sup>66</sup>

Before we look at Reid's exposition of the geometry of visibles, one further, though related, point of clarification is needed. The purpose of this inquiry is to investigate the correct geometrical description of visual experience. The primary concern here is whether the considerations and arguments Reid offers represent a compelling case in favour of a spherical geometry. However, Reid distinguishes between 'visible figures', which he holds are external to the mind, and sensations, which are themselves defined as internal mental entities. Such a distinction may imply that the description of 'visibles' is something to be distinguished from the description of the experience. As such, I will initially present Reid's arguments for the geometry without any consideration of the complications that are generated by Reid's view on what 'visibles' are. Only when I discuss the modern reconstructions of Reid's argument will I turn my attention to the theoretical possibilities and difficulties generated by his views on what we are to understand 'visibles' to be. During the course of my initial exposition, therefore, where Reid refers to 'visible' figures, e.g. 'visible right line' I shall take him to mean some external object of which we can be aware by means of vision.

Reid's presentation of his geometry of visibles in the chapter 'The Geometry of Visibles' of his *Inquiry* falls into three broad sections. First, he helps himself to the standard definitions of basic geometrical terms, such as point, line, etc., from Euclidean geometry. Second, he offers eight 'evident principles', which culminate in the thesis that the surface of a sphere centred on the eye is a representation of visible

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<sup>66</sup> Thomas Reid. *An inquiry into the human mind, on the principles of common sense: a critical edition*, edited by D. R. Brookes (Edinburgh: Edinburgh University Press, 1997), pp. 102-103

space. Finally, he offers a group of twelve propositions about visible figures that follow from these eight 'evident principles'. These twelve propositions are, according to Reid, "not less true nor less evident than the propositions of Euclid, with regard to tangible figures."<sup>67</sup>

The twelve propositions are intended as a sample of the theorems of this geometry, which is supposed to illustrate the divergence of Euclidean geometry from the correct description of the figures vision acquaints us with. However, it does not appear that Reid intended them to do any significant work in the argument for this geometry. He says:

This small specimen of the geometry of visibles, is intended to lead the reader to a clear and distinct conception of the figure and extension which is presented to the mind by vision; and to demonstrate the truth of what we have affirmed above, namely, That those figures and that extension which are the immediate objects of sight, are not the figures and the extension about which common geometry is employed....<sup>68</sup>

So we must look at the eight 'evident principles' to find the central moves in Reid's argument.

The eight principles are intended to be an elucidation of the appearances of external figures presented to the eye, which is identified with the single central point of a sphere of an arbitrary radius  $r$ . The appearances of such external figures are compared with the appearances of figures on the surface of the sphere. The eight principles can be divided into four groups of propositions, each dealing with distinct geometrical elements. Principles 1, 2 and 3 deal with the appearances of straight lines; principle 4 deals with the appearances of angles, or intersecting straight lines; principles 5 & 6 deal with the appearances of specific shapes; and principles 7 & 8 draw general conclusions about the relationship between the surface of a sphere and visible space.

Principle 1 asserts basically that every great circle on the surface of the sphere centred at a point identified with the eye will present the same appearance to the eye that it would if it were in fact straight. It also asserts that any external line lying on the

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<sup>67</sup> Ibid. pp. 103-105

<sup>68</sup> Ibid. p. 105



plane that intersects the eye and the all the points of the great circle, will itself present the same appearance as the great circle does:

1. Supposing the eye placed in the centre of a sphere, every great circle of the sphere will have the same appearance to the eye as if it was a straight line. For the curvature of the circle being turned directly toward the eye is not perceived by it. And for the same reason, any line which is drawn in the plane of a great circle of the sphere, whether in reality it be straight or curve, will appear straight to the eye.<sup>69</sup>

Principle 2 asserts basically that every point on an external line will have the same position relative to the eye that some point on the great circle will:

2. Every visible right line will appear to coincide with some great circle of the sphere; and the circumference of that great circle, even when it is produced until it returns into itself, will appear to be a continuation of the same visible line, all the parts of it being visibly *in directum*. For the eye, perceiving only the position of objects with regard to itself, and not their distance, will see those points in the same visible place which have the same position with regard to the eye, how different soever their distances from it may be. Now, since a plane passing through the eye and a given visible right line, will be the plane of some great circle of the sphere, every point of the visible right line will have the same position as some point of the circle; therefore they will both have the same visible place, and coincide to the eye: and the whole circumference of the great circle continued even until it returns to itself, will appear to be a continuation of the same visible right line.<sup>70</sup>

As should be evident, both principles 1 and 2 rely on the absence of depth perception. The absence of depth perception is a consequence of the restriction to a

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<sup>69</sup> Ibid. p. 103

<sup>70</sup> Ibid. pp. 103-104

static monocular case. This is a point that will be returned to when I consider Van Cleve's discussion, but for now this just requires highlighting.

Principle 3 is intended as a consequence of 1 & 2; it asserts a more general relationship between external lines and the great circles of the sphere: that the latter can always 'represent' the former:

3. That every visible right line, when it is continued *in directum*, as far as it may be continued, will be represented by a great circle of a sphere, in whose centre the eye is placed.<sup>71</sup>

Principle 4 shifts the discussion from how lines appear to the eye, to how angles appear to the eye. It asserts that when the above 'representing' relation holds between a pair of external lines and a pair of great arcs, then where those lines intersect the angles formed by the former pair will be identical with those formed by the latter. As a matter of fact the angles on the surface of a sphere will be spherical angles, so consequently visible angles must be equal to spherical angles:

4. ...the visible angle comprehended under two visible right lines, is equal to the spherical angle comprehended under the two great circles which are the representatives of these visible lines. For since the visible lines appear to coincide with the great circles, the visible angle comprehended under the former, must be equal to the visible angle comprehended under the latter. But the visible angle comprehended under the two great circles, when seen from the centre, is of the same magnitude with the spherical angle which they really comprehend, as mathematicians know; therefore the visible angle made by any two visible lines, is equal to the spherical angle made by the two great circles of the sphere which are their representatives.<sup>72</sup>

Once Reid has dealt with lines and angles, it is just a small step to deal with triangles:

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<sup>71</sup> Ibid p. 104

<sup>72</sup> Loc. Cit.

5. Hence it is evident, that every right-lined triangle, will coincide in all its parts with some spherical triangle. The sides of the one will appear equal to the sides of the other, and the angles of the one to the angles of the other, each to each; and therefore the whole of the one triangle will appear equal to the whole of the other. In a word, to the eye they will be one and the same, and have the same mathematical properties. The properties therefore of visible right-lined triangles, are not the same with the properties of plain triangles, but are the same with those of spherical triangles.<sup>73</sup>

Principle 6 deals briefly with circles:

6. Every lesser circle of the sphere, will appear a circle to the eye, placed, as we have supposed all along, in the centre of the sphere. And, on the other hand, every visible circle will appear to coincide with some lesser circle of the sphere.<sup>74</sup>

Having discussed the relation between figures formed by external objects and figures on the surface of the sphere centred at the eye, Reid is now in a position to assert that the surface of the sphere ‘represents’ visible space, which he does in principle 7:

7. Moreover, the whole surface of the sphere will represent the whole of visible space: for, since every visible point coincides with some point of the surface of the sphere, and has the same visible place, it follows, that all the parts of the spherical surface taken together, will represent all possible visible places, that is the whole of visible space.<sup>75</sup>

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<sup>73</sup> Loc. Cit.

<sup>74</sup> Loc. Cit.

<sup>75</sup> Loc. Cit.

The final principle is a corollary of principle 7, and deals in a general way with the relations between any external object and the figures on the sphere centred at the eye:

8. ...every visible figure will be represented by that part of the surface of the sphere, on which it might be projected, the eye being in the centre. And every such visible figure will bear the same *ratio* to the whole of visible space, as the part of the spherical surface which represents it, bears to the whole of visible space.<sup>76</sup>

It is widely accepted now that what Reid is trying to establish in this section of the *Inquiry* is an equivalency between a geometry of what is visible and spherical geometry. The discussion of the relation between visible lines, angles, triangles and circles and their counterparts on the surface of the sphere makes this clear. The idea is that if spherical figures represent visible figures, then there is a sense in which one can, as Yaffe puts it, 'speak for the other'. If you want to know the properties of the former you can look at the properties of the latter. However, it is not entirely clear how the considerations Reid presents in this section are supposed to establish them. There is, as I have already mentioned, the worry surrounding how Reid's phrase 'visible figure' is to be understood, but there are others. For example, just what is the role that the eye plays in this argument and how is the eye to be conceived? The idea that two external objects can have the same position with regard to the eye appears to be central to establishing the identity of visible elements, such as lines and angles, with their spherical counterparts, but it is not clear exactly what that role is. Many of these points have been discussed over the course of the contemporary attempts to explicate exactly what Reid's argument for the proposed equivalence is. Owing to these unclarities in Reid's exposition I will focus my critical discussion upon the contemporary reconstructions, to which I now turn.

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<sup>76</sup> Ibid. pp 104-105.

## **Contemporary Interpretations**

The most thorough attempt to reconstruct Reid's demonstration of the equivalence of the geometry of visibles and spherical geometry is given by Gideon Yaffe in his paper 'Reconsidering Reid's Geometry of Visibles'. In this paper Yaffe offers an interpretation of Reid's argument that is avowedly *a priori*. It deliberately eschews empirical support, but instead turns just on what he calls 'natural and appealing mathematical analyses of ordinary concepts'. These concepts he calls 'visible' concepts, which are intended to be those that describe the phenomenal character of our visual experiences. His idea is that once we understand these 'visible' concepts clearly we can construct a geometry using these concepts, which is the geometry of visibles; then we can investigate which mathematical geometry this is equivalent to in some sense. Yaffe argues that this equivalence of the two geometries consists in them being what he calls 'proof-theoretically' equivalent. By this Yaffe means roughly that corresponding sentences between geometries have the same truth value and the sentences that can be used to prove one sentence in one geometry have their corresponding sentences in the other.

Yaffe's general discussion of Reid's argument is particularly interesting for a further reason: it suggests a way of resolving the apparent conflict between Reid's Direct Realism and the idea that the geometry of visual experience is a spherical geometry. This is related to his claim that Reid did not develop a genuine non-Euclidean geometry; moreover, Yaffe's suggestion is similar in general strategy to those responses to the traditional form of the argument from illusion, based on spatial properties, which I discussed in chapter 1. However, a discussion of this point will need to wait until we have an overall view of the argument.

The argument for the equivalence, as it is presented by Yaffe, falls into three stages: the first consists of an articulation of what Reid means by the 'visible' concepts used in his presentation. This section forms the basis upon which we can construct the geometry of visibles. The second stage involves a demonstration on the basis of these newly clarified concepts that the geometry of visibles cannot be Euclidean. The third stage is the demonstration that the geometry of visibles is proof-theoretically equivalent to a spherical geometry. Because the core of the argument consists in establishing the equivalency between the two geometries, I will leave out

discussion of the second stage of Yaffe's presentation for a more appropriate place later on. As such, I will look first at how Yaffe proposes to construct the geometry of visibles from the 'visible' concepts and then move directly on to a discussion of his demonstration of the equivalence.

## **The Eye As A Single Point In Space**

The contemporary versions of this kind of argument all follow Reid in his identification of the eye with a single point in space – the point in relation to which visible figures of physical objects are defined. The first thing to do is to consider possible reasons for accepting this identification.

The first possibility is the reason ascribed to Reid by Norman Daniels for accepting this identification. The proposed justification for the identification is bound up with how Daniels thinks Reid's argument proceeds: he thinks that Reid attempts to establish the geometry of visibles by considering the projections of physical objects outward from the eye onto a sphere enclosing them. The choice of a sphere as a surface of projection, it is proposed, is based on the empirically false claim that the eye is a sphere and that the retina is part of this spherical surface. The idea, quite bizarrely, is that the eye can therefore be identified with the central point of this sphere.<sup>77</sup>

This proposed justification is unacceptable on at least two counts. Firstly, as Daniels and Yaffe have both observed, it relies on empirically false claims. Secondly, it is just not clear why, if the justification of the choice of surface of projection has to do with facts about the eye, the eye should be identified with a point *inside* a sphere. Whether this proposal is correctly ascribable to Reid or not, it is clearly not acceptable.

In light of the worry that the above justification turns on empirically false claims about the eye, Yaffe offers a justification for the identification of the eye with a point that does not rely on any empirically false claims about the eye. His proposal turns on a consideration of the function of the lens in providing focussed images. He proposes that we restrict our discussion to objects that are in focus.

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<sup>77</sup> See Norman Daniels. *Thomas Reid's 'Inquiry': the Geometry of Visibles and the Case for Realism* (Stanford, CA: Stanford University Press, 1989), p. 10

When an object, or some portion of it, is in focus there is a one-one correspondence between points on the object, or the relevant portion of it, and points on the retina. The function of the lens is to achieve this one-one correspondence of points by focussing multiple light rays emanating from a given point on the object onto a single point on the retina.

Yaffe observes that this function of the lens of the eye is fulfilled equally by a point sized hole in a pinhole camera. The function of the point in such a camera is to ensure that only a single light ray emanating directly from each point on the object will pass into the camera at the angle required to hit the photosensitive screen. It is on the basis of this functional identity that Yaffe suggests we are entitled to identify the eye with a single point in space:

To show that it is appropriate to identify the eye with a point, given that the discussion only concerns objects that are in focus, it is instructive to compare the eye with a pinhole camera, where the 'lens' is a hole the size of a single point, and the 'retina' is a flat screen onto which the light passing through the pinhole is projected. In the eye, by contrast, the retina is an irregularly curved surface, and the lens is a complex structure that fills an opening larger than a point. When we consider only objects that are in focus, however, the lens of the eye has in common with the pinhole the following property: both collect the rays emanating from a point and focus them onto a single point on the retina/screen. Therefore, given that the only relevant objects are those that are in focus, the lens of the eye is functionally equivalent to a single point in space....

[Reid] is allowed to associate the eye with a single point in space because the lens of the eye collects light in just the way a single point in space collects it, if we limit our discussion to objects in focus.<sup>78</sup>

This is a more sophisticated justification for the identification than that considered above, but there is a reason for remaining unconvinced by it. This is just

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<sup>78</sup> Gideon Yaffe. "Reconsidering Reid's Geometry of Visibles" *The Philosophical Quarterly* 52(209) (2002): 605-606

that it is not obvious that the genuine similarity of function identified by Yaffe is really relevant. What we are interested in establishing is something beyond the functional properties of the lens – we are interested in the geometry of the visual experience. To reason from the fact that two things are functionally identical to the claim that they have some further property in common involves the assumption that the same function is realised in a way that is similar in relevant respects. This assumption need not be true. Consider the case of what it is like to see by means of sonar. Seeing by means of sonar is functionally equivalent to seeing by means of eyes, in that they both provide a richly structured, three dimensional awareness of the shape and location of objects at a distance. However, it would be unwarranted to infer *from this alone* that what it is like to see by means of sonar is the same as what it is like to see by means of eyes. In light of this, it is legitimate to remain sceptical about whether such an identification of the eye with a point in space is justified.

One final proposal remains to be considered, offered by Van Cleve. Van Cleve does not appear to be too concerned with offering a justification for identifying the eye with a single point. Rather, he builds this assumption into his characterisation of his conclusion. He asserts that the geometry of visibles is a geometry of a single point of view.<sup>79</sup> On this view, whether the assumption is warranted will depend upon whether the geometry of visibles is convincingly argued for. I will now turn to a consideration of this question.

### **Yaffe's 'Visible' Concepts**

I noted above that Reid's principles 4 & 5 discuss the relationship between external figures, such as external triangles and external circles, and figures lying on the surface of spheres. He calls these external figures 'visible triangles', etc., but how are we to understand these phrases? The discussion of these specific 'visible figures' relies upon the general 'representing' relation that is asserted in Reid's principle 3, that lines on the surface of spheres can 'represent' visible lines. This principle was arrived at by considering the relation between physical lines and lines on the surface of spheres, where Reid noted that they would present the same appearance to the eye because the eye 'will see those points in the same visible place which have the same

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<sup>79</sup> James Van Cleve. "Thomas Reid's Geometry of Visibles" *The Philosophical Review* 111 (2002): 396.



position with regard to the eye'. So, it looks as if this notion of 'visible place', or 'visible position' as Yaffe calls it, is used to build up the notion of 'visible figure'. This naturally makes one ask what is meant by 'visible place', and as I have noted it is not obvious what is meant by 'visible figure'.

It seems that the 'visible' concepts we need to analyze before we can construct the 'visible geometry' are of two different kinds: general spatial concepts such as 'visible position' and 'visible figure'; and specific shape concepts such as 'visible line' and 'visible triangle'. These are the 'ordinary concepts' that Yaffe is referring to when he talks about giving a 'natural and appealing mathematical analysis' that is supposed to furnish our geometry of visibles. Yaffe is not explicit on this point, but it seems safe to assume that this means they are connected to pre-theoretical notions like 'the shape an object appears to have'. This has the following consequence for the definitions of the general spatial concepts of the geometry we are constructing for 'visibles': any formal definition of 'visible position' and 'visible figure' must conform to the use of their informal definitions. In light of this consideration, Yaffe offers the following two desiderata for our formal definitions of 'visible position' and 'visible figure':<sup>80</sup>

(D1) Each position within the visible figure of an object can be occupied by one and only one point.

(D2) The visible figure of an object must be 'path-connected'. This means that from any point in the visible figure to any other, also in the visible figure, there must be a path that passes only through points that are also within the visible figure of the object.

Yaffe follows Reid's suggestion of deriving the concept of 'visible figure' from the concept of 'visible position'. The idea is that a visible figure is determined by a full specification of the visible positions of its parts. So first we need to look at what we are to understand by the 'visible position' of something. Roughly speaking, the visible position of something is given by its relation to the eye. Reid's definition of visible position is given in this sentence from the *Inquiry*:

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<sup>80</sup> Gideon Yaffe. "Reconsidering Reid's Geometry of Visibles" *The Philosophical Quarterly* 52(209) (2002): 606-609.

Objects that lie in the same right line drawn from the centre of the eye,  
have the same position... but objects which lie on different right lines  
drawn from the eye's centre, have a different position....<sup>81</sup>

Yaffe points out that this definition is ambiguous between the following two definitions:

(VP1) The visible position of a point in space is the *line* passing through both the eye and that point.

(VP2) The visible position of a point in space is *any point on the line* passing through both the eye and that point.

Yaffe holds that definition (VP1) is unsatisfactory because it fails to meet desideratum (D1). If a visible position is a line, then it is constituted by an infinite number of points, contrary to (D1). On the basis of this, Yaffe concludes that we should think of visible position as given by definition (VP2).

I do not think that this can be correct. To express visible position in terms of some sort of identity with either a point or a line suggests that a visible position is some sort of thing, which runs counter to the way in which the phrase 'visible position' is usually used: i.e. as a predicate. At any rate, according to (VP2) if we take two distinct points lying on a line radiating out from the eye they will both be identical to the visible position of the point referred to on the left hand side of the identity statement. By the transitivity of identity, these two external points will be identical. But *ex hypothesi* they are not identical: they are distinct. So, (VP2) cannot be an adequate definition of visible position.

On the other hand, (VP1) does run counter to desideratum D1. So what are we to do? Well, these difficulties can be sidestepped if we do not consider ourselves to be specifying the visible position of some point, but defining the concept 'visible point.' This is just what Gordon Belot does in his 'Remarks of the Geometry of Visibles'. He defines 'visible point' and then derives 'visible figure' from this. However, he defines 'visible point' as the line radiating out from the origin: i.e. in accordance with

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<sup>81</sup> Thomas Reid. *An inquiry into the human mind, on the principles of common sense: a critical edition*, edited by D. R. Brookes (Edinburgh: Edinburgh University Press, 1997), p. 96

something closer to (VP1). Belot claims that his formulation is neater, which it is in one sense – if we were to define ‘visible point’ as *any* point on the line radiating from the eye, the above problem emerging from the non-identity of distinct points on that line would recur: we can still take two distinct points on the line, which by the transitivity of identity will be two non-identical points that are identical. No such problem arises for the definition in terms of lines. Belot’s decision to begin by defining ‘visible point’ has a further advantage – it avoids the problem that (VP1) ran into: that it runs counter to the intuition that one and only one point can occupy each position. Belot’s construal avoids any mention of visible position. However, as I will argue later, Belot’s way of defining ‘visible’ concepts does have its own problems.

Although there may be considerations that suggest Belot’s approach to articulating this ‘visible’ concept is preferable to Yaffe’s, this difference in choice of definition only affects the way the subsequent ‘visible’ concepts are defined, but not in such a way that ultimately affects the demonstration of the equivalency of the geometry of visibles with spherical geometry. This is because the lines radiating out from the eye will bear the same relations to the spherical figures that are relied upon to establish the equivalency.<sup>82</sup> As such, although there are features of Belot’s presentation that cut through some of the difficulties Yaffe considers, I will continue with Yaffe’s presentation, as it is more thorough and systematic. With the exception of the proceeding discussion of ‘visible figure’ the only difference in Belot’s definition of the other ‘visible’ concepts is that where Yaffe defines them in terms of sets of points on lines radiating out from the eye, Belot defines them just in terms of sets of lines radiating out from the eye.

So, moving on to visible figure, Reid defines it the following way:

... as the real figure of a body consists in the situation of its parts with regard to one another, so its visible figure consists in the position of its several parts with regard to the eye....<sup>83</sup>

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<sup>82</sup> Belot observes this in Gordon Belot. "Remarks On The Geometry Of Visibles" *The Philosophical Quarterly* 53(213) (2003): p. 583

<sup>83</sup> Thomas Reid. *An inquiry into the human mind, on the principles of common sense: a critical edition*, edited by D. R. Brookes (Edinburgh: Edinburgh University Press, 1997), p. 96

Again, Yaffe points out that this definition is ambiguous between the following two definitions, each of which can be derived from the corresponding articulations of visible position articulated in (VP1) and (VP2):

(VF1) A visible figure is a set of lines.

(VF2) A visible figure is any one of an infinite number of different sets of points. Which visible figure is in question will depend upon which of the infinite number of points on each of the relevant lines is selected.

Yaffe finds (VF1) unsatisfactory because he found its corresponding definition of visible position (VP1) unsatisfactory. However, (VF2) is also unsatisfactory because it permits visible figures that violate desideratum D2, that visible figures must be path connected. (VF2) allows that the set of points constituting a visible figure can be at wildly disparate distances from the eye. This means that they would not be path connected. On the basis of these considerations, Yaffe offers the following definition of visible figure:

(VF) A set  $VF$  of points in physical space is the visible figure of an object  $O$  iff

- (1) for each point on the surface of  $O$ , there is a member of  $VF$  with the same visible position;
- (2) no two members of  $VF$  share a visible position;
- (3) the set of points is path connected.

This defines visible figure as a set of points in physical space outside the eye and, as Yaffe points out, this is emphatically not any sort of mental object.<sup>84</sup> Nonetheless, it is intended as an analysis of the ordinary notions connected with phrases like 'the shape an object appears to have'. Here 'appears' should be taken in its phenomenological sense. So, it seems that Yaffe's account suggests that what is seen directly is a set of points in physical space.<sup>85</sup> This is just an expression of a form of Direct Realism. As we saw in chapter 1, Indirect Realism involves denying at least

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<sup>84</sup> Gideon Yaffe. "Reconsidering Reid's Geometry of Visibles" *The Philosophical Quarterly* 52(209) (2002): 609.

<sup>85</sup> For Belot, what is directly seen is a set of lines.

this. So, Indirect Realism is ruled out by Yaffe's identification of visible figure with a set of physical points.

This general commitment to Direct Realism, although consistent with Reid, is quite the exception amongst contemporary philosophers who have maintained that visual experience has a non-Euclidean geometry. The reason for this is that contemporary philosophers have recognised the tension between this account of what a visible figure is and the idea that the geometry that describes the possible visible figures is equivalent to a spherical geometry. As I have mentioned, parts of Yaffe's discussion provide a caveat that explains away this apparent conflict. The move that allows him to avoid the difficulty just discussed is bound up with the claim that the geometry of visibles is not a genuine non-Euclidean geometry. I shall examine how successful this is later on in this chapter; for now I will leave it to one side.

Up to this point, what the argument has done is to pick out a set of points in physical space and stipulate that they are the figures that our geometry of visibles is supposed to describe, they are the 'visibles'. As Yaffe says:

A visible figure is just a set of points in three-dimensional space defined by further reference to the position of an object and the position of an eye.<sup>86</sup>

We now need to define some of the specific shape concepts, such as line and triangle. First 'visible line' is defined, along with 'visible length', from which we can define 'visible triangle'. The definition of visible line is:

**Visible line segment** – A visible line segment is a visible figure, all the points of which lie on a plane with the eye.

This definition can be arrived at by reflecting on what would be the possible visible figure if we substituted 'line' for 'object *O*' in the above definition. No points of the set *VF* could lie on a plane other than the plane the other members of *VF* lie on, as this

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<sup>86</sup> Gideon Yaffe. "Reconsidering Reid's Geometry of Visibles" *The Philosophical Quarterly* 52(209) (2002): 609

would contradict the condition that the physical point must have the same visible position as the point in the set  $VF$ .

The definition of visible length is:

**Visible length** – Visible length is measured by the angle produced by the lines through the eye and the two extreme points on a visible line.

The definition of ‘visible triangle’ is built up from taking three intersecting visible lines:

**Visible triangle** – A visible triangle is a visible figure which is equal to the union of three visible lines. The intersections of these lines contain only one point.

The final definition given is that of ‘visible angle’:

**Visible angle** – A visible angle is a dihedral angle, i.e. the angle between two planes that meet at a line.

The idea here is that the planes through the eye not only specify which sets of points can count as visible lines, but, in addition, the angles between these planes specify the ‘visible angle’ between the visible lines.

### **Yaffe’s presentation of the proof-theoretical equivalence:**

From the ‘visible’ concepts discussed above we can construct sentences, which, taken together, constitute the geometry of visibles. It is proposed that there is a relation of some kind of equivalence between this geometry and the geometry describing the surface of the sphere – spherical geometry. So what is this relation?

Well, Yaffe thinks of the equivalence that Reid is indicating in terms of the existence of a kind of mapping relation of sentences from one geometry onto sentences of the other. So what does this mean? First of all, such a mapping relation is a general relation that could hold between various geometries – it is not specific to visible and spherical geometries. To see this let us consider ‘visible’ geometry and

Euclidean geometry. Once we have the terms of our ‘visible’ geometry, we can construct sentences from them that are truth apt. Moreover, these sentences can be decomposed into their constituent concepts. Now, each of the ‘visible’ concepts have counterparts in Euclidean geometry: so, ‘visible line’ has as its counterpart ‘planar line’; ‘visible angle’ has its counterpart ‘planar angle’. As a consequence, each sentence from the first geometry has a counterpart in the other: pairs of sentences are counterparts if and only if they can be decomposed into concepts that are themselves counterparts.<sup>87</sup>

In virtue of this correspondence of concepts and sentences, we can map the sentences of each of these geometries onto the sentences of the others when the concepts in one sentence are the counterparts of the concepts in the sentences of the other. The idea is that where there is a one-one correspondence of the basic terms, there will be a one-one correspondence of sentences containing corresponding terms. The example Yaffe gives is of the correspondence of Pythagoras’ theorem in Euclidean geometry to the ‘visible’ Pythagoras’ theorem:

[This] maps the Pythagorean theorem, ‘In any right triangle, the square of the length of the hypotenuse is equal to the sum of the squares of the lengths of the other two sides’, to the visible Pythagorean theorem, ‘In any visible right triangle, the square of the visible length of the visible hypotenuse is equal to the sum of the square of the visible lengths of the other two visible sides.’<sup>88</sup>

In addition to having counterparts in Euclidean geometry, the ‘visible’ concepts and the sentences constructed from them also have counterparts in a spherical geometry – ‘visible line’ corresponds to ‘spherical line’. A ‘spherical line’ is in fact a section of the great circle of the sphere.

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<sup>87</sup> As a point of clarification, this idea that one concept has a counterpart in another geometry is not related to the point that the ‘visible’ concepts were defined using concepts from another geometry. So, for instance, ‘visible line’ was defined in terms of a set of path-connected points in Euclidean space. This does not mean that ‘visible line’ as it occurs in this geometry of visibles has as its counterpart in Euclidean geometry the concept ‘set of path-connected points in Euclidean space’. Its counterpart is just ‘Euclidean line’.

<sup>88</sup> Ibid., p. 611

Yaffe expresses this idea of a mapping relation in terms of there being some function that takes as input a sentence from one geometry and gives as output the sentence that would be arrived at by substituting all the concepts with their counterparts from the target geometry. However, these corresponding sentences from the different geometries need not have the same truth value, so all we have thus far is the notion of some kind of formal equivalency. The conception of equivalence between ‘visible’ and spherical geometry must be stronger than this if it is to exclude Euclidean geometry as equivalent, as both Reid and Yaffe assert it does.

Yaffe’s notion of proof-theoretical equivalence is stronger than this general kind of mapping equivalence in that it requires that such a mapping function preserve two things: (1) the truth values of the corresponding sentences and (2) the relation of corresponding sentences to other corresponding sentences that are used in the proof of the former. This latter condition means that if a set of sentences in a geometry is necessary to the proof of a sentence  $p$  from that geometry, then the corresponding sentences from the other geometry are also necessary to the proof of the correspondent of  $p$ .

So, what Yaffe is doing here is defining a relation between sentences in different geometries: sentence  $x$  is ‘proof-theoretically equivalent’ to sentence  $y$  if and only if:

- (1) the truth value of  $x$  is the same as the truth value of  $y$ , and
- (2) if  $x$  is proved or proved false by a set of sentences  $P$ , then the set of counterparts of the sentences in  $P$  is a proof of  $y$ , or of its falsity.

Therefore, the claim that a given geometry is proof-theoretically equivalent to another is that this relation holds between the sentences that are mapped to each other in the above mapping relation.

So, what is the argument for the proof-theoretical equivalency of these geometries that Yaffe finds in Reid? The argument consists of the following three stages:<sup>89</sup>

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<sup>89</sup> Ibid., p. 612.



- a) A demonstration that any sentence about a ‘spherical line’ is proof-theoretically equivalent to its ‘visible’ counterpart.
- b) A demonstration that any sentence about a ‘spherical angle’, formed by two spherical lines, is proof-theoretically equivalent to its visible counterpart.
- c) A demonstration that any sentence about a ‘spherical triangle’ is proof-theoretically equivalent to its visible counterpart.

Part (a) draws upon the material from Reid’s discussion in principles 1-3, concerning visible lines; part (b) encompasses Reid’s discussion in principle 4, concerning visible angles; and part (c) from his discussion of principle 5, which deals with visible triangles. Part (c) is very straightforward – as Yaffe says:

...the parts of a triangle are just three line segments and three angles; the parts of a spherical triangle are three spherical line segments and three spherical angles; and the parts of a visible triangle are three visible line segments and three visible angles. Therefore anything that is said about a (planar, spherical or visible) triangle can be paraphrased into a sentence that makes no mention of triangles, but mentions only (planar, spherical or visible) sides of certain (planar, spherical or visible) lengths, and (planar, spherical or visible) angles of certain (planar, spherical or visible) magnitudes.<sup>90</sup>

If we establish a proof-theoretical equivalency of geometries in respect of sentences concerning lines and sentences concerning angles, then we will *de facto* have established a proof theoretical equivalency of sentences concerning triangles. So we need to focus upon parts (a) and (b), each of which I will discuss in turn.

## **Part (a)**

According to Yaffe, the argument in part (a) aims to reach its conclusion by showing that there is a proof-theoretical equivalence between sentences about the

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<sup>90</sup> Ibid., p. 619

slopes of spherical lines and the slopes of 'visible' lines. The argument appears to have the following structure:<sup>91</sup>

- (1) Two points have the same visible position,  
if and only if,  
the two points have the same spherical position with respect to a  
sphere centered at the eye.
- (2) Every plane passing through the eye and a visible straight line will be the  
plane of some great circle of a sphere.
- (3) So, every point on such a visible line will have the same visible position as  
some point on the great circle.
- (4) Therefore, any continuation of the spherical line will be a continuation of the  
visible line,  
and,  
any continuation of the visible line will be a continuation of the  
spherical line.
- (5) The thing that tells us what direction to continue any line in is its slope.
- (6) Therefore, whatever the spherical slope of the spherical line is at any point, the  
visible line must have the same visible slope,  
and,  
whatever the visible slope of the visible line is at any point, the  
spherical line has the same spherical slope.
- (7) So, any theorem about spherical lines and spherical slopes is proof-  
theoretically equivalent to the analogous theorem about visible lines and  
visible slopes.

First, just some comment on how these premises are related to Reid's claims:  
Yaffe's premise (1) comes from the following sentence from Reid's principle 2:

For the eye, perceiving only the position of objects with regard to itself,  
and not their distance, will see those points in the same visible place

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<sup>91</sup> Ibid., pp. 612-614.

which have the same position with regard to the eye, how different soever their distances from it may be.<sup>92</sup>

Yaffe suggests that what Reid means by 'position with regard to the eye' here cannot just be 'visible position' on pain of triviality, so 'position with regard to the eye' should be understood as 'spherical position', which is given by the angles from the x, y and z axes of a radius drawn to a point from the point identified with the eye. Premises (2), (3) and (4) are taken straight from the subsequent sentence in the principle (2):

Now, since a plane passing through the eye and a given visible right line, will be the plane of some great circle of the sphere, every point of the visible right line will have the same position as some point of the circle; therefore they will both have the same visible place, and coincide to the eye: and the whole circumference of the great circle continued even until it returns to itself, will appear to be a continuation of the same visible right line.<sup>93</sup>

The use of premise (5) to arrive at premise (6) is suggested by Reid's claim that the great circles of spheres will appear to have the same direction:

The circumference of that great circle, even when it is produced until it returns into itself, will appear to be a continuation of the same visible line, all the parts of it being visibly *in directum*.<sup>94</sup>

So, we have an argument with a clear structure: how does it fare? I think that there are reasons for thinking this argument is suspect. Premises (1)-(3) are fine: Yaffe offers an independent demonstration of premise (1)<sup>95</sup>; premise (2) is just a consequence of (1), together with the definition of visible figure; and premise (3) is

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<sup>92</sup> Thomas Reid. *An inquiry into the human mind, on the principles of common sense: a critical edition*, edited by D. R. Brookes (Edinburgh: Edinburgh University Press, 1997), pp. 103-104.

<sup>93</sup> Loc. Cit.

<sup>94</sup> Loc. Cit.

<sup>95</sup> See Gideon Yaffe. "Reconsidering Reid's Geometry of Visibles" *The Philosophical Quarterly* 52(209) (2002): p. 614

just a consequence of (2). Premise (5) is also unproblematic. The problems start with premise (4) and the step to premise (6), given (4) and (5).

Regarding premise (4), we need to give a more precise sense to the claim that any continuation of the spherical line ‘will be’ a continuation of the visible line. The most likely sense is that in which any continuation of the visible line will have a corresponding part of a continuation of the spherical line, and *vice versa*. In this sense (4) is true.

However, (6) does not follow from (5) and (4), given this sense. This is because the direction we need to go in to continue the visible line is not the same as the direction we need to go in to continue the spherical line. This is because the visible line is actually a physical line, and its direction is given by its planar properties, just as was the case with visible angles in my discussion of visible angles. For the sake of providing a concrete case to consider, I will assume that the visible line being discussed is a physically straight line that is orthogonal to the line of sight. We can see that the directions are different by taking a spherical line that is a section of a great circle that is a greater, but arbitrarily chosen distance away and considering the relations between it and the visible line.

Now, if the two lines have the same direction, and so the same slope, then a continuation of the visible line should not intersect anywhere with the great circle. However, this is manifestly what will happen if we continue the visible line – at some point, depending on the radius of the great circle from the eye, they will intersect at two places. So they cannot have the same direction, and so must have a different slope. It would seem to follow, then, that it is not the case that any theorem about spherical lines and spherical slopes is proof-theoretically equivalent to the analogous theorem about visible lines and visible slopes.

Yaffe would most likely respond to this objection by claiming that it places the emphasis on the wrong part of his argument. He can claim that the crucial thing is not that the direction of continuation is in fact the same, what is relevant is that when they are both continued there will be a one-to-one mapping of all the points of one line onto those of the other, and that the two lines will be visually indistinguishable. For this reason they would be able to ‘speak for’ each other, which is the sense in which

he claims that great circles are ‘representatives’ of visible lines.<sup>96</sup> In this case, whether the two lines intersect or not is of no consequence.

The problem with this response is that there are many figures that can be ‘representatives’ of visible lines in this sense. One such figure is that made by two lines that intersect and form an angle along the principle line of sight. It seems that at first pass we have no reason to take the fact that a given line can be a ‘representative’ of the visible line to be indicative of which geometry is the correct description of visible figures. This problem is one that Yaffe recognises in connection with the proposed proof theoretical equivalency of visible and spherical angles, and has been articulated further by Van Cleve. Accordingly I will return to this criticism later: all that is needed for the present is the observation that the notion of what it is for one figure to be a ‘representative’ of another appears to be a term of art, but it is not clear how it is to be understood.

**Part (b)**

The second part of the argument aims to show a proof-theoretical equivalence between sentences about visible angles and sentences about spherical angles. This step of the argument throws up some interesting considerations. The argument is extracted from Reid’s principle 4:

4. ...the visible angle comprehended under two visible right lines, is equal to the spherical angle comprehended under the two great circles which are the representatives of these visible lines. For since the visible lines appear to coincide with the great circles, the visible angle comprehended under the former, must be equal to the visible angle comprehended under the latter. But the visible angle comprehended under the two great circles, when seen from the centre, is of the same magnitude with the spherical angle which they really comprehend, as mathematicians know; therefore the visible angle made by any two

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<sup>96</sup> Ibid., p. 615.

visible lines, is equal to the spherical angle made by the two great circles of the sphere which are their representatives.<sup>97</sup>

What Yaffe takes Reid to be arguing in this section is the following: take a visible figure that is in fact a spherical figure, i.e. formed by the great arcs of circles centred on the eye. The visible angles of such a figure will be the same as its real angles. This is because ‘visible angle’ was defined as a dihedral angle, and the real angle formed by intersecting great circles of spheres is the angle between their tangents, which is the same as the dihedral angle. So, if we take any other visible figure whose parts lie on the same planes as the parts of this spherical figure then the dihedral angle will be the same – consequently so will the visible angle. So, if we want to know the visible angle of any figure, we just need to look at the real angle of a spherical figure whose elements are coplanar with the elements of the visible figure. In this way there is a proof-theoretical equivalence between sentences about visible angles and sentences about spherical angles.

James Van Cleve offers a very similar interpretation of this section of the argument and shows how it can be fitted into a condensed and formalised version of Reid’s overall argument. The portion of the argument concerning angles runs:

(P1) The visible angle made by any two visible straight lines = the visible angle made by the two great circles representing these lines.

(P2) The visible angle made by two great circles = the real angle made by these great circles.

(C) The visible angle made by any two visible straight lines = the real angle made by the two great circles representing them.

Premises (P1) and (P2) are, in turn, supported by a set of auxiliary assumptions. The assumptions supporting (P1) are as follows:

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<sup>97</sup> Loc. Cit.

(A1) Any two visible straight lines appear to coincide with two great circles.

(A2) If the angle making lines 1 and 2 appear to coincide respectively with the angle making lines 3 and 4 then the visible angle made by 1 and 2 will be the same as that made by 3 and 4.

Van Cleve takes (A1) and (A2) to be unobjectionable and, taken together, they imply (P1).

(P2) is supported by the following set of auxiliary assumptions:

(A3) The visible angle made by two great circles = the visible angle made by their tangents.

(A4) The visible angle made by the tangents of two great circles = the plane angle made by those tangents.

(A5) The plane angle made by the lines tangent to them at their point of intersection = the real angle made by two great circles.

(P2) The visible angle made by two great circles = the real angle made by these great circles.

Assumption (A3) is a consequence of the previous assumption (A2), assumption (A5) is just a standard mathematical convention, and (A4) captures the same idea as Yaffe's definition of 'visible angle' – that visible angles are dihedral angles.

(P1) and (P2) can be used to produce the following valid argument for the geometry of visibles:

(1) Every visible triangle is indistinguishable from some spherical triangle, and therefore has its visible angles equal to the visible angles of the spherical triangle.

(2) The visible angles in a spherical triangle equal its real angles.

(3) The real angles in a spherical triangle add up to more than  $180^\circ$ .

(4) Therefore, the visible angles in a visible triangle add up to more than  $180^\circ$ .

Sentence (3) is just a truth about spherical geometries. The inference in sentence (1) is supported by (P1). Sentence (2) is just a reformulation of (P2).

## **Problems**

The arguments that have been discussed here aim to establish an equivalence between the geometry constructed from the ‘visible’ concepts and the geometry of the figures on the surface of a sphere. They do so by observing that if we take a spherical figure whose parts lie on the same plane as the parts of the visible figure, then the spherical figure can speak for the visible figure. Looking at the properties of spherical figures will tell us about the visible figures. This is because the real angles of spherical figures are just the same as the visible angles of such spherical figures. However, there is a problem that arises which *prima facie* suggests that this result may not be very significant.

The problem can be articulated in the following way: ‘visible angle’ was defined as the dihedral angle of the planes passing through both the eye and the lines of the figure. So, if another figure  $f$  is to tell us about the visible angles of the visible figure then the real angles of  $f$  must be the same as the visible angles of  $f$ . One way of articulating why looking at spherical figures will tell us about other visible figures that are visually indistinguishable from them is that the following conditional is true:

(1) If figure  $f$  is a spherical figure, then its visible angles = its real angles.

However, whilst this is true, spherical figures are not the only figures whose visible angles are the same as their real angles. As such, for any given visible figure if we want to know about its angles we can look at certain figures that are not spherical figures to find out about them. What can be made of this? Well, as Yaffe says:



What this means is that although sentences about spherical angles are proof-theoretically equivalent to sentences about visible angles, they are not the only sentences that are.<sup>98</sup>

He then goes on to say that:

What this implies is that the geometry of spherical angles is not the *only* geometry of visible angles.<sup>99</sup>

Van Cleve has observed that there are two reasons for not thinking that this casts doubt on the significance of spherical geometry as a geometry of visibles. The first reason is that all the angular properties that we would ascribe to visibles on the basis of those non-spherical figures will be compatible with spherical geometry – they will always be equal to spherical angles. The second reason becomes apparent when we reflect on the fact that this worry arose when we considered individual visible figures in isolation. There may be non-spherical figures that can represent individual visible figures, but could there be a non-spherical surface encompassing the eye where the real angles of the figures on it will always be the same as their visible angles? The idea here is to consider the complete description of a total surface encompassing the eye – that complete description is what must be equivalent to the complete description of all possible visible figures. Van Cleve conjectures that the surface of a sphere is unique in this regard. He expresses this in the following way:

If S is a surface such that any figure seen from e can be represented by a figure on S, then S is a sphere centred on e.<sup>100</sup>

If this conjecture is true, then it is irrelevant that there are other individual figures that can tell us about the visible properties of figures, because the total description of the surface such figures lie on will not be the same as the total description of all possible visible figures. Gordon Belot has offered an argument for

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<sup>98</sup> Gideon Yaffe. "Reconsidering Reid's Geometry of Visibles" *The Philosophical Quarterly* 52(209). (2002): 618

<sup>99</sup> Loc. Cit.

<sup>100</sup> James Van Cleve. "Thomas Reid's Geometry of Visibles" *The Philosophical Review* 111 (2002): p. 409

something like this conjecture. However, this requires articulating some of the ways in which Belot differs from Yaffe in understanding the kind of equivalence between spherical geometry and the geometry of visibles.

## **Belot's Argument for Van Cleve's Conjecture**

Earlier I commented on one feature of Belot's construction of the argument that is different to Yaffe's: the definitions of the general visible concepts, i.e. 'visible point' and 'visible figure'. Yaffe defined them in terms of points on lines radiating from the eye and sets of path connected points on lines radiating from the eye. Belot defined them in terms of lines radiating from the eye and sets of such lines. This difference, Belot urges, is of no material significance to the proof.

Where Belot does substantially disagree with Yaffe's attempt to establish the equivalency of the geometry of visibles and spherical geometry is in the conception of what that equivalency consists in. Yaffe holds the equivalence to consist in there being a function that maps sentences of one geometry to another which preserves truth. So, where one sentence is true in one geometry, the sentence it maps onto in the other geometry will also be true; this holds *mutatis mutandis* for false sentences. Yaffe also insisted on the further condition that a proof of one sentence in the first geometry be transformable into a proof of the corresponding sentence in the other geometry. Belot observes that this is a superfluous requirement, as it is guaranteed by the first two.

There is a problem with taking the proposed equivalency of the geometries to consist just in the existence of such a mapping relation and the preservation of truth values by such a mapping. This is quite simply that it does not guarantee that the two geometries are the same.<sup>101</sup> Instead, Belot conceives of the equivalency in terms of an isomorphism. This isomorphism is given by a projective mapping which relates the elements of one geometry to the elements of the other, such that there is a bijective correspondence between them. These elements are those picked out by the definitions of such concepts as 'visible point' and 'spherical point'. This bijective correspondence between the set of 'visible' points and the set of points on the surface of the sphere naturally exists: each line radiating from the eye corresponds to the point

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<sup>101</sup> See Gordon Belot. "Remarks On The Geometry Of Visibles" *The Philosophical Quarterly* 53(213) (2003): 584 for a fuller discussion of this point.

at which it intersects the sphere centred on the eye. So, visible points are related to spherical points because the latter are projections of the former.

So, we get the equivalence of the two geometries because there is a bijective correspondence of the elements the geometries describe. The bijective correspondence is given by the 'projective map'. The idea seems to be, then, that the two geometries are equivalent because the points of visibles can be projected onto points on spheres, such that there is a one-one correspondence. At this point, as with Yaffe's and Van Cleve's discussion, we realise that the surfaces of spheres are not unique in this regard: there are non-spherical surfaces that visibles can be projected on, such that there is a one-one correspondence of visible points and points on the sphere. Van Cleve's conjecture is intended as response to this objection.

To get us in a position to offer an argument for Van Cleve's conjecture, Belot introduces the following condition on projectible surfaces: that the projections of figures onto their surfaces be distortion free. The idea here is that for the geometry of a surface of projection to be equivalent to the geometry of visibles it is not sufficient that there be a correspondence of points. The distances and angles must be preserved by the projection, and lines must map onto lines on the surface of projection. What the counterexamples to the original suggestion show is that for any visible figure there is a non-spherical surface that carries a distortion free projection of it. If we consider Van Cleve's conjecture in terms of possible surfaces of projection for visibles, what it asserts is that the sphere is the only surface on which *all* possible visible figures could be projected without distortion. Belot argues that non-spherical surfaces could not satisfy this more stringent condition, that all visible figures could be projected onto it whilst preserving all angles and distances and such that visible lines project onto lines on the surface of projection.

The reason Belot thinks that non-spherical surfaces cannot satisfy this more stringent condition is that the geometry of visibles is invariant under rotations of Euclidean space. What this means is that if we rotated all visible figures around the eye, the description of them would not change. So, any surface whose geometry is the same as the geometry of visibles must also be invariant under such rotations. The idea here seems to be that if we took a triangle on the surface of a spiky sphere and moved it around on that surface, then its description would change depending on whether it lies on the portions of the surface with constant curvature or the portions with variable

curvature. For this reason the geometry of the surface is not invariant under rotations around the eye.

### **Van Cleve's Conjecture And The Overall Strength of 'Reid's' Argument**

We now have all the pieces in place of this modern articulation of Reid's argument. The question is, How persuasive an argument is it? Well, there is one initial reason why someone may be suspicious of it. Consider Van Cleve's conjecture, it holds that the only surface on which all possible visible figures can be represented is the sphere. This by itself might warrant the response, 'So what?' This tells us something interesting, to be sure, about the surfaces of spheres, but why does it tell us anything interesting about the nature of visual experience? Consider Belot's idea of establishing Van Cleve's conjecture in terms of projective features of surfaces. Why should we take this as at all significant for the visual experiences of subjects who do not have spherical or hemi-spherical, or even roughly hemispherical light sensitive visual apparatus onto which images are projected?

The thought here is that the kind of argument being presented by Yaffe, Van Cleve and Belot may be too strong. One of its strengths is that it does not turn on any empirical facts – no contingent features of the visual system, nor any introspective evidence. Moreover, the conclusion accords at least roughly with what we might be led to conclude is contingently true of human visual experience, if we *were* to consider the actual shape of the human retina. However, it is at least possible that there can be creatures with visual systems whose retinas are flat, or indeed any shape that is radically different from the sphere. The problem is that such a consideration is defeated by the *a priori* argument for a spherical geometry: the shape of the retina has no significant bearing on the geometry of visibles. No matter how the subject's visual system is composed, the geometry of visibles will always be the same.

In view of this point the argument may seem too strong. This does not mean advocating a straightforward inference from the shape of the retina to the geometry of the experience. The point is that it at least seems plausible that what is true of human visual experience is at least dependent upon what is contingently true of the visual

system. By ‘dependency’ here all that is meant is that relevant changes in the visual system will produce relevant changes in the correct description of the experience. On Yaffe’s, Van Cleve’s and Belot’s way of approaching the issue there is no such dependency because when you change a feature of the visual system that *prima facie* seems highly relevant, there will still not be any change in the correct description of the visibles. This seems unusually strong.

Now, it is not sufficient to just highlight a possible reason for being suspicious of this approach, what we need is a demonstration that some step in the argument is erroneous, or at least seriously questionable. This is especially demanding in this case, as Yaffe, Van Cleve and Belot will point out that Van Cleve’s conjecture tells us something interesting about the geometry of visibles because of the *role* it plays in establishing that the surface of the sphere is the only surface for which all figures will have visible angles that equal their real angles. This is why the description of the total surface of the sphere can ‘speak for’ the total description of ‘visible space’. This in turn is why the geometry of the surface of a sphere is the geometry of the visible. However, there are two different questions here that must be treated separately. The first is whether Yaffe, Van Cleve and Belot have managed to show that what they have presented as ‘the geometry of visibles’ is equivalent to spherical geometry. I think the answer to this question is that they have. The second question concerns whether such a demonstration represents a convincing case for the claim that a spherical geometry is equivalent to the correct description of visual experience. I think that it is much less certain that this is the case.

What this second question effectively asks is whether there are reasons for thinking that what Yaffe, Van Cleve and Belot say by way of defining the ‘visible’ concepts is questionable. If it is doubtful that the definitions of the ‘visible’ concepts are appropriate to visual experience, then, even though spherical geometry is equivalent to the geometry constructed from these ‘visible’ concepts, there will be no reason to think that either geometry is an appropriate description of visual experience.

The point at which all three modern articulations of this argument, Yaffe’s, Van Cleve’s and Belot’s, are weakest is in the way visible angles are specified. The other ‘visible’ concepts, such as ‘visible line’, just serve to pick out sets of physical points or lines. ‘Visible angle’, on the other hand, specifies how we are to measure the angle between those lines. We are told that the visible angle is the angle between the planes common to the visible lines and the eye. The immediate question that naturally

arises is, why among the infinite number of possible relations we could possibly use to define 'visible angle' should we take dihedral angles to be the correct one?

To see what is at issue here, let us look at an argument Yaffe offers for the claim that the geometry of visibles cannot be Euclidean. This argument proceeds by attempting to show that on the basis of the definitions offered above there can be visible triangles that have visible angles greater than  $180^\circ$ . This would provide a counterexample to the thesis that visual experience is Euclidean.

To show that such a figure does in fact exist, Yaffe offers the following thought experiment: Imagine the eye placed in the corner of a room where two walls and the ceiling meet. Then take three points, each on one of the three lines that meet in the corner and each one foot from the corner. If we connect these points with lines drawn on the walls and the ceiling we will have a visible triangle. Because visible angles are dihedral angles in this case they will all be  $90^\circ$ , because these are the angles between each of the walls and the ceiling. This gives us a visible triangle with visible angles totalling  $270^\circ$ , which means that at least one proposition from Euclidean geometry is contradicted. This means that the geometry of visibles cannot be Euclidean.<sup>102</sup>

The importance of the definition of 'visible angle' in terms of dihedral angle for this argument is obvious - if it was defined differently, different conclusions would follow about the sum of the angles in visible triangles. In fact we can go further than this: until we have given a definition of 'visible angle' then the definition of 'visible triangle' does not pick out a unique object.

Yaffe defines 'visible triangle' as simply three connected visual lines. This merely picks out a set of lines that could potentially constitute an infinite number of different triangles. The reason for this is that a triangle is a figure constituted by three lines lying on a common plane; an infinite number of objects could be taken to serve as the plane common to all three lines. A sub-manifold of any curvature would serve, so long as the three lines could lie on it in some orientation without distortion. This indeterminacy is only eliminated when we specify a common plane, which need not be flat; we are entirely free to choose and, from a logical point of view, each choice is as valid as any other. This is significant to the issue at hand because what counts as the common plane determines what the internal angles of the triangle are, as these

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<sup>102</sup> Gideon Yaffe. "Reconsidering Reid's Geometry of Visibles" *The Philosophical Quarterly* 52(209) (2002): 610.

must also lie on the common plane in order to be considered ‘internal angles’ in any significant sense.

This suggests an important difference between the concept ‘internal angle’ and the concept ‘visible angle’. ‘Internal angle’ is a concept that is constrained by what is taken as the common plane – in specifying the common plane we restrict the class of objects that can count as internal angles. This works the other way too: in specifying what are to be counted as the internal angles we restrict the class of objects that can count as the common plane. On the other hand, when we specify what is to count as the ‘visible angle’ we do not thereby restrict the class of objects that could count as the common plane. Moreover, the concept ‘visible angle’ should surely bear a semantic relation to something like ‘the angle presented to us by vision’. In light of this it is not acceptable to just define ‘visible angle’ as dihedral angle until we have an argument to show that the angle presented to us by vision is a dihedral angle. To do so just looks like an act of *fiat*.

Someone may observe at this point that this objection only applies if we accept Yaffe’s way of defining ‘visible figure’. It could be argued that this objection does not apply if we run Yaffe’s anti-Euclidean argument using Belot’s definitions. This is because for Belot a visible line is a set of coplanar lines passing through the eye. As such, a visible triangle is the conjunction of three such sets, such that the planes each set of lines lie on all intersect. On this way of construing ‘visible triangle’ it perhaps seems more plausible to say that ‘visible angle’ should be measured by the dihedral angle.

There are two responses to this objection. The first is that this simply is not the right kind of reason that can justify a definition of visible angle. What we need is something that warrants the claim that dihedral angles are the angles we are aware of in vision. Our intuitions about the ‘right’ way to measure the angle between two physical planes to get an appropriate description of their relations are irrelevant here. The second point is that there are reasons for being sceptical about accepting Belot’s definition of ‘visible line’ and the consequent ‘visible’ figure concepts, such as ‘visible triangle’. The reason is that it seems implausible to say that when I am visually aware of some external object, what I am aware of is a set of lines, or the conjunction of sets of lines. If the motivation for making visible figures external objects is to preserve Direct Realism, this way of defining them seems to defeat that

purpose. Recall that Indirect Realism is an error theory: Direct Realism is the commonsense view about which objects we are directly perceptually aware. If it is desirable to defend Direct Realism because it is closer to commonsense views than Indirect Realism, then to do so by defining ‘visible’ concepts in such a counterintuitive way seems unacceptable.

So, it seems that Belot’s way of construing the ‘visible’ concepts cannot be used to avoid the criticism that the definition of ‘visible angle’ seems to be unwarranted. But why is this criticism a serious concern for this attempt to argue that the geometry of visibles is a spherical geometry? Well, it has to do with part (b) of the demonstration of the equivalence, which concerned visible angles. This part of the argument is looking for figures whose real angles are the same as their own visible angles. Because visible angles are defined as dihedral angles, we are looking for figures whose real angles are also dihedral angles. Spherical figures fit this bill, which was the significance of the truth of the conditional we looked at earlier:

If figure  $f$  is a spherical figure, then its visible angles = its real angles.

This was why spherical figures could speak for visible figures. This is really the heart of the argument in part (b), the later step involving Van Cleve’s conjecture was really just tying up loose ends.

It should be plain that, as was the case with Yaffe’s anti-Euclidean argument, if we were to define ‘visible’ angle other than in terms of dihedral angle we will get different results, depending on our choice of definition. Different figures would turn out to have visible angles that are the same as their real angles. Consequently, which figures can ‘speak for’ or represent ‘visible’ figures will depend upon this choice of definition. Now, if it is a condition on a definition of ‘visible angle’ that the *definiens* be the angle we are aware of in vision, then just knowing which figures have the same real angles as some arbitrarily specified *definiens* of ‘visible angle’ will not put us in a position to decide whether those figures are representatives of what we are aware of by means of vision. The arguments that establish equivalence do not cut to the heart of the matter: this is because they are neutral on whether the initial geometry is articulated in a way that is appropriate as a description of what we are aware of by means of vision. Just calling the geometry a ‘geometry of visibles’ does not secure this. In Yaffe’s case, definitions of concepts like ‘visible position’ and ‘visible figure’





are defined with sensitivity to phenomenological considerations, such as those exemplified in his first desiderata for the definition of 'visible position'. However, no direct attempt to justify the definition of 'visible angle' is to be found in any of these modern reconstructions.

What is needed is some attempt to justify the definition offered. As neither Yaffe, nor Van Cleve, nor Belot have much to say on this point, we must explore the various possibilities that seem to be available. To do so, let us return to Yaffe's thought experiment of the eye placed in the corner of a room with a triangle drawn on the walls and ceiling. One way of justifying the definition might be to appeal to the fact that the three lines lie on physical walls, which represent the planes common to a pair of the lines. But this is surely not sufficient, as the physical walls are not essential to the thought experiment. We could take a wire-mesh triangle and produce the same argument by imagining the common planes intersecting at the eye.

So, perhaps the justification has to do with the fact that the planes forming the dihedral angles all intersect with the eye. If we add to this the observation that those planes coincide with the path of the light rays that produce images on the retina, we might feel we are approaching a justification. But how is this to support the proposed definition? All it shows is that the light rays that produce the retinal image of the triangle converge at that point – and this alone is not a good reason for taking the angles between the planes to be those we want to call 'visible'. This is because facts about the retina and the projections onto it cannot get us straightforwardly to facts about our visual experiences.

It seems to me that the only way to provide an independent justification of any proposed definition of any concept that deserves to be called 'visible' is to show that the definition is appropriate to the phenomenology of what we are in fact immediately aware of by means of vision. So, in the case of 'visible angle', if the proposed definition is in terms of dihedral angles then the justification for that definition should be that those are the angles we are immediately aware of by means of vision. The only way I can see of doing this is to produce introspectively based evidence that this is so: the kind of evidence appealed to by Angell and French. If this is right, then the argument is no longer *a priori*.

Before continuing, it is worth underlining the point here: the demonstration of the equivalence of spherical geometry and the geometry of visibles as it is presented in the modern arguments of Yaffe and Belot cannot stand on its own. If we need to

produce introspective evidence to support the definition of 'visible angle' in terms of dihedral angles, then the argument no longer stands up by itself as *a priori*. This kind of argument can only be as compelling as the introspective evidence that supports the definition. To this extent, I think, the dismissal Suppes made of purely *a priori* approaches to the question is appropriate in this case. Before I turn to discuss the kinds of considerations put forward by Angell and French I look more closely at some interesting features of Van Cleve's discussion, before I proceed to highlight some consequences for the issue of Direct Realism that emerge from the arguments considered.

### **Van Cleve and Direct Realism**

I have argued that the contemporary attempts to establish that the geometry of visual experience is a spherical geometry, in the way outlined above, are unsuccessful. This is because the strategy involves defining certain crucial terms in a certain way, but without offering any real attempt to justify the proposed definitions. However, we cannot yet conclude against all of the contemporary attempts to establish a spherical geometry for visual experience. We can only conclude against those that employ what might be called the 'define and prove' strategy discussed above.

Recall Van Cleve's discussion of the portion of Reid's argument dealing with visible angles. Van Cleve's reconstruction involved no attempt to give a *general* definition of the term 'visible angle'. As such, it is questionable whether the previous line of objection will affect this argument. There is a further reason for looking more closely at Van Cleve's discussion: there are a number of points about the connection with Direct Realism and the argument from illusion that emerge upon further consideration of his discussion. However, I will begin by considering the strength of his overall argument concerning visible angles.

Here is an analytic reconstruction of Van Cleve's argument concerning the equivalency of visible angles and the angles of representative great circles:<sup>103</sup>

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<sup>103</sup> This argument is found in James Van Cleve. "Thomas Reid's Geometry of Visibles" *The Philosophical Review* 111 (2002): 389-391.

- (1) Every visible triangle is indistinguishable from some spherical triangle, and therefore has its visible angles equal to the visible angles of the spherical triangle.
- (2) The visible angles in a spherical triangle equal its real angles.
- (3) The real angles in a spherical triangle add up to more than  $180^\circ$ .
- (4) Therefore, the visible angles in a visible triangle add up to more than  $180^\circ$ .

Sentence (3) is just a truth about spherical geometries. Sentences (1) and (2) are supported by further arguments. The inference in (1) is supported by the following premise:

(P1) The visible angle made by any two visible straight lines = the visible angle made by the two great circles representing them.

This premise is, in turn, supported by a set of auxiliary assumptions, as is sentence (2).

The assumptions supporting (P1) are as follows:

(A1) Any two visible straight lines appear to coincide with two great circles.

(A2) If the angle making lines 1 and 2 appear to coincide respectively with the angle making lines 3 and 4 then the visible angle made by 1 and 2 will be the same as that made by 3 and 4.

Van Cleve takes (A1) and (A2) to be unobjectionable and, taken together, they imply (P1). However, I will argue in due course that these two premises *can* be objected to and that they are not as straightforward as they may appear at first sight. Initially, however, I will set aside concerns about the correctness of this part of the argument, as the issues involved are more complex than those surrounding the argument for sentence (2). Moreover, the motivation for questioning sentence (1) can be most clearly seen after a consideration of the presuppositions that underpin the arguments

for sentence (2). I will therefore return to these two assumptions (A1) and (A2) later, where I discuss the relationship between visual indistinguishability and the identity of visible figures.

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Sentence (2) is supported by the following premise:

(P2) The visible angle made by two great circles = the real angle made by these great circles.

This is in turn supported by the following set of auxiliary assumptions:

(A3) The visible angle made by two great circles = the visible angle made by their tangents.

(A4) The visible angle made by the tangents of two great circles = the plane angle made by those tangents.

(A5) The plane angle made by the lines tangent to them at their point of intersection = the real angle made by two great circles.

(P2) The visible angle made by two great circles = the real angle made by these great circles.

Assumption (A3) is a consequence of the previous assumption (A2) and assumption (A5) is just a standard mathematical convention. Previously I said that it may appear that (A4) just relies on the same definition of 'visible angle' that Yaffe employs: that visible angles are dihedral angles. Indeed, such a definition would be sufficient for (A4). However, (A4) itself involves no explicit endorsement of a general definition of 'visible angle' – indeed, it is explicit only about what the visible angle made by the tangents of great circles must be. However, Van Cleve offers some reasons for accepting (A4) that are *independent* of any such general stipulations as are found in

Yaffe and Belot's work. This means that before we can conclude against all of the modern forms of Reid's argument, we will need to assess these considerations. I will discuss these independent motivations for (A4) shortly, but first I intend to underline its central place in Van Cleve's version of the argument. My strategy for doing so is to offer an argument for the claim that the visible angles of physical triangles add up to  $180^\circ$ ; but offer such an argument as has the same structure as Van Cleve's argument for the contrary. Such an argument is easily arrived at by replacing all reference to spherical lines and angles with phrases referring to planar straight lines and angles. If it turns out that the weak point of this new argument is the new version of the same assumption that appears weak in the original, then the strength of each argument will depend upon how well each assumption can be supported.

### **A Structurally Isomorphic Argument Concluding the Contrary of Van Cleve's**

Here is the 'alternative' argument to Van Cleve's, which has the same structure but a contradictory conclusion:

(1\*) Every visible triangle is indistinguishable from some other planar triangle, and therefore has its visible angles equal to the visible angles of that other triangle.

(2\*) The visible angles in a planar triangle equal its real angles.

(3\*) The real angles in a planar triangle add up to just  $180^\circ$ .

(4\*) Therefore, the visible angles in a visible triangle add up to just  $180^\circ$ .

The inference in (1\*) is supported by the following premise:

(P1\*) The visible angle made by any two visible straight lines = the visible angle made by the other planar lines representing them.

This is in turn supported by the following auxiliary assumptions:

(A1\*) Any two visible straight lines appear to coincide with some other straight lines.

(A2\*) If the angle making lines 1 and 2 appear to coincide respectively with the angle making lines 3 and 4 then the visible angle made by 1 and 2 will be the same as that made by 3 and 4.

Both assumptions are no more problematic than their originals and, taken together, imply (P1\*).

For the purposes of underlining the claim that the arguments have the same structure it is worth spelling out how Van Cleve’s premise (1) is related to the premise (1\*). The crucial point is that both premises turn on the indistinguishability of some visible figure from some real figure, spherical or otherwise. In Van Cleve’s case, the real figure can be any arbitrarily selected triangle on a sphere of radius  $r$  from the eye. In the case of premise (1\*) it can be any arbitrarily selected triangle whose vertexes are all of distance  $r$  from the eye. How the feature of visible indistinguishability secures the identity of visible angles in each case is spelt out by the supporting premises (P1), (A1) and (A2) and their corresponding premises (P1\*), (A1\*) and (A2\*) in the alternative argument. The premises of the alternative argument are generated just by replacing concepts like ‘great circles’ with the relevant ‘planar’ concepts.

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Sentence (2\*) in this alternative argument is supported by the following premise:

(P2\*) The visible angle made by straight lines = the real angle made by these lines.

This is, in turn, supported by the following set of auxiliary assumptions:

(A3\*) The visible angle made by straight lines = the visible angle made by their tangents.

(A4\*) The visible angle made by the tangents of two straight lines = the plane angle made by those tangents.

(A5\*) The plane angle made by the lines tangent to them at their point of intersection = the real angle made by the two straight lines.

(P2\*) The visible angle made by two straight lines = the real angle made by these straight lines.

Now, this argument and Van Cleve's cannot both be correct at the same time, because in this argument physical triangles turn out to have visible angles that total just  $180^\circ$  and in his argument to have visible angles that total more than  $180^\circ$ . Given that the two arguments are structurally identical, we need to examine the assumptions that support the major premises. As with the original, assumption (A3\*) is not problematic, especially if we accept, following Van Cleve, the convention that straight lines are identical to their own tangents.<sup>104</sup> (A5\*) is also unproblematic. However, (A4\*) is as substantial a claim as the corresponding assumption was in the original argument.

So, we have two arguments with contradictory conclusions. They each turn on alternative, substantial assumptions about the visible angles made by some real figure. It would seem, then, that the strength of each argument will turn on the strength of their corresponding assumptions. This requires us to consider the relative merits of these two premises. I will begin by considering precisely how these assumptions should be construed, i.e. what they should be taken to be asserting, by contrasting them with the other assumptions. I will then turn more specifically to considerations that tell on the relative strengths of these assumptions.

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<sup>104</sup> Ibid., p. 391

## **The Source of the Difficulty with (A4) and (A4\*)**

It makes sense to ask why there appears to be more difficulty with (A4) and (A4\*) than with any of the other assumptions in the arguments for (P2) and (P2\*); after all, they are all identity statements and the argument proceeds quite elegantly by means of the transitivity of equality. So what is the reason for this apparent difficulty?

The difficulty arises with these and not the other premises because there is a difference in the correct analysis of the phrases on either side of the identity sign. In the other assumptions, except (A5), which is a mathematical convention, the identity sign is flanked on both sides by phrases about visible angles. These sentences should not be thought of as expressing the identity of two ‘visibles’, but as expressing the similitude of the way certain geometrical elements appear visually. Interpreting them in this way neither implies nor rules out the reification of the references of phrases like ‘visible angle’, which seems desirable given the connection between this topic and Direct Realism. In the argument from illusion, the description of whatever we are immediately perceptually aware of must remain neutral on the identity of these objects until the application of Leibniz’s Law, on pain of circularity. So it should be possible to reformulate these statements in such a way that they do not have the appearance of quantifying over peculiarly ‘visible’ objects.

So (A3) seems best read as saying that however the angle formed by two great circles appears visually is just how the angle formed by their tangents will appear visually, and vice versa. The same is true of the consequent in sentence (1) – it says that however the angles of a planar triangle appear visually is just how the angles of a spherical triangle will appear visually. This reading corresponds well with the kinds of considerations in (A1) and (A2) that were adduced to motivate (P1), which were reasons about how things appear to the eye. (A3) and the consequent in sentence (1) are informative statements, but we will not have the correct description of the ways the angles will appear, until we have the correct way of describing at least one of those appearances.

The role of (A4) and (A4\*) in their respective arguments is to provide such a description. On one side of the identity sign is a phrase referring to how the angle formed by two lines appears, but on the other side is a phrase that refers to some geometrical property of the lines that is not dependent upon how they appear visually.



In this case there is no way of recasting the whole sentence to say that the angles will appear visually the same. Nor should there be; what (A4) and (A4\*) both seem to be saying is that the correct description of how the angle referred to on the left hand side appears is the same as the correct description of the angle referred to on the right hand side. This is consonant with the desire, stated above, to avoid constructions that may suggest the reification of 'visible' objects. As yet, we have been given no good reason to think that the correct description of the visible angle formed by the tangents of two great circles will be the same as the description of the plane angle formed by those tangents.

### **An Argument Supporting (A4)**

Van Cleve does offer a brief argument in support of (A4), which has to do with the fact that the plane of the tangents of great circles will be orthogonal to the line of sight. The argument is given in the following parenthetical comment on (A4):

This [assumption (A4)] is true given our assumption that the tangents are viewed from the center of the sphere containing the great circles, since in that case one's line of sight will be orthogonal to the plane of the tangents.<sup>105</sup>

So, take both a line of longitude on the Earth and the Equator, with the central point of the Earth as the point from which they are viewed. Where they intersect, the tangents of these spherical lines both lie in a plane that is orthogonal to one of the lines radiating out from the central point; call this line the 'line of sight'. But, what is it about this that guarantees that the description of the plane angle between the tangents will be the correct description of how the spherical angle appears visually? What is it about these plane, or dihedral angles that guarantees that they will appear visually in such a way that the description of that visual appearance is the same as the description of the angle between the planes?

At this point someone could make the following response: although the consideration Van Cleve appeals to does not conclusively demonstrate that his assumption (A4) is correct, the fact that the plane containing the tangents is assumed

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<sup>105</sup> Ibid, p. 390

to be orthogonal to the line of sight points us in the direction of a strong reason for thinking that (A4\*) in the alternative argument is deeply implausible and for thinking that (A4) is highly plausible. This would put the dialectical weight in Van Cleve's favour.

The reason for thinking this is something like the following: consider two straight lines forming an angle in a plane that is orthogonal to the line of sight. Now tilt the lines towards you and then away from you. There is an obvious sense in which the way it appears changes: crucially, this provides reasons for thinking that the visible angle need not be the same as the planar angle, contrary to (A4\*). This point should be familiar from chapter 1 – it is just the same sense in which a circle tilted toward an observer in some sense appears elliptical. This consideration suggests that (A4\*) is implausible. No comparable implausibility attaches to (A4); so, it may be argued, there is no good reason for thinking the alternate argument is any good and so no reason for being suspicious of Van Cleve's.

The problem with this response is that the implausibility of (A4\*) relies on the presupposition that the only way to do justice to the phenomenal character of how the lines appear in such cases is to describe them in terms that diverge from a description of how they really are. In the case of a triangle tilted toward the eye, the temptation is to describe it as appearing such that the angles appear not to equal  $180^\circ$ . To put it another way, suggesting that (A4\*) is implausible *forces* us to interpret such cases as cases of illusion.

Now, it is true that support must end somewhere, and perhaps this looks like an ideal place to stop. However, this presupposition really goes to the heart of the matter, as has been articulated by one recent commentator on these kinds of cases, who has provided a considered appraisal of this presupposition and sophisticated reasons for denying it. A. D. Smith, whose rejection of the traditional form of the argument for illusion we looked at briefly in chapter 1, has argued that what lies behind the temptation to say that a circle turned toward an observer appears elliptical is the presupposition that what we are immediately aware of visually is a two-dimensional array of colour patches.<sup>106</sup> This is a presupposition that Van Cleve's argument inherits from Reid. In effect, this presupposition obliges us to cash out descriptions of how things appear in terms of their projective properties. Smith has

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<sup>106</sup> A. D. Smith. *The Problem of Perception* (Cambridge, Mass.: Harvard University Press, 2002), p 182; see also pp. 180-183.

advanced several sophisticated arguments that aim to show that there is no good reason for assenting to this presupposition, some of which we will look at more closely in connection with some of the direct phenomenological evidence for a spherical geometry.<sup>107</sup> What is crucial is the observation that we can accept the phenomenon that motivates this presupposition – that we are tempted to say that circles can appear elliptical and the angles of triangles can appear other than they do – whilst denying the stronger claim – that the only way to do justice to this temptation is to treat how something appears as given by its projection onto a two dimensional surface, spherical or otherwise.

Accepting this stronger presupposition would of course rule out the possibility that the visible angles of triangles turned toward an observer just would be its actual angles, and in doing so would rule out (A4\*). But this in turn allows us to run the argument from illusion, which puts pressure on Direct Realism. Moreover, it is not the straightforward phenomenal character, or the temptation to describe the phenomenal character in a certain way, that has this consequence, but an unargued stricture concerning what is to count as an adequate description of an ‘appearance’ in the phenomenological sense. Given that this presupposition is of such enormous significance to a major philosophical problem, I suggest it is not the minimal presupposition where justification can stop that it may appear to be in this kind of objection. Consequently, it cannot provide adequate justification for accepting (A4) over (A4\*).

It is important to see why (A4\*) is significant. It is not an arbitrarily chosen premise among many. It represents precisely what a Direct Realist is likely to want to claim about planar triangles, if he wants to avoid the argument from illusion based on spatial properties, instead of being forced to respond to it. Premise (A4\*) asserts that the apparent angular sum of a triangle just will be its actual angular sum. This leads us to a curious observation: Van Cleve’s (A4) is actually something that a Direct Realist will also be likely to want to accept. It asserts that the correct description of how the angles formed by tangents of great circles appear just will be the correct description of those angles. If this is right, then the Direct Realist will want to accept *both* premises. So, this dialectical defence of the plausibility of (A4\*) only goes so far – one of its

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<sup>107</sup> Smith’s most detailed discussion of this point is found in A. D. Smith “Space and Sight” *Mind* 109 (2000): 481-518. Some of these arguments will be discussed in both the next chapter, pp. 134-135, and chapter 5, pp. 182-187.

consequences is that this section of Van Cleve’s argument supporting premise (2) must be accepted. However, we still have the problem that there are two arguments with contradictory conclusions. If there is a problem with Van Cleve’s argument, it must be to do with premise (1), which *prima facie* seems to rest on entirely plausible premises. I will now turn to a discussion of this earlier section of Van Cleve’s argument.

**Visual Indistinguishability and the Identity of Visible Figures**

I have noted that premises (1) and (1\*) are alike to the extent that they both turn on the feature of indistinguishability of figures to secure the identity of the visible angles of separate and qualitatively different figures. In both arguments these premises function as a generalising move. If the results of the previous section are correct then it is this part of Van Cleve’s argument that must be rebutted to avoid the argument from illusion based on spatial properties. This is because, as we have seen, premise (2) does not imply any discrepancy between how the angles of spherical triangles are and how they appear. It is premise (1), though, that warrants assigning the visible angles of spherical triangles to *all* triangles, if they are indistinguishable from such spherical triangles. To avoid the argument from illusion in this case, what is needed is some way of denying that the truth of premise (2) has any significance for planar or other non-spherical triangles. There is, I suspect, no direct way of rebutting premise (1); however, it will be sufficient to show that despite being *prima facie* quite plausible, it can coherently be denied.

The relevant steps of this section of the argument are those considerations that support the premise (P1), which was the premise underpinning the inference in (1). Here is a reminder of the premise and those steps:

(P1) The visible angle made by any two visible straight lines = the visible angle made by the two great circles representing them.

The assumptions supporting (P1) are:

(A1) Any two visible straight lines appear to coincide with two great circles.

(A2) If the angle making lines 1 and 2 appear to coincide respectively with the angle making lines 3 and 4 then the visible angle made by 1 and 2 will be the same as that made by 3 and 4.

(A1) is quite straightforward – it is just a way of expressing the fact that the two pairs of lines will be visually indistinguishable, given that there is a one-to-one correspondence between the visible positions of all the parts of one figure with all the parts of the other and given that depth perception is not perceived. (A2) is where things get interesting. Let us assume that lines 1 and 2 are physical straight lines and assume that lines 3 and 4 are the great arcs of some sphere centred on the eye. By (A1) we can say that these will all ‘appear to coincide’ with each other. Exactly what this amounts to will be discussed in the next section, for now I will not question this.

Now, what (A2) asserts is that, despite the figures all having different real angles, they will all have the same visible angle. This is because it asserts a connection between two figures appearing to coincide (or the visual indistinguishability of two figures) and the sameness of the properties that can describe the way in which the two figures appear. At first glance this seems just as straightforward as (A1), but there are reasons for thinking it is not. If we accept the connection it asserts, then we do not even need to know what the actual geometrical properties of visual experiences are to know that the argument from illusion based on the geometry of visual experience can be run. (A2) means that there is a one-many relation between the apparent properties of the figures and the real properties of those figures. That is, there is only one way that visually coincident figures can all be described correctly in respect of their apparent properties. However, if we are to block this version of the argument from illusion before it gets under way, we need to find some way of claiming that there must be a many-many relation between the apparent properties and the real properties of the figures in the visually indistinguishable cases given above. This is to say that, even though figures can be visually indistinguishable, they can each have different apparent (spatial) properties.

This result has interesting consequences for the best method for approaching the issue of the correct geometrical description of visual experience. One possibility is, naturally, to accept (A2) and go on to find the proposed single, correct geometrical description of the way visually coincident figures appear. The problem with this

approach is that it does not allow for the possibility that an answer to the question of the geometry of visual experience can be evidence that could support a philosophical theory of perception. Such a possibility certainly appears to exist, by virtue of the reciprocal relationship between the issue of Direct Realism and the issue of the geometry of visual experience. This returns us to the *prima facie* worry that led us to criticise defining of 'visible angle' in terms of dihedral angles as an act of *fiat* – surely the geometry of visual experience cannot be determined in this kind of *a priori* fashion. As I have emphasised, along with Suppes, the question of the geometry of visual experience is at least in part an empirical one. This suggests that it should at least be possible to motivate a response to the question that does not involve an antecedent commitment to an overarching philosophical theory of perception. Such a response would be generally more convincing.

It is true that this problem is not conclusive against this kind of approach, but they at least provide some reasons for being sceptical about the results obtained by such an approach. It seems to me better to treat (A2) with scepticism and to construct an argument in favour of a particular geometry that remains philosophically neutral on this point.

## **Visual Indistinguishability and Binocular Cases**

At this point someone may object that we cannot leave the argument here. This is because there is a problem with denying (A2) but still accepting (A1). It leaves (A1) unexplained and also requires an account of how two figures can both be visually indistinguishable, without it being true that the geometrical description of how they appear is the same for both. I do not know in any detail how this could be done, if one can even make sense of such a possibility. However, there is a simpler alternative – to deny (A1) as well. However, (A1) seems straightforward and unproblematic, so how could it sensibly be denied?

Well, (A1) is certainly correct in the monocular case considered by Van Cleve, but is it also correct in the binocular case? Van Cleve does briefly discuss the implications upon his argument of adding a second eye.<sup>108</sup> He thinks that (A1) is

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<sup>108</sup> James Van Cleve. "Thomas Reid's Geometry of Visibles" *The Philosophical Review* 111 (2002): 397

unaffected by the addition of a second eye, because (A1) is a consequence of the unperceivability of depth. Van Cleve thinks that it is not clear that this will be affected by the addition of a second eye.

I will first provide a reason for thinking that he may be wrong. Take a monocular instance of (A1), where a straight line coincides with a great circle. They will be visually indistinguishable. Now take a point in physical space, different from that representing the first eye, to represent a second eye. To the second eye the two lines in question will not appear to coincide and will be visually distinguishable. To be sure, there will be other straight lines and great circles centred on this second point of which (A1) will be true, but this is irrelevant to the question of what the correct geometrical description of the binocular experience is. This is because to have a binocular visual experience is not to simply have two eyes, or two retinal images; it is to have a single experience that combines the information contained in both images. So, a conjunction of the separate descriptions of how things 'appear' to each eye will not be a good description of the binocular experience. It follows that, for any two lines that are instances of (A1) in the monocular case, in the binocular case there is no guarantee that they will also be instances of (A1). This at least leaves logical space to deny that (A1) holds in the case of binocular visual experience and, as the above example shows, there are cases where it does not.

In light of this, what can be made of the reasons that Van Cleve gives for thinking that Reid's argument will not be undermined by the addition of a second eye? Well, as we have seen, (A1) rests on the unperceivability of depth, and to deny (A1) is to assert that depth can in some sense be perceived. This is effectively the concern I raised in relation to Van Cleve's argument for (A4). Here, Van Cleve points out that if we grant that depth can be perceived in the binocular case, we must specify whether we take this in an epistemological sense or a phenomenological sense. He argues that in neither of these senses does the claim that depth can be perceived undermine (A1). Firstly, the epistemological sense does not even get off the ground, because the contention is not about what can be inferred from our visual experiences or about the kinds of judgments those experiences warrant. The relevant sense must be the phenomenological sense.

The example Van Cleve considers for the phenomenological sense in which depth could be claimed to be perceived is H. H. Price's claim that tomatoes look

bulgy.<sup>109</sup> Van Cleve argues that even if this were true, it would remain true that ‘a straight line can perfectly occlude a curved line, as assumed in the crucial assumption A1.’<sup>110</sup> The problem is that this is only straightforwardly true in the monocular case. In binocular experiences occlusion is rarely, if ever, *perfect* – binocular experiences of occlusion along a line have a vagueness or ‘fuzziness’ to them that is noticeable if one alternately opens and closes one eye. The presence of occlusion in the binocular experience is presumably the contribution made by the eye for which the occlusion is perfect; the ‘fuzziness’ of the occlusion in binocular experiences is presumably the result of the perfect occlusion for the one eye and the total lack of occlusion for the other. Van Cleve is not thinking of (A1) being applied to the binocular *experience*, but is thinking of it being applied to each individual eye in respect of different sets of occluding lines and curves for each eye. This is the mistake I pointed out above.

However, it may be argued that in cases of binocular experience *some* straight lines can occlude some curved lines perfectly, such as those pairs of lines whose distance from the observer is such that retinal disparity is negligible. It is not clear how useful this point would be to Van Cleve, as it would not warrant (A1) – we could not claim that for *any* visible line it would perfectly occlude some spherical line.

Van Cleve is right in claiming that *if* (A1) holds of binocular visual experiences then the presence of the perception of depth, in the phenomenological sense, need not obviously be a problem for this argument. This is because the kinds of considerations that *may* motivate the belief that spherical geometry is correct of monocular cases would also apply to binocular cases. The consideration in question is that perfect occlusion of spherical figures by visible figures. What I have shown is that there are reasons for thinking that (A1) does not hold of binocular visual experiences, at least in the sense that there can be perfect occlusion.

So, it appears possible to deny (A1) in the case of binocular visual experience; no good reason has been given for thinking that it will apply; and there are some reasons for thinking that it will not apply in the right sort of way. This leaves open the possibility of denying (A2). It may be strange to think that in either the monocular or the binocular cases that two figures can be visually indistinguishable, but have different apparent properties. However, two physical figures can be visually

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<sup>109</sup> Loc. Cit.

<sup>110</sup> Loc. Cit.



indistinguishable in the monocular case, but have different apparent properties in the binocular case. This removes any appearance of strangeness.

### **The Geometry of Visibles As a Geometry of Projective Qualities, and Other Such Reconciliatory Strategies**

I have argued that none of the contemporary arguments, modelled on Reid's discussion, to establish a spherical geometry for visual experience are without problems. I have, during the course of the discussion of Van Cleve's version of the argument, highlighted the various ways in which the difficulties for Direct Realism emerge from the position argued for. Now, both Gideon Yaffe and James Van Cleve are sensitive to the pressure that a non-Euclidean geometry for visual experience places on a commitment to Direct Realism, pressure that comes from the argument from illusion. Both of these philosophers have offered attempts to show that the difficulties here can be met, but their suggestions differ in the details. Both are similar, however, to the extent that they both employ the kind of strategy discussed in chapter 1, which has been employed in response to the traditional form of the argument from illusion based on spatial properties. That general strategy was to find ways in which the apparent, or 'visible', properties can be construed as legitimate properties of the objects we pre-theoretically hold to be the objects of perceptual awareness.

Let us, for the sake of argument, then, assume that the preceding criticisms can be responded to and a more successful argument can be given. The question then is, how compelling would these attempts to avoid the difficulties associated with Direct Realism be? I think that neither of the attempts offered by Yaffe and Van Cleve is particularly effective. I will look at each in turn, but I shall begin by considering the proposal offered by Yaffe, as the proposal offered by Van Cleve is considerably more radical than that offered by Yaffe.

In his article, Yaffe suggests a move that may offer a way out of the difficulty that arises from at once being a Direct Realist and holding the geometry of visibles to be a spherical geometry, however it is fair to say that he is sketchy on the details. The move involves suggesting a way in which the geometry of visibles is not incompatible

with the local geometry of external objects, i.e. Euclidean geometry. In the following passage, taken from the conclusion of his anti-Euclidean argument, he appears to suggest that the ‘visible’ predicates denote properties that are possessed by the set of lines in question, but are nonetheless compatible with predicates from other geometries. Recall that the anti-Euclidean argument invited us to consider three lines drawn on two walls and the ceiling of a room, with the eye in the corner. The angles between the walls, the dihedral angles, add up to  $270^\circ$ :

The result is that the three visible angles are each equal to  $90^\circ$ , and so the visible angles add up to  $270^\circ$ . But triangle  $ABC$  is also a planar triangle, in addition to being a visible triangle. So although its visible angles add up to  $270^\circ$ , its planar angles add up to  $180^\circ$ . This is noteworthy only because it illustrates the discrepancy between the planar and visible features of single objects.<sup>111</sup>

The problem is that Yaffe is not explicit about how there can be both ‘planar and visible features of single objects’. Unless we have an account for how this is possible it may be suspected that Yaffe is just brazenly asserting that these contradictory predicates can in fact coherently be ascribed to single objects. To avoid this objection what is needed is a way to coherently ascribe both ‘visible’ predicates and Euclidean predicates to single objects. In Yaffe’s discussion there seems to be the material for at least two possible ways of doing this. The first exploits the observation made earlier that three lines alone do not uniquely determine a triangle; the second is to deny that predicates from *different* geometries are being ascribed to the object of perception.

If we ignore the phrase ‘But the triangle  $ABC\dots$ ’, the passage quoted seems to concede the point that the three lines alone do not uniquely determine a single triangle. However, the three lines *do* uniquely constitute an object: viz. a set of lines. It is this set that can be variously described as a planar triangle or as a visible triangle, depending on what you take to be its internal angles. This suggests the first possible way we could coherently ascribe both ‘visible’ predicates and Euclidean predicates to the figure – it’s just a matter of which geometry you choose to describe the set of three lines with.

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<sup>111</sup> Gideon Yaffe. "Reconsidering Reid's Geometry of Visibles" *The Philosophical Quarterly* 52(209) (2002): 610.

Now it is certainly true that the three lines can be so variously described, but this suggestion concedes too much to conventionalism, for the following reason: if we have good reasons for thinking that at a local level physical space is Euclidean, then we have good reason to think that the set of three (physical) lines is adequately described by Euclidean geometry. In this case, the internal angles of the triangle constituted by the three lines are planar angles. If this is right then the internal angles cannot be dihedral angles, for the reasons given earlier. Now, I take it that Yaffe's assertion in the passage quoted - that the set of three lines constitutes a planar triangle in addition to constituting a visible triangle - to mean that he does think we have good reasons for thinking that physical space is locally Euclidean. So this suggestion does not appear to escape the difficulties posed by Direct Realism.

Given that the first way of interpreting the passage does not avoid the difficulties posed by the commitment to Direct Realism, we should turn to the second interpretation. This is the suggestion that 'visible' concepts should be taken to specify a particular sub-set of those properties that are picked out by the correct, Euclidean description of the physical figures. In particular, they pick out the projective properties of the objects. The 'visible' concepts and the Euclidean concepts are therefore not incompatible. This kind of move can be easily justified by the convenience and usefulness of such a separate description for dealing with certain philosophical problems: in this case, for explicating concepts like 'what we see'. This is the strategy that Yaffe appears to be endorsing – he denies that the geometry of visibles is a genuinely non-Euclidean geometry, but is just an explication of the projections of objects onto spherical surfaces:

But Reid is... really applying projective geometry to perspectival shape, rather than developing a genuine non-Euclidean geometry.<sup>112</sup>

The idea here is that all of the 'visible' concepts constitute a description of a set of relations that can be given an alternative description in Euclidean terms. This is not surprising, as the 'visible' concepts have all been defined in terms of Euclidean concepts, such as 'dihedral angle'. However, the description of the set of lines in

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<sup>112</sup> Gideon Yaffe. "Reconsidering Reid's Geometry of Visibles" *The Philosophical Quarterly* 52(209) (2002): 603. My concern here is not with whether this is the interpretation of how Reid thought about these matters, but whether the proposed solution is philosophically adequate.

terms of 'visible' concepts is itself justifiable if it is intended as a handy way of describing those relations that we are immediately aware of by means of vision.

The claim that the geometry of visual experience is just the sub-set of theorems from Euclidean geometry that concern projections on a sphere avoids the difficulty presented by the commitment to Direct Realism because there is no incompatibility between the Euclidean properties of external objects and any of their projective properties. The projective properties do not require the ascription of a different intrinsic curvature to the external object. But is it really plausible to make such a claim? On this view it does not merely turn out that the geometry of visibles is in some sense equivalent to a sub-set of propositions from Euclid's geometry: it turns out that there is no 'geometry of visibles' distinct from Euclidean geometry. The 'geometry of visibles' just is the part of Euclidean geometry dealing with the projective properties of external objects (in this case, projections onto spheres).

One advantage of holding this position is that if it is correct, then there should be no more difficulty in claiming that visible properties are real properties of external objects than there appeared to be for Mundle or Armstrong in chapter 1. The difficulty of carrying out this reductive strategy emerges only when we have genuinely non-Euclidean predicates applied to visual experience. Here the difficulty is dodged, because the 'visible' concepts turn out not to be genuinely non-Euclidean predicates.

Although this position ultimately results in the surprising conclusion that after all the geometry of visibles is Euclidean, why might this be thought to be a problem? Well, it at least presents a problem for those who accept that there is some sensory or qualitative aspect to visual experience: a phenomenal character. The first point to note is that if you do accept this, then it is difficult to hold that we can *only* correctly describe the phenomenal character of our visual experiences once we have the correct description of the external objects. To see this, reflect on the possibility of hallucination. The very possibility of hallucination means that anyone who accepts that visual experience has a phenomenal character ought to accept that we can correctly describe it without there even being an external object. Now, consider the concept 'visible line' from the geometry of visibles: this either picks out geodesics or it does not. If it does not pick out geodesics then it can be a concept from Euclidean geometry, just not the one we might ordinarily take it to be. However, if it does pick out geodesics then it cannot be a concept from Euclidean geometry, because the objects it picks out are not geodesics in Euclidean geometry.

The present attempt to avoid the problems arising in connection with Direct Realism must therefore hold that 'visible line' does not pick out geodesics. But now return to the case of hallucinatory experiences; is it plausible to claim that in such cases 'visible line' does not pick out geodesics? To see that it is not, reflect on what this would entail: that hallucination involve awareness of the projective properties of objects that do not exist. This seems implausible. The only thing that is plausible, and which is remotely close to this idea, is that hallucinatory experiences can have a phenomenal character that is qualitatively identical to veridical perception. But this is not something that excludes the contrary position.

It seems to me, then, that in hallucinatory cases we must say that 'visible line' picks out geodesics and not that it picks out Euclidean projective properties. Now, if the concept 'visible line' picks out geodesics in non-veridical cases then, unless we give an analysis of perception that treats veridical and non-veridical cases differently, i.e. unless we are disjunctivists, we must accept that it also does in veridical perception.

I have argued here that this attempted solution is implausible if you accept that there is phenomenal character to visual experience. Now as a matter of fact, Yaffe and Van Cleve consider themselves to be presenting a reconstruction of Reid's argument, and Reid himself did acknowledge sensations. So, this solution is not available if a proper exegesis of Reid is our primary concern. If we set Reid scholarship aside, however, it does remain possible to employ this solution, but only by denying there to be a phenomenal character to visual experience. This, it might be thought, does not do justice to what is quite distinctive of visual experience: its phenomenal character.

Now, despite the charge of phenomenological inadequacy, this move has not been without its supporters. However, there is a further dialectical consideration that has a bearing here. This requirement that we deny a phenomenal character to visual experience should give a moment of pause for a proponent of the kind of *a priori* argument for the geometry that modern commentators of Reid have proposed. This is for dialectical reasons: denying a phenomenal character to visual experience only once we have seen the tension between the claim about the geometry and the commitment to Direct Realism would be grossly *ad hoc*. So, one would need to independently argue for it: let us, for the sake of argument, assume that there is such an argument available. Why then would someone who antecedently denied a phenomenal character to visual experience require an *a priori* argument to establish

the geometry of visibles? They would not accept that the geometry of visibles provides a description of a phenomenal character, as they hold there is none to be had. The only thing that is really left for the geometry of visibles to be is an account of the relationship between the external objects and the organ of sight. This would make the nature of the visible predicates a thoroughly empirical issue – this adds further weight to the point made earlier, that *a priori* approaches to the question of the geometry of visual experience are misguided.

I will now turn to Van Cleve's proposal for avoiding the difficulties posed for Direct Realism by Reid's argument. Van Cleve's suggestion is considerably more radical than the proposal just considered. In contrast to Yaffe's position, Van Cleve does not attempt to question the apparently genuine non-Euclidean status of the spatial properties that the argument attempts to ascribe to the immediate objects of perceptual awareness. However, as with Yaffe, this is not just a brazen attempt to assert contradictory predicates to these objects, a qualification is made. For Van Cleve, these non-Euclidean properties are contrasted with the 'real', or 'intrinsic' properties of the object of perceptual awareness. The non-Euclidean properties are some other sort of property, which Van Cleve calls a 'relational', or 'relativised' property.

The suggestion I wish to make now is that visibles simply *are* objects that are visible – they include tables, trees, and all the furniture of the earth. Visible figure is a property of objects – not an intrinsic property, like real figure, but a relational property (or perhaps better, a relativised property), possessed only in relation to a point of view.... [W]e may say that objects have certain shape properties in themselves and other shape properties relative to various points of view.<sup>113</sup>

The relational properties are specified relative to some point of view. So, the idea seems to be that two lines may, for instance, subtend an angle of 90 degrees, but those lines may *also* subtend an angle of more than 90 degrees *from a particular point of view*. The view Van Cleve is proposing is radical because it amounts to claiming that objects can be both Euclidean *simpliciter* and non-Euclidean from a point of view.

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<sup>113</sup> James Van Cleve. "Thomas Reid's Geometry of Visibles" *The Philosophical Review* 111 (2002): 404

It is, therefore, *not* an attempt to make sense of the claim that objects are Euclidean but can *appear* non-Euclidean. Van Cleve is quite explicit on this point:

The proposal may sound like the suggestion of our direct realist in section 11, who resisted the introduction of visibles as a class of entities and insisted that visible objects merely *appear* to have non-Euclidean properties.... I am only going part of the way with that direct realist. I am identifying visibles with ordinary objects, as he does, but in the relativized approach to visible figure I am suggesting, I am not saying that quadrilaterals merely *appear* to have angles that sum to more than 360 degrees.<sup>114</sup>

The move that Van Cleve is making here is analogous in one respect to that made by Armstrong in the passages we looked at in chapter 1. There Armstrong argued that in our awareness of perspectival shapes there is some relational content – we are aware of the shapes of objects relative to our bodies. The same point is being made here; however, I think it is fair to say that the strategy seems much less plausible in the present case.

The first reason for thinking so appeals to the point I made in chapter 1, that Euclidean and non-Euclidean predicates are just incompatible. This is what motivated a distinction between the traditional form of the argument from illusion based on spatial properties and the form based on the geometry of visual experience. If we are forced to defend Direct Realism by asserting that a given object has both Euclidean and non-Euclidean properties it might be thought that Direct Realism is in desperate straits indeed. However, this response is not sufficient, as Van Cleve can point out that it is the *relational* nature of the non-Euclidean properties that makes them compatible with the Euclidean real properties. An example Van Cleve uses to illustrate this is the property angles can have of being obtuse. There at least appears to be no logical contradiction between saying that a given angle is not obtuse and saying that it is obtuse from where I am sitting.

The first concern with this proposal is that for the non-Euclidean properties to be relational in this way they will need to be two-place predicates. The problem is that

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<sup>114</sup> Ibid., p 405

properties like ‘are obtuse’ and ‘subtend an angle of just 180 degrees’ seem to be just monadic properties – they seem to have a one place structure. Language already has words that mark out a distinction between relational and non-relational spatial properties. So, for example, constructions like ‘being wider than’ express spatial relations, whereas ‘being a geodesic’ or ‘being obtuse’ express non-relational spatial properties. Given this, Van Cleve’s strategy seems to go against the actual meaning of terms like ‘obtuse’.

Moreover, what should we make of the semantic relation between the spatial properties mentioned in sentences like the following:

- 1) Angle x is obtuse.
- 2) Angle x is obtuse from here.

In each sentence, should we construe ‘obtuse’ as having the same meaning? It seems that we cannot, because in (1) it is one-place, whereas in (2) it is two-place. This is, of course, what allows Van Cleve ascribing incompatible spatial properties to physical objects and visibles – because whatever ‘is obtuse from here’ means, it cannot be the same as ‘is obtuse’ and so need not exclude contrary predicates like ‘is acute’. However, to say what it does not mean is not to say what it does. Moreover, it just not clear what meaning can be attached to the phrase ‘... is obtuse from here’. It is far from clear what sense can be made of this proposal in any detail.

Also, the use of examples seems to me to make some difference to the strength of Van Cleve’s case. Compare the above sentences with the following:

- 3) Lines x and y subtend an angle of more than 180 degrees.
- 4) Lines x and y subtend an angle of more than 180 degrees from here.

In this case, it seems plausible to say that the lines subtend whatever angle they subtend; where one is makes no difference. The thing that makes (2) more plausible than (4) seems to be that (2) is closer in construction to sentences about appearances, i.e.:

- 5) Angle x appears obtuse from here.



It may be argued that the suggestion trades on the plausibility of these kinds of constructions. In cases like (5) the modifier ‘appears’ makes intelligible the claim that the property is genuinely relational. It is comprehensible to talk about objects appearing a given way from a point of view, and then showing that this is a perfectly objective property of the object. No comparative plausibility, or even comprehensibility, attaches to constructions that do not have the modifier ‘appears’, as in (4).

I have suggested that Van Cleve’s proposed solution to the challenges that are posed by concluding that the geometry of visual experience is non-Euclidean is not to be accepted. It is highly counterintuitive, but more crucially it is not clear what sense can be made of the proposal that non-Euclidean visible properties should be ‘relativised’.

## **Conclusion**

In this chapter I have considered a contemporary line of argument for a spherical geometry for visual experience, based on Reid’s original discussion. The strategy of this line of argument consists in arguing for the geometry in an *a priori* fashion by means of establishing an equivalency between a spherical geometry and the proposed ‘geometry of visibles’. I argued that this entire strategy is inconclusive, despite being successful in establishing the desired equivalency. This is because what we are after is some demonstration that the geometry of visibles we are being offered is the right description of visual experience, before we can accept the conclusion.

In the work of Yaffe and Belot we find no good reason for accepting that the proposed ‘geometry of visibles’ is actually an accurate description of visual experience. Van Cleve provides a line of reasoning that can be seen as an attempt to justify such a definition. It does so by offering a reason for generalising to all triangles the identity claims that can be made about the visible angles and real angles of triangles formed by great arcs of spheres. This generalisation was made possible by appealing to the indistinguishability of all triangles from some such spherical triangle. I argued that there are ways of rejecting this generalising move, by rejecting the assumptions supporting the claim that visible angles of all triangles just are the visible angles of the spherical triangles.

I have suggested that the failure of these arguments was tied up with their aprioristic approach to solving the problem, and that a better strategy would involve some appeal to the phenomenology of visual experience, or other empirical data. This means that we cannot yet conclude that the conclusion of these arguments is incorrect, as there are philosophers who have attempted to provide a battery of arguments for this conclusion that make appeal to just such kinds of evidence. It is these arguments that I will consider in the next chapter.

## **Chapter 4 – Spherical Geometry – Phenomenological Arguments**

### **Introduction**

In the previous chapter I looked at a number of arguments, inspired by Reid, which aim to show *a priori* that the geometry of visual experience is spherical geometry. I suggested that the failure of these arguments was tied up with their aprioristic approach, and that a better strategy would involve some appeal to the phenomenal character of visual experience. In this chapter I will consider a number of arguments that have attempted to establish a spherical geometry for visual experience by taking such an approach. The arguments that will be discussed here are given by Robert French in his *The Geometry of Vision and the Mind-Body Problem* and his 'The Geometry of Visual Space', and considerations offered by Richard Bradshaw Angell in his 'The Geometry of Visibles'.<sup>115</sup> These arguments all involve some appeal to such phenomenological features to reach the conclusion that the geometry of visual experience is a spherical geometry. For the purposes of brevity, and following my practice in chapter 2, I will from now on refer to this as the *spherical thesis*. I will argue that only one form of this argument that is extant in the literature provides some compelling evidence for the spherical thesis.

In French and Angell's work there can be found broadly two different arguments that follow this phenomenological approach. The first kind of argument attempts to show in a quite straightforward way that the figures found in our visual field are most naturally described in terms of the propositions of spherical geometry. The second argument, due to French argues from the absence of certain distortions in the visual field to the conclusion that the geometry of visual experience is a spherical geometry. I will begin with the former argument.

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<sup>115</sup> R. E. French. *The Geometry of Vision and the Mind Body Problem* (New York: Peter Lang, 1987) and R. E. French. "The Geometry Of Visual Space" *Nous* 21 (1987): 115-133 and R. B. Angell. "The Geometry of Visibles" *Nous* 8 (1974): 87-177.

## **The Argument That Phenomena in the Visual Field Are Best Described By Propositions From Spherical Geometry**

The first of the two phenomenological based arguments for the spherical thesis turns on the general fit between the features that are present in visual experience, and a spherical geometry. The argument takes many forms. This is partly because different arguments focus on different propositions or groups of propositions from spherical geometry; some arguments focus upon propositions about angular size, some focus on propositions about straight lines.

Versions of this argument are to be found in both Angell's and French's papers. However, there is an important distinction to be drawn between the formulation of the argument given by Angell and the formulation given by French. Angell's argument relies on appeal to figures which are only possible in the kind of extensions of the visual field that a single eye is capable of furnishing. French's version of the argument is intended as a modification of Angell's to avoid some of the criticisms that can be levelled against Angell's argument arising from this point. I will begin with Angell's version of the argument.

### **Angell's Version**

As has been said, Angell's argument in favour of the spherical thesis appeals to the phenomenal character of visual experience: he lists seven features of a spherical geometry and argues that these can be found, by means of introspection and measurement, to apply to the phenomenal character of visual experience. The argument makes no appeal to facts about the retina:

Were we physicalists, we might try to *explain* the geometry of visibles in terms of the geometry of impressions on the retina of the eyeball (spherical geometry). But the purely phenomenological account of the geometry of visibles given here in no way depends upon such an explanation: the non-Euclidean properties of actual visibles are

determined in a natural way which is quite independent of any such physical, or metaphysical explanations.<sup>116</sup>

Although Angell claims that features of spherical geometry apply to what he variously calls ‘visual appearances’ or, echoing Reid, just ‘visibles’, exactly how they apply is not straightforward. This is because Angell distinguishes between three different senses of ‘visual field’, which is the field containing the ‘visibles’. These three senses correspond to what might be called ‘levels’ at which something may be called ‘visible’. These three senses of ‘visual field’ are:

- 1) The momentary visual field.
- 2) The visual field extended over time.
- 3) The visual field spatially extended beyond the momentary visual field.

The momentary visual field is defined as a two-dimensional continuum:

The term ‘visual field’ refers to the two-dimensional continuum which contains visibles. Intuitively, the momentary visual field of a given observer is the two-dimensional expanse of visual colors and shapes which the observer can be aware of at that moment.<sup>117</sup>

Each of the two senses of ‘visual field’ that are extensions to the momentary visual field are introduced to satisfy the need to account for certain features of visual experience. The first is the possibility of changes in location in the visual field over time, for this reason Angell introduces the sense of a visual field extended over time:

But we shall also want to speak, eventually, of motions and changes as well as fixity among the visual objects in the visual field. We must therefore think of the visual field of a given person as persisting through time and containing or having as members, a series of the observer’s momentary visual fields.<sup>118</sup>

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<sup>116</sup> R. B. Angell. "The Geometry of Visibles" *Nous* 8 (1974): 90

<sup>117</sup> *Ibid.*, p. 91

<sup>118</sup> *Loc. Cit.*

The second extension of the sense of 'visual field' beyond the momentary visual field seems to concern the need to account for the sense that the visual figures in the momentary visual field can be continued beyond the boundaries of the visual field:

But, also, we shall want to speak of a given person's visual field as more extended spatially than any momentary visual field is in fact. In following a line with the eye (e.g. scanning the horizon), we take portions of the line previously scanned but no longer in the momentary field to be continuous with the portions later scanned. We will therefore speak of a person's total visual field as that expanse which includes all possible continuous extensions of lines or regions in his momentary visual field.<sup>119</sup>

This kind of extension implicitly allows the eye the possibility of rotation in any direction.

It is this 'total visual field' to which Angell claims the sample features of spherical geometry apply. These features are very similar to the sample of the geometry of visibles that Reid proposed, which were intended to 'lead the reader to a clear and distinct conception of the figure and extension which is presented to the mind by vision.'<sup>120</sup> In Angell's argument they are given pride of place and are appealed to directly. They are<sup>121</sup>:

- 1) A straight line can be a circle.
- 2) No straight line is infinitely extendible.
- 3) Every pair of straight lines intersects at two points.
- 4) Two straight lines, cut by a third line perpendicular to both, always intersect.
- 5) All equilateral triangles do not have the same interior angles.

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<sup>119</sup> Loc. Cit.

<sup>120</sup> Thomas Reid. *An inquiry into the human mind, on the principles of common sense: a critical edition*, edited by D. R. Brookes. (Edinburgh: Edinburgh University Press, 1997), p.105

<sup>121</sup> R. B. Angell. "The Geometry of Visibles" *Nous* 8 (1974): 95

- 6) The sum of the interior angles of a triangle is always greater than two right angles.
- 7) The four angles of a rectangle are always larger than right angles.

There is a significant difference between the first four and the final three of these propositions in how Angell demonstrates them to hold of the visual field. The first four rely on the disposition of visible elements outside the momentary visual field, whereas propositions (5), (6) and (7) do not. However, these three propositions do rely on some ability to measure the angles of the visible figures. Both of these issues leave room for objecting to Angell's argument for the spherical thesis.

I will begin by looking at the propositions that rely on the ability to measure the internal angles of visible figures. Angell describes a measuring instrument that is intended to provide for the 'objective corrigibility of judgments about the geometrical relations among visibles.'<sup>122</sup> Here is his description of the measuring instrument:

Take a stick 14.35 inches long, attach a six inch metal strip marked off in quarter inches to one end of it, and bend the metal strip so that each point on it is equidistant from the free end of the stick. When the free end is placed just below the eye, the quarter-inch marks on the metal strip at the other end each mark off just one degree (or  $1/360^{\text{th}}$  of a complete horizon) of visual distance.<sup>123</sup>

This allows the possibility of measuring the visual length of lines in terms of the radians subtended by the physical object at the eye – this is in keeping with the notion of 'visible length' employed by Reid *et al*, as seen in the preceding section. However, further adaptations are needed to measure visible angles:

For objective measurement of visible *angles* among visibles (e.g., for measuring the actual size of visible angles in the trapezoidal visible which is produced by the square table-top when viewed from one side), it suffices to attach a protractor perpendicularly to the same stick, with its center at the end where the metal strip is attached. When this device

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<sup>122</sup> Ibid., p. 92

<sup>123</sup> Ibid., p. 93

is held to the eye and the angles in the protractor are aligned with the angles in the visible, an accurate, objective measure of the angles in the visibles is provided.<sup>124</sup>

There is a need for a measuring device to determine the geometry of visibles because, as we observed in chapter 1 and as Van Cleve points out, if a figure is small enough to be seen in a single momentary visual field then it will not be noticeably non-Euclidean:

Angell is on safer ground, it seems to me, insofar as he rests his case on the measurement of visible figures that we *can* take in at a single view – for example, triangles and quadrilaterals that take up only a small portion of the visual field. Such figures are not noticeably non-Euclidean.<sup>125</sup>

Rough estimates of the size of visible angles will not be enough to guarantee that the geometry is a spherical geometry. This is because where the spherical figures are small, the divergence of the angular sum from that of Euclidean figures will be small. For this reason, Angell's device is in principle a useful one.

However, despite being a useful device in principle, there is an attendant problem concerning the reliability of such measurements. Van Cleve illustrates this problem in the following passage:

I find myself that I cannot measure small visible figures with any precision. Too much depends upon how I hold the protractor or how I cock my head – problems that do not affect the measurement of stable lines on paper – and I seldom get the same result twice. So far as I can tell by measurement, visible triangles might contain 180 degrees, or 175 or 185.<sup>126</sup>

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<sup>124</sup> Loc. Cit.

<sup>125</sup> James Van Cleve. "Thomas Reid's Geometry of Visibles" *The Philosophical Review* 111 (2002):

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<sup>126</sup> Ibid., p. 389



I take Van Cleve to be correct in respect of measuring small visibles where the difference between Euclidean and spherical figures is very fine. Here accuracy of measurement is crucial. However, as we will see when we come to Robert French's discussion, it is not obvious that the only figures that can be taken in at once are such that their non-Euclidean character will always be unnoticeable. Regarding the present point, however, Van Cleve's point stands.

Turning now to the first four propositions, in order to establish these (and in an informal way, also the last three propositions) Angell relies on the disposition of the elements of the visual field beyond the single momentary visual field. So, for instance, he argues for the proposition that in the visual field a straight line can be a circle by pointing out that the horizon is such a line:

... i.e., a visual straight line can be a closed line with all points on it equidistant from a polar point in the visual field. Consider the horizon, with a point directly overhead as its center.<sup>127</sup>

For propositions (3) and (4), which both concern pairs of straight lines intersecting at two points, even when both are intersected by a third line perpendicular to both, Angell appeals to an extension of the familiar example of the appearance of railway lines retreating from the observer:

Imagine standing in the middle of a straight railroad track on a vast plane. The *visual* lines associated with the two rails are demonstrably visually straight in every segment – they appear perfectly straight, not curved, visually. Yet these visually straight lines meet at two points which are opposite each other on the horizon, and they enclose a substantial region on the visual field.<sup>128</sup>

It should be clear that the demonstration of these propositions assumes that we are talking about Angell's 'total visual field', which includes extensions beyond the momentary visual field. The dispositions of visual lines is determined, as we have

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<sup>127</sup> R. B. Angell. "The Geometry of Visibles" *Nous* 8 (1974): 95

<sup>128</sup> *Ibid.*, p. 95

seen, by the possibility of the rotation of the eye to enable it to scan physical lines and take previously scanned portions as continuous with those presently in the visual field.

There is an obvious problem attending this way of establishing any propositions about visual experience, which concerns this seemingly counterintuitive way of thinking about the visual field. It requires you to accept that there are visible lines that cannot be seen, but which nonetheless intersect 'behind' the observer. Van Cleve expresses the strangeness of this admirably:

As developed so far, Angell's geometry is a geometry of appearances that never appear.<sup>129</sup>

Van Cleve thinks that this presents an insuperable problem for Angell. Non-appearing appearances may be just about accepted, Van Cleve reasons, provided that the appearances nonetheless exist. For this, Angell needs to offer some principled way of justifying the existence of such a 'total visual field':

... he needs a principle assuring us that certain total appearances exist even though nothing more than various alleged parts of them are ever given to us at once. It would have to be a principle allowing us to identify extensions of the lines that meet at the horizon with extensions of the lines that cross the tie at our feet. I am skeptical whether there is any acceptable principle that will fit the bill.<sup>130</sup>

For this reason, Van Cleve thinks that it would be strategically more sound to focus on figures that can be seen all at once in a momentary visual field.

However, there are two distinct issues here: first a question about how to justify the *existence* of extensions to the momentary visual field; second, once their existence has been granted, there is a separate question about whether Angell's characterisation of the 'total visual field' is the correct one. Whilst I share Van Cleve's scepticism about whether Angell is warranted regarding the latter issue, there is a line of reasoning that can be employed to justify the *existence* of a field of visual

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<sup>129</sup> James Van Cleve. "Thomas Reid's Geometry of Visibles" *The Philosophical Review* 111 (2002):

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<sup>130</sup> Ibid., p. 388

experience that goes beyond the momentary, where that is specified simply by the light cone entering the eye. The line of reasoning holds that it is a mistake to think of the visual field, in the sense of the field of visual experience, as entirely determined by the kind of information that a single eye could in a moment be aware of. This is a line of reasoning that we have already encountered in chapter 3 in connection with Van Cleve's argument for the equivalency of visible angles and spherical angles. There I argued that visual indistinguishability in monocular cases does not warrant generalised claims about visual experiences: in particular about visible angles.

This line of reasoning is exactly analogous to that employed by Smith in his rebuttal of an objection by Berkeley that vision does not 'originally' afford phenomenal awareness of depth. There the question is whether visual cues, such as the convergence of the eyes or lens accommodation, could be possible grounds for the three-dimensionality of vision. These have been rejected by a number of theorists as good grounds for phenomenal three-dimensionality because 'they were taken to be themselves either merely elements in a two-dimensional visual scene, not properly visual, or not consciously registered at all.'<sup>131</sup> Smith argues that this ignores the quite plausible idea that pre-conscious processes can result in an experience that is three-dimensional, but not as a result of operations on some already conscious experience:

This, however, ignores, somewhat surprisingly, a possibility that today suggests itself to us quite naturally: that pre-conscious processes can extract three-dimensional information from what is given to the eye, and can issue, as their *first* conscious upshot, in phenomenally three-dimensional experience.<sup>132</sup>

This line of reasoning can be applied in principle to Angell's claim of the existence of a field of visual experience that goes beyond the momentary. The idea is that the first conscious upshot of the eyes scanning lines as it rotates is an experience of a line that goes beyond what one is or could be aware of in a momentary visual field.

Whilst this line of reasoning provides a principled justification for accepting the existence of extensions to a momentary visual field, as I have observed it does not,

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<sup>131</sup> A. D. Smith. "Space and Sight" *Mind* 109 (2000): 491-492

<sup>132</sup> *Ibid.*, p. 492

warrant Angell's conclusions about the geometry of such appearances. Moreover, I think Angell's conclusions on this separate issue are questionable for the following three reasons: firstly, Angell requires for some of his propositions the ability to rotate the eye beyond our contingent physical limits. For instance, his demonstration of proposition (2), that no line is infinitely extendible, relies on the ability to rotate the eye 360 degrees:

If we extend any straight line segment in the visual field, it eventually returns on itself. It is thus finite, though unbounded. Again, consider the horizon.<sup>133</sup>

This is perhaps not a devastating objection, as one may consider sufficient for Angell's purposes the truth of the subjunctive conditional that if one were able to rotate the eye beyond its actual limits, the figures Angell describes would be seen. I do not find this very compelling, but as we shall see when we discuss French's treatment of the evidence relating to phenomenal character, there is something in this idea.

The second problem is that there is nothing about the phenomenal character of the momentary visual field that guarantees that the extensions to it will result in the kinds of figures that Angell has in mind. Consider the example of the railway lines; what reason do we have for not supposing that beyond the edges of my visual field such lines veer off to form hyperbolas? We could appeal to the fact that rotation of the eye, together with the assumption of the absence of depth perception makes this very unlikely. The problem is that this implicitly assumes something further about how the information from the eye is processed – i.e. that the scanning process is itself processed by means of some constant function. There may be contingent reasons for thinking that this is that case, but to appeal to this is to be no longer appealing just to the phenomenal character of visual experience.

The final, most problematic reason why this line of argument does not secure Angell's conclusions about the geometry of such appearances is that Angell's restriction to the monocular case and his decision to only allow the rotation of the eye is arbitrary. If we are to allow that the visual field extended beyond the momentary

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<sup>133</sup> R. B. Angell. "The Geometry of Visibles" *Nous* 8 (1974): 95

visual field can be the first conscious upshot of pre-conscious processing of eye movement, then why consider only rotational eye movement as relevant to the geometry of appearances? If we are prepared to allow rotation of the eye, why not also allow translation, or for that matter stereopsis? Surely in normal visual experience these things pre-consciously also contribute to the determination of the geometrical character of the 'first conscious upshot', as Smith puts it? As we will see in the next chapter, there is some evidence to suggest that when we add a second eye and certain limited kinds of movement, the geometry of visual experience is a hyperbolic geometry.

From the preceding it seems clear that Angell's attempt to establish the spherical thesis is not successful. His appeal to features of a visual field extended beyond the momentary visual field is not, as Van Cleve suggests, without a principled justification. However, once one concedes the principle I have suggested, which warrants talking about features of a visual field beyond what a single eye can take in at once, we see that Angell has specified his assumptions in a way that prejudices the propositions he aims to establish.

## **French's Version**

I will now turn to French's discussion of the phenomenological considerations that support the spherical thesis. French offers two arguments in favour of the spherical thesis, both of which are based on phenomenological considerations. The first that we will look at is just a modification of the phenomenological considerations we have just looked at from Angell. The second is a more sophisticated argument, based on the absence of marginal distortions in the visual field. I will argue that while these considerations are compelling evidence for a spherical metric, there are responses available that can block the conclusion they *prima facie* suggest. I will begin by looking at French's modification of Angell's argument.

As we saw in the previous section, Angell cited as evidence for the spherical thesis a number of features of 'visibles' that extend beyond the momentary visual field. Van Cleve objected to this extension beyond the momentary visual field. French, although less concerned about the metaphysical propriety of appearances that do not

appear, nonetheless concurs with Van Cleve's estimation that it is strategically more sound to consider just figures that can be seen all at once:

If by 'visual space' Angell is referring to phenomenal visual space itself, then many of his examples are not very satisfactory for the purpose of establishing that this space possesses a spherical metric structure, inasmuch as they cover too large of an area for them to be seen all at once, and since head movement is thus required to take in the complete effects.... However, inasmuch as in this chapter I am only concerned with the determination of the metric structure of a purely phenomenal visual space, and not with extensions of that space to a space of 'potentialities,' it would seem to be a valid criticism of many of Angell's examples with respect to this space, that the field of view of vision is too narrow for them to take place there.<sup>134</sup>

French argues that in spite of this restriction to the momentary visual field, a number of the propositions of spherical geometry can be shown to hold. He briefly considers that the absence of similar figures of different sizes in spherical geometry can be shown to hold of the momentary visual field. Presumably this relies on the measurement of angles, on which he also relies for the claim that we can test for the angular sums of figures in the visual field:

However, the axioms of spherical geometry which do not require the existence of a complete sphere in order to hold, such as the absence of similar figures of different sizes, and the sums of the angles of figures being greater than their Euclidean counterparts, can be tested for in visual space.<sup>135</sup>

As we have already seen, Van Cleve has questioned the reliability of such claims that require measurement as a basis for the geometry of visual experience. However, a more compelling consideration French raises concerns the convergence of

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<sup>134</sup> R. E. French *The Geometry of Vision and the Mind Body Problem* (New York: Peter Lang. 1987), pp. 87-88

<sup>135</sup> Ibid., p. 88

parallel lines in spherical geometry. Angell argued that railway lines, seen retreating into the distance from just above ground level, furnish an example of straight lines in the visual field that meet at two points, one in front and one behind, and enclose a portion of space. This naturally relies on Angell's extension of the momentary visual field.

French argues that really the relevant feature of Angell's example is the convergence of the lines: it is not necessary in order to confirm the spherical thesis that the lines in the visual field actually meet. This is because in spherical geometry, parallel lines would be expected to converge:

Inasmuch as Euclidean geometry holds that parallel lines are always equidistant apart, and hyperbolic geometries that they tend to diverge, any such convergence would constitute evidence that visual space possesses a spherical metric structure.<sup>136</sup>

So, rather than considering parallel lines seen retreating from the observer at ground level, French proposes that we consider parallel lines seen lying on a plane perpendicular to the lines of sight, such as parallel lines on the ground when seen from above:

It is possible to amend Angell's example of the converging railroad tracks so that the effect can be taken in all at once, by considering the case of viewing a pair of tracks from a position directly overhead, and then seeing whether both ends tend to converge, even though one's field of view is not sufficiently wide in order for both ends to converge to actual points.<sup>137</sup>

French cites some cases which confirm the convergence of parallel lines in the visual field. The first is that when one observes a long, low lying building 'head-on from a position around half-way along its length', the outline of the top and bottom of the building will appear wider at the centre than at the edges. The second and third

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<sup>136</sup> Ibid., p. 89

<sup>137</sup> Loc. Cit.

cases French cites are examples from Helmholtz's *Physiological Optics*. The first of these is just a repetition of French's own example, but using a strip of paper:

If a long strip of paper, with parallel edges about three inches apart, is laid on the top of the same table, it will be noticed, on looking at the middle of it, that by indirect vision it appears to be narrower at the ends than at the middle, and that it is apparently bounded by two arcs with their concavities toward each other. In short stretches of straight lines the apparent curvature is generally not noticed, because we are disposed to regard and interpret them as being straight lines on material objects rather than as being great circles in the field of view.<sup>138</sup>

The final example from Helmholtz illustrates this convergence effect by means of its distorting effect on curved lines which bend away from each other. When viewed at the right distance with one eye closed, such lines can appear straight. This effect can be tested using Helmholtz's checkerboard, which consist of black and white checkers formed by such curved lines, such that those at the edge are increasingly unlike squares. Below is a picture of Helmholtz's checkerboard:<sup>139</sup>

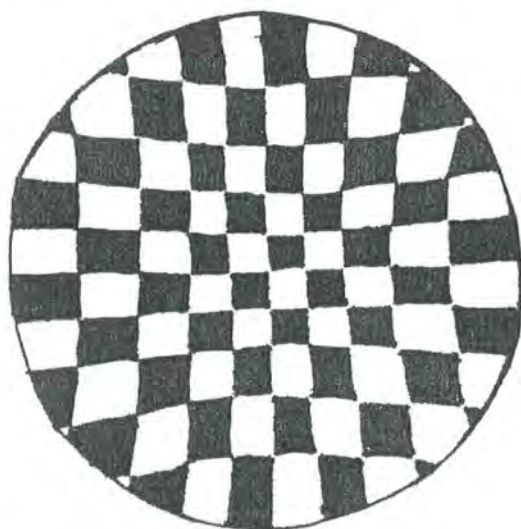


Figure 1 - Helmholtz's Checkerboard

<sup>138</sup> Herman Von Helmholtz. *Treatise on Physiological Optics*. Vol. 3 (New York: Dover Publications, 1962) p. 183.

<sup>139</sup> Ibid., p. 181.



These are the phenomenological considerations that French offers in support of the spherical thesis. There is an obvious enough initial objection to the evidence discussed, which also applies to the considerations Angell offered concerning the extended visual field. This is to accept that the elements, such as lines, angles, etc, converge or sum to more than 180 degrees; however, it can then be denied that the lines constituting the figure are straight. This leaves open the possibility that visual space possesses a Euclidean geometry:

... it is possible to raise an objection to virtually all (a notable exception is the evidence from marginal distortion) of the evidence cited in this chapter as favoring a spherical metric for visual space, by questioning whether the lines involved in the effect are actually seen as being straight. If these lines were not in fact straight, one could claim that visual space still possesses a Euclidean metric structure, with the effects involved, such as "parallel lines" converging in both directions, being due to these lines being curved.<sup>140</sup>

French concedes that this is a possible objection to the evidence he gives and even concedes that there appears to be a 'primitive' notion of straightness we use to judge in these cases and in cases of illusion, although he does not specify in what sense this notion is 'primitive'.

...indeed there does appear to be a primitive visual sense of straightness, by which, for example, we tell in certain optical illusions, such as the Hering illusion, that physical straight lines are not visually straight.<sup>141</sup>

However, French thinks it adequate to say in response to this that although those visual lines correlated with physical straight lines may not satisfy our primitive notion of straightness, there is an explanation of this fact, which is intended to compete with the above Euclidean explanation of the phenomenal character. This is

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<sup>140</sup> R. B. Angell. "The Geometry of Visibles" *Nous* 8 (1974): p. 92

<sup>141</sup> R. E. French *The Geometry of Vision and the Mind Body Problem* (New York: Peter Lang. 1987), p. 75

simply that if visual experience has a spherical geometry then we would be able to claim that such lines as do not correspond with our ‘primitive’ notion of straightness *are* straight, in the sense that they are geodesics – they are the straightest possible in visual experience.

It is possible to find counterexamples to this kind of response; one just needs to find cases of lines that French claims are ‘the straightest possible’ in the visual field and show that we can find lines (in visual experience) that we would quite naturally want to say are straighter. Such lines are not hard to find. Consider again the case of the railway lines seen from above that appear to converge at the edge of the visual field. If we take two points on each of those physical lines, such that when seen they correspond to visual points located somewhere near the edge of the visual field. Presumably two gently curved, physical arcs can be made to connect these pairs of points, on the same line, such that the effect of the distortion that makes the parallel lines appear to converge will make the arcs appear straight. These two visual lines correlated with the arcs will be equidistant at all points, and so could count as evidence for a Euclidean geometry. The converging visual lines that are correlated with the physical, straight lines may be geodesics on the surface of the retina, but that need not mean that they are the straightest possible in visual experience.

This attempt to explain away French’s phenomenological considerations in a way that is consistent with the Euclidean thesis does not come without a cost, however. One of the implications of it is that visual straight lines never correspond to physical straight lines. Indeed, if the argument is right, then visual straight lines will systematically correspond with physical arcs. This is quite the opposite of the explicit assumption French makes at the outset of his *The Geometry of Vision and the Mind-Body Problem*: he assumes ‘by convention’ that visual straight lines are always correlated with physical straight lines.

Thus, unless there is some *prima facie* evidence to the contrary... that physical straight lines are not constituted in visual space as straight lines also, I shall assume by convention that the two are in fact correlated.<sup>142</sup>

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<sup>142</sup> Loc. Cit.

As French has already mentioned illusions as special cases, it seems reasonable to conclude that the *prima facie* evidence should concern veridical cases where there is no correspondence of straight lines.

In the case of a spherical metric, French's assumption can be motivated by appeal to the shape of the retina: projections of physical straight lines onto a sphere are always geodesics on the surface of a sphere – and the eye is roughly hemispherical. In the case of an advocate of a Euclidean geometry, though, there seems to be no immediately obvious reason why he must make this assumption. Moreover, there are considerations that tell against the assumption that visual straight lines always correspond with physical straight lines. These considerations turn on the observation that, in respect of other geometrical properties, visual lines sometimes systematically fail to correspond with physical straight lines.

One such example of this failure to correspond is the most commonly cited cases of parallelism: railroad tracks seen retreating from the observer from just above ground level. The visual lines by means of which we see such parallel railroad tracks quite obviously intersect, have different directions, and are not equidistant at all points. This implies that, as a general point, visual lines need not correspond with physical lines in respect of their geometrical properties. This provides *prima facie* evidence that is sufficiently general to call into question French's assumption that, at least in standard cases, there is the correspondence.

However, before rejecting French's assumption we must consider the potential cost of doing so. To reject it would certainly leave room to claim that the geometry of visual experience is Euclidean. However, if the interest in defending this claim is to avoid the argument from illusion, then rejecting French's assumption may be equally problematic, as we will be exchanging one case of a systematic divergence of physical properties and apparent properties with another. This is because, as has been noted, the property of being curved will systematically apply to physical lines wherever being straight applies to their appearances.

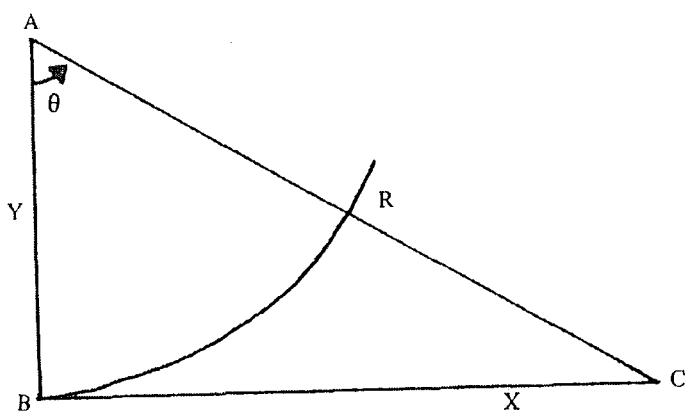
It seems that in light of these points, there is some compelling phenomenological evidence supporting the spherical thesis. An assessment of how compelling such evidence ultimately is will need to be postponed until we have considered the evidence for a hyperbolic metric, however. Before we turn to that, though, there is one final argument French offers in favour of the spherical thesis.

## **French's Argument From Marginal Distortions**

I said that the phenomenological arguments in the literature can be divided into those that direct attention toward figures in the visual field, and one particular argument that considers the absence of certain features of the visual field. French offers two variations of this latter kind of argument, which takes as its data the absence of distortions at the margins of the momentary visual field. The first of these variants appeals to facts about the retina, facts about wide angle projections onto planes and spheres, and the phenomenal character of visual experience. The second argument, intended as a response to objections that may be moved against the first argument, appeals just to the facts about wide angle projections and the phenomenal character of visual experience. I will begin my discussion with the first variation of the argument.

The first move of the argument is the observation that the field of view of the eye is quite wide, particularly horizontally. The next point is the observation that wide angle projections onto planes produce distortions the further one gets from the central point of projection on the surface of projection. By contrast, wide angle projections onto the interior of the surface of spheres do not produce any such distortions.

To get an idea of the kind of effect being appealed to here, it is helpful to think about the kinds of distortions that can be observed in wide angle photography, where the surface of projection is a flat photographic film. The extent of the distortion is directly proportional to the distance from the central point of projection: the point at which the line of projection is perpendicular to the surface of projection. The further from this point one goes, the more severe the distortion. In the case of a flat surface of projection, the reason for this distortion is that as one moves further away from the central point, the lines of projection intersect the surface at increasingly acute angles. The absence of such distortions in the case of projections onto the interior of a sphere is because the lines of projection intersect the sphere at equal angles, wherever they fall on the sphere. French offers the following illustration of this fact:



**Figure 2 - French's illustration of why marginal distortions do not arise for projections onto sections of spheres**

This can be given mathematical expression by saying that for projections onto spheres the ratio  $dx/d\theta$  is constant, whereas for projections onto planes the ratio is not constant and given by:  $dx/d\theta \propto \sec \theta$ .

Finally, the argument appeals to the observation that there are no marginal distortions in visual experience. This puts us in a position to provide an argument for the spherical thesis.

... the fact that these [marginal] distortions are not present in visual space would seem to constitute strong evidence that that space possesses the same metric structure as that of the surface of a sphere; i.e., a spherical metric structure.<sup>143</sup>

However, whilst it is clear what considerations French's argument appeals to in aiming to establish its conclusion, it is not as clear just what the structure of the argument is. One thing that is clear, though, is that the argument cannot work as a deductive argument. Here is a possible deductive argument, with a claim about the metric structure of visual experience as its conclusion, which employs the considerations discussed above:

- (1) When an image is projected onto a sphere, equal areas of the sphere subtend equal solid angles. The ratio  $dx/d\theta$  is constant.

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<sup>143</sup> Ibid., p. 85

- (2) When an image is projected onto a plane, equal areas of the plane do not subtend equal solid angles. The ratio  $dx/d\theta$  is proportional to  $\sec\theta$ .
- (3) A consequence of this is that marginal distortions will occur in the peripheral regions of a wide-angle projection onto a plane, but not in a projection onto a sphere.
- (4) The field of vision of the eye is very wide ( $170^\circ$ ).
- (5) There are no marginal distortions in visual experience.
- (6)  $\therefore$  Visual experience possesses the same metric as that of the surface of a sphere.

This argument cannot work because, as stated, it is unsound. All that we are justified in concluding at (6) is that the projection of physical objects onto the retina is a projection onto a (hemi) sphere and that there are no marginal distortions in the image on the surface of the retina. For the purpose of establishing the geometry of visual experience this is an uninteresting conclusion.

Premises (1)-(3) just represent the facts about the projective properties of objects that have just been discussed, so they need no comment. Premise (4) is a fact about the eye. At this point the argument looks like it should proceed to a conclusion about the properties of the projected image on the retina. Instead, the argument introduces a premise about a feature of visual experience: the absence of marginal distortions. It is not clear how we are supposed to get from this to the conclusion (6). The premise that there are no marginal distortions in visual experience, premise (5), is unhelpful because no connection has been established between the features of the image on the retina and the features of visual experience. It is the properties of visual experience that we are investigating, not the properties of the retinal surface.

If we supply an additional premise that visual experience is identical to the image projected onto the retina, then the argument becomes sound, but bad. It is just not true that visual experience is the image on the retina: an eyeball that has been detached from the brain may still be photosensitive for a time, but will not possess a visual field, despite having a retina. The retina is certainly causally connected with our visual experience, but is not identical to it. French is entirely sensitive to this point and a similar one made by J. J. Gibson:

The argument, from the fact that the retina is curved like a sphere to the conclusion that visual space possesses a similar curvature, is inconclusive, inasmuch as it would be possible to map the retina onto a Euclidean planar surface....<sup>144</sup>

... as J. J. Gibson argues, it is relatively unimportant that the retinal image actually be an image, due to the role of eye movements in the retina's picking up of information from the optical array impinging on it....<sup>145</sup>

It seems fair to say that French does not conceive of his argument as being deductive; rather, it seems to have the character of an abductive argument. The evidence French appeals to can be put into an argument that has the following structure:

- (i) There are no marginal distortions in visual experience.
- (ii) There are no marginal distortions in projections onto the surface of a sphere.
- (iii) There is a dependency of the metric properties of visual experience upon the metric properties of the surface of the retina.
- (iv) The retina is (roughly) hemispherical.
- (v) The absence of marginal distortions in visual experience is best explained by the hypothesis that visual experience has a spherical metric.

An explanation of this kind certainly works, but I am dubious about how powerful an argument it is for the spherical thesis. I will try to show why by analyzing the explanation.

Step (i) is the datum to be explained. Step (ii) points to an interesting parallel between projections onto the surface of a sphere and a particular property of visual experience. At this point, though, no substantive reason has been given for why datum (i) is in need of explanation – why is it such an odd result?

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<sup>144</sup> Ibid., p. 84

<sup>145</sup> Ibid., p. 87

Naturally, the answer lies in the fact that marginal distortions do occur in projections onto a plane. When this is coupled with the dependency stated in step (iii) the need for the explanation becomes apparent. If there were marginal distortions in visual experience then they would depend upon marginal distortions in the image on the surface of the retina. The existence of such marginal distortions on the retina would mean that the retina was flat. It is not. So the conundrum would be: if visual experience is Euclidean then why are there no marginal distortions? The obvious response to make, as French does, is to dispense with the idea that visual experience has a Euclidean metric. This is supported by the dependency of the metric of visual experience on the metric of the surface of the retina (Step iii) and the hemispherical shape of the retina (Step iv).

The weak point in this explanation is the claim about the dependency of the metric of visual experience upon the metric of the surface of the retina. This step is suspicious because (iii) looks just like a statement of a principle that warrants the inference directly from (metric) facts about the retina to (metric) facts about visual experience. In order to assess (iii) we need a more precise sense for the dependency relation it is intended to express. So, what can be said about this dependency relation? Well, this much is obvious:

(C) Visual experience ordinarily stands in a causal relationship to the image produced by the light rays falling on the surface of the retina.

This is undoubtedly true, but not helpful. At the most general level, there appear to be two different kinds of sense that could be expressed by (iii):

(I) The dependency relation implies the identity of metrics.

(NI) The dependency relation does not imply the identity of metrics.

To work as an inference to the best possible explanation, this first variation of French's argument from the absence of marginal distortions requires the assumption of (I). One problem with assuming (I), which French accepts, is that Gibson's claim that the retinal image is relatively unimportant, because the visual system picks up information from eye movement and other cues such as those discussed earlier in



connection with Van Cleve's criticisms of Angell, actually provides a reason for rejecting (I) in favour of (NI).

French is aware that this reliance on appeals to the shape of the retina is a point of weakness for this argument. It is for this reason that he offers his second variation of the argument, which cuts out any appeal to facts about the retina. In doing so, French aims to undercut the Gibsonian line of criticism. This alternative variant of the argument from marginal distortions just appeals to the absence of such distortions in visual experience, as compared with photographs:

In fact, the preceding argument can be made without even appealing to the shape of the retina, since phenomenal visual space itself can be compared with the photographic image of a scene constituted in it, and the fact that the marginal distortions present in the photographs of this scene are not present when it is constituted in visual space will be strong evidence that visual space possesses a spherical metric.<sup>146</sup>

Whilst it is plausible that cutting out any appeal to the shape of the retina avoids the Gibsonian objection, it is no longer clear just what French's argument is. How is a straightforward appeal to the qualitative differences between the phenomenal character of a visual experience and a wide angle photograph supposed to recommend a spherical geometry for the visual experience? Presumably we must bring in facts about projections onto planes and onto spheres. This does not, however, require us to appeal to the actual shape of the retina. The qualitative features of the photograph are predicted, indeed determined, by the facts about wide angle projections onto a plane. Equally, qualitative features of the momentary visual field, such as the absence of marginal distortions, are predicted by the facts about projections onto spheres. It is then proposed that the best explanation of the absence of marginal distortions is that visual experience has a spherical metric. The argument has the following kind of structure:

- (i) Photographs and visual experiences differ in that there are no marginal distortions in visual experience, but there are in photographs.

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<sup>146</sup> Loc Cit.

- (ii) There are no marginal distortions in projections onto the surface of a sphere.
- (iii) There are marginal distortions in projections onto flat surfaces.
- (iv) The absence of marginal distortions in visual experience is best explained by the hypothesis that visual experience has a spherical metric.

The problem with this argument is that it is perfectly legitimate to respond that even though the fact that in visual experience there is an absence of marginal distortions lines up with facts about projections onto spheres, it is nonetheless contestable that the proposed geometry of the experience need enter the explanation at all. Gibson's point that the visual system can pick up information other than that provided by the image on the retina makes space for this possibility, just in the same way that it did in connection with Van Cleve's objections to extending the momentary visual field. The idea is that the first conscious upshot of transformations of the image on the retina by the visual system is an experience with a metric structure different from that of the sphere. Consequently, the geometry of the experience could still be other than a spherical geometry, in spite of the projective facts lining up with the phenomenal character of the experience in a way that naturally suggests them.

Given the kind of evidence that French generally wants to appeal to, he would be well served here if he had some way of showing that transformations of the pattern of retinal excitation could never result in a space with a geometry other than that suggested by comparing visual experience with photographs and reflecting on projective facts. In fact French does have the resources to construct an argument to this effect, but never really employs it to its full effect.

French's argument turns on a mathematical point, that if you have a continuous function that transforms a spherical image into a flat image you will get marginal distortions:

...any continuous mapping of the surface of a sphere onto a plane will involve marginal distortions, as in the case of the Mercator projection of the globe onto a flat map, and thus it is impossible to embed a

Cartesian coordinate system onto the surface of a sphere without affecting the metric relationships of these coordinates.<sup>147</sup>

A consequence of this is that it should be the case that if the visual system transforms the retinal image into an experience with a non-spherical metric structure, then there should be marginal distortions present. As French has observed, there are no such marginal distortions, so the experience that results from the operation of the visual system must have a spherical metric:

Thus it is impossible to map the optical projection on the curved retina onto a flat visual space without introducing marginal distortions, and therefore the fact that these distortions are not present in visual space constitutes very strong evidence that visual space is not flat, but is instead curved, at least approximately like the surface of a sphere.<sup>148</sup>

This argument, however, is not sufficient to eliminate the possibility that the transformation by the visual system of the retinal image results in a space with a different geometry than that of the sphere. This is because French's argument only concerns continuous mappings of the retinal image, and only concerns mappings of a single retinal image. It is a fact about the retina that the distribution of the light sensitive cones and rods is not equal on all parts of the retina – there is greater concentration in the fovea than elsewhere. This shows that the mapping of points of the retinal image onto the pattern of retinal excitation cannot be an injective mapping. There is no good reason, therefore, for supposing that the mapping must be continuous.

More importantly, there is no good reason for thinking that visual experience must be thought of as the result of a transformation of just a single retinal image. The point of Gibson's objection was that there are a number of different sources of information available to the visual system, such as binocular or motion parallax. There is no reason to suppose that the functional transformation of these disparate pieces of information could not have as their 'first conscious upshot' an experience with a different geometry from that of the sphere.

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<sup>147</sup> Ibid., p. 86

<sup>148</sup> Ibid., p. 87

## **Conclusion**

The line of argument I have considered in this chapter can be seen as an attempt to remedy the failings of the strategy considered in the previous chapter. This is an attempt to use phenomenological considerations to support a spherical geometry for visual experience. I argued that both Angell's argument concerning the geometry of the extended visual field and French's argument from the absence of marginal distortions can be blocked. In the case of Angell's extended visual field, this is because it was necessary to his conclusion that the only possible extensions of the visual field be the result of rotations of the eye. I argued that if we are prepared to allow such extensions to the visual field, then there is no reason to restrict the possible sources of information available to the visual system in this way. In the case of French's argument from the absence of marginal distortions, a similar point applies, if we accept that information other than that provided by a single retina can inform visual experience, then there is no reason to suppose that the geometry of visual experience can never diverge from that of the image on the retina.

There remains one piece of evidence that does suggest a spherical geometry, and this is the quite straightforward phenomenological evidence French discusses in his restricted treatment of the phenomena Angell discusses. The fact that physically straight parallel lines appear to converge at the edges quite naturally suggests a spherical geometry. Moreover, as we saw, there are difficulties for rejecting the correspondence of physical geodesics with visual geodesics that is involved in reaching this conclusion. What, then, are we to conclude? The first point to note is that the evidence turns only on phenomena observable in monocular experiences. So, whilst the considerations do offer some compelling support for a spherical geometry in the case of static monocular experiences, there remains a question about binocular experiences. In the next chapter I will turn to a discussion of theories that have claimed a hyperbolic geometry for visual experience.

## **Chapter 5 – Hyperbolic Geometry**

### **Introduction**

In the previous two chapters I argued that a number of proposed arguments for a spherical geometry need not be accepted. Part of the strategy for rejecting these arguments involved rejecting two salient assumptions of the approaches taken by proponents of those arguments. The first concerned the general strategy of focusing on static monocular considerations: the assumption that such considerations could provide an adequate account of visual experience. The second assumption underpinned that strategy: the assumption that vision does not as its first conscious upshot provide depth awareness.

However, after rejecting these assumptions we cannot automatically claim that the Euclidean thesis is correct. This is because there appears to be evidence to suggest that the geometry of binocular visual experience is a hyperbolic geometry. If this is correct then, in spite of the concession that visual experience originally provides awareness of depth, the Euclidean thesis will be false. If this is so then, in spite of all that has been said so far, we will not have avoided the argument from illusion based on geometry. Before we can claim that there is nothing about the underlying spatial structure of visual experience that puts pressure on Direct Realism, we need to assess the proposal of a hyperbolic geometry.

In this chapter I will discuss the various arguments that have been proposed for a hyperbolic geometry for visual experience. Historically, the proposal of a hyperbolic geometry emerged from the experimental work of the psychologist Rudolph K. Luneburg on the metric structure of binocular vision; consequently, his empirical work and the further refinements made by his followers constitute the first body of theory that will be considered. Luneburg argued that the results of experiments conducted in a dark room into binocular vision suggest that visual experience has a non-Euclidean hyperbolic geometry. The second body of theory that I will look at is the work of Patrick Heelan. Heelan has developed Luneburg's proposal of a hyperbolic metric for visual experience by proposing a hyperbolic model for visual experience outside the restricted experimental conditions of the dark

room and adducing evidence of various kinds in favour of it. This evidence comprises phenomenal character, explanatory power for the presence of certain kinds of visual illusion, and evidence from the history of art.

A discussion of these two bodies of theory presents some considerable exegetical challenges. Regarding the experimental work of Luneburg *et al*, it is not immediately obvious in what way the evidence presented is relevant to the question of the phenomenal character of visual experience. Consequently I will begin my discussion of Luneburg's line of argument by highlighting the reasons for taking these experiments to be of significance to the question of the geometry of visual experience. Regarding Heelan's work, on the other hand, his discussion takes place within a foundationalist epistemological project of establishing an epistemic warrant for scientific knowledge. He also considers the investigation into the geometry of visual experience to be 'hermeneutical' in character – by which he appears to mean that the metric structure of visual experience varies depending upon a number of factors. My aim here is to extract from Heelan's discussion the basic core of his argument for a hyperbolic geometry, so it can be evaluated independently of his methodological commitments.

### **Luneburg's Theory**

Before discussing Heelan's work, which in many ways is the most thoroughgoing attempt to motivate the hyperbolic thesis, I will discuss Rudolph Luneburg's theory. The reason for this is twofold: the first is exegetical; because Heelan's work is an extension of Luneburg's it will be helpful to see it in context. Secondly, recalling Suppes' complaint that was discussed in chapters 1 and 2, while Luneburg's theory has received considerable attention from experimental psychologists, who have criticised the accuracy of the early experiments and have rerun them, his theory has received very little critical attention from philosophers interested in this issue. What critical attention it has received is, I think, not very thorough.

I will begin by providing an exposition of the theory, with the following questions in mind: is it a genuine competitor to the claims, examined in the previous chapters, that have been made by philosophers about the geometry of visual

experience, or does it deal with an issue that does not generate the same philosophical interest? What is the significance of the fact that it is a theory of binocular vision? These questions serve the exegetical purpose of placing Luneburg's theory within the context of the wider philosophical debate. In this exposition I shall draw on the writings of Luneburg, A. A. Blank and a number of later contributors to the debate. I shall then present the arguments that are typically adduced in favour of this theory and consider two separate criticisms that have been offered by Richard Angell and Robert French, who, as we have seen, both endorse the spherical thesis. These criticisms aim to show that the arguments presented in favour of the theory are not conclusive. I argue that this general assessment of the argument is correct, but the reasons offered by these philosophers are not themselves adequate to their purpose. Finally I consider a criticism of Luneburg's theory that was made by J. J. Gibson.

## **The Theory and Its Relation To The Philosophical Theories**

Luneburg's theory that the metric of visual space is hyperbolic is one that is arrived at inductively from experimental evidence. The general structure of the argument is as follows: there are certain facts about the features of form and location in binocular experiences, given by experiments in a dark room and under specific conditions, which need accounting for within a general theoretical framework. Providing such an account consists of describing 'visual space' as a metric space. This is achieved by means of a transformation of a description in terms of bipolar coordinates, where each point represents an eye, of objects arrayed in the physical space surrounding an observer into a space described by a single polar coordinate system. This single polar coordinate system is supposed to represent the space of cyclopean visual experience: the binocular experience of the world as from a single point of view. Luneburg then offers a formula for determining the distance function between points in this visual space. The formula contains an undetermined, real valued constant  $K$ , which represents the curvature of visual space. The value of  $K$ , and consequently the metric of the space, can be tested for by measuring the divergence of the reports of parallelism and of equidistance found specifically in the Hillebrand and Blumenfeld alley experiments, which will be described later on in this chapter. It turns

out that these measurements, despite variations of the amount of curvature from subject to subject, support the conclusion that the metric is hyperbolic in character.

Owing to its empirical nature, there are certain peculiarities of the approach that need clarifying in order to properly place this theory in the context of the other philosophical theories concerning the geometry of visual experience. As I have mentioned, there is a concern about whether Luneburg's theory addresses the same issue as those philosophers we have looked at in previous chapters. The aim in the previous chapters was to provide an adequate description of the phenomenal character of visual experiences. Given that these theorists are mostly philosophers, and that the main proponents of and commentators on Luneburg's theory have been psychologists, it is therefore worthwhile highlighting the reasons for thinking that they are not discussing something different.

According to A. A. Blank, who was the main expositor and proponent of Luneburg's theory:

The ultimate objective of a theory of three-dimensional space perception is to state in some precise way what an observer really "sees" when he looks out on the physical world.<sup>149</sup>

The use of the inverted commas around 'sees' in this quote would suggest that the theory is supposed to describe the phenomenal character of visual experiences. The modification of 'sees' by 'really' suggests that he has in mind the same kind of contrast Reid had in mind when he asserted that the geometry of visibles is not what we might naively take it to be: just the geometry of the world.<sup>150</sup> Moreover, in Luneburg's paper 'The Metric of Binocular Visual Space', in the passage where he introduces the purpose of his theory, he frequently refers to 'visual sensations' as that which the theory is intended to describe. Occasionally what he writes comes very close to straightforwardly asserting Indirect Realism; for example, he says of visual sensations:

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<sup>149</sup> A. A. Blank. "The Luneburg Theory of Binocular Visual Space" *The Journal of the Optical Society of America* 43(9) (1953): 717

<sup>150</sup> See chapter 3, pp. 65-66.



They are the result of an activation of our mind by physical light stimuli and are distinguished by a remarkable degree of certainty and definiteness which tempts us to believe that the external world itself is revealed to us, *and not merely an image of our own making*.<sup>151</sup>

The implication here is that sensations, the images of our own making, are what we are immediately aware of, not the external world. It would appear, then, that Luneburg's theory and the arguments of previous chapters are in fact competing explanations of the same thing – the actual, but commonly unnoticed phenomenal character of visual experiences.

One peculiarity of Luneburg's theory that needs some preliminary comment is that it explicitly concerns the metric of *binocular* visual space. Although the preceding discussion of the arguments for a spherical geometry has provided some philosophical motivation for this, it is natural to ask why Luneburg's theory is concerned with binocular experiences. There seem to be two answers to this, one of which is explicitly given in his writings and one that can be extrapolated from the general comments he makes about the notion of 'visual space'. The first answer is that it appears from dark room experiments that form and localisation in monocular experiences are erratic. Presented with a number of point-like light stimuli from the same place in front of the subject, the reports about the apparent location vary. Luneburg writes:

There is a great difference between monocular and binocular vision under the artificial conditions just described. With one eye our judgment of spatial form and localization is erratic and inconsistent.<sup>152</sup>

Even if these reports are taken as reports of judgments about the location of the stimulus in the physical space surrounding the subject, they still imply a report on the phenomenal character of the experience. This is because the fact that the points appear to be in different places cannot be explained either by differences in the physical location of the source or differences in the locus of stimulation on the retinas:

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<sup>151</sup> Rudolph K. Luneburg, "The Metric of Visual Space," *The Journal of the Optical Society of America* 40(10) (1950): 627. Emphasis added.

<sup>152</sup> *Ibid.*, p. 628

the same results are obtained when these are kept constant. There is nothing left to appeal to, then, other than the difference between the phenomenal characters of the separate experiences.

The conclusion Luneburg draws from this feature of monocular experiences is that they do not have a constant structure in respect of spatial form and localization. On the other hand, binocular experiences under the same experimental conditions do result in greater consistency in the reports of form and localization. If monocular experiences do not have a constant spatial structure then it seems correct to take binocular experiences as the proper subject of an investigation into the metric of visual experiences.

One observation about Luneburg's line of reasoning on this point must be made: the experimental evidence really concerns the relationship between temporally distinct monocular experiences, i.e. between two different monocular experiences had by the same person. The subject is presented with one point-like light source and is then invited to comment on its apparent location; a few moments later he is presented with another light source and invited to comment on its location. There is nothing that shows it cannot be the case that each monocular experience does have its own spatial structure. All that is shown is that the deliverances of the first experience bear no relation to the deliverances of the subsequent experience. If this is right, then there remains logical space *in respect of this argument* for proponents of the spherical thesis to claim that the monocular case is the philosophically relevant one and that their arguments reveal the metric structure of individual monocular experiences.

The second, this time implicit, motivation for Luneburg's choice of binocular experiences as the subject of his investigation has to do with his conception of 'visual space'. In contrast with most proponents of the spherical thesis, the notion of 'visual space' Luneburg begins his analysis with is very broad. Those proponents of the spherical thesis who follow Reid, as we saw in chapters 3 and 4, tend to specify the visual field in terms of the relationship between a point of origin, associated with the eye, and the lines radiating out from it or the points lying on them. In their case, after the metric has been argued for, there remains a question about how this 'space' relates to the phenomenal character of what we are aware of visually after the brain has processed this: and after the 'fusion' of the information from the retina. Luneburg begins at the other end; he takes the full panoply of what we are visually aware of under normal conditions as the reference of 'visual space'. He conceives of this as a

‘three dimensional continuum, endowed with certain sensed qualities of color, brightness, form and localization.’<sup>153</sup> Some properties of the continuum, it is proposed, are partly determined by binocularity; so what he is investigating in his experiments is the contribution made by binocularity to this normal kind of visual experience. In particular, he is interested in the contribution made by binocularity to the properties of ‘form’ and ‘localization’ in such experiences. The contrast of the inconsistency exhibited by monocular experiences and the consistency exhibited by binocular experiences suggests that binocularity is what determines the underlying spatial structure of visual experiences. So, Luneburg’s choice of binocular experiences as the subject of his enquiry is partly the result of what he implicitly takes to be the correct phenomenological starting point for a description of what we are aware of visually. In his case, by contrast to taking the monocular case to be primitive, there will then be a question about how this relates to what is provided by the information coming from each individual eye. This question is one that people engaged in research into visual science have made some advances on in recent decades.<sup>154</sup>

### **The Experimental Evidence and Luneburg’s Argument For a Hyperbolic Metric**

Having provided a brief explanation of the general strategy of this approach and some of the contrasts between Luneburg’s theory and the theories discussed in previous chapters, I will now turn to the core argument that is generally used to support Luneburg’s theory. This argument appeals to evidence generated from the alley experiments conducted by Hillebrand and by Blumenfeld: the ‘parallel alleys’ experiment and the ‘equidistant alleys’ experiment. These were experiments performed in a darkened room, where the subject has the freedom to move his eyes but his head remains fixed in place. The purpose of these conditions is to focus on the contribution of binocularity to visual experience by eliminating as many other perceptual cues as possible, such as textural cues and motion parallax. This deliberate restriction of the perceptual cues available to the subject is a consequence of

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<sup>153</sup> Ibid., p. 627

<sup>154</sup> For a comprehensive discussion of the strategies employed, see B. Bruce and P. Green. *Visual Perception: Physiology, Psychology and Ecology*, 2<sup>nd</sup> Edition (London: Lawrence Erlbaum Associates, 1990), especially parts II, III & IV.

Luneburg's idea that binocularity is the source of the only consistent underlying metric structure of visual experiences.

In the parallel alleys experiment, the subject is presented with two rows of point-like lights, which lie on either side of the median plane extending out from the observer. The two farthest lights are fixed in their starting position, whereas the other lights are movable along the horizontal plane. The subject is invited to arrange the lights:

... so that they no longer converge but seem to form a parallel  
"alley".<sup>155</sup>

There is some unclarity here about what these instructions mean, but the idea seems to be something like this: physical parallel lines, such as railway tracks, normally present, in some sense, the appearance of converging lines, when you are standing in the middle of them. In this experiment the subject is in the same kind of position relative to the physical lines, but is required to avoid the expected appearance of convergence: he is to arrange the lights so that they appear to form a parallel alley in another sense. It is nowhere stated explicitly, but presumably the appearance of parallelism that is sought after here is something akin to the experience of viewing the railway lines from above. The only problem with this suggestion is that the instructions seem to have been unclear because some reports of the experiment suggest that the instructions also required the subjects to arrange the lights so they appeared to be straight, parallel lines *extending toward the observer*.<sup>156</sup> In this case it could not be like the appearance of railway lines seen from above. In fact, based on the descriptions of the experiments available it is not clear in these papers what was expected of the subject.

Setting this worry aside for the present, when the lights have been arranged to present the right appearance to the subject, the physical location of the lights is recorded. The physical arrangement of the lights is not of parallel lines, or indeed of straight lines at all; the arrangement is roughly that of two hyperbolas diverging as they recede from the subject.

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<sup>155</sup> Rudolph K. Luneburg. "The Metric of Visual Space." *The Journal of the Optical Society of America* 40(10) (1950): 629

<sup>156</sup> J. M. Foley. "Binocular Space Perception." in *Vision and Visual Dysfunction 9: Binocular Vision*, edited by David Regan (London: The Macmillan Press, 1991), p. 81

In the equidistant alleys experiment the general setup is the same as that of the parallel alleys experiments; the only difference being that when the experiment begins, only the two most distant lights are illuminated. The second most distant pair of lights is then illuminated and the subject is instructed to arrange the lights so that they appear to be the same distance apart as the fixed lights. This is done for all the remaining lights, to:

... set up alleys of apparently equidistant walls.<sup>157</sup>

Luneburg's claim that what is being asked for is 'apparently equidistant walls' suggests that they should appear to the observer to be extending toward them. This is what is suggested also by the general description of this experiment as found in John Foley's paper 'Binocular Space Perception' where he says that the lights were to be moved so they appear to correspond to the following perceptual criterion:

... two rows extending toward the observer in which the points at each distance [i.e. along the median plane] are separated by the same perceived distance.<sup>158</sup>

The results are similar to those from the parallel alleys experiment, in that the physical arrangement of lights is not of straight lines equidistant at all points. Again, the arrangement is roughly that of two hyperbolas diverging as they recede from the subject.

When the results of the two experiments are compared, the physical hyperbolas from the parallel alleys experiment consistently lie inside the physical hyperbolas from the equidistant alleys experiment. Luneburg and his followers have taken this result to be a *reductio* of the thesis that the geometry of visual experiences is Euclidean. As A. A. Blank puts it:

Since the two criteria, equidistance and parallelism, do not give the same result, it is clear that the geometry is not Euclidean.<sup>159</sup>

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<sup>157</sup> Loc Cit.

<sup>158</sup> Loc Cit.

Moreover, the tendency of the physical lines to diverge suggests that the geometry cannot be spherical.

The result that the geometry of binocular visual experience is hyperbolic is further confirmed mathematically, by describing visual space using a system of polar coordinates, with the midpoint between the eyes as the origin, and using the data from the alleys experiments to mathematically derive the metric. Thorne Shipley, in his paper ‘Convergence Function in Visual Space. I. A Note on Theory’, gives perhaps the clearest account of this mathematical form of the argument, in terms of the version angles from the origin of the points constituting the alley and the hyperbolic-right-triangle relations. Summing up the mathematical description of his argument, he says:

This may be phrased as follows: given the basic structure of the theory, if the version angles of the distance alley are found, by experiment to be greater than those of the corresponding parallel alley... *then the hyperbolic-right-triangle relations would hold in visual space....* In this point lies the crucial test of the alley experiments: is it true that, on the average, the points of the distance alleys are wider than those of the corresponding parallel alleys?<sup>160</sup>

It appears from the results of the alley experiments that this relation does hold – on average the points of the equidistance alleys are wider than those of the corresponding parallel alley. This is, then, the structure of Luneburg’s central argument for the thesis that the geometry of visual experiences is hyperbolic.

**Some Criticisms**

There is a large volume of psychological literature on this theory, much of which is concerned with improving the accuracy of the original experiments and with extending the ways in which the geometry of visual experience can be tested for using

<sup>159</sup> A. A. Blank. “The Luneburg Theory of Binocular Visual Space” *The Journal of the Optical Society of America* 43(9) (1953): 719

<sup>160</sup> Thorne Shipley. “Convergence Function in Visual Space. I. A Note on Theory” *Journal of the Optical Society of America* 47:9 (1957): 800

dark room experiments of a similar kind.<sup>161</sup> Some of the repetitions of the experiments have produced some interesting results: for instance there has been found to be considerable variability in the amount of curvature as indicated by the experiments, even for the same subject. This variability appears to depend in some measure upon the size of the array of lights used. These results provide motivation for some interesting aspects of Heelan's approach to Luneburg's theory. However, for the present I will restrict my discussion to just three direct criticisms of Luneburg's theory that have been offered by French, Angell and J. J. Gibson, as these criticisms all propose that for various theoretical reasons the results of the experiments reveal nothing about the geometry of visual experience. I broadly agree with this claim, but find the reasons they adduce in favour of it to be inadequate.

I will begin with Angell's central criticism, which is the criticism most likely to arise on reflection about the experimental method used in these experiments. Angell objects that the method of instructing the experimental subjects to arrange the lights according to some criteria does not provide *any* evidence about the phenomenal character of visual experience. Comparing Luneburg's theory and his own he argues that:

...they deal with a different domain of data than that of our thesis. The domain within which these hyperbolic properties and relations are alleged to exist is the field *Jp*, of judgments about, or perceptions of, geometrical relations and properties among physical objects. The domain which we assert satisfies the axioms of elliptical geometry is the domain of *Av*, the actual geometrical relations and properties which are found among visibles. The data on which Luneburg bases his theory are *Jp*, reported judgments or perceptions about three-spatial relations from subjects, and *Ap*, actual physical measurements of actual physical objects. The data we appeal to for our thesis are *Av*, actual visual distances and angles....<sup>162</sup>

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<sup>161</sup> For instance, Blank developed experiments where the subject is instructed to construct isosceles triangles with the cyclopean point of view as one of the vertices; Foley later developed experiments testing the relations between apparent equivalence of frontal and egocentric extents by instructing subjects to construct pairs of equilateral triangles sharing a line in the median plane for one side. These experiments are all described in J. M. Foley. "Binocular Space Perception." in *Vision and Visual Dysfunction 9: Binocular Vision*, edited by David Regan (London: The Macmillan Press, 1991), p. 81

<sup>162</sup> R. B. Angell. "The Geometry of Visibles" *Nous* 8 (1974): 99-100

It seems to me that the reason Angell has identified for dismissing Luneburg's theory and the evidence supporting it is not very convincing. This is because Luneburg's theory is not intended to be a systematisation of the judgments we make about the properties and relations of physical objects. As I highlighted at the beginning of this chapter, the avowed intent of the theory is to describe the phenomenal character of visual experience: Angell's *Av*. The fact that the theory relies on a mathematical analysis of the arrangement of physical objects is not itself grounds for thinking that the experiment does not reveal anything about the actual geometrical relations and properties which are found among the elements of visual experience. The arrangement of external objects is taken to be indicative of the relations in visual experiences, which it is quite reasonably supposed defy direct measurement. Finally, it is not obviously correct that the theory is based on judgments of the physical objects: it seems that the subject is instructed to judge that the visual appearance match some 'perceptual criteria'. It is true that there is some unclarity about what exactly these perceptual criteria are, but this is not sufficient for Angell's conclusion, which seems to me altogether too quick. Angell has just assumed without further ado that the theory concerns judgements of relations between physical objects, because it takes as its data how physical objects are arranged in response to instructions. Foley has commented on how common this misconception of the research programme is:

The question of the intrinsic geometry of visual space is a question that is frequently misunderstood. It is not a question that can be answered by a consideration of the relation between physical locations and perceived locations or physical shapes and perceived shapes. Rather it must be answered by considering the relations among perceived magnitudes (lengths and angles). In principle, questions about intrinsic geometry can be answered by phenomenological description without reference to the physical configurations. This is why it is called intrinsic geometry. In practice hypotheses about intrinsic geometry are usually tested by having observers construct configurations that satisfy



certain perceptual criteria and using assumptions about the relation between physical and perceived coordinates in analysing the results.<sup>163</sup>

The second criticism of Luneburg's theory is that offered by Robert French, who takes his cue from J. J. Gibson's dismissal of it. In his *The Perception of the Visual World* Gibson claimed himself to be 'bewildered' by Luneburg's conclusion of a hyperbolic geometry for visual experience:

When Luneburg suggests that perceptual space is the hyperbolic type of non-Euclidean geometrical space it only confuses me.<sup>164</sup>

Gibson's main objection to Luneburg's theory concentrates on the fact that the conclusion is reached solely on the basis of experiments conducted under highly unusual perceptual conditions: i.e. in the dark, with no possibility of head movement. He argues that it is mistaken to conclude that visual experience has a non-Euclidean geometry *solely* in order to account for the data generated by these experiments, to the exclusion of our patent ability to use sight accurately to get around and interact with objects in the world:

The obtruse and theoretically unclear set of facts which [Luneburg's theory] seems to account for (the alley experiments) are not obtained in a situation with full illumination and optimal conditions for depth perception. The argument is based entirely on an analysis of binocular disparity of images, leaving out of consideration the geometry of perspective as it applies to size, texture, motion, and other types of stimulation. Perceptual space as we get it under optimal conditions – with constancy of size and shape – is so plainly and simply the space from which Euclid extracted his geometry, and this conception is so illuminating for all the constancy experiments which yield 100 per cent

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<sup>163</sup> J. M. Foley. "Binocular Space Perception." in *Vision and Visual Dysfunction 9: Binocular Vision*, edited by David Regan (London: The Macmillan Press, 1991), p. 81

<sup>164</sup> Gibson, J. J. *The Perception of the Visual World* (Cambridge, Massachusetts: The Riverside Press, 1950): 189

constancy, that to deny it for the sake of the alley experiments seems unjustified.<sup>165</sup>

One implication of this passage is that it should be possible to explain the results Luneburg bases his conclusions upon, the alley experiments, in terms of the size constancy tendency. This is the tendency to perceive objects as having their true size: this tendency sometimes fails, generating a large number of perceptual illusions. For size constancy to operate successfully the visual system must make use of a number of visual cues, many of which are removed by the experimental setup of the both alley experiments. As French points out, the results of the alley experiments were originally ascribed to size constancy effects:

...historically the results of the alley experiments were thought to be due to size constancy effects, and the construction of the equidistant alley can be interpreted in a straight-forward manner as being an experiment in size constancy, since the experimental subject is asked to construct alleys which are the same distance apart at various depths.<sup>166</sup>

However, this suggestion of Gibson's cannot be left undeveloped. This is because it is not immediately clear why the explanation of the results of the alley experiments is incompatible with Luneburg's conclusion that visual experience has an underlying hyperbolic geometry. This is because the hyperbolic geometry just provides a mathematical *description* of visual experience, based on the disparity between the results of the equidistant alley experiments and the parallel alley experiments. In the passage above, Gibson has only pointed out the undesirability of Luneburg's theory, which he claims 'violates common sense'; he has just pointed out that it leaves unexplained the relationship between this underlying geometry and our perceptual success in normal viewing conditions. French attempts to offer a more robust argument for why the results of the alley experiments should not be considered as at all significant for the phenomenal character of visual experience.

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<sup>165</sup> Gibson, J. J. *The Perception of the Visual World* (Cambridge, Massachusetts: The Riverside Press, 1950): 190.

<sup>166</sup> R. E. French. *The Geometry of Vision and the Mind Body Problem* (New York: Peter Lang., 1987): 99.

Luneburg's argument for a hyperbolic geometry was based upon the disparity between the equidistance alleys and the parallel alleys. French's strategy for resisting the conclusion is twofold: he first argues that one set of results, those concerning the equidistance alley experiments, are straightforwardly irrelevant to the determination of the geometry of the phenomenal character of visual experience. Secondly, he argues that the results of the parallel alleys experiment, the divergence of the arrangements of lights, can be adequately accounted for by the unusual instructions given to the experimental subjects. Taken together, these two parts of his strategy would provide an adequate explanation of the evidence from the alley experiments without suggesting a hyperbolic geometry for visual experience.

As I have indicated, French follows Gibson in interpreting the equidistance alley experiment as informing on the size constancy phenomenon. One feature of such experiments is that the results obtained by the experiments depend to a large extent upon how the instructions are given to the subject: they can be asked to consider either the projective size and shape of objects or the actual size and shape of objects. On the basis of this feature of such experiments, French offers the following argument intended to show that the equidistance alley

- (6) Therefore, the results of the equidistant alley experiments are not relevant to a determination of the geometry of the phenomenal character of visual experience.

There appear to be no problems with premises (1) and (3), which are straightforward empirical data. The problem with this argument concerns premise (4). Why must we, if our interest is in the geometry of visual experience, restrict ourselves to experiments that concern a subject's responses to requests for information about the projective sizes or shapes of objects? Why should the equidistant alley experiment be ruled out as irrelevant? Here is French's argument:

The question thus arises as to whether data from objective or projective matches in size constancy experiments should be taken as primitive in the determination of the geometry of visual space. Inasmuch as in the determination of this geometry we are concerned only with the nature of phenomenal space itself, and not with inferences of judgments from that space to the nature of something else, in this case the physical sizes being seen, it would seem that the projective match data would be the relevant set.<sup>167</sup>

French appears to be making a similar mistake to that I observed in Angell. French supposes that the results of the alley experiment are just indicative of 'inferences of judgments' made by the subject. I have argued that this is not the correct way to think about these experiments. French appears to take this view of the alley experiment because he thinks that the third dimension is not part of the phenomenal character of the visual experience. This however, is a standing assumption of this approach, and we looked at some of the reasons for accepting it in the previous chapter. Anyone convinced that vision is phenomenally three-dimensional need not accept French's conclusion that the equidistance alley experiments are irrelevant for the recovery of the geometry of visual experience.

French's position would mean that size constancy cannot be part of the phenomenal character of visual experience, but there appears not to be any clear

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<sup>167</sup> Ibid., p. 101.

reason why we must accept this. Moreover, size constancy just does seem to be part of the phenomenal character of visual experience in a way that is not inferential. This is why we find it surprising or disappointing when the photograph of a mountain range does not capture how dramatic they appear to the naked eye. If this is right, then, despite the fact that these experiments may have an explanation in terms of size constancy, the results of the equidistance alley experiment will not be, on these grounds, irrelevant for the determination of the geometrical properties of visual experience. There is no reason, other than a belief in the absence of a phenomenal awareness of depth, to think that an explanation of the results in terms of size constancy and a description of the experience in terms of a hyperbolic geometry are mutually exclusive. The particular metric facts of the phenomenon of size constancy in the phenomenal character of visual experience themselves stand in need of a geometrical description, which the hyperbolic geometry may be ideal for.

I have argued that the first part of French's strategy for resisting Luneburg's conclusion is unconvincing, given that it assumes something that anyone seriously considering Luneburg's strategy will not accept. This first part of the strategy consisted of an attempt to discredit the equidistance alley experiment as an investigative tool for recovering the phenomenal character of visual experience. The second part of the strategy focuses upon the parallel alley experiment. French argues that the results of the parallel alley experiment can be dismissed, because they are just artefacts of the unusual instructions given to the experimental subjects. Recall that in these experiments the subjects are asked to construct parallel alleys extending toward them, but are required to avoid the appearance of convergence which is normal when we see parallel lines retreating into the distance. French argues that in this case it is no surprise that the resulting physical arrangement of lines diverge.

The instructions typically given subjects for the construction of parallel alleys differ significantly from those given for equidistant alleys, not only in that parallelism (and thus straightness), rather than equidistance, is to be the determining factor in the positioning of the lights of the experiment, but also that the subjects are told to avoid an effect which is characteristic of viewing physical parallel lines, that of convergence, as in the case of railroad tracks which appear to converge as they go off into the distance. Thus, it is not surprising that in order to avoid this

converging tendency in the appearance of physical parallel lines, the parallel lines have to be constructed so as to physically diverge as they become more distant, giving a result closer to a null match than in the equidistant alley case.<sup>168</sup>

I think that French is right to be suspicious of this evidence, but is this enough to resist Luneburg's conclusion? If French's argument for the irrelevance of the equidistance alley experiment had been convincing, then his argument would have carried considerable weight. However, as it was not convincing we can at best conclude that we have grounds for being suspicious of Luneburg's theory.

On the basis of this suspicion there is at least room for being sceptical about how conclusive these experiments are for the geometry of visual experience. There remains room for the possibility that the results obtained are not representative of the geometrical relations between elements in experience. The results may just be artefacts of the unusual instructions given to the subjects. This possibility may be thought more probable upon reflection on Gibson's observation that the alley experiments only deal with a small set of facts about visual experience: those obtained under highly restricted, non-normal viewing conditions. It would therefore be advantageous to the proponent of Luneburg's theory if there were a wider range of more salient phenomenological evidence that could support the claim of a hyperbolic geometry, rather than just relying on results obtained under unusual conditions in response to opaque instructions. In fact this is precisely one way that Patrick Heelan has attempted to motivate his own version of the claim that visual experience is hyperbolic. I will now turn to a discussion of his position.

### **Patrick Heelan's Hyperbolic Model**

Luneburg's approach to the question of the geometry of binocular visual experience was to construct a model of visual experience and test it by means of experiments conducted in a dark room with point-like light sources. I have discussed, and largely rejected, some of the reasons for being suspicious of how compelling such

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<sup>168</sup> Ibid., pp. 102-103.

evidence is for a hyperbolic geometry. However, Patrick Heelan has offered further support for a hyperbolic geometry by arguing effectively that there are a large number of features of the phenomenal character of visual experience, which occur under normal viewing conditions, that confirm predictions made by a hyperbolic model of visual experience.

Heelan's 'hyperbolic model' of visual experience differs from that of Luneburg's model in one important respect. Luneburg assumed that the structure of visual experience is determined by unvarying, presumably biological factors – this has the consequence that the basic structure of binocular visual experience should not vary as the configurations of the physical objects observed vary. As was mentioned in my initial presentation of the evidence for Luneburg's theory, further experiments have suggested that this is not the case.

Heelan attempts to account for the variability in the results of these further experiments by means of the assumption that an observer has 'at his disposal two kinds of spaces, a Euclidean space and a family of hyperbolic spaces....'<sup>169</sup> For Heelan, whether a particular configuration of physical objects appears visually in such a way that the geometrical relations satisfy either the hyperbolic model or a Euclidean model depends upon what he calls 'hermeneutical considerations'. His idea is that at some (presumably sub-personal) level, the observer assesses what 'makes good visual sense of the situation as a perceptual opportunity.'<sup>170</sup> This assessment fixes a determinate geometrical character for the experience only if certain conditions are met. The conditions for a Euclidean 'perceptual opportunity' and a hyperbolic 'perceptual opportunity' concern the possibility of congruence standards being employed for the arrangement of objects, and this differs for the two visual spaces. If these conditions are not met then the experience will be ambiguous or indeterminate.

Heelan thinks that in spite of the failure of Luneburg's theory to account for the variability found upon further experimentation, there are considerations that do support the claim that there is a primitive hyperbolic way the visual system delivers up objects of perception. This is commonly overridden by what he calls 'Cartesianism', by which he seems to mean certain assumptions about the world, in

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<sup>169</sup> Patrick Heelan. *Space Perception and the Philosophy of Science* (Berkeley and Los Angeles: University of California Press, 1983), p. 50

<sup>170</sup> Loc. Cit.

particular that objects are arrayed in a Euclidean three dimensional space.<sup>171</sup> This ‘Cartesianism’ is what provides the second, Euclidean kind of perceptual space Heelan postulates. However, Heelan is not explicit about what he means by saying that the hyperbolic way that visual system delivers up the objects of perception is ‘primitive’. In addition, it is far from clear how we are to construe these ‘Cartesian’ assumptions: are they propositional or non-propositional, conscious or sub-personal?

Regarding a ‘Euclidean perceptual opportunity’, Heelan writes:

For a World to appear Euclidean to a visual observer, it must then be virtually populated with familiar (stationary) standards of length and distance, and be equipped with instantaneous means for communicating information about coincidences from all parts of space to the localized visual observer, wherever he/she happens to be.<sup>172</sup>

Regarding a ‘hyperbolic perceptual opportunity’, Heelan says:

...the visual observer must be able to use the rule of congruence which, it is claimed, is embodied in the capacity of the unaided visual system to order the sizes, depths, and distances of all objects in the unified spatial field of vision. This is done by purely visual estimation....<sup>173</sup>

Before continuing I should just comment on some of the slightly obscure claims Heelan makes in these passages, and explain some of the quasi-technical uses of terminology he employs. Firstly, for Heelan the capitalised word ‘World’ is used as something of a term of art. He says:

Our World is the general background reality context that is experienced as given to our perception together with the individual objects that we perceive.<sup>174</sup>

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<sup>171</sup> If this is right, then Heelan’s official position seems much closer to Reichenbach’s position in that there are plurality of geometries that visual experience can instantiate. In both cases, which geometry is instantiated depends upon the independent status that is accorded to the choice of unit of measure.

<sup>172</sup> Ibid., p. 51

<sup>173</sup> Loc. Cit.

<sup>174</sup> Ibid., p 10



For Heelan, a ‘World’ is indexed to particular communities at particular times and ‘is not the only World.’<sup>175</sup> It is far from clear to me what a ‘World’ is, but it seems safe to say that if it is meant to be a metaphysical notion, then Heelan is not a Realist. If it is not a metaphysical notion, then it may be a part of a metaphysics of perception. In this case, I do not know what it means to claim that one experiences a context, whether given with individual objects that we perceive, or not.

However, there does not appear to be any need to commit to Heelan’s theoretical assumptions to see the basic point that he appears to want to make: in fact, it is quite straightforward. It is just that for a physical arrangement to appear Euclidean there must be standards for length and distance that the observer can employ, and employ to any part of what is seen. For a physical arrangement to appear hyperbolic, the visual system ‘unaided’ must be able to order the size, distance and depth relations. This, it is assumed, is the situation of the experimental subject in the experiments Luneburg bases his analysis on – the arrangements of point-like lights are based solely on ‘visual estimation’ because movement is restricted.

In what follows I will attempt to give as austere a reading to Heelan’s discussion without relying on his less than clear terminology, in order to fillet out of Heelan’s discussion the various considerations that support a hyperbolic metric for visual experience.

**The Model**

Heelan’s general approach to showing that there are considerations that support a hyperbolic geometry seems to have the following form:

- 1) Assume that visual space is hyperbolic and physical space is Euclidean.
- 2) There are formulae for the transformation of shapes in physical space to the hyperbolic space.
- 3) From these formulae we can derive models, which (when interpreted) give the visual shapes of the physical object under the proposed hyperbolic model.

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<sup>175</sup> Loc. Cit.

- 4) We can then compare the visual shapes the model specifies with those found in veridical visual experience, illusory visual experience and art to test the appropriateness of the model.

Both the shapes specified by the model and the kinds of evidence mentioned in (4) are cashed out by Heelan in terms of qualitative features of the space – in familiar non-mathematical language, such as statements about lines converging or diverging. The first thing to do is to look at the qualitative description of the predictions made by the model. After this, I will look at the evidence Heelan appeals to from the phenomenal character of visual experience under normal viewing conditions, before moving on to his discussion of illusions.<sup>176</sup>

First, here is a summary of the general features of the hyperbolic model:

- 1) The model has the form of a non-Euclidean metric space.
- 2) The model is linked to a model of physical Euclidean space by transformation laws. These are based on what are believed to be the relevant psychological parameters.
- 3) It is assumed that the parameters are governed by considerations of what makes sense of the physical environment.

These are:

- a) The need for a standard for size, depth, and distance.
- b) That this standard is projected across the whole of visual space by a process of ‘visual measurement’.
- c) There are Gestalt-type relationships between ‘foreground’ and ‘background’, ‘near zone’ and ‘distant zone’, etc.

## **Predictions and Confirming Phenomenological Evidence**

Although I have hitherto been referring to Heelan’s model in the singular, in fact he offers two different hyperbolic models for visual experience. This is because

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<sup>176</sup> I will only briefly look at one use of Heelan’s appeal to art to support his conclusion. This is because his appeal to art forms the weakest part of his argument.

the specific qualitative descriptions given by the hyperbolic model depend crucially upon whether it is assumed that the hyperbolic space is finite or infinite. This means that different predictions are made by the model for each assumption. However, there are a number of qualitative features that both finite and infinite hyperbolic spaces share. I will begin by discussing these common features and the phenomenological evidence that Heelan cites as confirming them. I will then list the qualitative characteristics that are specific to the two kinds of hyperbolic space, finite and infinite. Heelan argues that the majority of phenomenological evidence confirms the assumption that visual space is a finite hyperbolic space.

### **General features predicted by the model**

Heelan claims that the most significant qualitative feature of the model is what he calls the ‘natural’ division into a *near zone* and a *distant zone*, relative to the observer:

Potentially the most significant qualitative characteristic of the model of a finite hyperbolic space is the division of the visual field into the near and distant zone.<sup>177</sup>

In the near zone the dimensions of shapes in the visual model do not differ much from the dimensions of the shapes of the physical objects of which they are mathematical transforms. In the case of the distant zone, however, this is not true. The distant zone is just the region outside of the near zone from the observer and it extends toward the theoretical limit of visibility, which Heelan calls the ‘horizon sphere’. The major qualitative feature of the distant zone is the shallowness of apparent depth. As we shall see, Heelan suggests that this zone may be responsible for a number of the kinds of illusions investigated by psychologists.

Heelan describes the division of the model into a near zone and a distant zone as ‘natural’. What he appears to have in mind here is the fact that when you map physical space onto the hyperbolic space, there will always be a region surrounding some point in front of the observer where the shapes and sizes of the physical objects

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<sup>177</sup> Ibid, p. 75

roughly coincide with the shapes and sizes of the corresponding figures in the hyperbolic model.

The most important theorem about the relationship between physical and visual space is that there always exists a region surrounding some definite point directly in front of the observer in which visual and physical sizes and shapes roughly coincide.<sup>178</sup>

Heelan calls the point in physical space that this is true of, the 'true point'. He seems to think that the near region forms a sort of Gestalt and that when the true point is occupied the scaling standard for size, depth and distance for this 'region' is located at the true point. It is not clear whether Heelan wishes to make any metaphysical hay out of this distinction between near zone and distant zone, but it is certainly clear that one need not – the division of the space into the two zones can be unproblematically construed as grouping roughly similar features of the hyperbolic space.

In the model, there is a parameter ' $\sigma$ ', which represents the distance from the observer to the true point. The true point is also the point in front of the observer where the visual standard for size, depth and distance for the whole space is located. On Heelan's account, this varies depending on what is appropriate for the physical setup being seen. So, for example, in a small enclosed space like the interior of a room, the value should be somewhere between the distance to the farthest visible point at eye level in the room and half that value. The reason Heelan offers for this is that the interior of the room falls within the near zone of the visual space and that the true point does not lie outside the room. The significant point is that the value of  $\sigma$  varies from situation to situation, and the parameter  $\sigma$  specifies the region of the near zone. It is therefore a consequence of Heelan's model that exactly what the limits of the near zone and the distant zone are will depend upon the situation.

Whilst it is true that, generally speaking, in the near zone the apparent size and shapes predicted by the hyperbolic model do not diverge as much as in the distant zone from the sizes and shapes of the physical objects they are transforms of, there is some variability nonetheless within the near zone. The principal such qualitative

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<sup>178</sup> Ibid., p. 58

feature of the near zone is that in the area between the viewer and the true point, apparent length and breadth contract, while apparent depth expands.

In the domain between the viewer and the true point, size (i.e. length and breadth) visually contracts or “shrinks,” while depth expands or “swells”.<sup>179</sup>

The main feature of the distant zone is that the objects within it will appear not to have much depth. The further from the observer that objects are situated, the more significant the loss of depth perception in viewing them. One consequence of this shallowness of depth is that the physical objects seen at a distance will be ambiguous as to their shape, size and orientation.

In... both finite and infinite spaces, there would be a very significant loss of depth perception with distance. Very distant objects would appear to have little discernible depth or thickness, papered, as it were, on the inside of a large surrounding sphere....

Given the shallowness of visual depth in the distant zone, objects with significant physical depth will appear as affected with unresolvable ambiguities of shape, size and orientation.<sup>180</sup>

In the model, flat physical planes in the distant zone that are horizontal to the observer and below the line of sight are transformed into bowl-like shapes, with the observer in the centre – so the model predicts that this is how they will appear. Pairs of parallel vertical planes on either side of observer should appear to diverge in the near zone. In the distant zone, the apparent disposition of such parallel vertical planes depends upon whether the space is assumed to be finite or infinite.

On the basis of these points, Heelan describes how the model predicts a chequered grid limited by a circle would look in visual space at different distances from the observer. The centre of the pattern is to be understood as in each case being directly in front of the observer, and that the perimeter of the limiting circle of the chequered pattern always subtends 90 degrees at point occupied by the viewer. These

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<sup>179</sup> Ibid., p. 63.

<sup>180</sup> Ibid., p. 64-65

predictions of the model are supposed to be predictions about how such a physical pattern will appear at certain distances.

When the chequered pattern is centred on the true point (i.e. where there is the greatest similarity of physical shapes and sizes with the shapes and sizes specified by the hyperbolic model), the visual frontal plane of the chequered pattern has a central region that is congruent with the physical pattern. The limiting circle of the chequered pattern is transformed into an oval that stretches in the horizontal dimensions. Horizontal lines bend away at the edges from the horizontal line passing through the true point.

When the pattern is located at half the distance from the true point, the central region of the visual figure will be similar to the physical pattern, but smaller. The limiting circle is a more pronounced oval than at the true point – the vertical dimension is flattened and the horizontal dimension is even more stretched. The surface will appear as convex.

From these predictions Heelan predicts how a cube will appear as it is moved toward and then away from an observer. As it is moved toward an observer, closer than the true point, the side facing the observer will contract. As it is then moved away, it will expand to a maximum size where the near zone ends and the distant zone begins. In the near zone vertical lines close to the median line of vision bend outward at their ends from the median line of vision. Vertical lines further from the median line do the reverse – they are concave to the observer.

I have collected together the various predictions that Heelan's hyperbolic model makes of how physical objects will appear. These predictions hold generally of hyperbolic spaces, irrespective of whether they are finite or infinite. I'll now cover the phenomenological evidence Heelan offers as confirming these predictions.

The first piece of evidence Heelan adduces in support of the hyperbolic model concerns the existence in the model of a region surrounding the 'true point' where the apparent sizes and shapes of objects do not diverge greatly from the actual sizes and shapes of objects. The evidence is the phenomenological feature of a "Newtonian oasis" in the near, central region of the visual field, which has been described by R. Arnheim in his *Art and Visual Perception*:

There are what might be called “Newtonian oases” in perceptual spaces. Within a frontal plane, space is approximately Euclidean; and up to a few yards of distance from the observer, shape and size are actually seen as unchangeable. It is from these areas that our visual reasoning obtains confirmation when, at an elementary level of spatial differentiation, it conceives size, shape and speed as independent of location.<sup>181</sup>

Heelan connects the existence of a near zone where length and breadth contract to the absence in experience of distortions that are to be found in photographs where objects are oriented toward the camera. For instance, when an arm is outstretched toward the camera, the closer parts seem disproportionately large, at suitably close distances.

Some photographic distortions can be explained as related to phenomena of the near zone. For example, a photograph of a hand outstretched toward the camera lens appears to depict a grossly enlarged hand.<sup>182</sup>

This distorting effect is absent in experience and would be explained by a systematic shrinking of the appearances of objects when in increasingly close proximity to the observer. He also ventures that it explains certain distortions that *are* to be found in experience, such as how checkerboard patterns seem to swell ‘like a shield’ as one approaches it.

The evidence that confirms the general predictions about the loss of depth in the distant zone concerns the ambiguity of the shape, size and orientation predicted of distant objects. Heelan claims that it is a consequence of the shallowness of depth in the distant zone that those physical objects will be ambiguous as to their shape, size and orientation. As evidence for the presence of such ambiguities in the distant zone, Heelan appears to a case study of what he refers to as the ‘visual profiles’ of Eero Saarinen’s Jefferson Memorial Arch in St. Louis. This case study was made by Edward G. Ballard, and is a description of how this arch, triangular in cross section

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<sup>181</sup> Ibid., p. 44

<sup>182</sup> Ibid., p. 64

and 200 meters high and wide, appears visually. Here is Heelan's summary of the relevant features:

To Ballard's perception, every approach to the Arch is characterized by visual ambiguity about form, solidity, orientation and materials. He describes the first appearance of the Arch through the morning haze as a thin grey band in the sky "paper thin, no more solid than a gap in the color of the cloud." From his position upriver, the Arch with its two great legs comes into view at an oblique angle, and he notices a strange phenomenon: "as soon as one leg was identified as the South leg, the Arch would 'spin around' and the appearance reversed."<sup>183</sup>

Another feature related to the loss of depth predicted by the model is that the visible facets of distant objects would appear to be oriented in a 'frontal way' toward the observer – i.e. would appear as if they were closer to orthogonality than they actually are. Even surfaces that are orthogonal to each other, such as the sides of a house, would appear turned toward the observer. This does not mean that we see any part of the surfaces that are hidden from sight. It is just the visible surfaces that appear to have this non-veridical orientation. Heelan points out that in visual experience depth between distant objects is foreshortened and that their plane facets appear turned toward the observer:

Distant objects are perceived with a noticeable telephoto effect; that is, they are brought closer, depth between distant objects is foreshortened, and plane facets are turned toward the observer.<sup>184</sup>

He also argues Cezanne's painting 'Turning Road at Roche-Guyon' captures this aspect of experience. If the painting is compared with a photograph of the same scene (see appendix 1), then it will be noticed that the visible surfaces of the buildings in the painting have different geometrical features than the visible surfaces of the buildings in the photograph. The phenomenal character of visual experience, it seems Heelan is claiming, is qualitatively like the painting rather than the photograph.

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<sup>183</sup> Ibid., p. 36

<sup>184</sup> Ibid., p. 67



The further feature predicted of the distant zone, the bowl-like apparent shape of horizontal planes in experience is demonstrated by the phenomenon of how the sea appears when in a boat with no land in sight – you appear to be in a great bowl of blue. The same effect occurs on land if you have a high vantage point. The appearance of the sky as a flattened vault overhead is the same phenomenon – a horizontal layer of clouds appears like a ceiling that moves down toward the horizon.

## **Finite and Infinite Hyperbolic Spaces**

The preceding features predicted by the model are general features that obtain in the hyperbolic model, irrespective of assumptions made about the space. However there are two different classes of space within the family of hyperbolic spaces that Heelan considers: finite and infinite. These correspond to different models obtained by setting the parameters of the general hyperbolic model differently. They each have distinct features that the other does not. The relevant parameter in the model is  $\tau$ , which establishes the limits of visual space in the model. If  $\tau = 0$  then the space is infinite and the horizon sphere is at infinity. If  $\tau < 0$  the space is finite and the radial distance to the surface of the horizon sphere is finite. Heelan asks the further question of whether visual space is a finite or infinite hyperbolic space, by means of the same method followed above: adducing evidence that confirms the predictions of one of the models.

### **Features of Finite Hyperbolic Spaces:**

If the parameters are set such that the hyperbolic space is assumed to be finite, then physical Euclidean space is mapped onto the interior of a hyperbolic sphere surrounding the observer. Euclidean infinity is mapped onto a limiting hyperbolic sphere, which Heelan calls the ‘horizon sphere’.

First, concerning the near zone, in a finite hyperbolic space, when you have physical parallel lines receding from the observer, the visual lines initially diverge in the near zone and at a distance of roughly  $2\sigma$  they begin to converge. The visual lines eventually will meet at a point in the distant zone. They describe a pair of converging

arcs. In such a case, you would expect parallel lines in the horizontal planes to appear to diverge at first as they approach the end of the near zone. They would reach a maximum apparent separation distance at the distance from the observer specified above, and then begin to appear to converge. Such lines eventually meet at the horizon sphere. Related to this phenomenon is the feature of the distant zone of a finite hyperbolic visual space where the visual size of physical objects reduces to a point on the horizon sphere.

For a finite hyperbolic space, in the frontal plane the chequered pattern discussed above will be very similar to that of the physical pattern, when located at a distance that is ten times the distance to the true point. The limiting circle is transformed into an oval, but not very pronounced at all. The lines of the visual pattern will appear concave relative to the centre of the pattern and the surface will appear concave to the viewer. On the basis of this we can predict that if visual space is a finite hyperbolic space then a cube moving away from the observer beyond the near zone will diminish until it becomes a point on the horizon sphere.

### **Features of Infinite Hyperbolic Spaces:**

These spaces are closer to Euclidean space than finite hyperbolic spaces. However, the most salient qualitative difference between finite and infinite hyperbolic spaces is that for the latter parallel lines in the horizontal plane will appear to diverge rapidly near the observer, then more slowly until they reach their maximum separation at infinity. This matches the results obtained by Luneburg in his alley experiments conducted in dark rooms. In spite of this, Heelan thinks that the majority of the phenomenological evidence supports the assumption that visual experience is finite.

### **Evidence for a Finite Hyperbolic Space**

There are two pieces of phenomenological evidence that Heelan offers for thinking that visual space is a finite hyperbolic space, rather than an infinite hyperbolic space. The first concerns the existence of a 'turning point' where parallel lines begin to converge. Heelan claims that the presence of such a point can be identified by the means of the following experiment: take a small rectangular card

3"x5" and hold it horizontally in front of the eyes, then move this card outward along a horizontal plane. It is possible to locate the point at which the edges of the card switch from appearing to diverge to appearing to converge. The existence of such point indicates a finite space. If you draw lines on the card parallel to the edge, but closer to the middle, and repeat the experiment then they will form different, curving shapes as they are moved closer to or further from the eyes.

The second piece of phenomenological evidence concerns the way in which physical parallel lines on a horizontal plane distort as one moves toward them in a car. Here is Heelan's description:

The pavement seems to dip and swell and undergo dynamic changes as it passes under and around to the right and left; in front, the road far ahead seems to climb rapidly and that hill appears to retreat in contrast with the rapidly approaching foreground; the width of the road ahead decreases in the distance until the margins eventually join; closer in front, the road dips and swells to receive the moving vehicle; the road seems to unroll before the driver's gaze in one continuous swell. All these descriptive elements are in keeping with an experience of movement in a hyperbolic visual space.<sup>185</sup>

## **Critique of the Phenomenological Evidence**

Heelan has offered a number of observations about the phenomenal character of visual experience that are intended to match the predictions of his hyperbolic model and thereby confirm that visual experience is hyperbolic in character. To block this conclusion we need to ask whether the phenomenal character of visual experience is as Heelan describes it. My discussion will concentrate on two of the phenomenological features Heelan discusses which seem to be particularly important to his argument. The reason these features are particularly important is because they concern *systematic* changes in the visual appearances of external objects – systematic changes that are in keeping with a hyperbolic geometry. If these claims can be

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<sup>185</sup> Ibid., p. 68

blocked, then the other features Heelan appeals to can be dealt with in a piecemeal fashion.

The first feature concerns systematic changes in the apparent shapes and orientations of the external objects as they change physical location relative to the observer. Heelan claimed that objects that are distant from the observer appear in orientations that are systematically more orthogonal to the line of sight than they would be if visual experience was Euclidean in character. The second feature concerned systematic changes in apparent depth in visual experience. There is an increased shallowness of apparent depth the further from Heelan's 'true point' the external object recedes away from the observer; conversely, as the external object approaches the observer from the 'true point' apparent depth expands.

I will begin with the first concern, as the second is more difficult to deal with. The claim that supports the hyperbolic model is that the apparent orientations of objects far away from the observer are such that they appear oriented 'toward the observer'. Moreover, this apparent orientation is not the orientation that they actually have. I find this claim about the phenomenal character of visual experience rather dubious. Having stared for some time at distant objects, I must confess not to be aware of the surfaces standing in noticeably non-veridical apparent orientations to me. However, there is a danger that debates about this kind of issue can collapse into a shouting match about what experience is like. As a way of avoiding this, one question that naturally arises concerns how Heelan's claim could be supported. As we saw earlier, in illustrating his claim Heelan appeals to a difference between the geometrical character of a photograph of a scene and the geometrical character that a visual experience of such a scene would have. The geometrical character of the photograph is, Heelan seems to suppose, Euclidean. The geometrical character of visual experience would not like that of the photograph, so it must not be Euclidean. Such an argument, it might be thought, lends some support to Heelan's claim about the apparent orientations of external objects.

The problem with this line of reasoning is that we already have reasons for thinking that the geometrical character of visual experience is not like that of a photograph. This is because we have assumed that visual experience is not two dimensional. Why should the fact that the visual experience of a scene is not like a photograph of a scene be put down to, or explained in terms of, a difference in the *geometry* of the experience? Heelan's line of reasoning would be convincing if we

thought that for three dimensional visual experiences to be Euclidean in character they must be something like a photograph, but we have not been offered any good reason for thinking this. To put the point slightly differently, why should we assume that for a visual experience to be Euclidean in character it must be anything like looking at a photograph? In the absence of such a reason, the fact that having a visual experience of a portion of the environment is not like looking at a photograph of that same portion cannot support Heelan's claim that the surfaces of the immediate objects of visual experience are oriented toward the observer in a non-veridical way.<sup>186</sup>

I will now turn to the second systematic feature that Heelan claims holds of visual experience: that there is a systematic shortening of apparent depth the further from the observer one goes. This claim, if it can be established, is crucial to Heelan's argument because it would warrant ascribing to the phenomenal character of visual experience certain metric relations that do not hold of physical space, or the objects in it. As we saw, such a warrant is argued for on the basis of the claim that as an object moves away from the observer, the same changes in physical place result in increasingly small changes in apparent or visual place. The question is, is this claim about visual experience true?

It is not hard to see how someone may be convinced that it is true, after all it is well enough established that binocular vision becomes rapidly less effective after a relatively short distance from the observer. After a certain distance we are decreasingly able to discriminate distance on the basis of binocular cues like retinal disparity and vergence. However, I think that it is instructive to compare the line of reasoning that is being offered here to that offered by Berkeley in respect of awareness of depth in visual experience.

In both the present case and in Berkeley's case, what is being attempted is to establish a claim about phenomenal awareness of depth by appeal to variability in visual experience relative to the actual variability of external objects. Take Berkeley's claim that distance is "a line directed end-wise to the eye...." A consequence of this is that *any* variability in the physical position of objects along that line will result in no apparent variation in their depth: "... [the line] projects only one point in the fund of

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<sup>186</sup> This line of argument is equally effective against Heelan's appeal to the existence of a 'turning point' for parallel lines and to the distinctive character of changes in parallel lines on a road as one moves forward in a car.

eye, which point remains invariably the same, whether the distance be longer or shorter.”<sup>187</sup> The claim this is supposed to support is that there will be no change in the phenomenal character of visual experience if there is change in place along that line. This is supposed to be a warranted claim about visual experience because there is no information available to the visual system to provide such awareness of depth. As we saw in the previous chapter, one response to this, due to Smith, is to argue that the relevant information that would be necessary for an awareness of depth can be found in a number of visual cues other than variability in the pattern of retinal activity in the left-right and up-down dimensions. The idea was that after the visual system has processed such cues we get depth awareness as the first conscious upshot.

Now in the present case the concern is not about whether there is *any* awareness of depth content, it is here assumed that there *is* such awareness, but whether the phenomenal character of that awareness of depth has the same metric structure as the physical objects. Heelan does not think, as Berkeley does, that any change in physical place along a line endwise to the eye would result in *no* variability in visual experience. Instead he appears to hold that equal changes in physical place on a line endwise to the eye result in decreasing variability in visual experience. This is presumably because there must be decreasing variability in the pattern of retinal excitation and a decreasing discrepancy between the pattern of light falling on the left and right retinas. To then use this to justify the claim about the phenomenal character of depth awareness we need to hold that it is only on the basis of the magnitude of such variability that the visual system calculates or assigns magnitude of depth.

It is, however, open to object that there are a number of other cues according to which magnitude of depth can be assigned; in visual science these cues are commonly referred to as pictorial cues. Consider a car moving off toward the horizon at a constant speed – as it begins to move off there is considerable variability both in one’s experience and in the pattern of activity at the retinas. However, it seems very strange to describe such an experience as being of a car changing its apparent position *less rapidly* the further it recedes. Such a description would, however, be a consequence of Heelan’s claim that less apparent distance has been covered by the car. Movement toward the horizon is a cue to depth when combined with other cues, such as patterns of variability in retinal excitation.

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<sup>187</sup> George Berkeley. *Philosophical Works: Including the Works on Vision* (London: J. M. Dent, 1996), p. 7

There is, however, an important point of difference between the case against Berkeley's denial of the very presence of depth awareness in vision and the case of what the metric characteristics of that depth awareness are. In the case against Berkeley it was plausible to say that awareness of depth is the first conscious upshot of the processing of certain visual cues. This is because all we are after is *some* awareness of depth, so we need just *some* information processing to have occurred before the first conscious upshot. It perhaps seems less plausible in the present case, simply because it seems that so much more processing must occur to get the right awareness of the relevant metric relations; processing that it seems sensible to presume *need not* occur for the subject to have some visual awareness with a phenomenal character. If we combine this point with a reflection on the phenomenal character of seeing distant objects, Heelan's claim may seem much more plausible. Take a scene with two objects reasonably close to the observer and at a given distance apart in the outward dimension, and with two objects set at the same physical distance apart, but which are much further away along the same dimension. There seems to be a difference between the observer's awareness of the relations between the close pair and the far pair. Does this not support Heelan's position?

Well no doubt it offers some support, but there is an alternative explanation of the phenomena, which makes more plausible use of the empirical facts about the decreasing richness of the information available to the visual system as objects recede from the observer. The suggestion is that we should not interpret the difference between my awareness of the two pairs of objects as consisting in a determinately shorter apparent distance between the distant pair than between the closer pair. Instead we should interpret the difference in terms of there being a *determinate* distance between the closer pair and there being a *less determinate* distance between the further pair. A generalization of this would be the phenomenological claim the awareness of depth becomes less determinate *in respect of magnitude*, the further from the observer objects recede.

This phenomenological claim coheres with the observation that the information available to the visual system to assign measures of depth drops off increasingly rapidly as objects recede from the eyes. Moreover, it does not have the strange consequence that Heelan's position has, viz. that we must say that the car moving off into the horizon at a regular speed appears to change its position less rapidly as its physical position gets further and further from the observer. No doubt it

may be objected that there is less variability in experience, but the point of the criticism of Berkeley's discussion is that depth awareness may be gained from cues other than such variability.

It is crucial to underline the distinction between Heelan's claim which says that depth is foreshortened as one recedes from the observer, and my claim which is that depth awareness becomes less distinct, or determinate, as one recedes from the observer. Heelan's view implies a denial of a Euclidean geometry for visual experience, because it explicitly assigns metrical relations that are incompatible with those between the external objects. My claim is that there is decreasing determinacy in the metric relations between the objects of visual awareness, which is more like remaining silent about the metric relations between the objects of visual awareness.

The move that I am making to block Heelan's claim bears some similarity to the strategy James Hopkins employed to avoid the challenge to Direct Realism, which we looked at in chapter 2. Hopkins observes that whilst it is a contradiction to say that the object we pre-theoretically take ourselves to be aware of is both Euclidean and non-Euclidean, it is not a contradiction to say that  $x$  is Euclidean and approximately non-Euclidean.<sup>188</sup> He then goes on to argue that visual experience is indeterminate and that 'approximately non-Euclidean' is the best that could ever be said about it. I argued in chapter 2, though, that Hopkins' own case for indeterminacy is no good.<sup>189</sup> Here Heelan seems to be interpreting the falling away of information about depth as resulting in a determinate metric for the whole of visual experience - a hyperbolic metric. I have argued that there is a better explanation of the falling away of information about depth - a decrease in the determinacy of depth awareness: a falling away of awareness. This interpretation does not put any pressure on the claim that external objects are the immediate objects of visual awareness: we must just say that such awareness is decreasingly determinate at increased ranges.

I have argued that both of Heelan's main pieces of phenomenological evidence do not support his claim of a hyperbolic geometry for visual experience. This is because in both cases the phenomenal character of experience has been interpreted in ways that support Heelan's claim, but there are alternative ways of describing the phenomenal character of such experiences that do not provide confirmatory support

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<sup>188</sup> James Hopkins. "Visual Geometry" *The Philosophical Review* 82 (1973): p 55

<sup>189</sup> See Chapter 2, pp. 44-52



for Heelan's hyperbolic model. I believe that the remaining evidence Heelan cites can be dismissed along similar lines, or as particular cases of illusion that do not reflect some underlying geometrical structure for visual experience. For instance, Heelan's claim about the ambiguity of distant figures is perfectly compatible with the proposal that the phenomenal character of such figures is indeterminate in respect of the metric relations between its objects of immediate awareness. Moreover, in this way we can account for such cases without needing to appeal to Heelan's rather strange claim that observers have different kinds of 'perceptual spaces' available to them.<sup>190</sup>

We are also now in a position to vindicate the suspicions that French and Gibson raised regarding Luneburg's theory. French observed that the parallel alley experiments involved giving the experimental subjects unusual instructions: they were asked to avoid making the alleys present precisely the appearance that parallel lines normally do in such orientations to an observer. Moreover, Gibson observed that the theory was based on results obtained under experimental conditions which were highly restricted, compared with normal viewing conditions. Gibson presented us with a choice between Luneburg's theory and the commonsense view of a Euclidean geometry that informs the abilities we have under normal viewing conditions. It was not clear, though, precisely what theoretical mistake Luneburg *et al.* had made. After all, Luneburg was quite clear that he was interested in the geometrical structure of our visual experience under such restricted conditions. However, we are now in a position to offer an explanation for why it is a mistake to take the alley experiments as indicative of the geometry of visual experience. In taking the responses given by the experimental subjects in unusual and highly restricted viewing conditions as indicative of the geometrical relations to be found in visual experience, Luneburg *et al.* have assumed that under such conditions visual experience exhibits *determinate* metric relations. I have suggested that the correct way to explain the phenomenological features of depth perception Heelan discusses is to say that visual experience becomes less determinate as depth information becomes less rich. If this is right, then under the viewing conditions found in the alley experiments we are not entitled to assume, as Luneburg's theory does, that visual experience exhibits determinate metric relations.

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<sup>190</sup> See pp of this chapter for Heelan's assertion of this claim.

To the extent that this criticism offers a way to accept data generated by the experiments and the phenomenal character, but to nonetheless deny that any substantive conclusion about the underlying spatial structure of visual experience, the criticism provides a warning to philosophers who may resist the idea that this is a philosophical question at all. In chapters 3 & 4 I defended Suppes' criticism of those who may take entirely aprioristic approaches to the question of the geometry of visual experience. Here we see that critical reflection on the line of reasoning reveals a hidden assumption: that the spatial properties that characterise visual experience are determinate under the experimental conditions described. Apriorism is not the only danger to avoid when approaching this question: scientism is another.

I have argued that interpreting the falling away of information about depth in terms of a decreased determinacy in visual experience provides a way of avoiding the argument from illusion based on the geometry of visual experience. However, I have not said much in any detail about what this claim amounts to, which I will now proceed to do.

The most obvious question to ask is what is meant by 'indeterminacy' here? I have argued that there is some similarity between the claim I have made here and that made by James Hopkins, who argued that the geometry of visual experience is indeterminate. However, my claim differs from Hopkins' in two crucial ways. Firstly I do not claim that the geometry of visual experience is indeterminate, just that certain properties of visual experience can be indeterminate. Secondly, the sense in which I use the term 'indeterminacy' is different from that which appears to be used by Hopkins. The first difference will be best explained after an account of the second.

In Hopkins' discussion there appeared to be two senses in which he claimed visual experience is indeterminate. As I observed in chapter 2, the first sense seemed to be better described as a claim about the under determination of the geometry of visual experience. The second sense of indeterminacy was the sense in which the phenomenal character of visual experience can never accurately represent certain spatial relations. I argued that Hopkins' argument for the indeterminacy of visual experience involves sliding illegitimately into the second sense. I do not mean 'indeterminacy' in either of these senses.

The sense in which I do intend to use 'indeterminacy' can be specified by means of the 'determinate-determinable' relation between properties. This

relationship was first formulated in these terms by W. E. Johnson.<sup>191</sup> Certain properties stand in a particular kind of relationship to each other, such as the property 'red' and the property 'coloured', such that having one of the properties is a particular way of having the other: being red is a particular way of being coloured. In the colour example, 'coloured' is the determinable in this relation and 'red' is the determinate. The relation is one that holds between pairs of properties, which form a hierarchy: 'red' is a determinate of the determinable 'coloured', but 'scarlet' is a determinate of the determinable 'red'. The relation is transitive in that 'scarlet' is a determinate of 'coloured'; however, it is asymmetric in that being scarlet implies being red, but not *vice versa*.

One question that can be asked about this relation is, if something instantiates a property like 'coloured', a determinable, must it also instantiate a further property like 'red', one of its determinates? This question is crucial for my claim that certain properties of visual experience can be indeterminate. The sense in which I wish to use 'indeterminate' is to express the situation where a determinable is instantiated, but no determinate of that determinable is instantiated. To say that under certain circumstances visual experience can be indeterminate regarding depth and shape is to say that while under those circumstances it instantiates the properties 'extended in dimension z' and 'is a Euclidean shape', it does not instantiate a specific measure of extension or a specific Euclidean shape property – both determinates of the respective determinables.

One problem with what I have suggested is that intuitively the answer to the above question seems to be that such determinates of determinables *must* be instantiated: something that is coloured must be coloured in a certain way; something that is red must be a particular shade of red. This was a point capitalised on by Berkeley in his objection to abstract ideas. Funkhouser agrees with this intuition, although he is in principle prepared to make exceptions for cases of quantum indeterminacy:

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<sup>191</sup> See W. E. Johnson. *Logic, Part I* (Cambridge: Cambridge University Press, 1921), chapter 11.

An object instantiating a determinable must also instantiate some determinate under that determinable.... No object is merely colored *simpliciter*.<sup>192</sup>

This intuition is problematic for my claim that visual experience is indeterminate because it denies the very possibility of indeterminacy in the sense I appeal to.

There is, I think, a response to this objection. Whilst the intuition may hold good for the cases that most obviously leap to mind, viz. physical objects, it is not clear that the intuition holds for experiences. It is not obvious that when we characterise experiences, this characterisation must be given at the level of specificity and exactness we expect for a characterisation of physical objects.<sup>193</sup> Moreover, the view I am advocating for experience does not mean that such determinables should be thought of as being instantiated *simpliciter*. To say that a determinable can be instantiated *simpliciter* surely means something like that there is nothing to decide between any of the determinates falling under it: this would be a case of underdetermination, such as that appealed to by Hopkins. What is distinctive about my claim is that visual experience can become more determinate and, under normal circumstances, the determinates that become instantiated are preordained by the determinates of the physical object seen.

The suggestion I have made here about how to construe the claim that visual experience can become more or less determinate is, I believe, theoretically quite useful because it applies quite generally. It can account for the general fact that the qualitative character of visual experience changes under conditions of reduced visual information. Moreover, it can do so in such a way that does not generate situations to which we can apply Leibniz's Law. This is because it remains open to claim that at comparable levels of specificity the properties instantiated by the external objects and by the experience are the same.

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<sup>192</sup> Loc Cit.

<sup>193</sup> There is a question of whether there is some maximal specificity and exactness for physical objects. Funkhouser claims there is, David Sanford demurs. See Sanford, D. H. "Determinates vs. Determinables." *Stanford Encyclopedia of Philosophy* (October 2006) <http://www.seop.leeds.ac.uk/entries/determinate-determinables/> (accessed January 9, 2007). I leave it entirely open here whether this is the case.

## **Evidence From Illusions**

The evidence for Heelan's hyperbolic model discussed thus far concerns features of normal binocular experience – 'everyday experiences', as Heelan describes them. This comprised the main body of evidence for Heelan's conclusion. I believe I have shown that such evidence need not be taken to support a hyperbolic geometry. However, Heelan contrasts these 'everyday' experiences with a class of experiences he calls 'perceptual illusions' and attempts to draw further support for his hyperbolic model from such illusions. In the remainder of this chapter I will consider to what extent this is true.

Heelan appears to have in mind a number of the kinds of phenomena studied by psychologists, such as the Muller-Lyer illusion, the Poggendorff illusion, etc. In particular, he is not concerned with illusions resulting from damage to the optical and neural array; instead he is concerned with the class of illusions that seem to result from what R. L. Gregory has described as 'the use of inappropriate cognitive strategies', where the visual system misinterprets certain ambiguous figures.<sup>194</sup>

However, it is not entirely clear how this distinction between 'everyday' experiences, where physical objects appear other than they are, and 'perceptual illusions' is to be drawn. From the point of view of the argument from illusion, at any rate, they are on an equal footing – both classes of experience contain experiences where the physical objects appear other than they are. However this may be, Heelan thinks that the kinds of phenomena that psychologists study as cases of illusion in respect of geometrical form can provide ancillary support for his hyperbolic model of visual experience. The way he argues for this is to show that the transforms from physical space to the hyperbolic space of his hyperbolic model can account for some features of some versions of these kinds of illusions.

My aim in this chapter is merely to show the relevance of hyperbolic visual space to the problem of visual illusions and to show in what way

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<sup>194</sup> For a useful introduction to this idea see R. L. Gregory. *Eye and Brain: The Psychology of Seeing* (London: World University Library, 1966), chapter 9.

hyperbolic vision may actually be a factor in (some versions of) the paradigmatic cases mentioned above.<sup>195</sup>

Heelan distinguishes between three classes of illusions to be considered: those arising from two dimensional figures, those arising from rotations of three dimensional objects, and those arising from the appearance of distant three dimensional objects. This third class of illusions has already been covered in his discussion of everyday experiences – the experience of the ground and sky as a flattened vault is one example – so I will concentrate mainly on the first two classes of illusion.

### **Two Dimensional Illusions**

Heelan's account of the way his hyperbolic model for visual space gets support from two dimensional illusions depends to some degree upon a distinction he draws between how the two dimensional figures can be held to be related to some hyperbolic space. The two dimensional lines on paper can either be considered in respect of the transformation equations specified by Heelan's model, in which case the question is whether the transformation to a hyperbolic space accounts for the features of the illusion, or the lines can be taken themselves as constituting a representation of a hyperbolic space. In the latter case it is not the visual experience that is hyperbolic, rather as Heelan says, 'it is the illusionary space that is hyperbolic.'<sup>196</sup> By 'illusionary space' Heelan just seems to mean the space that the figure is a representation of.

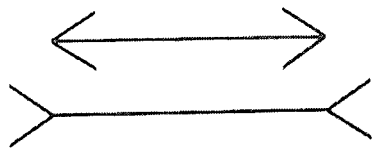
Now, direct confirmation of Heelan's hyperbolic model would be achieved if the transformation equations predicted the illusory phenomena, but Heelan thinks that indirect confirmation can also be achieved, if the figures are interpreted by the visual system to be representations of a space whose geometrical character matches that predicted by the hyperbolic model. This is perhaps best illustrated by means of one of Heelan's own, more detailed examples: the Muller-Lyer illusion.

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<sup>195</sup> Patrick Heelan. *Space Perception and the Philosophy of Science* (Berkeley and Los Angeles: University of California Press, 1983), p. 78

<sup>196</sup> *Ibid.*, p. 82

The Muller-Lyer illusion is a much discussed phenomenon, so I will be brief in my description of it. It consists of two physical lines of equal length, one ending with arrows pointing inward (the lower line in the figure below), the other ending with arrows pointing outward (the upper line in the figure below). The line with the arrows pointing outward appears shorter than the line with arrows pointing inward:



**Figure 3 - The Muller Lyer Illusion**

Given Heelan’s distinction between the two ways that two dimensional shapes can be related to a hyperbolic space, the first thing to consider is whether the transforms in Heelan’s hyperbolic model account for the difference in the apparent length of the two lines. If they do, then this illusion can be considered as directly confirming Heelan’s model. However, they do not: both lines and pairs of arrows will transform into the hyperbolic space without any significant change.

The first question that suggests itself is whether there is any feature of the visual transform of the Muller-Lyer figure (as a physical object) that could account for the illusion. The horizontal lines will transform with little significant change, likewise the arrowhead serifs and the reverse serifs.<sup>197</sup>

What this means is that this illusion does not provide direct confirmation of Heelan’s hyperbolic model. However, Heelan seems to think that his hyperbolic model could be confirmed indirectly if the two dimensional figures are taken to be a representation of a space. This is because, Heelan argues, the features of the illusion can be explained if the space the figure is taken to represent is considered to be hyperbolic.

Heelan claims that when the line with the outward pointing arrows lies above the other line, and both lines are in a horizontal orientation, the line with the outward

<sup>197</sup> Ibid., p. 84

pointing arrows appears to be at a greater distance from the observer than the other line. In this case, the two lines are being interpreted as a representation of objects arrayed in a three dimensional space. Now, because the line that appears further away also appears shorter, Heelan argues, the space being represented cannot be a Euclidean space. This is because it is in violation of Euclidean constancy scaling laws, which require that if the angle subtended by an object at the observer remains constant, it will be increasingly large at increasingly greater distances from the observer. In the Muller-Lyer case the two lines subtend the same angle, but the one that appears further also appears shorter.

Although, according to Heelan's interpretation, the Muller-Lyer illusion represents a space that violates the Euclidean constancy scaling laws, the space represented is in agreement with the scaling laws of a hyperbolic space. Objects in a finite hyperbolic space that subtend equal angles at the observer can be of different sizes – in particular, after a certain distance from the observer they gradually decrease in size.

... (in a finite hyperbolic space) at sufficiently large distances from the viewer, the visual size of an object may appear to be smaller than a nearby object of equal angular size.<sup>198</sup>

There are at least two problems with Heelan's attempt to use the Muller-Lyer case as confirming his hyperbolic model for visual space. The first problem is quite serious; quite bluntly, there is no reason to suppose that an explanation that accounts for how a two dimensional image could represent a non-Euclidean space confirms the hypothesis that visual experience has the geometrical properties of the space represented. If this is right then illusions could provide confirmatory evidence for Heelan's hyperbolic model for visual experience only if the illusion is accounted for by the transformation laws of the model.

The second problem concerns Heelan's interpretation of the Muller-Lyer illusion. Heelan's argument takes it that, in the orientation he has described, the line with outward pointing arrows looks at once further away and shorter than the other line. This is precisely the feature that fits finite hyperbolic spaces. However, it is

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<sup>198</sup> Ibid., p. 81



questionable whether this really is the case. This illusion has typically been interpreted as a case of application of size constancy to an ambiguous pattern of retinal stimulation. The line with outward pointing arrows is interpreted by the visual system to be shorter than the other line, because the direction of the arrows indicates it to be closer; as the lines subtend the same angle at the observer, they cannot be the same size. This is just the application of Euclidean constancy scaling laws by the visual system. Note that in this standard interpretation, the line is interpreted as *closer*, and therefore represented as shorter, contrary to Heelan's interpretation.

However, Heelan is right that because it appears shorter, the line with the outward pointing arrows can be taken to represent a line further away than the other line. But if we take Heelan's suggestion that the line with the outward pointing arrows appears further away to mean that the visual system can interpret it as being so, then there should be no surprise that the visual system should try to make it appear shorter than the other line. This is because it is just what the Euclidean constancy scaling laws would require when the lines they represent are actually the same length. What produces the ambiguous phenomenal character generated by the Muller-Lyer illusion is that both lines subtend the same angle at the observer. However, in both this case and the case where the line is interpreted as being closer it is the *Euclidean* constancy scaling laws that explains the peculiar phenomenal character.

I have suggested that the only even vaguely plausible way that illusions could provide confirmation of Heelan's hyperbolic model for visual experience if the transforms of the model themselves explain the phenomenology of the experience. There are three cases of two dimensional illusions that Heelan claims can be accounted for by the transformations of the model: the Hering illusion, the Poggendorf illusion, and phenomenal regression to the real object.

The Hering illusion consists of lines radiating out from a point, intersected by two horizontal parallel lines at equal distances wither side of the vertex of the radiating lines. The parallel lines appear to bulge in the region where they intersect the radiating lines, as in the figure below.

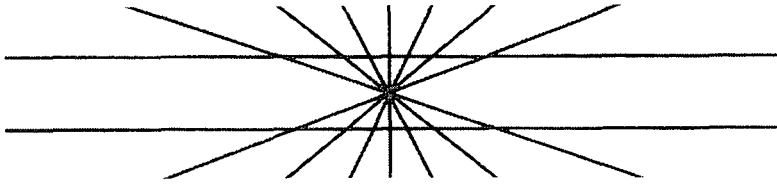


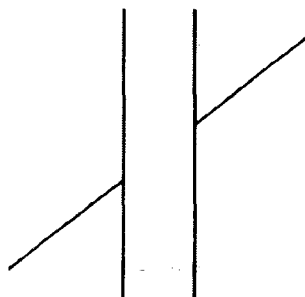
Figure 4 - The Hering Illusion

Heelan claims that the transform of this two dimensional figure, if it occurs in the distant zone, will result in a figure whose horizontal lines bulge in the way they appear to in the illusion.

...all of the figure's lines, radial as well as horizontal, will lie in one continuous two-dimensional surface that is the visual transform of the physical plane of the paper; on this surface, the visual shape of the figure will depend on whether it is found in the near visual zone or the distant zone of the viewer. If the figure falls in the distant zone, the shape of the horizontals will be bowed in the way the figure appears in the normal Hering illusion.<sup>199</sup>

However, the illusion usually occurs when the page with the lines on is relatively close to the viewer. However, it is true that as the page is drawn closer to the viewer the extent to which the lines appear curved is lessened, whereas the further from the viewer, the lines effect of the illusion is more pronounced.

The second illusion that provides direct confirmation of the hyperbolic model is the Poggendorf illusion. This is an illusion in which an interrupted diagonal line appears to be offset by the gap separating its two parts, shown in the following figure:



<sup>199</sup> Ibid., p. 82

**Figure 5 - The Poggendorf Illusion**

Heelan claims that the transformation of this figure in his model results in just this offsetting of the diagonal lines.

The Poggendorf Illusion can be understood as a result of such a transformation of the figure, *provided the plane of the figure lies in the near foreground relative to the true point.*<sup>200</sup>

He provides the following two diagrams to illustrate this, the second being a representation of the transformation of the first diagram.

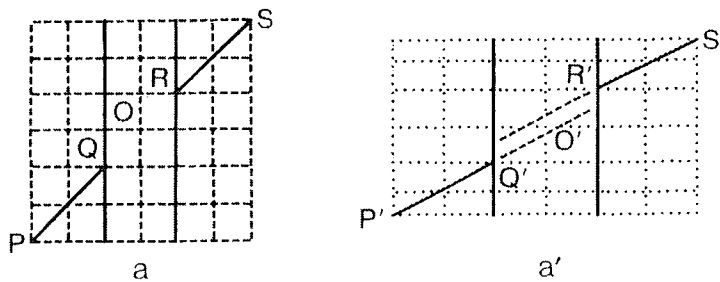


Figure 5.6: (a) Two-dimensional map of diagram of Poggendorff Illusion in a Euclidean plane; (a') possible two-dimensional map of the same diagram in a hyperbolic visual plane (based on table 4 of the Appendix).

**Figure 6 - Heelan's illustration of the difference between mapping the illusion onto a Euclidean plane and a hyperbolic plane**

The final illusion that provides direct confirmation of the hyperbolic model is that of phenomenal regression to the real object. This is a phenomenon that was studied by the psychologist R. H. Thouless by looking at the two dimensional shapes subjects judged to occlude a circular object tilted relative to the observer.<sup>201</sup> Surprisingly, when the instructions were suitably clarified to eliminate ambiguity over instructions using the word 'appears', the subjects tended to chose two dimensional shapes that did not match the occlusion shape, but instead chose a shape intermediary between the occlusion shape and the shape of the object being occluded. The

<sup>200</sup> Ibid., p. 86

<sup>201</sup> See R. H. Thouless. "Phenomenal regression to the real object, I and II" *British Journal of Psychology* 21: 339-359; 22: 1-30; also see C. W. K. Mundle. *Perception: Facts and Theories* (Oxford: Oxford University Press, 1971), pp. 16-19 for a critical discussion.

explanation of this phenomenon offered by Thouless was that the shape seen is the result of a compromise, at the sub-personal level, between retinal stimulation and knowledge of the real shape of the object.

Heelan rejects Thouless' explanation of the phenomenon, although it is really not clear why. This is all the reason he offers:

Such an account, however, is unsatisfactory because it rests on a metaphor and the abuse of categories.<sup>202</sup>

Instead, Heelan thinks that a simpler explanation is that the metric of visual experience is hyperbolic.

There is a simple solution in terms of visual space: what we see is a tilted oval object that is (or appears on the surface to be) congruent with the three-dimensional visual shape of a circular plate as construed in hyperbolic space.<sup>203</sup>

It seems fair to say that Heelan is confusing the description of a phenomenon with its explanation: in fact, there is no conflict of explanation here. Heelan is claiming that the description of what we are aware of visually satisfies the same geometrical description as does some object in a hyperbolic space. However, Thouless is offering an explanation of why what we are aware of satisfies that description and not the same geometrical description of either the physical object or the image on the retina.

The important question is whether the support that these illusions offer the hyperbolic model is convincing. I think that in light of the reasons for rejecting the more systematic non-veridical features of visual experiences that are not illusory in the present sense, we should deny that they offer any significant insight into the underlying geometry of visual experience. Instead we should treat them as what they are: individual illusions that can be dealt with by any adequate response to the general form of the argument from illusion.

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<sup>202</sup> Ibid., p. 90

<sup>203</sup> Loc. Cit.

## **Illusions and Rotations of Three Dimensional Objects**

The second class of illusions Heelan appeals to in support of his hyperbolic model concerns three dimensional objects that are rotated in space at a considerable distance from the observer. Take two wire mesh squares, linked by a rigid wire, and rotate them around a vertical axis perpendicular to the middle of the linking wire. The squares will either appear to glide flatly over each other, or appear to oscillate about a vertical so that they reverse direction every half turn. Heelan argues that these illusory effects are connected with the increased shallowness of depth in hyperbolic space the further one gets from the observer, a feature touched illustrated in Heelan's discussion of the Saarinen Gateway Arch of St. Louis. At a sufficient distance beyond Heelan's near zone, the depth of equal increments of the hyperbolic space decreases to the point where the only two possibilities for how the object could appear are (1) as a flat object that has a fixed height, but varying width, or (2) as a three-dimensional figure oscillating slightly.

...the shallowness of the space available would permit no more than the following two options: (1) the perception of a flat nonrigid object of fixed height and pulsating width, or (2) the perception of a rigid figure performing a shallow oscillatory motion about a vertical axis.<sup>204</sup>

It is claimed that these features of the hyperbolic space match what is observed in these illusions, and so provide some confirmation of the model.

Given that these illusions are dependent upon Heelan's claim that there is increased shallowness of depth in visual experience, we can discount these kinds of illusions as providing any confirmation of the hyperbolic model. We will need to resist describing the appearance of distant objects in the situation Heelan appeals to as flat or as oscillating slightly.

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<sup>204</sup> Ibid., p. 91

## **Conclusion**

In this chapter I have argued that neither Luneburg nor Heelan offer a convincing case for a hyperbolic geometry for visual experience. Although I defended the experimental evidence against the charge that the nature of the experiments is such that they cannot tell us anything about visual experience, I acknowledged that there were concerns about the nature of the instructions given to the subjects and the fact that the theory explains only a set of data obtained under highly unusual viewing conditions. These concerns leave room to question whether the results of the experiment are due to the ambiguous character of the instructions, or the geometrical character of visual experience.

In response to the concerns with the experiments Luneburg appeals to, I considered Heelan's argument that the phenomenal character of visual experience under normal viewing conditions confirms a hyperbolic geometry. Against both Heelan and Luneburg, I argued that the conclusion of a hyperbolic geometry turns on a tacit assumption that visual experience exhibits determinate spatial relations when that visual system is deprived of the information necessary to assign determinate spatial relations. I argued that no good reason had been offered for accepting this. I then provided an account of the sense of 'indeterminacy' I intended by appealing to the determinate-determinable relation between properties. I conclude then that none of the arguments considered in this chapter establish a hyperbolic geometry for visual experience.

## Conclusion

The purpose of this thesis has been to examine what the geometry of visual experience is; and in particular to vindicate the claim that the geometry is at least roughly Euclidean. This investigation was undertaken with an eye on two philosophical concerns, which served to focus the course of the inquiry. The first concern was regarding the ways and extent to which the question is an empirical one. In what respects and to what extent did the question fall within the remit of philosophical investigation? The question is of contemporary relevance, owing to the attempt by Yaffe, Van Cleve and Belot to provide an *a priori* argument for a spherical geometry. The second concern was related to the first in that there is a traditional philosophical concern about the directness of perception, which would be generated if the geometry of visual experience were other than that of physical space. This concern arose by acknowledging the challenge that the question of the geometry of visual experience posed for Direct Realism. That challenge arose from the recognition that it is possible to run the argument from illusion by appealing to the spatial properties of visual experience. Objects, or spaces, cannot be at once Euclidean and non-Euclidean, or be hyperbolic and spherical. By an application of Leibniz's Law, it follows that if the geometry of visual experience is other than that of physical objects, or physical space, then they cannot be identical. This identity claim is partly constitutive of Direct Realism: denying it implies the denial of Direct Realism.

I began by distinguishing between two forms of the argument from illusion: a general form and one particular form. The general form of the argument arises from the occurrence of perceptual illusion, irrespective of the class of illusory property. The particular form is generated by illusions in respect of particular classes of properties: the class of properties relevant to this debate were spatial properties. It was noted that one strategy of avoiding this particular form of the argument from illusion has been to dismiss traditional concerns about spatial properties by finding ways in which such properties can be made compatible with the real shape of external objects. I argued that this strategy is only plausible on the assumption that the appearance property is a Euclidean property. This fact was what made pressing the need to establish the

geometry of visual experience, because the success of such strategies depends upon whether the geometrical properties that correctly describe visual experience are properties from the same geometry as that which correctly describes physical space, or physical objects.

Two concerns needed to then be dealt with. One of these concerns was whether it was even possible for there to be a geometry of visual experience. I argued that there appear to be no good reasons to think that it is not possible. I argued that a number of reasons for denying such a possibility, which have been imputed to Berkeley, do not appear to be directed at this question. Moreover, those reasons are not convincing anyway. The other concern arose from the possibility that physical space is not Euclidean. This is a concern because the pressure on Direct Realism from the argument from illusion, based on the proposed non-Euclidean character of visual experience, depends upon the assumption that physical space is Euclidean. I argued that the possible non-Euclidean character of physical space does not affect the validity of this enquiry, because even if it is true then the local objects, those which we can see, are roughly Euclidean. This provides a way of testing for the incompatibility that gives rise to the concern about Direct Realism. If the geometry of visual experience is not roughly Euclidean, it cannot match the geometry of the world: if it is roughly Euclidean, this remains a possibility.

In light of this way of testing for such incompatibility, in chapter 2 I began by considering a number of the arguments that have been discussed in the literature for taking the geometry of visual experience to be Euclidean. Any such argument would also count as an argument for the claim that the geometry of visual experience is *roughly* Euclidean. The evidence that I discussed concerned the claim, taken from Strawson and Bennett, that the intersection of parallel lines seemed not to be possible in visual experience. I then considered an objection to this claim by Hopkins that there is no reason to claim, as Strawson did, that Euclidean geometry holds necessarily for visual experience. I responded, on Strawson's behalf, that what is crucial is the unfalsifiability of Euclidean geometry for visual experience. This does not imply the claim that the visual experience is necessarily Euclidean in character, in the sense that Hopkins means it: that it ranges over all possible sighted creatures. I observed, though, that this really warrants no more than the claim that Euclidean geometry is contingently true of visual experience.



Next I argued against two proposals, which take their starting point from Strawson or Kant, that there is no determinate geometry of visual experience. The first proposal, due to Hopkins, was that there is no *fixed* determinate geometry of visual experience. This claim was motivated by fact that there are restrictions on the acuity of vision. I argued that this is not good grounds for rejecting a determinate geometry of visual experience.

The second proposal, due to Reichenbach, was that the phenomenal character of visual experience underdetermines a geometry. He claimed that visual experiences take on a phenomenal character, and so determine a geometry only when certain tacit assumptions are made by the observer. I argued, with Hopkins, that there is no reason to assume that this is so, as there are no convincing cases of people being able to alter the phenomenal character of their visual experiences in this way, so as to have non-Euclidean relations.

In chapter 3 I turned to the first group of arguments that are intended to establish that the geometry of visual experience is a non-Euclidean geometry. I distinguished between arguments based on appeals to the phenomenal character of visual experience and arguments based entirely on *a priori* considerations; these were then dealt with separately. Those arguments based solely on *a priori* considerations take their inspiration from the justly famous chapter of Reid's *Inquiry* on the geometry of visibles, which I discussed in outline by way of introduction to the contemporary arguments.

These contemporary arguments aimed to establish a spherical geometry for visual experience by showing that there is some form of equivalence between spherical geometry and their proposed 'geometry of visibles'. These strategies involved identifying the eye with a single point in space. Yaffe offered a consideration supporting such an identification, to the effect that the lens of the eye performs the same function as a point in a pinhole camera: it collects and focuses rays of light onto the medium of projection. I argued that this reason is not compelling.

The general strategy of such contemporary arguments is first to define a number of terms that are to feature in the proposed geometry of visibles, such as 'visible line'; next to construct sentences of the geometry of visibles; then show that the proposed equivalency holds. The demonstration of equivalency is built up in stages: first sentences about visible lines are shown to be equivalent to sentences

about spherical lines; then sentences about visible angles and sentences about spherical angles; then the conjunction of these entails the equivalency of sentences about triangles.

The equivalency was partially established by showing that the following holds:

- (1) If a figure  $f$  is a spherical figure, then its visible angles = its real angles.

This, however, was not sufficient to establish the equivalency, as other, non-spherical figures may have their visible angles as equivalent to their real angles; i.e., that other geometries may hold of visual experience. Van Cleve offered a patch for this problem in the form of a conjecture:

- (2) If  $S$  is a surface such that any figure seen from  $e$  can be represented by a figure on  $S$ , then  $S$  is a sphere on  $e$ .

I argued that Belot's conception of equivalency, given in terms of a 'projective map' provides a justification for Van Cleve's conjecture.

However, in spite of the success in establishing the proposed equivalency, I argued that this tells us nothing conclusive about visual experience. This is because we have no reason for thinking that the 'geometry of visibles' is the right description of visual experience. The geometry of visibles is constructed simply by stipulating the meanings of the 'visible concepts', which looks like an act of *fiat*. As such, the argument is not successful in establishing anything *a priori* about visual experience.

I then moved on to consider Van Cleve's argument for the claim that the visible angles of triangles add up to more than 180 degrees. Van Cleve's argument differed from the others in that it does not begin by defining 'visible angle', so the preceding line of argument may not affect his argument. Van Cleve's argument attempts to provide a warrant to move from the fact that the visible angles of figures constituted by arcs of great circles centred on the eye can be greater than 180 degrees to conclusions about other figures. The warrant is based upon the further claim that such figures may be visually indistinguishable.

I argued that the move is not warranted: that the claims that Van Cleve makes about the visible angles of figures constituted by arcs of great circles are claims that the Direct Realist will want to accept. However, the Direct Realist will want to deny

that this has any significance for the visible angles of figures formed by straight lines. I argued that one way to block Van Cleve's argument is to deny the assumptions underpinning the move to conclusions about other figures. I argued that both such assumptions *could* be coherently denied, if we look beyond the static monocular case.

As a last point of consideration of the modern articulations of Reid's argument, I considered some of the attempts to reconcile a non-Euclidean geometry for visual experience with Direct Realism in such a way that the argument from illusion does not arise. The first of these attempts was taken from Yaffe's discussion, the second from Van Cleve's. Yaffe's approach was to *deny* that the geometry of visibles he offered is really non-Euclidean. This move was bound up with Yaffe's exegetical concerns about how to interpret Reid. I argued that that irrespective of how Reid conceived his own argument, if the modern articulations of the argument are successful, the conclusion must be that visual experience is genuinely non-Euclidean. This is because the possibility of hallucination means that the visible concepts pick out non-Euclidean properties, not Euclidean properties. Van Cleve's attempt at reconciliation turned on the suggestion that, while we accept the genuinely non-Euclidean character of the geometry, we relativise the non-Euclidean properties. I argued that this suggestion is highly counterintuitive, and it is not at all clear how the proposed 'relativised' properties are to be construed.

Having shown that the contemporary *a priori* versions of Reid's argument can be responded to, I turned in chapter 4 to look at the arguments for a spherical geometry that are based on phenomenological and empirical evidence. I distinguished between two kinds of arguments for a spherical geometry: those that appeal to features of visual experience; and one particular argument that appeals to the absence of marginal distortions in visual experience.

I began by considering an argument appealing to features of visual experience, which was offered by Angell. In order to establish a number of propositions from spherical geometry, Angell suggested that we consider extensions to our actual visual field beyond its actual limits at any given moment. I considered an objection to this device for demonstrating a spherical geometry, due to Van Cleve, to the effect that such extensions are metaphysically suspicious. I argued that claiming that visual experience consists of more than the momentary visual field, as given by the cone of light that enters the eye, could be warranted if we locate the first conscious upshot of

visual experience as occurring after the processing of multiple ‘momentary visual fields’ by the visual system. I argued that this does not, however, help in establishing Angell’s claim that the geometry of the resulting visual experience will be a spherical geometry. The main reason I offered for this is that Angell’s claim relies on only allowing rotation of the eye to generate a visual field greater than that of the momentary visual field. I suggested that there is no good reason for consenting to such a restriction.

Next I looked at two arguments from French for a spherical geometry, one of which appealed to the absence of marginal distortions in visual experience. I considered and rejected a number of forms of this argument. The strongest version of this argument I considered began with the observation that there are no marginal distortions in visual experience, or on the retina. Any continuous transformation of the retinal image would result in marginal distortions, so visual experience cannot be the result of such transformations that result in a different geometry, as there are no such distortions in visual experience. I argued that while this may be true, there is no immediately obvious reason for thinking that the transformations of the pattern of light falling on the retina by the visual system need be continuous, or to take as the input information for such transformations just the pattern of retinal excitation of one eye. There was therefore no reason to suppose that the geometry of visual experience need be the same as that of the image on the retina: i.e. spherical.

The other argument I looked at from French was a more conservative version of Angell’s argument. It consisted of reflecting on certain features of the phenomenal character of visual experience and claiming that they are best explained by a spherical geometry. The general feature that French considered was the apparent convergence of parallel lines at the far left and right of the visual experience. I observed that such features seem to be related only to static monocular considerations.

The rejection of purely monocular considerations would not, of itself, put us in a position to avoid the argument from illusion, and so avoid the worries it generates about Direct Realism. This is because there is a body of experimental evidence and philosophical argument which suggests a hyperbolic geometry for binocular visual experience. In chapter 5 I argued that the conclusion of a hyperbolic geometry for visual experience can be resisted.

I examined two sources of such an argument: the first was a body of experimental evidence from Rudolph K. Luneburg and his followers, based on a number of experiments conducted in darkened rooms. The second argument, due to Heelan, appealed to a number of claims about the phenomenal character of ordinary binocular experience, outside such artificial experimental conditions, that support the conclusion of a hyperbolic geometry.

Regarding the experimental evidence from Luneburg, I argued that such experiments should be construed, contrary to the view of Angell, as an attempt to provide the basis of a geometrical description of the phenomenal character of visual experience. Moreover, I defended the validity of the experiments as an investigation into the geometry of visual experience against an argument from French. He claimed that the only experiments that are valid for determining the geometry of visual experience are those which elicit responses from the subjects to projective features of visual experience. I argued that the only reason for supposing that this is true is the claim that visual experience is phenomenally two dimensional. This, as we have seen, need not be assumed and is contrary to the standing assumption of the approach taken by such experimenters.

In spite of the ways in which I defended the experiments from the criticisms of French and Angell, I acknowledged that the experiments appeared to involve very strange instructions. Also, as Gibson observed, Luneburg's theory only explained a small set of data, obtained under highly restricted viewing conditions. These two points raised legitimate concerns about how convincing the conclusion of a hyperbolic geometry should be taken to be. In light of this, I turned to the work of Patrick Heelan, who argued that there are many features of visual experience under normal viewing conditions, which are available upon introspection, that support a hyperbolic geometry. Unfortunately, Heelan's discussion involved a number of controversial or unclear presuppositions and terms of art. I argued that, in spite of this, it was possible to extract the core argument for a hyperbolic geometry from his discussion without committing to any of these parts of the discussion.

Heelan offered a hyperbolic model of visual experience; this model makes a number of predictions, based on mathematical transformations of the physical objects, of what the phenomenal character of hyperbolic visual experience would be. Heelan argued that these predictions are confirmed upon introspection of ordinary visual experience. There were two features of Heelan's claim that the phenomenal character

of visual experience confirms the model that I objected to. The first feature was his claim that the surfaces of physical objects appear in experience to be oriented toward the observer more orthogonal to the line of sight than they are. Underpinning this claim was the observation that visual experience is unlike a photograph. I argued that this observation does not provide good grounds for thinking that visual experience differs from a photograph in respect of its geometry.

The second feature of Heelan's argument for a hyperbolic geometry that I objected to was his claim that depth is foreshortened in visual experience. I argued that there is no good reason to think that units of depth in visual experience get shorter with physical depth. I argued that it is open to claim that depth awareness simply becomes less determinate with depth. This idea of variable determinacy was articulated in terms of the 'determinate-determinable' relation between properties, where determinates of determinables could be uninstantiated.

I argued that my claim of indeterminacy for visual experience coheres better than Heelan's with the psychological facts that there is decreasing information available to the visual system the further away objects are from the observer. I argued that this objection works equally well as an objection to the experimental evidence offered by Luneburg, thus vindicating the worries that French and Gibson raised for his theory.

Before concluding, it is worth highlighting one important, but not commonly emphasised feature of the debate that arises from acknowledging that there is a geometry of visual experience, irrespective of what it may be – Euclidean or Non-Euclidean, 2-dimensional or 3-dimensional. This is that a geometry provides a complete, systematic description of the spatial *structure* of visual experience. Visual experience is spatially structured, as are the experiences generated by other sense modalities: this quite simple fact has not been adequately appreciated. It at least casts some doubt on the plausibility of modelling perceptual experience along the lines of propositional thought. This is because it is of the nature of such propositional thought that it has a certain kind of structure, i.e. syntactic structure, which is different from that of perceptual experience. If the structure is different, it is at least plausible that the components that are structured are different also, which in the case of propositional thought are commonly held to be concepts. This strategy of emphasising the differences between perceptual experience and thought may provide some returns

for the debate about whether perceptual experience is helpfully modelled along the lines of propositional thought.

I have considered what I believe are the main lines of argument for a determinate geometry that is very different from the geometry of physical objects. I have offered a number of considerations against most of these in such a way that we can avoid the argument from illusion based on a non-Euclidean geometry for visual experience. I do not propose that I have *shown* that the geometry of visual experience is roughly Euclidean, although those considerations discussed in chapter 2 count in favour of this position. Nor have I shown that there is no possible argument for a geometry of visual experience that is not Euclidean geometry. However, insofar as I have shown that the main extant arguments for such a claim can be resisted I hope to have been successful in resisting one form of an attack on Direct Realism. So, have I shown this? Unfortunately, there does remain at least one area of difficulty for the attempt to resist an argument from illusion based on geometry that I have given in this thesis. This has to do with the phenomenological evidence that suggests a spherical geometry for static monocular visual experience. This evidence has not been adequately dealt with, as it can obviously be argued that static monocular visual experiences are perfectly good visual experiences. It is surely not acceptable to just arbitrarily rule out static monocular visual experiences as relevant to the question of the geometry of visual experience.

What is needed is some principled way of ruling out such data. Now, If Luneburg's argument, discussed in chapter 5, to the effect that static monocular visual experiences have no spatial structure were good then it would provide a principled rejection of static monocular experience, but as we saw in chapter 5 it is not. In recent philosophical and psychological literature there is a tendency to view movement as central to an adequate account of perception. Now, this alone will not be helpful here unless such an account also makes movement central to an analysis of the nature of perceptual experience – i.e. an analysis of the mental state, rather than the cognitive process. One recent attempt to do just that has been given by Smith, and in particular as a response to the general form of the argument from illusion. In this thesis I have not employed that approach – I restricted myself to attempting to account for the phenomenal character of visual experience in a way that does not generate

applications of Leibniz’s Law, but without recourse to a general analysis of perceptual experience. This result can be taken to highlight the limits of such an approach.



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## Appendix 1



Figure 2.2: (a) *Turning Road at Roche-Guyon*, by Paul Cézanne (Art Gallery, Smith College, Northhampton, Mass.).



(b) Photograph by John Rewald of the same motif.

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<sup>205</sup> Images from Patrick Heelan. *Space Perception and the Philosophy of Science* (Berkeley and Los Angeles: University of California Press, 1983).

