## *The relationship between visual illusions and cues to distance*

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# THE RELATIONSHIP BETWEEN VISUAL ILLUSIONS AND CUES TO DISTANCE

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A Thesis Presented for the Degree of Doctor of Philosophy

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## ABSTRACT

The theory of R. L. Gregory that certain visual illusions are caused by the inappropriate action of a constancy-scaling mechanism was critically examined. Several unsuccessful attempts were made to replicate his experimental findings that certain ambiguous figures, such as the M-L illusion, appear 3-dimensional in a particular way when presented in reduced cue conditions.

It was noted that the depth effect reported by Gregory was not large enough to explain all the illusory distortion in his figures. It was suggested that this might be because his apparatus allowed certain cues which could be used to determine the true form of the figures and thus destroy or reduce any 3-dimensional effects. The experimental results suggested that this was not so.

In later experiments it proved possible to repeat Gregory's results only by inducing Ss to adopt a specific perceptual set. If this was not done Ss tended to see the figures in different ways which often changed over time. Combined analysis of the results of all Ss on many different figures showed a slight tendency for the central part of any Gestalt or figure to appear nearer than other parts. Two possible hypotheses were advanced to explain this result but further experimentation suggested that both were inadequate.

Experimental evidence is provided that the Ponzo illusion is the result of a shrinkage of the lower line rather than an expansion of the upper line, as is generally thought. This and other evidence is interpreted as suggesting that even this illusion may not have a perspective component.

Taken as a whole the results suggest that any perspective theory of the illusions will prove inadequate. It is finally suggested that further research be directed towards inhibition type theories.

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## INTRODUCTION AND AN OVERVIEW OF GREGORY'S THEORY OF ILLUSIONS

## A SUMMARY OF GREGORY'S POSITION

A brief summary of Gregory's position is presented at this point in the hope that it will help the reader to keep in mind the theory as a whole and that he will therefore more easily discern the relevance of the ensuing discussion to its various parts.

Basic to the theory is the notion of size-constancy. This may be thought of as an internal scaling mechanism which allows size perception to remain constant even though an object's retinal image size changes with changes in distance. This involves the corollary that if two objects subtend the same retinal angle then the one with the greater apparent distance will be seen as larger. This is what is thought to happen in the illusion figures. Although they are flat, Gregory believes that perspective cues exist in them which trigger the constancy mechanism. Of course, in this case its functioning is inappropriate because no actual differences in depth exist, hence the distortions.

The older perspective theories were very similar to this in regarding the illusion figures as flat representations of 3-dimensional objects. However, they made no attempt to explain how flat figures - and subjects do report that the figures are flat - can trigger a mechanism which is assumed to work only when differences in apparent depth are seen. Gregory extends his theory to meet this difficulty and to this end he postulates that two independent types of constancy exist. He calls them primary scaling and secondary scaling.

Secondary scaling is thought to be the more normal type of constancy, well documented in the literature, which functions simply according to apparent distance - as set out in Emmert's Law (1881). Primary scaling is thought to be "set directly by depth cues, even when these are countermanded by background texture so that the figure appears flat." Were we to remove these "countermanding" background cues, the figures would then give the differences in apparent depth necessary to account for the distortions. This is testable by removing these other cues and measuring any differences in apparent depth which then occur. Back-ground cues are easily disposed of by using luminous models in a dark room and viewing them with one eye. A large part of Gregory's writing is devoted to establishing the independence of these two scaling mechanisms.

One further assumption is necessary to complete the theory. As mentioned above, illusion figures are regarded as flat representations of 3-dimensional objects, yet any illusion figure may give rise to several different depth interpretations e.g. the 'long' Muller-Lyer arrow can be interpreted either as an open book or as a house roof viewed from above. These two views place the 'distorted' shaft of the figure as alternately further and nearer than the rest of the figure. A strict perspective theory would demand that the direction of the illusion should reverse according to which interpretation was entertained. This does not happen. Gregory explains this by assuming that one particular view is more 'typical' than any other i.e. we experience it much more frequently than any other, and therefore it is this view which fixes the direction of the illusion permanently. This is known as the 'typical view' hypothesis.

The majority of this thesis is concerned with examining the validity of this theory.

## A NOTE ON GREGORY'S TREATMENT OF SOME OF THE OLDER ILLUSION THEORIES (See Gregory 1963 P 76-79)

It was not until the late I9th. century that scientists began to notice the discrepancies between appearances and actuality which we now refer to as the geometric illusion, and began to speculate as to their cause. Carter and Pollock (I968) have recently reviewed the early literature and traced the origins of the controversy which still continues today.

After an initial surge of activity, which lasted several years, interest died down somewhat. However, there has recently been a sharp increase in the number of papers published on the topic (Zusne, I968) and it would seem that Gregory's theory has been instrumental in this.

Paradoxically some of the new evidence has indicated that some of the older theories, which Gregory lightly dismisses, might not be so sterile after all. For instance, the eye-movement theory, first prompted by Wundt and others, has recently received a new lease of life from a paper by Festinger, White and Allyn (I968).

It has been well documented that the M-L and other illusions decrease with repeated exposure (Judd and Courten, I905; Mountjoy, I958, I96I, I963, I966; Mountjoy and Cordes, I958) while Lewis noted as early as I908 that eye-movements seemed necessary for this decrement to occur. With an exposure too fast for eye-movements the illusion remains undiminished.

Festinger et al. replicated these results and photographed eye-movements. Saccades across the perceptually short side of the figure were found to be shorter than those across the perceptually long side. Free inspection of the figure allowed the saccades to 'even up' but this did not occur if subjects fixated one point during the inspection period.

Festinger et al. propose that the illusions figure causes 'efferent instructions' to be drawn up which result in saccades that are too long on the 'long' side and vice-versa. If a subject makes actual eye-movements across the figure, it becomes obvious that the 'efferent instructions' are in error and they are modified accordingly, hence the slow decrement in the size of the illusion. If no eye-movements are allowed the error in the instructions remain undiscovered and the illusion maintains its size. This explanation accounts for the occurrence of the illusion with a stabilised retinal image (Evans and Marsden, I966; Pritchard, I958) and also its refusal to diminish with short exposures (Judd, I902).

However, as with most theories, there are still problems. Yarbus (I967) believes that eye-movements have nothing to do with the illusions - the change in eye-movements being caused by the change in the illusion and not vice-versa, and also the theory says nothing about why the illusion figure should cause the mistaken calibration of the efferent system in the first place. It might also be mentioned that Dewar (I967a) failed to reduce the illusion to zero, unlike the earlier studies of Judd (I902) and Lewis (I908), even after I,000 trials.

McLaughlin et al. (I969) dispute the conclusions of Festinger et al. in the light of previous experiments (McLaughlin, I967; McLaughlin et al., I968) in which they find 'efferent readiness' to have no effect on perception. However, they conclude that "these gross eye-movements may provide S with information which is at variance with the illusory percept - specifically information about the veridical lengths of the two line segments. In that indirect way, the eye-movements may cause the illusion to diminish; but this is a very different thing to saying that the illusions diminish because of a change in the way the eye-movements are programmed."

Gregory's theory would not predict any decrement of the illusions over time, nor would it predict any changes in eye-movements. An excellent paper by Cameron and Steele (1905) suggested that the Poggendorf illusion was the result of an amalgam of factors working together and sometimes opposing each other. It has been a tendency of theoreticians to attempt to explain all the illusions in one fell swoop. The more evidence we accumulate, the less possible does this seem. Gregory and Festinger both seem to have something to offer towards the final answer and their work should be thought of as complementary rather than mutually exclusive. There is much to be said for multiple determination.

Similar sentiments have been expressed by such as Farrimond (1968), Hotopf (1966), Robinson (1968) and Wagner (1963). Robinson says, "It seems that Hotopf (1966) is to a large extent correct in his claim that visual illusions are multiply determined." The latter two both think that Ganz's theory of retinal inhibition (1964, 1966) might explain the 'distortion' illusions but they cannot see it applying to the 'size - change' illusions. It is with the distortion figures that Gregory's case is weakest; with Ganz, it is the reverse, yet both try to stretch their theories in order that they might be able to encompass the whole and thereby prejudice their legitimate claims.

The eye-movement theory is not the only one to be passed over by Gregory after a perfunctory dismissal. The empathy theory, the limited acuity theory, the confusion theory and the pregnance theory are all treated in the same way. In these cases I can find no quarrel with his conclusion. However, his dismissal of the 'old' perspective theories is more questionable. He is correct in saying that Tausch's (1954) theory "does not suggest why or how perspective should produce distortions in flat figures," but in going further than this he falls into error. "The traditional perspective theory simply states that these figures suggest depth, and that if this suggestion is followed the more distant figures appear objectively larger. But why should suggestion of distance produce a change in apparent size? Further, why should suggestion of greater distance produce increase in size when distant objects are typically seen as smaller with increasing distance? The theory predicts not an increase but a decrease in the size of features having greater distance indicated by perspective, but this is the wrong way round." (1966). Now, it is true that as an object gets further away it subtends a smaller retinal angle. However, the object does not look smaller because of constancy, as Gregory very well knows. Constancy "is the tendency for objects to appear much the same size over a wide range of distance in spite of changes of the retinal images associated with distance of the object." (Gregory, 1963). Similarly, if two objects subtend the same retinal angle but one appears to be more distant than the other, as both Gregory and Tausch believe to occur in the illusion figures, then constancy allows us to correctly discern that the further object is larger. This is what Tausch meant and Gregory uses the same reasoning in his own theory. The only difference is that Tausch does not mention the magic word 'constancy'. Gregory's writings imply that because Tausch does not explicitly mention constancy then it ceases to function! This fallacious reasoning was first noted by Fisher (1968)[2]. Gregory has so far made no comment. A theory very similar to that of Tausch has been put forward by Kristof (1961)[1&2].

Having discussed Gregory's treament of his rivals, I now pass on to the examination of his theory.

## GREGORY'S THEORY EXAMINED IN GREATER DETAIL

Gregory's theory is superficially a very appealing one. Everything seems to fit together very nicely but a closer examination reveals a number of non-sequitors and a lack of experimental confirmation.

A major bone of contention is the typical view hypothesis. Note the use of the word 'typical' in the following quote. "The illusion figures may be thought of as flat projections of typical views of objects lying in 3-dimensional space. For example, the outward-going Muller-Lyer arrow figure is a typical projection of, say, the corner of a room - the fins representing the intersections of the walls with the ceiling and the floor - while the ingoing arrow is a typical projection of an outside corner of a house or a box,the converging lines receding into the distance." (1963).

Primary scaling is thought to be "set directly by visual depth features," yet we have noted that most illusion figures are ambiguous in this respect i.e. more than one depth interpretation is possible. However, the illusion does not change with a change in interpretation. To account for this embarassing phenomenon Gregory adds an appendage to his notion of primary scaling - "it is independent of the observer's perceptual set," (1968).[1] Constancy is only triggered according to one particular depth interpretation - even when we are consciously entertaining a different interpretation. How is this favoured view supposed to gain its dominance? Gregory is not very expansive on this point, merely saying that it is 'learned'. In a later paper he says, "features are selected for scaling according to early perceptual experience of the individual" (1968).[2] According to the evidence we have available it must be very early since illusions have been found by Segall et al.(1966) in children four years old. However, there is a wealth of evidence from both animal and human studies which indicates that the appearance of normal adult behaviour is often dependant on the organism obtaining a particular sort of experience during a critical period (Levine,1962; Dennenberg, 1962). Gregory's theory would seem to imply (although this is not explicitly stated by him) that here a critical period exists during which the direction of primary scaling is set. This would seem to require that most depth experience we have during this period is in accord with the typical view. However, human experience is extremely varied and in the general run of things one would expect a few individuals to emerge whose experience was atypical i.e. individuals whose depth experience during the critical period consisted of one of the alternative interpretations to which the illusion figures may give rise. These individuals would then see the illusion concerned in the reverse direction to most of us. To my knowledge no such person has ever been found.

The theory also implies that before and during the critical period the child would not be subject to the illusions. This is of course testable although the findings might be arguable on the grounds that the critical period is synonomous with the actual development of depth perception - in which case the non-illusion period would not exist.

Another implication of the theory is that children should be less influenced by illusions than adults. However, as Campbell (1964) notes this is not the case. "Data from a sample of Evanston (Ill.) children show the following progression in mean % of illusion for age groups from 4 to 9 years: 27%, 23%, 22%, 20%, 19% and 17%. This age trend is confirmed for almost all cultures, although culture-to-culture differences are in general larger for adults. The simplest application of our theory would have expected European adults, having had the longest influence of culture, to have had more illusion than European children. The outcome is thus puzzling and complicated." However, Segall et al. (1963, 1966) do not believe that the difficulty posed by these figures is insurmountable. They note that adults generally are less easily duped than children and that they are more skilled in the application of analytical techniques. "The child's first efforts to draw the box in front of him are impeded by his compulsion to draw all angles as right angles. It is only by effort and training that he learns to note what is in front of his eyes."

Of course, all the above is purely hypothetical. The only evidence that Gregory has produced so far is a rather unconvincing demonstration with a luminous cube (dealt with later) and the statement that, "Common sense is a fair guide to what is a typical perspective projection." (1967). An experiment by Pike and Stacey (1968) suggest that this is not true.

They used thirty Ss who viewed self-luminous Muller-Lyer figures monocularly in a dark room. The Ss were asked how the figures appeared to them and the results are shown below:-

| Figure seen as | Fins ingoing | Fins outgoing | Total |
|---|---|---|---|
| FLAT | 17 | 14 | 31 |
| 3-D | 13 | 16 | 29 |

Less than half the sample saw the figures in depth at all. When the figures were seen as 3-dimensional, most Ss (but not all) saw them as the perspective theory would predict:-

| | Fins ingoing | Fins outgoing | Total |
|---|---|---|---|
| Perception in accord | 8 | II | I9 |
| Perception not in accord | 3 | 2 | 5 |

Similar results were obtained by Green and Hoyle (I963). They presented a self-luminous Poggendorf display "under reduced cue conditions described by Gregory . . . . In fact, all 2I subjects reported the display in two dimensions only when asked for a free description of what they saw. When directly asked to describe the display 'as if it were' in three dimensions, most subjects were able to do so, although there was a considerable diversity of interpretations offered by the various subjects."

Hotopf reports, "Luminous models of the Muller-Lyer figures were constructed . . . These figures were presented in the dark to twenty-five subjects who viewed them monocularly at a distance of IO ft. None of these subjects saw both figures as Gregory's theory would predict; sixteen saw both figures as completely flat, and three saw some of the arrows as pointing in the wrong direction"(1966).

There is only one crumb of comfort that Gregory can extract from these results. All the estimates of depth are purely subjective. Only one study has used an objective method of measuring perceived depth in the reduced cue situation and that was done by Gregory himself. Not surprisingly his results were more congenial to his theory. However, Gregory has specifically predicted that, "If the illusion figures are presented to the eye with no visible background . . . they should be seen in depth according to their perspective characteristics," (I966)[1]. This prediction would seem to have been refuted by the evidence presented above.

However, a number of other attacks have been made on Gregory's theory which are totally unwarranted. They have arisen out of a basic misunderstanding of the nature of primary scaling and the typical view hypothesis, and can be refuted purely on theoretical grounds.

For instance, Brown and Houssiadas (1964) stated the following:- "In Ponzo's figure the same one of the two central lines appears longer, no matter whether it is seen as nearer or farther after rotating the figure through 180 degrees. Similarly when the top or bottom half of either forms of Hering's illusion is inspected, the parallel lines appear to be distorted, no matter which way the perceived depth or distance appears to be in the background field."

In a later paper (1965) they repeat the error. Subjects were asked, "In which direction does the illusory figure seem to lie? That is, if this were a 3-D picture, which part looks as if it would be nearer?" Thus the figures were presented as drawings and not with the background texture removed as Gregory requires. Hence it is no surprise that the results do not favour Gregory's interpretation. In fact, a close look at their figures shows that many subjects did not even see an illusion in the illusion figure! What sort of experiment is it that uses a Ponzo figure which produces no illusion in 15 out of 25 subjects?

Day (1965) also reveals that he has missed the point of primary scaling when he says that Gregory's theory is based on the assumption that apparent size is a simple function of apparent distance.

In 1968[2] Fisher was still making the same mistake. "These Muller-Lyer figures can be interpreted in a way diametrically opposite to that assumed by the inapproprate-depth theories;... .. the two components of the Muller-Lyer figure will alternate readily in depth. It might be expected that the direction of illusory distortion would change upon reversal, but this fails to occur." He even mounted an experiment using IOO subjects to show that this was not true. Not only has Fisher failed to comprehend an important part of Gregory's theory (the typical view hypothesis), in addition he has repeated the mistake made by Brown and Houssiadas even though it was patiently explained by Gregory at the time.

In later experiments (I970) Fisher shows a better grasp of the theory and makes some telling points but Gregory can still invoke the 'typical view hypothesis' to answer many of his points. For example, Fisher presented Ss with Ponzo and M-L figures in which the perspective element was ingeniously removed and showed that the illusion still persists. Gregory would not expect otherwise.

Hamilton (I965) was another who designed a complex experiment without fully developing the implications of Gregory's theory. He reasoned that since misapplied constancy was thought to be responsible for the illusory distortions then, "degree of illusion and degree of constancy should be significantly correlated;" however, no such correlation was found. Hamilton's mistake, as with the others, was to ignore the distinction between primary and secondary scaling. "Primary scaling is entirely responsible for the distortion illusions presented on textured backgrounds, ..... Secondary scaling is mainly responsible for constancy for normal objects ..... Since the effective processes are different we should not expect any high correlation between constancy for normal objects and distortions which occur when three-dimensional information is presented on a flat plane." (1966)[2]. An experiment very similar to Hamilton's has been done by J. Carlson (1966).

Lester (1969) believes Gregory's theory to predict a decrease in illusion when the S is further away....."when O moves away from the M-L figures the difference in apparent distance of the two lines will remain the same in absolute terms but will decrease proportionate to the distance between O and the apparatus. Since the size of the retinal image is proportional to the distance of the object from the eye, size-constancy should produce less and less of an apparent difference in the line lengths as O decreases the proportionate difference in apparent distance of the lines by moving away from the apparatus." This prediction was not confirmed in an experiment.

Lester's reasoning seems to be based on the assumption that Gregory predicts a distance difference between the two shafts of the M-L, i.e. that the apparently longer outgoing shaft should be seen as further than the apparently shorter ingoing shaft. In fact, as Pike and Stacey (1968) point out, Gregory only predicts depth differences <u>within</u> the two figures - he makes no prediction at all concerning their relative depths.

Objections such as the above arose from misunderstandings of the theory. However, other points have been raised which are not so easily dealt with. Gregory claims that his theory can help to explain many different sorts of illusion, including the Orbison and Hering figures. It will readily be appreciated that these are a different sort of figure to the Ponzo and Muller-Lyer - in the former straight lines appear curved, while in the latter it is their length that is wrongly perceived. The one class may be referred to as the distortion illusions, while the other may be referred to as the size-change illusions. According to Gregory, the Orbison figure is seen as a cone, i.e. its 'typical view' is that the middle of the figure is seen as nearer than the margins. Any line drawn on the surface of the cone such that its two ends touch the base, which projects a straight image on the retina, is in fact, bowed. Were the figure to be interpreted as a tunnel, the direction of bend would have to be in the opposite direction; however, in the illusion figure the direction of distortion does not change, it is always appropriate to the 'cone' interpretation, even when the circles are deliberately spaced so as to give a 'tunnel' effect, as done by Green and Stacey (1966). At first sight this might appear to be the same argument that used concerning the non-reversal of the Muller-Lyer illusion. In fact, there is a slight difference. In the case of the Muller-Lyer, the <u>same</u> figure was used for both 3-dimensional interpretations. In the Orbison figure as illustrated by Green and Stacey two <u>different</u> figures are used in which the typical perspective interpretations would appear to be opposite, i.e. the 'typical view' of each figure should be different. However, as with the Muller-Lyer, the distortion refuses to reverse. However, who is to say how much change is necessary to counteract the established typical view and to replace it with another? The typical view would seem to be dependant on primary scaling, not perspective, and all that is being changed by Green and Stacey is perspective.

Perhaps the best way to resolve this dilemma would be to remove the background from the two figures and objectively test subjects' perception. If the 'tunnel' figure gives a 'tunnel' depth effect, then Gregory's theory would seem to have been refuted. If both figures gave a 'cone' depth effect, which is by no means impossible, the theory would be strengthened.

Humphrey and Morgan (I965) have made much the same point as Green and Stacey but neither performed the crucial test. Wallace (I966) was also on the same track when he published two figures "in which the perspective effect was the same but opposite distortions are produced". One was a Hering figure and the other was an Orbison figure. Wallace has no objective evidence for his statement that the perceptive is the same in both figures (although this may well be the case) and his argument loses weight accordingly.

Houck, Mefford and Wieland (I969) have done experiments with the Ponzo under reduction conditions similar to Gregory's. They found that Ss reported the apparent depths of the constituent lines to fluctuate in a similar manner to a Necker cube, but the line nearest to the vertex always seemed longer no matter at what distance it appeared to be relative to the other line. They concluded that Gregory's theory was inadequate.

Gregory's tendency to attempt to spread his theory over the widest possible area has brought him problems on other fronts. Illusions have been found in situations in which a depth interpretation is difficult, if not impossible. Day mentions the dumb-bell Muller-Lyer and Hotopf the 'Australia' Poggendorf. On the other hand, Fisher (I968)[3] has experimented with figures which he argues, convincingly I think, are just as open to depth interpretation as the conventional Muller-Lyer and should therefore produce similar illusory effects. Although he found this not to be the case, (there was no illusion at all) his case would have been much stronger had he presented these figures in Gregory's reduced cue situation without a textured background and shown that they did give rise to three-dimensional perceptions in the same way as the conventional Muller-Lyer figure. (Figures as in Fig. I:I).

Jeffrey (1968) has made the point that "nearly any 2-dimensional extensions to each end of a line .....increase its perceptual length." The existence of illusion figures in which a depth interpretation would be unlikely (the aforementioned dumb-bell Muller-Lyer, for instance) suggests that even if Gregory's theory were accepted it would not provide the whole answer.

One of the most thorough assessments of Gregory's theory is that of Hotopf (1966) and he has raised some further questions which Gregory has difficulty in answering. For example, he notes that in the Zollner illusion the transversals, which are supposed by Gregory to provide the perspective cues, do not in fact do so .... "As the lines get further away, the angle of the transversals should change and they should come closer to one another. Since this does not happen, the transversals are not providing the perspective cues i.e. the gradation, that primary scaling demands. The illusion occurs indeed even when the angles of the transversals alternate between 30 degrees and 40 degrees."

Following this, Hotopf gives another example of a set of figures in which he claims Gregory's interpretation is inconsistent with what is seen. "It is possible to combine the Muller-Lyer figure with Hering's and Wundt's figures, if the former is slightly modified.

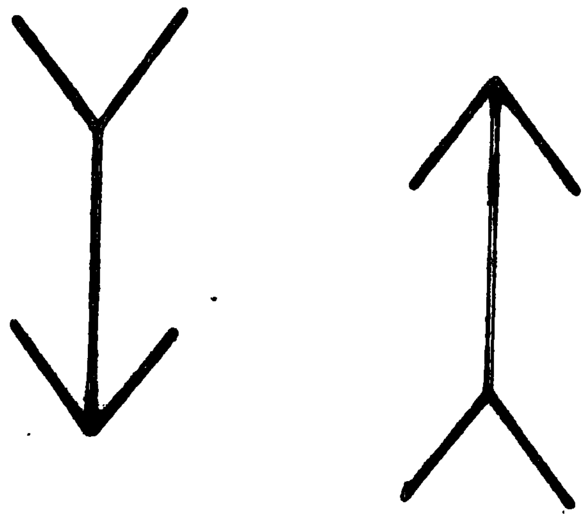


Fig. 1.1 - Figures used by Fisher (1968)[3]. He believes Gregory's theory should predict illusory distortions in them.

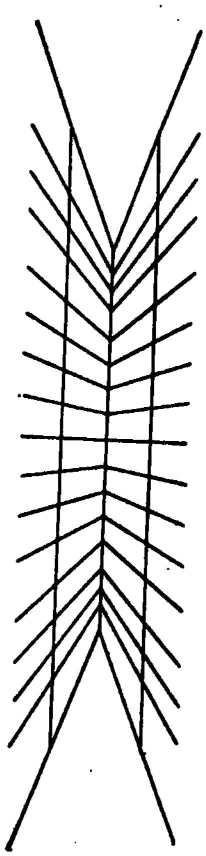


Fig 1.2

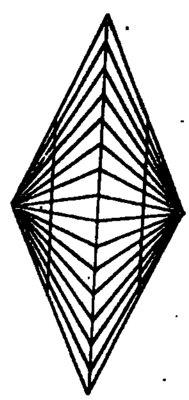


Fig 1.3

It is clear that both pairs of illusions work. In Fig. I:3 (Wundt's figure and the shorter Muller-Lyer) the two illusions can be explained by Gregory's theory. But the same explanation cannot be used for Fig. I:2 (Hering's figure and the longer Muller-Lyer). If the vertical line in the centre of the figure looks longer than its counterpart in Fig. I:3 because it is seen as furthest from, instead of nearest to the observer, the observer's distance from the parallels must increase as they rise above or fall below eye-level. By Gregory's principle, they should therefore seem wider apart as in Fig. I:2, but they are seen as nearer." However, one can never be sure of Gregory's exact position because he never states it clearly for these figures. All he does is to throw out vague suggestions, e.g. "It seems possible that the curvature distortions given by radiating background lines (e.g. Hering's and Wundt's illusions) should be attributed to mis-scaling from the spherical perspective of the images on the hemispherical surface of the retina to the effective linear perspective of perception. The distortions are in the right direction for such an interpretation, but precise experiments remain to be completed." (1968)[2]. The last phrase is unfortunately true in more situations than this.

However, no amount of wriggling can avoid the dilemma set by Hotopf's third point. By adding transversals to a von Bezold circle the shape of the circle appears to change. The transversals do not change the perspective of the circle at all so Gregory would not predict any change at all. Hotopf notes that this distortion, like the others he mentions, are all consistent with a regression to right angles hypothesis. This tendency seems to occur in all situations but unfortunately, its mere statement does not serve as an explanation.

On one point, however, Gregory does give a complete answer. Hotopf suggests that since the M-L illusion is at its strongest when the angle enclosed by the arrow is $60^{\circ}$ and that this is a narrower angle than could be projected on the retina from a corner of $90^{\circ}$, then "a more likely interpretation that is still in accord with the perspective theory would be that the illusion is determined by our experience with wedges, doors open at an angle and so on, rather than by 'typical views' such as corners in houses." This is much the same view as was put forward by Thiery (1895, 1896), who regarded the M-L arrows as a drawing of a saw-horse seen in three dimensions. Gregory explains that "Thiery's choice of a 'saw-horse' (a horizontal beam supported on legs forming triangles at each end) is a poor example for the legs are not at any specific angle, such as a right angle. He may not have seen that for perspective to serve as a depth cue, reliable assumptions about angles must be possible. The legs of a saw-horse can be at almost any angle, so it is not a good example of depth being given by perspective projection" (1968). Although, this does answer Hotopf's question of "why not wedges?", it does not answer his assumption that $60^{\circ}$ is a narrower angle than could be projected on the retina, from a corner of $90^{\circ}$. Gregory has chosen to ignore this point.

From the above it is apparent that although some criticisms are unjustified, the typical view hypothesis has a lot to answer. A few simple experiments would clear up some points but there seem to be other questions which find the theory inadequate. Objective evidence supporting the theory is conspicuous by its absence. However, Gregory has quoted some anthropological studies in support of his theory. These are considered next.

## THE ANTHROPOLOGICAL ARGUMENT

"Western societies provide environments replete with rectangular objects; these objects, when projected on the retina, are represented by non-rectangular images. For people living in carpentered worlds, the tendency to interpret obtuse and acute angles in retinal images as deriving from rectangular objects is likely to be so pervasively reinforced that it becomes automatic and unconscious relatively early in life. For those living where man-made structures are a small portion of the visual environment and where such structures are constructed without benefit of carpenter's tools (saw, plane, straight edge, tape measure, carpenter's square, spirit level, plumb bob, chalk line, surveyor's sight, etc.) straight lines and precise right angles are a rarity. As a result the inference habit of interpreting acute and obtuse angles as right angles extended in space would not be learned, at least not as well." (Segall, Campbell and Herskovits, 1966).

The above is a good summary of the 'carpentered world' hypothesis. Segall et al. trace this theory back to Sanford (1908) and beyond, and it seems to be one of the roots from which Gregory's theory grew. He refers to the findings of the Segall group on several occasions and makes particular reference to the Zulus. "We may ask whether people living in other environments, where there are few right angles and few parallel lines, are subject to the illusions which we believe to be associated with perspective ..... The people who stand out as living in a non-perspective world are the Zulus. Their world has been described as a 'circular culture' - their huts are round, and have round doors; they do not plough their land in straight furrows but in curves and few of their possessions have corners or straight lines. They are thus ideal for our purpose. It is found that they do experience the arrow illusion to a small extent, but they are hardly affected at all by the other illusion figures." - (Eye and Brain, p.160-167).

"It has been known for 60 years that people who live in environments largely free of right angular corners and parallel lines - such as Zulus, who live in a 'circular culture' of round huts - do not suffer these distortion illusions." (Nature, 1965, underlining mine).

"The Zulus live in a 'circular culture' of round huts, with few corners or straight parallel lines, and they see the figures with very little distortion (Segall et al., 1963)" -(New Horizons, p.80).

I repeat the point to show that it is not an isolated comment on Gregory's part. His assertions about Zulus' susceptibility to illusion is, at best, highly misleading as reference to the results of Segall et al. will show. Zulus show an 11.2% illusion to the Muller-Lyer, while South African Europeans show 13.5%. With horizontal-vertical ( T ) the figures are 9.5% to 15.0%; with the horizontal-vertical ( L ), 7.8% to 5.0%, i.e. the Zulus show a greater illusion than their western cousins. This happens also in the Sander's parallelogram when the figures are 18.5% to 17.4%. It is difficult to comprehend where Gregory got his impression that Zulus "do not suffer these distortions" since Segall's is the only study to which he refers in his bibliography.

It is all the more amazing that this deception should have gone unchallenged for so long. Nor is this the end of the matter. Campbell (1964) explained the difficulties in administering the tests to natives with whom direct verbal communication by the test administrator was often impossible. If their responses differed considerably from the western norm one would assume that they had misunderstood the test, therefore the possibility that they might perceive the figures in an <u>entirely</u> different way from us is ignored. They can be slightly different but not too different. Obviously this is unsatisfactory but there is no alternative; it is considered better to err on this side than to accept differences caused by irrelevant factors. With this in mind it was decided to discard any subject who responded in a different way to the same stimulus figure on more than one occasion. "By these strict standards, 10% of the Evanston cases were lost, 22% of the bushmen, 18% of the European South Africans, 9% of the Basongye, <u>65% of the Zulu</u>, and so on. (Underlining mine). Discarding cases is a dirty business, rightfully suspect." Surely generalising to the whole population when the results from almost 7 out of every 10 of them have to be ignored is a very chancy affair. Even if Segall's results had supported Gregory they would still have been questionable on these grounds.

It seems certain that Gregory can never have read Segall's completed work otherwise the following could scarcely have avoided his notice:- "Since relative non-scalability is characteristic of two societies (Zulu and Sengalese) over all five illusions we might wish to question <u>all</u> Zulu and Sengalese sample scores." And also:- "From these results it is clear that the Evanstonians were significantly more susceptible to the Muller-Lyer illusion than any of the non-western samples. The same is true of the North-western University students and the S.A. Europeans, except for the fact that the Sengalese and Zulu means were not significantly different from the two Western sample means"......."The Evanston children had significantly higher scores on the Muller-Lyer illusion than did <u>all</u> the non-western groups of children, except for the Sengalese and Zulu children."

Although Gregory only refers to the study of Segall et al., it is quite possible that he based his comments on the earlier work of Allport and Pettigrew (1957). Note the similarity between this quote and the one taken from Eye and Brain, mentioned earlier:- "Zulu culture is probably the most spherical or circular of all Bantu cultures, possibly the most spherical of all native African cultures ..... Huts are invariably round ..... Fields follow the irregular contours of the rolling land and never seem to be laid out in the neat rectangular plots so characteristic of western culture.... cooking pots are round or globe shaped ..... It is commonly said in Natal that Zulus straight from the reserves cannot plough a straight furrow and are unable to lay out a rectangular flower bed. Linguistically the same bias towards circularity is seen. While it is possible to say 'round'in Zulu, there is no word for 'square'."

However, Allport and Pettigrew did not use any of the illusion figures whose results Gregory claims to be able to explain. They used Ames' trapezoidal window illusion. They compared two groups of rural Zulus with a group of urban Africans and a group of Europeans under four different viewing conditions i.e. 10 feet binocular, 10 feet monocular, 20 feet binocular and 20 feet monocular. The subjects were asked to describe what they saw and they were questioned until the experimenter was satisfied as to whether they saw the illusion or not. The results were as below:-

| | Nongama Rural | | | Polala Rural | | | African Urban | | | European Urban | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Yes | No | Unc. | Yes | No | Unc. | Yes | No | Unc. | Yes | No | Unc. |
| 10' bin. | 3 | 17 | 0 | 4 | 14 | 2 | 13 | 7 | 0 | 11 | 9 | 0 |
| 10' mon. | 14 | 6 | 0 | 16 | 4 | 0 | 19 | 1 | 0 | 19 | 1 | 0 |
| 20' bin. | 8 | 12 | 0 | 17 | 1 | 2 | 16 | 3 | 1 | 16 | 4 | 0 |
| 20' mon. | 18 | 2 | 0 | 17 | 2 | 1 | 18 | 2 | 0 | 19 | 0 | 1 |
| Totals | 43 | 37 | 0 | 54 | 21 | 5 | 66 | 13 | 1 | 65 | 14 | 1 |

Nongoma is a more 'isolated' area than Polela. Note that there are no differences between Urban Africans and Europeans nor between the rural Zulu groups except that there is a tendency for the Polela results to lie between the Nongoma and the Urban. The monocular data is very similar over all samples at both distances but this is not so with the binocular data. Only 7 out of 40 rural Africans see the illusion at 10 feet binocularly. At 20 feet the Polelas are little different from the Urbans, but the Nongomas still have a majority against the illusion. This seems to be strong evidence that experience with windows is important for the development of the illusion but there are no groundsfor saying that Zulus do not see the illusion. Under optimal conditions 90% of the Nongoma sample, whose experience of rectangles was considered minimal, saw the illusion. However, as more cues become available to assist one in making the correct perception, they are more likely to use them. Just to check. a second set of results were obtained. Both groups were Zulus. When asked, "What is this?", 67% of the urban sample replied 'window', but only 26% of the rural sample said it. The second set of results are shown below.

| | Ceza Rural | | | Lamontville Urban | | |
|---|---|---|---|---|---|---|
| | Yes | No | Unc. | Yes | No | Unc. |
| 20' mon. | 22 | 2 | 0 | 20 | 0 | I |
| IO' bin. | 2 | I8 | 4 | I4 | 7 | 0 |
| Totals | 24 | 20 | 4 | 34 | 7 | I |

Freeman and Pasnak (I968) claim that the interpretation of the trapezoid as a rectangle does not depend on past experience but is caused more by "ambiguity of cues in the retinal image," which is not quite the same thing. They also point out that the trapezoid figure does not have to be made to look like a window for the illusion to occur. However, this observation does not carry much weight since, even without the requisite shading, the trapezoid is still thought of as rectangular by most observers.

Haber (I965) found that the incidence of the illusion fell from 90 % to 40 % when his Ss discovered the trapezoid's true shape, hence experience produced a marked modification of perception but did not destroy the illusion completely.

Allport and Pettigrew concluded that their results could not be used to decide the nativist - empiricist controversy i.e. whether the illusion was the result of learning or whether it was inborn. However, Slack (I959) criticised this conclusion strongly. He believes the results to be in accord with a strongly empiricist theory. "The most important conclusion from the study is that the strong nativist position is no longer tenable." He argues convincingly that given the relative biases of the two groups, i.e. rectangularity as against circularity, in an illusion demanding a choice between rectangularity and trapezoidy, one would expect an illusion in both groups under optimal conditions. Only if you substituted a bias towards trapezoidy for the bias towards circularity, would you expect no illusion for this group under any conditions.

Thus we may conclude that for this illusion the carpentered world hypothesis is certainly valid. However, this is a very different illusion from the sort that Gregory seeks to explain. Although Gregory's use of the theory is puzzling to say the least, that is not to say that it is not applicable to the other illusions. Therefore I will consider Segall's development of it since it is rather more comprehensible than Gregory's. However, it is interesting to note that they quote an early paper by Gregory and Wallace (1958)as evidence that we must learn to perceive. In this study tests were performed on a man who had virtually been blind since birth but who recovered his sight in middle age when his cataracts were removed by surgery. This study is also mentioned by Gregory as providing an early guide to his subsequent theory. Segall et al. were particularly interested in this finding that the subject was "apparently unable to perceive depth in real space accurately" and that as regards the Hering, Zollner, Poggendorf, Necker cube and Muller-Lyer figures "the patient displayed no illusion susceptibility at all or a degree of susceptibility considerably less marked than that typical of normal observers."

Unlike Gregory, they give a full explanation of how their theory may be applied to the figures which they are considering. Their hypothesis for the Saunders parallelogram runs:- "This bias is understandable if one perceives a non-orthogonal parallelogram drawn on a flat surface extended in space. Given such a tendency it is clear that the represented distance covered by the left diagonal is greater than the represented distance covered by the right diagonal.

A tendency such as this constitutes a habit of inference that has great ecological validity - and great functional utility - in highly carpentered environments. Western societies provide environments replete with rectangular objects; these objects when projected on the retina are represented by non-rectangular images."

For the Muller-Lyer the reasoning is different. "In this instance the two main parts of the drawing represent two objects. In Fig I:5, for example, if the horizontal segment were perceived as the representation of the edge of a box, it would be a front edge, while in Fig. I:4, if the horizontal segment were perceived as the edge of another box, it would be the back edge along the inside of the box. Hence the right hand horizontal would 'have to be' shorter than the drawing makes it out to be and the left hand horizontal would 'have to be' longer." Actually, as Pike and Stacey (1968) have pointed out, this is slightly different from Gregory's interpretation and leads to a different prediction in their experimental situation. This will be referred to later.

As far as the Horizontal-Vertical illusion goes, the vertical component is thought of as representing something stretching into the distance. Forest dwellers who would not have much experience of this sort of situation ought, therefore, to experience less illusion than savana dwellers. Note, however, that this explanation says

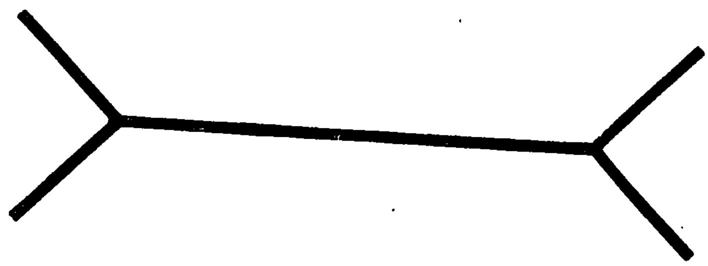


Fig 1.4 The back edge of a box.

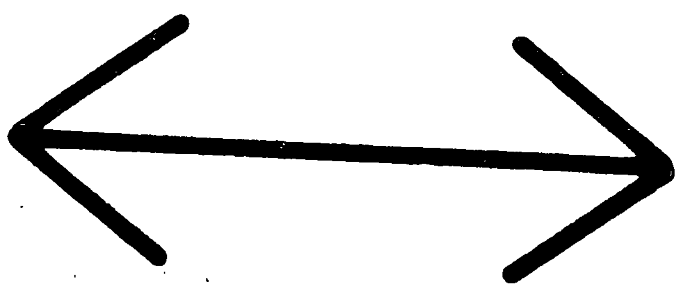


Fig 1.5 The front edge of a box.

nothing about the intersection component of this illusion which cannot be explained in this way.

Both the Segall et al. and the Gregory explanations depend on 2-dimensional drawings being interpreted as if they lay in 3-dimensions and both mention that images on the retina are in fact 2-dimensional. Only Segall, however, is gracious enough to attribute the observation of this fact to Gibson (p.2, 1950) - "The physical environment has three dimensions; it is projected by light onto a sensitive surface of two dimensions; it is perceived nevertheless in three dimensions. How can the lost third dimension be restored to perception? This is the problem of how we perceive space." In this respect it is interesting to note that most primitive peoples have difficulty in interpreting photographs or drawings in three dimensions. Evidence suggests that this ability is learned (Leibowitz et al., 1969) and a study by Hudson (1960) concluded that "cultural isolation was effective in preventing or retarding the process, even in candidates possessing formal education of an advanced level." Segall et al. acknowledge this fact (p.32-34) but seem unaware of the problem it sets them. If the interpretation of two-dimensional drawings as three-dimensional objects is as important a factor as they would have us believe, how come people incapable of this see illusions at all? Gregory is covered, as usual, by his distinction between primary and secondary scaling.

Morgan (1959) found that black South African mine labourers were as much affected by a perspective illusion (Ponzo) as were a group of white graduates. Yet Hudson found mine labourers to be incapable of three-dimensional responses to perspective drawings. Does this show that the Ponzo is not a perspective illusion or that Hudson was mistaken about the abilities of his sample? Obviously there is scope here for further investigation. It would certainly be illuminating if one could find a group which was not affected by, say, the cylinder illusion, but still saw the Ponzo illusion.

In this context it is interesting to note that Segall's results show that his Africans generally seem to experience a greater H-V illusion than do Europeans. Since Europeans are obviously more experienced at interpreting 2-dimensional drawings in 3 dimensions then it would seem that this illusion does not have a perspective component. The results of Avery and Day (1969) support this view. They presented the illusion to Ss in various orientations and found the illusion to occur only when one of the lines fell on the vertical meridian of the eye. Kunnapas (1953, 1955, 1957, 1958) has shown that this illusion is subject to influence by such things as the shape of the frame in which it is presented and other factors which would seem to have nothing to do with perspective.

In a futher experiment, Day and Avery (1970) made and confirmed the prediction that if their 'vertical meridian' theory was correct then the illusion should be a purely visual phenomenon and should have no haptic equivalent. A perspective theory would also predict this for all illusions.

However, returning to the theory of Segall et al., we find that they make certain testable predictions. That peoples whose cultures contain few rectangles will be less susceptible to illusions such as the Saunders parallelogram and the Muller-Lyer than will Western races. That people who live in forests will be less susceptible to the H-V illusion than will peoples who live in open country.

The data does fit the theory, but only in a gross manner. Europeans do seem to experience the Muller-Lyer to a greater extent but there are some odd discrepancies. As previously noted, the Zulus experience the illusion much more than many more angularly cultured tribes, while the Bete, from a 'moderately carpentered environment' rank very low. In the Sanders figure the issue is less clear cut but still favourable, while of the H-V, Campbell says ......."it is comforting to note that the Bete at the bottom have a jungle environment, and that the Batare and the Banjankole live in a high open country. But in detail the data do not fit well. The Bushmen should be at the top; the Zulu should be much higher."

It is also mentioned that results obtained by Bonte (1962) were not consistent with those of the Segall study and despite doubts about her experimental technique they conclude ......"the issue remains a puzzling one. Of the many idiosyncrasies in her technique few specifically point in the direction of the results she obtained." She found no significant differences between Europeans, Bambuti pygmies and Bashi on the Muller-Lyer. These two tribes live in round houses and should not have shown much illusion at all. Neither tribe was tested in the Segall study. Despite these deviations, Segall is able to conclude, "We know of no other hypothesis comparably plausible."

However, some recent work by Pollack (1967) may be of great significance in this respect. He seems to have good evidence for linking the size of Muller-Lyer illusion experienced with the density of pigmentation of the Fundus occuli. As a rule Negroes are more deeply pigmented than whites, but this is not always the case and Pollack used a sample in which were included two whites who were judged to be deeply pigmented and one negro who was judged to be lightly pigmented. "The results were clearcut. The more deeply pigmented Ss produced smaller illusion magnitudes than the lightly pigmented Ss. The difference was significant ($t=3.89$, $d.f.=33$, $p.<.01$). Biserial correlation of optical pigmentation with illusion score yielded a coefficient of -.745. The single negro judged as lightly pigmented indicated an illusion magnitude of 3.55 mm., somewhat smaller than the lightly pigmented mean but greater than that for six of the white boys with light pigmentation. The two whites judged as being deeply pigmented showed magnitudes of 3.15 mm. and 2.55 mm. Four of the Negro boys had means larger than 2.55 mm. Thus optical pigmentation rather than race membership seems to be the more important variable affecting sensitivity to the illusion." Pollack used 14 Negroes and 21 whites as subjects and they were equated as closely as possible for school grade, intelligence, etc.

Segall has heard of Pollack's early work but not of the paper quoted above since they "do not find the explanation very plausible ......It depends on an unchecked hypothesis relating sunlight and corneal density and it does not explain the direction of our results with the H-V illusion." This last comment is rather unfair, since, with regard to the H-V, Segall himself concludes, "that the processes underlying this class of illusion are different from those that underlie the Muller-Lyer and Saunder illusions." Personally, I would regard Pollack's explanation as far more plausible than that of Segall. Granted it says nothing about <u>how</u> corneal density affects perceived illusion size but it involves much less in the way of far fetched assumptions. Obviously Pollack's work has yet to be tested on a wider front and his small sample is to be regretted, yet I believe that Pollack's line of investigation will prove far more fruitful than either Segall's or Gregory's.

A study by Jahoda (1966) has added to the evidence against Segall's interpretation. The Lobi and Dagomba tribes both live in round huts set in open parkland. The Ashanti live in rectangular houses enclosed in high tropical rain forests. Jahoda tested the hypothesis that the Lobi and the Dagomba should be more susceptible to the H-V than the Ashanti but that the Ashanti should be more susceptible to the Muller-Lyer. In addition, a group of 41 Ghanaians was included who were students in Britain. The results offer no

| | H-V ILLUSION | | MULLER-LYER ILLUSION | |
|---|---|---|---|---|
| | n | Error | n | Error |
| Lobi | 34 | 1.65 | 34 | 15.18 |
| Ghanaian Europeans | 41 | 11.16 | 41 | 23.22 |
| Ashanti | 127 | 16.22 | 127 | 17.61 |
| Dagomba | 52 | 22.71 | 52 | 17.44 |

support whatsoever to Segall's hypothesis - he can offer no explanation of why the Lobi and Dagomba should differ so greatly in the H-V. In the Muller-Lyer the only significant difference was that the 'European' group showed more illusion than any of the tribes. Jahoda describes the 'European' sample as "undergraduates not taking Psychology and university administrative staff" but he makes no comment on their place of birth. At face value their greater susceptibility to the Muller-Lyer is directly in accordance with the prediction of Segall. However, this assumes that their critical early experience was obtained in this country - if this was not true then there would seem to be no reason for them to differ from the native sample. But there would seem to be another difference between the samples besides their environment which is pointed out by Jahoda. All his native samples were illiterate and as such it may be assumed that they were unpractised at interpreting 2-dimensional drawings in 3-dimensions whereas it seems probable that the student sample would have learned to do this. This leads to the hypothesis that the African may only be less susceptible to the illusion because he lacks the ability - this is easily testable. If it proved to be correct it would shoot to ribbons the cultural explanations of Segall and Gregory but it would at least help Gregory in that depth perception would be shown to be a relevant factor - something not yet proven. Of course, these comments apply only to the Muller-Lyer since, as regards the H-V, Africans seem to be more susceptible than do Europeans, but there is ample evidence, as previously mentioned, that the illusions do not belong in the same class. It is interesting to note that Gregory does not include the H-V in the list to which he believes his theory applicable.

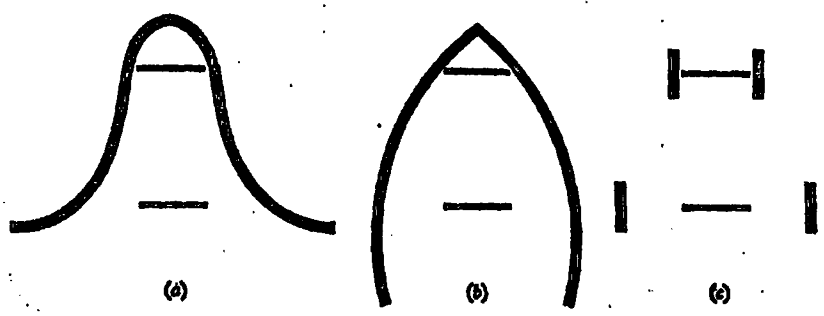
Jahoda adds further comment on the difficulties provided for Segall by the H-V. "Two successive expeditions to the Kalahari by the same team used the tests of Segall et al. (1966) with two groups of Bushmen; on one version of the H-V illusion results were substantially the same, with the other version they differed very significantly (p.<0.01); here again both tribe and ecology were constant. (based on data contained in Morgan, 1959; Mundy-Castle and Nelson, 1962)."

As it stands then, the carpentered world hypothesis remains unproven. Although some of the evidence is favourable and not a little puzzling e.g. the finding that the Evanston sample showed a significant difference on the Muller-Lyer between urban and rural dwellers (Rural mean 4.45, Urban mean 5.57; t=4.93; N's 33 & I52), there is a wealth of other evidence that just will not fit. Consider the problem set by the fact that children get larger illusory effects than adults. Pollack (I963), using 60 children between the ages of 8 and I4 years, has found that contour detectability threshold rises as the magnitude of illusion falls throughout this period ($r=0.49$ $p<0.0I$). Nothing is produced to show that a low contour detection threshold is linked with a large illusion per se, nor why this should be so, although Oyama (I960) has reported increased illusion as contrast increases, but how can Segall explain this correlation?

Odd facts like this must be tied up before a complete explanation can be achieved and once again I must state my belief that more than one factor will be found to be responsible.

I have left until last an experiment by Fisher (I968)' which might be considered the last word on the problem. Using the figures shown in Fig. I:6 and IOO subjects, Fisher obtained illusory effects. He mentions that natives will all be familiar with curves in their environment ....."We are forced to the conclusion that the carpentered world hypothesis, along with other theories that appeal to the influences of typical depth features in inducing illusory spatial distortions, requires reconsideration." The argument is that figures of this nature (similar results have been obtained using curved ends to the Muller-Lyer) cannot readily be endowed with a perspective interpretation.

It would seem then that Gregory's venture into anthropology is entirely unsuccessful and provides no support for his typical view hypothesis. On the contrary, the evidence strongly suggests that this hypothesis is false.



(a) (b) (c)

Three new Ponzo-type illusions.

*The extent to which each lower horizontal line was adjusted so as to appear of the same length as the upper*

| Display... | | 1(a) | 2(a) | 2(b) | 2(c) | Control |
|---|---|---|---|---|---|---|
| Adjustment of lower line | $\bar{X}$ | 13·44 | 12·57 | 10·85 | 8·57 | 0·37 |
| | $\sigma_{\bar{X}}$ | 0·52 | 0·41 | 0·31 | 0·38 | 0·35 |

Fig 1.6 From Fisher (1968).

PRIMARY AND SECONDARY SCALING

Gregory's theory differs from the other perspective theories in that he draws a distinction between primary and secondary scaling. It is vital that he should be able to demonstrate conclusively that these two mechanisms are, in fact, independent. "It is generally assumed that constancy scaling depends simply on apparent distance as Emmert's Law might suggest; but if we are to suppose that constancy scaling can operate for figures clearly lying on a flat surface we must challenge this assumption and suggest that visual features associated with distance can modify constancy scaling even when no depth is seen. If we are to suppose that the illusions are due to misplaced constancy scaling, we must suppose that the scaling can be set directly by depth features of flat figures, and that the scaling is not set simply as a function of apparent distance as is generally thought to be the case." (1963).

Primary scaling results from the presence of perspective cues in a figure. Gregory points out that he is not unique in stressing the effectiveness of such cues. "A wealth of evidence is given by Gibson (1950) that perspective, changes of texture, masking of further objects by nearer objects, and other such visual features are most important for estimating distance" (1964)[1]. However, Gregory is somewhat unique in his resort to luminous models to demonstrate his thoughts. As early as 1962[2] he was pondering on the reversals of Necker cubes and it is this figure that he uses most.

A luminous model of a Necker cube (i.e. a 2-D model) was found to reverse in the same manner as a Necker cube drawn on paper. However, unlike the drawn Necker cube, according to Gregory anyway, it was seen to undergo size changes appropriate to these reversals - "the apparently further face looking somewhat larger than the nearer, showing that constancy scaling is now operating. Since the retinal image remains unchanged it follows that the scaling is not set under these conditions as a simple function of apparent distance." (1963). The size change was thought not to occur in the drawn cube because of the inhibiting effects of the background texture. With a luminous model of a 3-D cube the effect was even more striking since, upon reversal, the figure no longer resembled a cube at all but a truncated pyramid. Here, of course, the apparently nearer face is casting a smaller image on the retina than the apparently further face. Hence the apparently nearer face must be much smaller.

Hotopf (1966) points out that Gregory is guilty of inconsistency even here. The argument is that the change in size is due to the perception of distance alone (secondary scaling) and not perspective (primary scaling). "If it had been due to perspective the Necker cube should have changed in size like a visual illusion when seen as a drawing on paper. The reasoning, however, is odd because if the Muller-Lyer figures are parts of cubes and are seen as they are because of the perspective indications, this must also be the case for a Necker cube, which has <u>more</u> of these cues." Hotopf also questions Gregory's assertion that the faces of a Necker cube "do not change in size" upon reversal. Sanford (1908) believed that they did and Hotopf himself produces experimental evidence to support this view. "On the assumption that the illusion is subthreshold, increasing the distance between near and far sides of the cube should magnify it sufficiently to become detectable (Fig. I:17). This figure with sides 2 inches long was presented at eye level, at a distance of from 3.5 to 4 feet to 32 subjects ..... Only two subjects saw the sides the same and the lines as parallel for both perspectives i.e. saw it in the way claimed by Gregory. Of the rest, 23 indicated a change in size consistent with the far side looking larger in answer to three of the four questions put to them. It is notable that comparison of size of the sides from the 'typical perspective position' was the least sensitive indicator; it should have been the most sensitive on Gregory's view." Hence we must conclude that the independence of secondary scaling has not been demonstrated. Hotopf's result has been replicated by Fisher (1970).

Primary scaling poses even more problems. "To get evidence for primary scaling entirely independent of the illusions is difficult but at least the following is suggestive ..... It has been noted by Humphrey that a straight line drawn across a corner of a Necker cube appears bent. Now this is particularly interesting because the direction of bending is the same which ever way the cube appears to lie in depth. It is bent in the direction to be expected if constancy scaling is operating from a typical perspective interpretation of the angle against which the line lies." (1963). Essentially what Gregory is saying is that if a line is drawn across two sides of a 3-D cube such that it casts a straight image on the retina, then the line will, in fact, be bent. If the Necker cube were a 3-D cube then the line would be bent in the direction it appears to be bent, assuming that the typical view (seeing the lower left face of the cube as to the fore) is entertained.

Wallace (1966) has taken exception to this interpretation. He points out that the whole of the Necker cube figure is not necessary for the distortion to take place (Fig 1.7). Gregory replies (1966)'

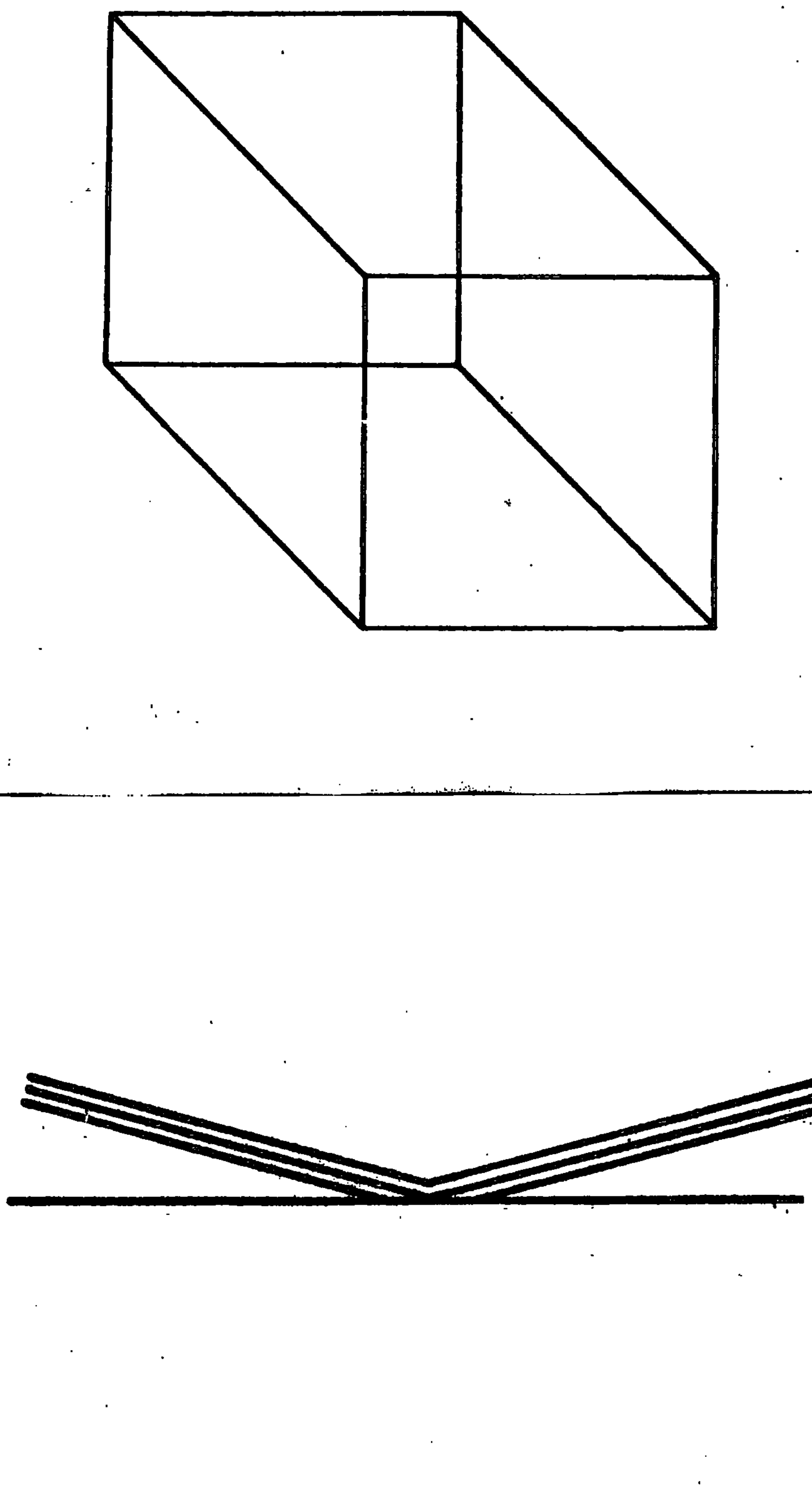


Fig 1.7   Hotopf's distorted cube (TOP) and Wallace's bent line (BOTTOM).

that he is well aware of this fact and that "The point of using a complete Necker cube rather than just a corner was to show, using the reversal properties of the figure as a whole, that the bending is in the same direction whether this corner is seen in inward or outward depth. It is a crucial observation to distinguish between primary and secondary constancy. Wallace evidently fails to see the significance of this point." On the contrary, I think Gregory has failed to see the significance of Wallace's point. Wallace shows that the distortion occurs in a figure which has no obvious depth interpretation - unlike the Necker cube. The obvious conclusion to draw, therefore, would seem to be that the bending of the line has nothing whatsoever to do with primary or secondary scaling and is not the result of misapplied constancy! Hence the direction of distortion would have no reason to change when the Necker cube reversed. In order to discredit Wallace's criticism, Gregory must show that Wallace's figure is seen in depth in the same way as a Necker cube when its background is removed.

Gregory has more to say on this matter, however, He claims that the direction of bend of the line drawn across a Necker cube does change when a 2-D cube is presented without its background. (1966)[3]. His point is that this is secondary scaling functioning purely according to apparent distance. As usual, no experimental evidence is presented to back up this casual observation. The difficulty here is that since the 2-D figure and the line drawn across it are functioning according to secondary scaling, so must the Muller-Lyer figures contained in the cube i.e. the illusion disappears. Gregory seems to acknowledge this corollary in his 1963 paper - "What happens to the distortions when we remove the background texture is complex......but, in general, distortions are reduced and disappear" (1963). Yet in Gregory (1966)[3] we find the following:- "If the illusion figures are presented to the eye with no visible background: .....the distortions should still be present and may be greater, for we should expect expansion as in Emmert's Law for after-images." This later position is in accordance with what is actually seen for, as Gregory notes, (1967), "The relevant fact is that distortions are observed in the illusion figures whether or not they have a visible background." Gregory has made his predictions fit the observed facts without changing his theory accordingly. In other words, it still follows from his theory that the distortions should disappear when background texture is removed.

Other writers have noted this inconsistency. Zanforlin (1967) and also Fisher and Lucas (1969) took photographs to show that illusions still persist in real situations. Gregory replied (1967) to Zanforlin's paper but seemed not to appreciate the flaw in his theory - he merely agreed that distortions do occur "whether or not they have a visible background." Fisher (1968)[3] is another who makes the point - "Although the perspective theory can be defended against criticisms levelled at it by previous writers, it carries a logical corollary which should be noted. According to the theory, the mechanisms which allow 3-Ds to be perceived accurately, operate inappropriately for 2-D displays. It follows that veridical perception of 3-Dimensional space is achieved at the cost of illusory distortion of 2-Dimensional space." Fisher and Lucas (1968) have produced a series of examples of illusions in real life situations. Weale (1968, p.104) seems to be alone in suggesting that the Poggendorf does not occur in 3-D.

Thus we must agree with Hotopf in his summing up. "There is no case on present evidence for distinguishing between primary and secondary scaling. It is indeed difficult to see how a form of scaling which was not itself due to 'perspective and other features associated with distance' (primary scaling) could be 'set simply by apparent distance' (secondary scaling)."

This is similar to the conclusion reached by Humphrey and Morgan (1965). "The hope of demonstrating primary scaling independently of the illusions is rendered forlorn by the nature of the concepts involved. The term 'illusion' may be taken to embrace all cases of plane figures the perceived configuration of which differs from the real physical configuration. But this inevitably includes any figure that is constructed to demonstrate primary scaling since such a demonstration must make use of plane figures in order to exclude the apparent depth effects which would activate secondary scaling. Thus the concept of primary scaling is tied to the illusions and cannot be adduced as a general phenomenon of which the illusions are only a specific instance."

In fact, the only concrete evidence Gregory has is his own experiment. This will be discussed at length shortly but first I would like to consider some other miscellaneous objections which provide embarrassment for the theory.

Particularly noteworthy in this respect is the work of such as Rudel and Teuber (1963) and Over (1966,[1&2] 1967).[1,2,&3] On numerous occasions these writers have shown that figures such as the Muller-

Lyer and T configurations produce distortions when touched by the subject e.g. "A haptic illusion of the same magnitude as the visual illusion is found when a blindfolded S moves his finger over the Muller-Lyer figure. Decrements in illusion obtained over repeated visual trials transfer to haptic judgements and vice-versa." However, these illusions are not identical with their visual counterparts in all respects. "Instructions which produce differences in amount of visual illusion do not differentially control magnitude of haptic illusion and age differences in magnitude of illusion have not been found with haptic judgement of the Muller-Lyer figure." (Over, 1968). Despite this, Bean (1938) reports the presence of these haptic illusions in subjects who have been blind since birth is enough to make any attempt to apply a perspective interpretation rather fatuous. Gregory comments ..... "I offer no comment at this stage. Haptic touch on the Muller-Lyer illusion is confounded by the poor touch acuity of the fingers which tends to produce a similar effect for figures such as the Muller-Lyer by quite different means." (1967). If Gregory is suggesting that the results are an experimental artifact, then the burden of the proof rests squarely on him. He continues, "At present the whole question of the relation between touch and vision is too uncertain for us to say how relevant touch experiments are to a theory of visual illusions, but perhaps a close relationship would indicate that the touch and visual spaces are neurally related in the nervous system <u>after</u> visual primary constancy scaling has taken place ..... the relationship between touch and vision in illusion situations is at present largely mysterious, and it is unwise to make any specific statement at the present stage of knowledge." Of course, there is interaction between the two senses e.g. Gregory's 'blind man' who recovered his sight could tell the time even though he had only felt the face of a clock previously. But taken at face value these results indicate that any perspective theory will be inadequate and that a more general mechanism is required.

In his review of the various illusion theories, Over (1968) lumps Gregory and Tausch together without bothering to venture into the intricacies of primary and secondary scaling. He notes that Gregory's own results are difficult to explain but that Green and Hoyle (1963) and Hotopf (1966) failed to replicate them. Conveniently this enables him to disregard Gregory's results but I do not think it is valid for him to pass over them so easily since neither of the two experiments that he mentions used an objective evaluation of depth as Gregory himself did. It is worth noting that few of the writers who have attacked Gregory seem to appreciate that his experimental results still need an explanation. Wallace is an exception in this respect.

Attention has been drawn to a number of figures which seem to produce difficulties for Gregory. Humphrey and Morgan (1965) note that, "Gregory explains the fact that the upper line in the Ponzo illusion appears longer than the lower by saying that it lies in a part of the figure which the converging lines indicate to be more distant so that primary scaling magnifies its apparent size. As it stands this explanation predicts the expansion of any line drawn in the upper (more 'distant') part of the figure, no matter what the orientation of the line might be. It follows that the illusion ought to occur when the two lines of the Ponzo illusion are drawn vertically instead of horizontally (Fig. I:8). This appears not to be the case.

Fisher (1968)[3] inquires how Gregory can explain the composite form of the Muller-Lyer. "What depth interpretation could possibly be placed on the arrowhead which bisects the figure? To mention a consistent explanation in the case of the complete Muller-Lyer figure, the ends of this arrow must be seen as being both in front and behind the shaft. If the illusion is to be explained in terms of apparent depth, the shaft must be seen simultaneously as both nearer to and farther from the observer. An interpretation of this kind seems highly questionable."

Virsu (1968) notes that Gregory's 'neutral' Muller-Lyer figure (see chapter on his experiment) is in actual fact Oppell's illusion. In his experiment Gregory finds no illusion with the fins at this orientation and claims that there is no apparent depth generated by them. However, Oppell's illusion is well established and it seems that Gregory does not include it in his list to which his theory is applicable. Since it <u>does</u> seem to contribute towards the overall effect of the Muller-Lyer, it would seem to indicate multiple determination.

Hotopf (1966) has drawn attention to the fact that once the length of the Muller-Lyer fins exceeds a certain ratio, the illusion begins to shrink. This is contrary to Gregory's predictions, since the longer the fins the greater should be the perspective cues and therefore the greater the illusion.

It is well established that the effect of an illusion figure tends to diminish with repeated presentation. This is particularly well documented with respect to the Muller-Lyer. Judd (1902) succeeded in reducing this illusion to zero as has been mentioned

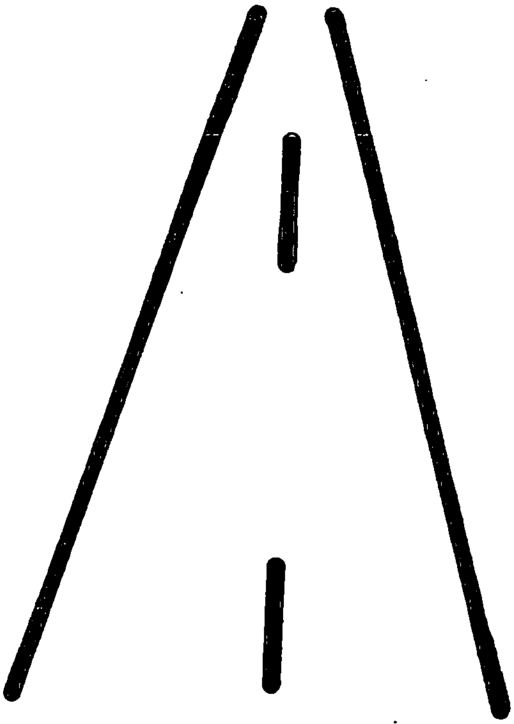


Fig. 1.8
Humphrey and Morgans
Ponzo 'illusion'

earlier (p. 4). Mountjoy (1964, 1965, 1966, 1967, 1968) has been particularly diligent in his researches on this topic. Incidentally, these results produce further evidence that the H-V and Muller-Lyer illusions are not part of a single class of illusions. Decrement with repeated trials was found to occur much faster with the H-V and the illusion actually reversed after 30 trials. Gregory makes no attempt to explain this phenomenon and indeed it is difficult to see how he could.

I would now like to pass on and consider Gregory's own experimental results and some associated points.

GREGORY'S APPARATUS, HIS EXPERIMENTS AND RELATED TOPICS

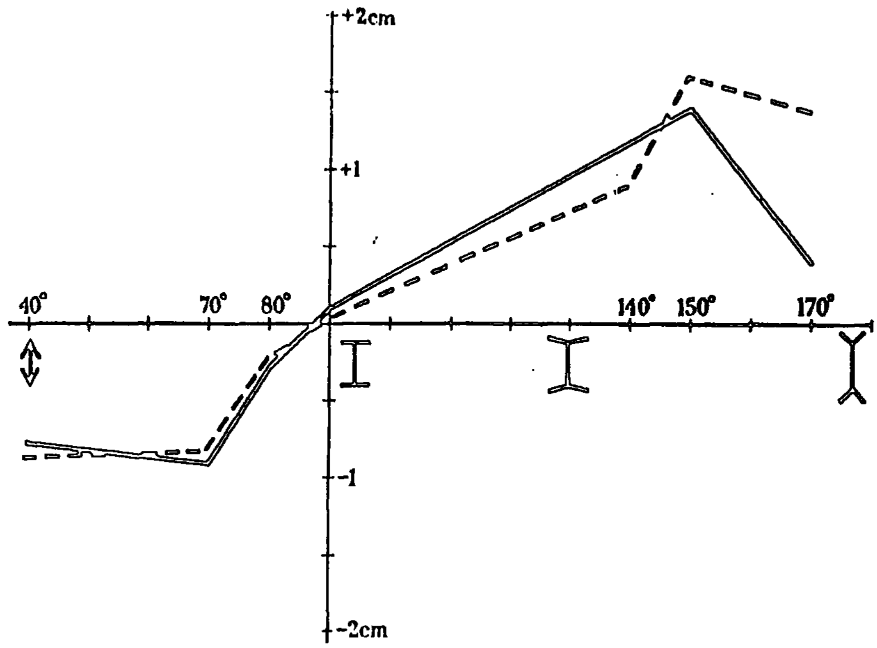
Gregory's first publication of his experimental results was in Nature (1965)[1]. Subsequently he repeated them in further articles published in New Horizons in Psychology (p.92 to 94), Scientific American (1968)[1] and in the Proceedings of the Royal Society (1968)[2]. The earlier articles contained no information about the number of subjects or how many readings had been taken at each point; however, this was revealed in the later articles (1968) i.e. 20 subjects, three readings at each point.

His ingenious apparatus was set up as follows. The textured background was removed from his illusion figures by presenting them in a 'Pandora's box' apparatus as photographic transparancies back-illuminated by an electroluminescent panel. The figure is placed behind a sheet of polaroid and the subject views it through polaroid glasses, one lens of these glasses being turned through 90 degrees; thus the figure is visible to one eye only. A half-reflecting mirror is placed diagonally across the box in front of the figure to allow the introduction of a reference light. This reference light is visible to both eyes and its exact distance is given to the subject by stereoscopic vision. The subject is required to indicate the distance at which he sees selected parts of the figure by adjusting the light until it appears to lie at the same distance as the part of the figure he is viewing. In this manner measurements were obtained from the central shaft of various Muller-Lyer figures and also from the tips of the fins. The actual distance of the figure from the subject was about 50 cms. and the graph* shows the seen depth difference in cms. between the central shaft and the

*See page 51

From the graph as published this breakdown would appear to occur only at the 'above 90 degree' end. However, Humphrey and Morgan (1965) quote Gregory as follows concerning the Muller-Lyer, "a model having the optimum angle for the fins (about 40 degrees) shows a marked illusion and is not seen in depth but appears, on the whole, flat, .... ...This evidently produces a discrepancy between constancy and apparent depth." They give the source of this quote as "R.L. Gregory, Stability and Distortions of Visual Space, International congress of human factors in Electronics, May,1962, Long Beach, Cal. (unpublished)". This may also be the source of the following remarks made by Pike and Stacey (1968), "Gregory, although maintaining that there is a general tendency to see Muller-Lyer figures in 3-D in the dark, nevertheless found that this is not the case for the Muller-Lyer with optimum fin angles (about 40 degrees) and that the illusion persists under dark room conditions." However, the results as shown indicate that a Muller-Lyer figure wuth a 40 degree fin angle <u>is</u> seen in depth. Of course, the quote above is of 1962 vintage while the experimental results did not appear until 1965 so we must assume that Gregory's early, non-objective observations were in error.

Although the results are convincing in that no other theory could explain them, they are not as convincing as Gregory would have us believe. Consider the graph once more. Does it really provide "evidence of a remarkably close tie up" between the amounts of illusion and seen depth? The theory as applied to an outgoing arrow runs as follows. The shaft is seen as most distant and being further away than the rest of the figure the image it casts on the retina




Müller-Lyer illusion and apparent depth, for various fin angles. The $x$ axis represents the angle between the shaft and fins in the Müller-Lyer figure. (With fins at 90° the figure is a capital I, giving zero illusion and zero depth.) For angles greater than 90° the illusion is positive, for smaller angles negative. The illusion is measured by adjusting a line to the same apparent length for each angle of the illusion figure. The *illusion* is shown in the *dotted* line. It was presented on a normally textured background. The same observers were used to measure apparent depth for the same angles; the illusion being presented without background texture and monocularly to avoid competing depth information. The results in both cases are the means of three readings for each of twenty observers in all conditions. The figure was ten centimetres in length viewed from half a metre. Depth was measured with the 'Pandora box' technique, the comparison light being set to the apparent distance of the shaft and the ends of the fins. *Depth* shown in the *solid* line.

Results taken from Gregory (1968).[2]

fin extremeties. "For comparison, the measured illusion for each angle for the same (20) subjects is plotted on the same graph. It is important to note that though the depth was measured with luminous figures, the illusion was measured (using an adjustable comparison line set to apparent equality) with the figures drawn on a textured background......The experiment shows that when the background is removed, depth very closely follows the illusion for the various fin angles. The similarity of the two curves provides evidence of a remarkably close tie-up between the illusion as it occurs when depth is not seen with the depth which is seen when the background is removed." Or so Gregory maintains, but more of this later.

It is seen from the graph that for fin angles of greater than 90 degrees (outgoing arrows) the central shaft appears farther away than the fin extremeties. When the fin angle is less than 90 degrees (ingoing arrows) the central shaft appears nearer than the fin extremeties. This is exactly what is predicted from Gregory's theory and it seems to show primary scaling actually in operation. It is interesting to note that since these results were first published, Gregory has omitted his discussion of the 'bent line' in the cube, apparently deciding that this new evidence made it irrelevant. However, this decision also means that he omits his only attempt to show the 'typical view hypothesis' at work. In the later papers he merely alludes to "commonsense interpretations" (1966)[4] as a means of deciding what the typical view of a figure is.

Returning to the graph, we see that the curves do not fit exactly. Gregory notes that "they break down together at just about the limits of perspective angles which can arise from corners."(1966)[3].

is not a correct reflexion of its actual physical size relative to the rest of the figure. Hence it is expanded to restore the balance. This is constancy at work - however, its operation is inappropriate in this case because the shaft is not more distant than the rest of the figure. Now how much does the shaft have to appear displaced in depth before it needs an expansion of I cm. to compensate? The diagram (Fig. I:9) suggests about 5 cms., i.e. the shaft must appear 5 cms. further away than the fin extremeties if its I cm. expansion is to be explained in this manner. Gregory finds only a I cm. seen depth difference - enough to account for 20% of the illusion. However, even in saying that we are probably giving Gregory more than his due.

It is a well documented fact that if you join the fin extremeties the resulting line, let us call it B, is distorted in the opposite direction to the shaft (Fig. I:IO). Hence the apparent depth difference from the shaft to B must be large enough to account for two distortions. Gregory would argue that the forces at work would be exactly those which caused the distortion of the shaft itself, i.e. the distance at which B is seen is mis-judged and the constancy mechanism is triggered inappropriately. Thus one would expect a difference between the shaft and B of more like IO cms. than the I cm. that Gregory finds, i.e. two 5 cm. differences.

Looked at in this way, Gregory's experimental findings look somewhat less impressive. Nevertheless, his is the only theory that would have predicted any apparent depth effect. Since this is what he found we must still explain it even if we reject his theory, a point that is often missed by his critics.

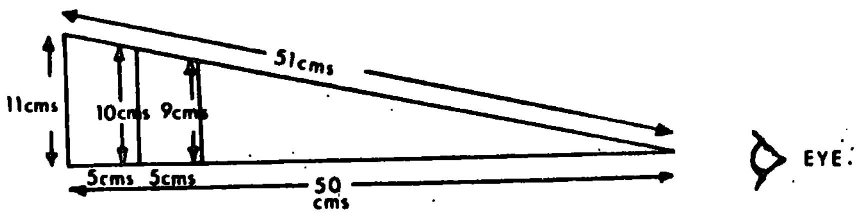



Fig. 1.9

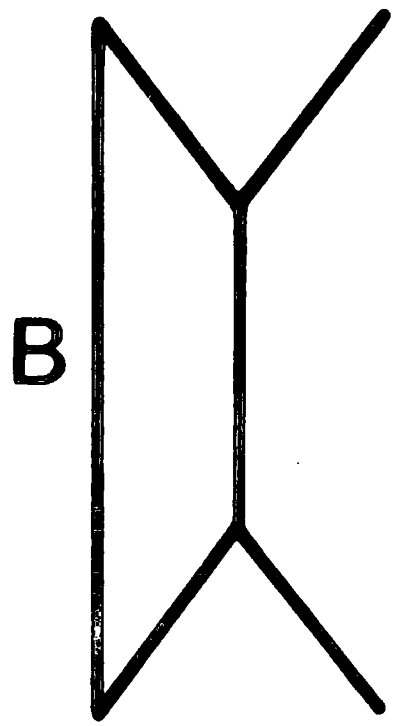



Fig. 1.10

Of course, although Gregory's figures were IO cms. long and viewed at a distance of 50 cms., there is no reason why they should be seen at this distance. Gregory himself remarks that there is no reason why a luminous object viewed with a single eye should be assigned <u>any</u> distance at all - since all distance cues are thought to have been eliminated. However, he did find that his figures had "a remarkably constant apparent distance," and in his I959 paper he shows a similar phenomenon occuring with after-images. But whatever distance the figures are seen at, and Gregory provides no information on this point, there would never be the one to one relationship he would seem to require. It is certainly not telling the whole story to claim that, "the correlation between apparent depth and extent of the Muller-Lyer illusion is better than 0.9" (I966)[3].

It is interesting that Gregory makes no prediction about what would happen if both Muller-Lyer figures were presented simultaneously without textured backgrounds i.e. whether one shaft would appear nearer than the other. This is pointed out by Pike and Stacey (I968) who examine the implications of both Gregory's development of the 'carpentered world hypothesis' and that of Segall et al.

Segall states (I966, p.206), "our analysis of the Muller-Lyer illusion contained the suggestion that the horizontal line with the obliques extending outward is seen as further from the viewer than the horizontal line with the obliques extending inward. When the two critical lines - the horizontals enclosed in the obliques - are in fact equal in length the differential distance inference forces the viewer to conclude that the 'farther away' line is 'really' longer. To check on this notion a laboratory study employing Muller-Lyer-like figures as stimuli in which distance judgements are required, seems essential." The prediction from this then is that if the two arrows are presented without textured backgrounds the outgoing arrow shaft will appear more distant than the ingoing arrow shaft.

Gregory, as usual, is not so explicit. Pike and Stacey remark that he seems to make "no prediction concerning the relative distance of the two shafts although he has shown that a depth effect exists within the separate figures when viewed without a visible background." They continue, "however it is clear that the phenomenal distortion of the shafts is likely to produce some effect with respect to apparent distance and it seems logical to predict that it would be the one usually associated with the size-distance effect. That is the phenomenally larger shaft will be perceived as nearer the observer - the direct opposite of the prediction of Segall et al." Note that this prediction is obtained by extrapolating from Gregory's writings and is not explicitly made by him.

Thirty subjects were presented with self-luminous Muller-Lyer figures at a distance of twelve feet in a dark room. Viewing was monocular and they were required to equate the two shafts for distance. The results showed that the outgoing arrow was seen as nearer and that it had to be moved back I4.I cms. (p.<.OI) on average before it appeared at the same distance as the ingoing arrow. Unfortunately, Pike and Stacey give no information about the height of their figures, i.e. was the subject making a retinal size match - allowing for the phenomenal distortion of course. However, they do note that, "the effect of apparent size does not need to have originated in a constancy scaling mechanism. Any process producing the phenomenal distortion could result in the distance judgements reported here." In other words, given that one figure is seen as larger than another similar figure, no matter how this judgement came to be made, then under reduced cue conditions the apparently larger figure will be seen as nearer.

Fisher (1968)[1] also seems to appreciate this point but his application of it seems a little misguided. In his discussion on p.382, l.15 of his article, he states that Emmert's Law "shows that a line which appears longer also appears to be situated at a farther distance than one that seems shorter." This is, of course, the wrong way round, as Pike and Stacey's experiment shows; it is the line which appears shorter that appears to be situated at a farther distance. However, he continues, "On Emmert's Law, it is to be expected that contours differing in apparent length should also appear to be situated at different distances. But such differences in apparent distance are irrelevant to explanation of differences in apparent size i.e. they fail to explain why the two lines should appear to differ in length in <u>the first place</u>. Thus the experiment intended to demonstrate the postulated relation exploits the nature of the two component parts of the Muller-Lyer illusion as seen. It fails entirely to indicate any mechanism which might be responsible for its appearance." In actual fact, as has been pointed out, Gregory does not use both Muller-Lyer figures in simultaneous presentation and Emmert's Law would certainly not predict that once the textured background is removed the fin extremeties should appear at a different apparent distance to the shaft.

Harking back to the theme of multiple determination, we might consider the work of Fellows (1967, 1968) at this point. He experimented using the figures illustrated in Fig. I:11. The distance between the apexes of his arrows was 160 mm. but he used shafts varying between 20 mm. and 140 mm., hence there was a gap at each end between the shaft and the arrow head. The usual illusory effect was obtained using shafts of from 140 mm. to 120 mm. (hence total gaps of 20 mm. to 40 mm.). The effect he got is illustrated in the graph (Fig. I:12). A reverse Muller-Lyer effect was obtained using shafts of between 100 mm. and 40 mm. (total gaps of 60 mm. and 120 mm.). When the shaft was only 20 mm. long with a gap on each side of 70 mm., no distortion at all occurred. These results would seem to be due to some sort of contrast effect and as such would present Gregory with a problem. However, he is undismayed.* "Now given that the separation between the arrow heads is changed by the usual illusion (and this occurs in the absence of any line joining them) what 'should' happen to a short line placed between the heads? If the heads correspond perceptually to the retinal projection of corners, a shorter line could represent, in the case of the outgoing arrows, some object nearer the observer than the extreme of the (inside) corner. In the real world this would give a larger retinal image than when placed at the corner; so to give constancy it must be shrunk with respect to the corner - which is what Fellows finds in his experiment. (I would regard

*(1967)

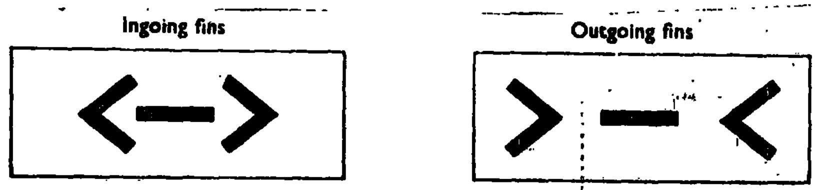
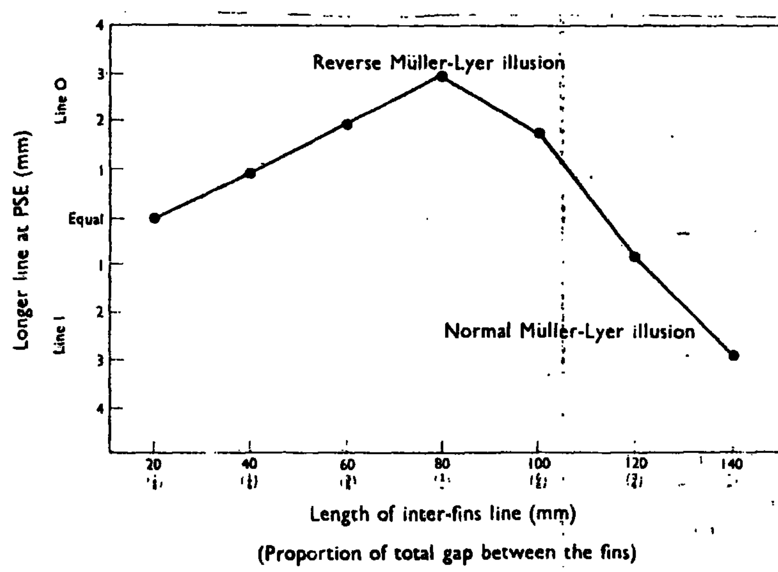



**Fig. 1.11 - From Fellows (1967).**




Direction and extent of illusion for different lengths of inter-fins line.

**Fig. 1.12 - From Fellows (1967).**

this figure as perceptually the same as the Ponzo illusion but, as it were, viewed end on. I would predict that the short line will be measured as perceptually nearer, with the depth measuring technique)....... This prediction has since been confirmed when measurements of apparent depth were made in the figures for I6 subjects. This experiment, suggested by Fellows' results, was carried out recently (June, 1967). It is hoped to publish full details later." However, no such details have yet appeared. What has appeared is another paper by Fellows (1968) completely rejecting Gregory's argument. He points out two problems arising from Gregory's suggestion. "First, as the previous experiment clearly showed, the <u>outgoing</u> fins have no significant effect upon the perceived length of a short line. Rather, the reversal of the Muller-Lyer illusion, at least with lines one half of the length of the gap between the fins, is entirely attributable to the expansion effect of the <u>ingoing</u> fins. Secondly, what could a short line between the ingoing fins represent? Surely not some object further away from the observer than the corner. But if, as seems reasonable, it represents something nearer, then by Gregory's argument it should also be shrunken - which it clearly is not."

Fellows had previously suggested that the effects he obtained might have been due to the fins'enclosing' the lines. He set out to test this, using 26 subjects, by replacing the fins by an "equivalent size non-fin enclosure". (Fig. I:I3). Results using this figure were very similar to those obtained using the fins. Hence Fellows could conclude, "the equivalence of the two P.S.E.s clearly supports the enclosure explanation of the Muller-Lyer

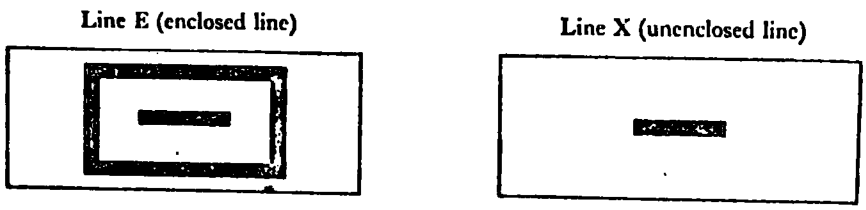



Fig. 1.13

Figures used by Fellows (1968)

reversal effect with short inter-fin lines. This implies that the pattern made by the fins plays no direct part in this effect."

Gregory has promised to publish more results on two other occasions. "We should expect different scaling systems to have somewhat different time-constants, and we are attempting to measure these to establish their separate existence quite apart from considerations of distortions of visual space." He also suggested that figural after-effects were caused by primary scaling that was still appropriate to the first pattern operating on the second - "Preliminary experiments are providing strong evidence....." (1963). In neither case has anything more been published.

In fact, Gregory has published some other results concerning the Ponzo illusion (1968)'. He found that a line of six bars were seen at different depths although the difference was only 0.8cms. at the most. No information is given as to how these results were tested for significance, nor as to how many subjects were used. The minute differences obtained would seem to be a very shaky basis on which to base conclusions and although the graphs shown for 'matching errors' and for 'depth differences' are quite a good likenesses, the same argument applies as was used against the Muller-Lyer graphs.

However, Gregory has received support from a rather unexpected source. Coren and Festinger (1967) found that the width of a curve tended to be overestimated whereas the height was not. Applying Gregory's theory they predicted that removing the background would results in the middle of a curve being seen as further away than the top and bottom. They likened the wings of a curve to the converging lines of the Ponzo illusion. Using 36 subjects and a variation of Gregory's apparatus, they found that this was the case. The figure was 40 cms. away. The bottom was seen at 4I.39 cms. and the middle at 45.7 cms. "One may calculate the magnitude of the width estimation that might be expected from the magnitude of rotation obtained in the monocular situation. The expected perceived width, if shape constancy were perfect, is 6.67 cms. If shape constancy were less than I00% then the obtained width estimate of 6.24 cms. would not be too far off." The actual width of the figure was 5.I cms.

Coren and Festinger also used a control figure made up of straight lines and found no seen depth difference. From my previous analysis it will be apparent to the reader that my opinion is that the only thing that holds Gregory's theory up is his experimental result. Hence the Coren and Festinger experiment is something of an embarrassment for: unlike Gregory's work, it seems to have had an efficient control situation and adequate experimental detail is reported. However, in the light of the theoretical difficulties and flaws that are apparent in Gregory's edifice, it was thought worthwhile to attempt to replicate the experimental findings of Gregory himself and also those of Coren and Festinger.

## AN ASSESSMENT OF THE LIKELY EFFECTIVENESS OF THE CUES TO DISTANCE PRESENT IN GREGORY'S APPARATUS

The purpose of most of the experiments in this series is to investigate further Gregory's claim that certain 2-dimensional figures appear 3-dimensional in a predicted manner when viewed under certain conditions, namely with one eye, in the dark and without any background being discernible. The apparatus he used has already been described on page 50 . It's purpose was to present the figures in such a way that the S would not be able to tell that they were flat, or, in Gregory's own words - "(the figures) are viewed with one eye in order to remove stereoscopic information that they are truly flat." Although it is true that his apparatus does remove all stereoscopic information, there still remain a number of other cues which could conceivably furnish the same information i.e. to tell the S that the figure is, in fact, flat.

These cues are referred to collectively as the oculomotor adjustments - accommodation and convergence and changing pupil size. All three change systematically with changes in target distance, this being the first requirement of any sort of visual stimulation if it is to serve as a distance indicator. However, although these cues satisfy this requirement, it remains to be demonstrated experimentally that they <u>do</u> serve as indicators of distance. The search for this experimental evidence has continued since Bishop Berkeley's first speculations on the subject in 1709, but we are still without a definite answer that would apply to the Gregory situation.

The best summary of the early literature is in Woodworth (1938, p.665-680). He traces the theory's progress through Wundt (1862), Hillebrand (1894), and Bourdon (1902) to Baird (1903) and Bourdon (1932). In the latter's experiment the targets were luminous discs illuminated in such a way as to conceal their surface detail, with the size of each target so related to its distance as to maintain a constant visual angle. The targets were presented successively, in pairs, at distances of 16.5 and 25cms; 25 and 50cms; 33.3 and 100cms. In half the cases observers were unable to discern any differences in distance. In 25% of the cases the difference reported was in the wrong direction and in only 19% were the judgements correct. Viewing was, of course, monocular, which allowed Bappert to place a mirror before the 'non-seeing' eye and to observe whether its convergence was appropriate to the object's actual distance. (Both accommodation and convergence in the 'non-seeing' eye tend to follow that of the 'seeing' eye, as may be observed by shifting one's convergence from a far object to a near one while keeping a finger-tip upon the closed lid of the other eye. The inward movement of the cornea may easily be felt). Bappert found that in the majority of cases convergence did move in the required direction i.e. outward when the observer's gaze shifted from the nearer to the further disc, and vice-versa. The inference of these findings is that if Ss are unable to correctly discern relative distances under these conditions then they will be unable to discern whether a figure is flat or 3-dimensional.

The weight of evidence against the theory was increased by a thorough study by Irvine and Ludvigh (1936), who were unable to trace any physiological mechanisms which might provide the information required for proprioception. However, the finding by Daniel (1946), confirmed by Cooper and Daniel (1949) and Sunderland (1949) of muscle spindles in the extraocular muscles of the human eye, raised the possibility that these spindles might be the means by which proprioception was achieved. Irvine (1950) appreciated the implications of these discoveries and tested them, but once again the conclusion was a negative one. Ogle (1962)[1] was thus able to write that the evidence against was "rather conclusive" (p.266). However, this ignores the work of Grant, (1942).

Grant criticised the early work on the grounds that the targets used were inadequate to stimulate the accommodative reflex. "The milk glass chosen for the target was intended as a perfectly homogeneous surface, free of any detail or markings which might offer visual cues of approach or recession as the distance of the target was varied. Unfortunately a perfectly homogeneous surface does not provide a stimulus to accommodation. This reaction, unlike such a response as that of a pupil to light, requires more than a certain kind of physical stimulation of the retina. It depends not only on a change of light, but on a perception of something to be seen."

Grant's target was an arrow and in one of his experiments this was presented to just one of the S's eyes and straight ahead of that eye on the primary visual axis. "In this instance accommodation occurred in both eyes although directly stimulated in but one; convergence likewise is a binocular reaction under monocular stimulation. In this case, since the eye in use remained directed straight ahead upon the target during accommodative increase, the only associated convergence change involved the unused eye whose axis rotated towards the near point for which accommodation was now adjusted ....However, under these conditions the adjustment of the unstimulated eye is seldom complete; the amount of convergence varies in amount, on the average, from two-thirds to three-quarters of that required for perfect binocular fixation, while accommodation approaches somewhat closer to the amount needed for adequate focus." This part of Grant's work is quoted so extensively because the behaviour of the 'non-seeing' eye was thought to be relevant to the Gregory-type situation, which was very similar to Grant's.

The S's task was to adjust a coin (seen binocularly with full cues) to the same distance as the arrow. The arrow was seen either binocularly or monocularly and the results are given below.

| Distance of the arrow . . . . . | 50 cms. | 25 cms. |
|---|---|---|
| Arrow seen binocularly . . . . | 41.4 | 34.3 |
| Arrow seen monocularly . . . . | 49.9 | 45.4 |

Grant blames the fact that the adjustments in the occluded eye were not enough for perfect fusion for the decreased accuracy in monocular vision. This is but one of several experiments done by Grant, all of which he believed to show that proprioceptive cues could be used with accuracy.

Accommodation is usually thought of as a relatively minor cue to distance and yet the quote given above indicates that in Grant's experiment it was accommodation in the 'seeing' eye which triggered convergence of the 'non-seeing' eye. This was in agreement with the views of Maddox (1893) who felt that convergence had no direct influence on accommodation but that any change in accommodation always necessitates a change in convergence. Experiments by Morgan (1944) largely confirmed this. In Bappert's experiment it was found that eye-movements in the 'non-seeing' eye were consistently made in the correct direction. On the above evidence these eye-movements must have been stimulated by accommodation in the 'seeing' eye, and yet Grant claims that accommodation did not occur because the targets were inadequate. If accommodation did occur, as seeme likely, it is puzzling why use was apparently made of the resultant cues in Grant's experiment but not in that of Bappert.

Another relevant series of experiments were performed by Heinemann, Tulving and Nachmias (1959). Again target presentation was monocular, directly in front of the 'seeing' eye. This time the targets were discs of various sizes presented at different distances although they subtended the same visual angle. The task was to match the size of the discs by adjusting a comparison disc and also to say which disc was nearer. The results showed that Ss could judge the size of the discs quite accurately but that they could not discriminate which was nearer with better than chance accuracy. These findings as regards the distance judgements are in agreement with those of Bappert but contrary to those of Grant. Neither Bappert nor Grant were concerned with judgements of size.

Heinemann et al. went on to show that the accurate size judgements were due to oculomotor adjustments by placing an artificial pupil in front of the 'seeing' eye and then repeating the experiment. An artificial pupil provides the eye with an unlimited depth of focus and thus makes accommodation unnecessary, and, of course, without accommodation, no convergence will occur in the 'non-seeing' eye. Ss were unable to make accurate size judgements under these conditions.

The next step taken by Heinemann et al. was to eliminate accommodation but to retain convergence. The accommodative mechanisms of the 'seeing' eye were anaesthetised with homatropine and as an added precaution artificial pupils were placed before both eyes. Convergence was induced in the following ingenious manner. Previously the 'non-seeing' eye had not been presented with any sort of target - its movement was controlled by the 'seeing' eye. Now, however, it was given a target to fixate in the shape of a cross. A similar cross was marked on the centre of each disc. The S was instructed to fuse the two. Thus convergence could be controlled. The crosses presented to the 'non-seeing' eye were placed so that convergence occurred that was appropriate to the particular distance of the disc being viewed. "As soon as the exposure of the first disc ended, O was required to view the second disc."

The results from this condition were identical to those obtained earlier when all oculomotor adjustments were allowed; "the same tendency toward size - constancy was present." The interpretation of this result was "that changes in the angle of convergence, unaccompanied by changes in accommodation or in the diameter of the pupil, produce variations in the apparent size of objects viewed." However Heinemann et al. were unable to offer an explanation of exactly how the cue worked.

The work of Morgan has shown that accommodation will trigger convergence, while Heinemann et al. have shown that convergence alone can serve as a cue. This would tend to support the view of Maddox that "the efforts of convergence and accommodation are intimately intersusceptible."

The results of Heinemann et al. also create further difficulties for the size-distance invariance hypothesis (SDIH) which states that there is an invariant relation between perceived size, distance and visual angle such that given any two, the third is automatically fixed. This theory has been the foundation of several well known theories of perception e.g. the Gestalt school, but in recent years it has been challenged with increasing frequency. Several reduced cue studies have produced results at variance with its predictions, (for a review see Epstein, Park and Casey, I96I), so that now it is no longer considered valid to infer apparent size from apparent distance or vice-versa. Of late more notice has been taken of this fact.

Gogel (I96I,I962) has investigated the effect of convergence alone as a cue to absolute distance in a long series of experiments. His method was to use stereoscopically generated objects as targets. These targets only appeared as a single object if a specific convergence value was maintained. If convergence was an effective cue to distance, changes in the convergence value necessary to fuse the target should result in perception of an appropriate change in its distance. Ss could see a visual alley in one eye while they viewed the targets. This alley contained many cues to the distance of its parts and Ss indicated their perceptions of target distance by moving a marker down the alley until it was the same apparent distance as the target. Mirrors were used to make the target appear to hang just above the floor of the alley. Using convergence values which varied from 0 to 12 degrees Gogel was able to conclude, "Even for those Ss who evidenced some changes in perceived distance, the magnitude of these perceived changes was considerably lessthan the range of physical distances required to produce the different values of convergence."

However, this conclusion needs some qualification. Most of Gogel's experiments were done with accommodation held constant at optical infinity. This was achieved by introducing a lens between the target and the eye which refracted light from the target so that it entered the eye in parallel beams. Gogel considered that this made his situation rather artificial. Accordingly he repeated the experiment in such a way to allow the accommodation cue while keeping everything else as before. In this situation the number of Ss showing some ability to use the cues doubled from three out of twelve to six out of twelve.

In another experiment, this time without the accommodation cue, two or more stereoscopic targets were presented at the same time, only one of which could be'fused' at any one time. Under these conditions Ss seemed quite capable of ordering the targets according to the amount of convergence required, i.e. the target requiring most convergence was placed nearest etc. Once again the distances indicated by the Ss were not nearly as large as the physical distance between targets would have been.

From this then it would seem that most Ss can use convergence and accommodation to tell them the relative distances of objects but that they are not so good as indicators of absolute distance. The finding that these cues can indicate relative distances is contrary to that of Heinemann et al. However, it should be noted that, whereas the Ss of Heinemann et al. were viewing monocularly, as were Gregory's Ss, those of Gogel were using both eyes.

Biersdorf, Ohwaki and Kozil (1963) have confirmed Grant's (1942) assertion that convergence under monocular viewing is not quite sufficient for fusion. Biersdorf et al. used three Ss and found convergence to be, on average, 98% of what was required at one metre, but this was reduced to 86% at five metres. These percentages are in excess of Grant's original estimates (66 to 75%).

Biersdorf (1966) went on to repeat Heinemann et al's. experiment in which Ss judged the relative distance of targets subtending the same visual angle under reduction conditions. As in the Heinemann study the cues of convergence and accommodation were allowed. Four of the eight Ss ordered the targets correctly. This is contradictory to Heinemann's results but in accordance with Gogel's. Biersdorf was able to point out a possible cause of the difference.

Biersdorf, unlike most others, measured convergence in the 'non-seeing' eye for all his Ss during the experiment. He found that three Ss who could not order the targets correctly were those who showed least contralateral convergence. Alpern (1962) states that Ss differ considerably on this factor. Biersdorf suggests that these Ss might not have been using the convergence - accommodation cue, but some other irrelevant cue.

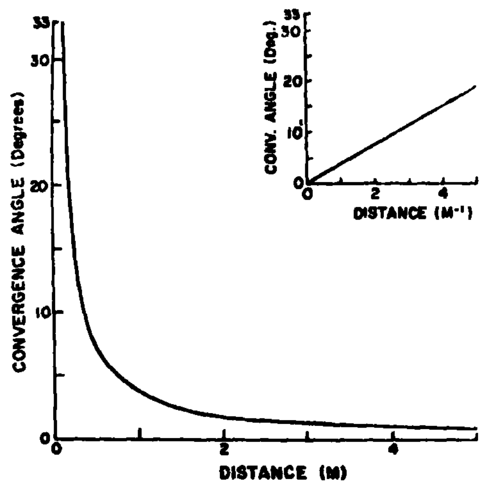
However the matter is further complicated by the results of Kunnapas (1968). Using an almost identical experimental set up to that of Biersdorf i.e. monocular viewing which allowed accommodation and convergence of the 'non-seeing' eye as the only cues, he found that Ss could not order targets correctly. He used a standard at 115cms while the other target was either at 25, 45, 75, 115, 195, 295 or 395cms. All targets subtended the same visual angle. "It is seen that accommodation did not provide sufficient information for accurate estimation of distance. All distances are judged to be approximately equal, but slightly larger than the standard distance. Subjective uncertainty is very large."

Rock and McDermott (1964) also obtained results at variance with those of Biersdorf. Again using the 'visual' reduced situation, allowing only accommodation and convergence of the 'non-seeing' eye as cues. However, their targets were positioned 32 feet away which is far in excess of the maximum distance at which it is thought possible for these cues to be effective. Their comment was "In preliminary work we noted that Ss often had no definite impression of distance at all wherever a reduction object was involved. In fact the relative distance responses ...... were often little more than guesses or random reactions, as volunteered by several Ss and admitted by several others on questioning." This is as one would expect at such great distances for 'optical infinity' i.e. the distance at which changes in distance produce such small oculomotor adjustments as to be, to all intents, undetectable to the S. Optical infinity isthought to begin at about 6 metres - the graph (Figure I: I4) illustrates this (from Baird, 1970). Baird also states that most of the accommodative mechanism has "run its course" by 2 metres.

Kunnapas' results are immune from this criticism and they have been replicated by other 'short distance' studies by Landauer and Epstein (1969) and Epstein and Landauer (1969), which used exactly the same distance as Kunnapas.

It would seem then that only Grant, Gogel and Biersdorf have produced evidence that convergence and accommodation can act as cues to absolute or relative distance. Most investigations have reached the opposite conclusion. In regard to this thesis it is important to assess how these cues would affect Gregory's work. Gregory presented his figures at a distance of 50cms which is well within optical infinity, but even if we concede that the cues are effective at this distance there is still another point to be discussed.

Most of the studies discussed have asked the question, can the S tell that object A is nearer than object B? In regard to the Gregory results the question is, can the S tell that object A is at the same distance as object B, which is not the same thing at all. Indeed we may recall Kunnapas' comment that, "all distances are judged to be approximately equal" under these conditions. It would seem, then, that the S in the Gregory situation should judge the different parts of the figure as equidistant, unless some hitherto unmentioned cue intervenes. Gregory contends that such a cue would be the 'obvious' 3-dimensionality of the M-L figures.




**Convergence angle in degrees as an hyperbolic function of target distance in meters. The hypothetical curve is based upon an interpupillary distance of 66 mm. The insert gives convergence angle as a linear function of reciprocal distance.**

Fig. 1.14 - From Baird (1970)

We may sum up as follows. If the figures are seen by Ss in the way Gregory believes then the cues of accommodation and convergence would not be sufficient to overrule this perception. If they are not seen in this way then they should be perceived as flat - not because of the influence of accommodation and convergence, but because this would seem to be how uncertain Ss react. However, it was pointed out in the Introduction (p. 54) that the depth differences reported by Gregory were only one-tenth the size expected. It was thought that the cues available in the Gregory apparatus might be acting in such a way as to reduce the size of the observed 3-dimensional effects.

The first experiment is an attempt to test this hypothesis. A new apparatus was constructed such that the cues of accommodation and convergence would no longer be available.

Another reason for modifying the Gregory apparatus was suggested by a pilot study conducted with an apparatus almost identical to his. It was found that even when the polaroid filters were crossed at exactly 90 degrees some light always penetrated them such that the outline of the figure could be made out by the 'occluded' eye. Even though a S might report that this was not the case when he first saw the figure, upon dark adaption the 'ghost' image might well appear without his being aware of it. The polaroid filters used in this pilot study were admittedly of an inferior sort but at the same time it is true that <u>no</u> polaroid filter is 100% efficient.

The result of a ghost image of the sort described would be that the S would be able to view the figures stereoscopically and hence perceive that they were truly flat. It is not suggested that this might have happened in Gregory's experiment since his results did not show his Ss to perceive the figures as flat but it is mentioned as a possible experimental artifact which care should be taken to avoid.

There is one further point to be made concerning the effectiveness of the cues of convergence and accommodation. None of the studies so far reported have exposed Ss to the same situation over a large number of trials. In some of the experiments to be reported here Ss will undergo as many as 100 trials on the same figure. Biersdorf (1966) has mentioned the possibility of Ss learning which cues to pay attention to. If Ss do learn to perceive more veridically with experience under reduction conditions then it certainly would be a possibility in some of the experiments in this study. This is a further reason for eliminating the cues available in the Gregory-type situation.

## SOME NOTES ON THE SIZE-DISTANCE HYPOTHESIS

Gregory's theory is connected with the SDIH in as much as Gregory predicts a difference in size to be associated with a difference in distance. The SDIH was defined by Epstein, Park and Casey (1961) in one of its forms as follows:-"The hypothesis proposes an invariant relationship such that the apparent size of an object is uniquely determined by an interaction of visual and apparent distance."

The classical experiment was performed by Holway and Boring (1941) who found that under conditions of complete reduction, judgements of size approximated visual angle matches. This result has been replicated many times since e.g. Lichten and Lurie (1950), Hastorf and Way (1952), Chalmers (1952, 1953), Renshaw (1953) and Zeigler and Leibowitz (1957).

However, Wallach and McKenna (1960) believe that these results are merely a 'special case' in that the S's reaction is dictated by the experimental situation rather than some underlying general tendency. It is true that a difference in apparent visual angle can mean <u>either</u> that two objects are of the same size but at different distances <u>or</u> that they are of different sizes at the same distance. Both these possibilities are in accord with the SDIH - Holway and Boring's results would seem to indicate that most Ss prefer the first assumption whereas Wallach and McKenna write that "the equation of image-sizes results from an implicit assumption of equal distance of the standard and comparison object."

A third possibility is expressed by Rock and McDermott (1964). They report that their Ss had no impression of distance at all and that their responses were merely guesses.

Epstein and Landauer (1969) and Landauer and Epstein (1969) provided further evidence. They varied the visual angle of their targets and obtained estimates of both distance and size. They found that the larger the visual angle, the larger and nearer the target appeared and vice-versa. These results were contrary to predictions from the SDIH in that if size was perceived as changing in proportion to visual angle, distance should have remained constant. Gogel (1969), on the other hand, has reported results in similar conditions which support the SDIH. Gogel found that Ss tended to place all figures, no matter what their visual angle, at a certain specific distance on their first presentation. After this they seemed to judge all the figures (rectangles) to be of the same size but at different distances according to their visual angle as the SDIH predicts.

One difference between the experiments which might well be important is that Epstein and Landauer presented Ss with two figures at once, i.e. the standard and the comparison, whereas Gogel's Ss only ever saw one figure at any one time. However, as regards his conclusions as to the relative distances at which figures of different visual angle would be located, Gogel agrees with Epstein and Landauer. "Suppose that two rectangles of different retinal size are presented simultaneously and are viewed monocularly under reduced conditions of observation. The difference in the retinal sizes of the two rectangles would result in the rectangles appearing at different distances." This conclusion is in substantial agreement with Holway and Boring's original findings.

Pike and Stacey (1968) investigated the relative apparent distances of an ingoing and outgoing M-L figure under reduced conditions, i.e. monocular viewing at 12 feet. They interpreted the work of Segall, Campbell and Herskovits as predicting that since the ingoing M-L was thought to be interpreted as an 'outside' corner while the outgoing M-L was thought to be interpreted as an 'inside' corner, then the ingoing M-L should appear to be further since outside corners usually are in our experience. They note that Gregory makes no predictions on this matter but is concerned only with distance differences that occur within the figures. "Gregory .... has made no prediction concerning the relative distance of the two shafts, although he has shown a depth effect exists within the separate figures."

They predict that the SDIH (in accordance with Holway and Boring) would expect the apparently longer outgoing shaft to be seen as nearer i.e. the exact opposite of the prediction of Segall et al. However, they also note that the difference in apparent length of the shafts is not a real one but the result of an illusory distortion and that they should both subtend the same retinal angle.

In fact, Pike and Stacey's results (already reported on p. 57) indicated that Ss did use the phenomenal difference in shaft lengths as a basis for judging relative distance in the manner predicted by the SDIH. The two M-Ls were mounted on runners and could be moved backwards and forwards. Ss were instructed to set them at the same distance. The responses of 15 of the 30 Ss indicated that when the two shafts were equidistant the outgoing shaft appeared to be nearer. However, six Ss showed the opposite tendency while another nine seemed to judge the situation veridically - or at least neither one way nor the other. Thus half the Ss responded as Holway and Boring might expect, but half did not.

Epstein and Landauer commenting on Kunnapas (1968) results, say, "Inasmuch as, in the absence of other distance cues, a difference in visual angle is compatible with a judged difference in distance or size, S avails himself of both alternatives depending on the task requirement. If the E solicits a size judgement, S translates the visual angle difference into a size judgement, proportional to visual angle. If a distance judgement is solicited S translates the visual angle difference into a distance judgement, proportional to visual angle." It would seem from Pike and Stacey's findings, that not all Ss react in the same way to the 'task requirements', and that using the group means covers up a great deal of individual variation.

In our first experiment, Ss were presented with an ingoing M-L and a vertical line of the same length as the shaft. They were asked to 'distance' both. In this case the ingoing shaft would look shorter without their being any possibility of Ss taking the fins into account. Such a figure also allowed further investigation of Pike and Stacey's findings concerning the behaviour of individual Ss.

The SD IH and Gregory's theories do not come into conflict over the apparent distances at which M-L shafts will be seen. However, the position is more complex when we come to the Ponzo illusion (Fig. I:15). In this illusion the line nearest the apex of the $\Lambda$ appears longer than the lower line even though they are both the same length. Gregory believes this effect to be due to S's interpreting the $\Lambda$ as some form of parallel way (such as a road or railway) receding into the distance. The horizontal lines are thought to be seen to rest on this 'way' and to be part of it. If this is so, then the upper line must be further than the lower line and since it subtends the same retinal angle, it must also be larger. Gregory believes that the workings of the size-constancy mechanism results in our perceiving it as larger. Gregory has published results (1968)' indicating that Ss do tend to see the apparently longer line as further away when the illusion is viewed in his apparatus.

However, if one rejects the premise that the $\Lambda$ is interpreted by the S as 'railway lines' one is lead to a different prediction. Recalling the quotes from Gogel and Kunnapas, it seems that most Ss will locate the longer of the two lines as nearer under reduction conditions. This, of course, depends on an assumption of equal size. The results of Pike and Stacey seem to indicate that not everyone makes this assumption. Some of their Ss saw the two shafts as equidistant, while a small number saw the apparently longer shaft as further. If the Ponzo illusion is not a perspective illusion but has its cause in some other mechanism (eg lateral inhibition) then one would expect Ss to judge the distances of the lines in accordance with their apparent size - rather than their apparent distance determining their apparent size, as the perspective explanation would have it. This might well lead to individual results conforming to the pattern established by Pike and Stacey with the M-L shafts, ie the apparently longer line being judged as nearer by about half the Ss, with some seeing the apparently longer line as further and some judging them as equidistant. This is very different from Gregory's position.

Gregory would seem to have settled the matter with his findings but a number of the experiments in this thesis call these into question.

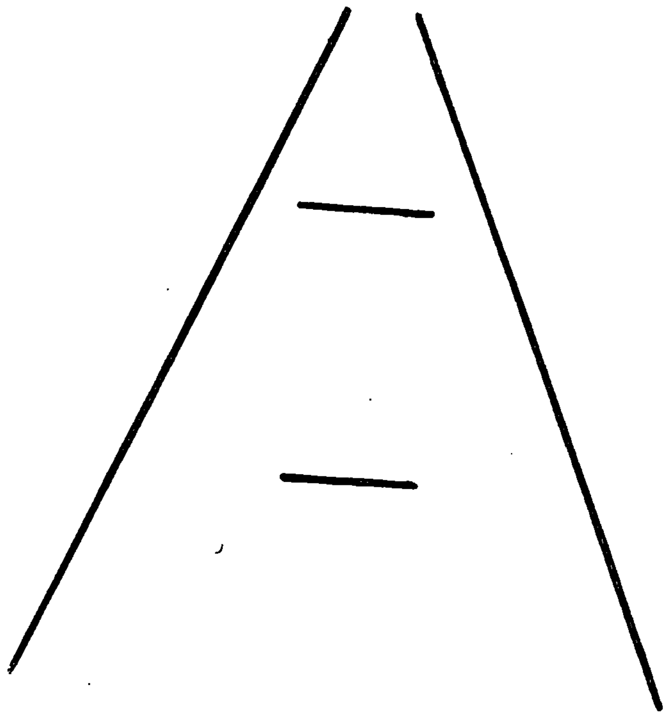


Fig. 1.15 - The Ponzo illusion

# P A R T 2

## EXPERIMENTAL ATTEMPTS TO REPLICATE GREGORY'S RESULTS

## A NOTE ON STATISTICAL PROCEDURE

"The empirical determination of functional relations between behaviour and its controlling variables forms a large part of modern behavioural research. One important aspect of this type of experimentation is the method of distributing subjects among the various points which determine an empirical curve. The most direct method is to use a single organism, to obtain every point on the curve. This procedure is not always practicable, however .......Faced with these problems most experimenters turn to group data" (Sidman, 1952).

The results Gregory has published from his experiments on the Muller-Lyer illusion (1966[3], 1968[1]) are what Sidman refers to as 'group data'. The points on Gregory's curve appear to be the averages from 20 Ss. However, as Sidman points out, the use of group data is not without its problems.

"The first point to be made is that the mean curve.....is not neccessarily of the same shape as the inferred individual curves......When different....subjects are used to obtain the points determining a functional relation, the mean curve does not provide the information neccessary to make statements concerning the function for the individual."

The inference Gregory obviously wants us to draw from his curve is that all Ss saw his figures in the way indicated but this need not have been the case. A significant number of Ss could have shown the exact opposite effect yet have been outweighed, on average, by the others. On an analysis of variance such a dichotomy would have been shown by a significant Points by Subjects interaction; however, Gregory provides no information as to the statistical tests, if any, that he performed on the data, so we are unable to judge whether or not the obtained curve does represent the trend shown by most Ss.

Estes (1956) says, "The group curve will remain one of our most useful devices both for summarising information and for theoretical analysis provided only that it is handled with a modicum of tact and understanding.....(however) the uncritical use of mean curves.....is attended by considerable risk...... Distortion arises only if unwarranted inferences are drawn from the mean curves."

Gregory has not given us enough information for us to decide whether or not his inferences were warranted. It occurred to the present author that while Ss are viewing figures which are acknowledged to be ambiguous, then it is quite likely that Ss' interpretations of them will vary, hence in this sort of experiment the mean curve might prove very misleading.

The experiments presented in this thesis are of a very similar type to those done by Gregory and, bearing in mind the above, it was decided to present the results in two ways. The results of each individual would be analysed separately as well as the more usual group analysis. This would allow a comparison of whether or not the group curve does in fact truly represent the trend present in the individual results.

There were some differences between experiments but the basic procedure was similar throughout. Each S was asked to 'distance' a number of points on a figure just as in Gregory's experiment. The number of trials for each point varied from six to twenty-four, and the number of points varied from two to six. Sometimes trials were divided into blocks. Analysis of variance was used to assess the various effects i.e. points or sometimes points, blocks and a points by blocks interaction. Our main interest was in the differences between points, however, and these were futher investigated by means of Duncan Multiple Range tests (see Edwards, 1965, p.136-140). This particular test was chosen because it was easiest to compute - an important factor considering the number which had to be done. It enabled us to see which means differed .

The overall analyses generally followed the same pattern as the individual analyses in that all the effects included in the individual analyses were also included in the overall analyses. One other effect was always added i.e. the effect due to differences between Ss. This may be called a 'random' factor since the Ss used represent a tiny fraction of all possible Ss. To obtain an F ratio a main factor mean square was divided by the mean square of its interaction with Subjects e.g. F for, say, Blocks is obtained by dividing the Blocks Mean Square by the Blocks by Subjects Mean Square.

The tables of results always contain full details of the overall analyses. The mean settings for each S at each point are shown alongside the S's name together with a summary of his individual analysis e.g.

| | | | | |
|---|---|---|---|---|
| Smith | 82.6 | 90.4 | 82.8 | Centre > Lft. & Rt. |

EXPERIMENT I--An attempt to replicate Gregory's findings using a modified apparatus and an investigation of the effect of perceived size on perceived distance under reduction conditions.

INTRODUCTION The apparatus described below was selected because it did not allow the S any cues which might be provided by oculomotor adjustments. The figures were so positioned as to be directly in front of the 'seeing' eye, thus removing the need for this eye to converge. As in some of Gogel's (1961) experiments a lens was placed between the figures and the S's eye such that light from the figures was refracted parallel, thus removing the need for the eye to accommodate. This in turn removes the stimulus for the 'non-seeing' eye to converge.

Where then should the S locate the figures? In a normal situation, no convergence and no accommadation would indicate that the figure was somewhere beyond six metres i. e. at optical infinity. This assumes, of course, that the S can use these cues to determine distance--an assumption that is by no means certain. A further difficulty arises here in that our apparatus does not allow the S to indicate that the figure lies any further than two metres. Under these conditions it might be expected that the S will choose an arbitrary distance for his first setting and then attempt to relate his further settings to this distance in an appropriate manner. Epstein, Park and Casey (1961) speculate on this point as follows......."Woodworth and Schlosberg note, 'we do not perceive free-floating objects at unspecified distances,' (1954,P.481). Indeed the object will be located at some specific distance .... However, since the reduced situation is ambiguous it is likely that apparent distance will vary for different Os."

Where the S locates the figures is not really important. What we are really interested in is whether or not he sees it in 3-dimensions and if so, how? We are interested in relative distances rather than absolute ones. By using sterooscopically viewed reference lights to distance various parts of the figure, as Gregory did, we are hoping that the S will answer the question, "Do these figures appear to you to be 3-dimensional?" By using reference lights we are hoping for a more exact answer than a mere verbal response could provide. In the past, verbal responses have been asked for by Pike and Stacey (1968) and Hotopf (1966), their results being at variance with Gregory's. However, it is thought verbal responses in reply to a direct question might well be biased in favour of 3-dimensional responses - by asking the questions explicitly, the possibility of such a response might be suggested to a S to whom it would not otherwise occur. The use of reference lights is not thought to be suggestive in this manner.

There is nothing unusual in the fact that 2-dimensional line drawings should appear 3-dimensional. Simon (1967) has argued persuasively that this is often the simplest way of interpreting them, as have Hochberg and Brooks (1960), Hochberg and McAlister (1953) and Kopfermann (1930), while Gregory (1966)[3] has pointed out that the image of an object on the retina <u>can</u> only be 2-dimensional, even though we interpret 3-dimensional visual space from it. What is unusual about Gregory's theory is that he predicts that ambiguousfigures of a certain type should appear 3-dimensional in one particular way, rather than in any of the other possible ways, by everyone. Only if this prediction is verified can we link the apparent 3-dimensionality to size - constancy and the illusory distortions. Gregory, himself, has verified his own predictions, but in the face of the contradictory evidence from Pike and Stacey and Hotopf, the issue is not settled.

Gregory used only the two sorts of Muller-Lyer (M-L) figure in his experiment. In this experiment one M-L figure is used (the ingoing M-L) and several other types of figures are used as well. Two of these figures are termed 'control figures' and they were included so that the results from them could be compared with those of other figures, which were termed the 'experimental figures.'

The experimental figures were all simple line drawings similar to the M-L, while the control were far more'concrete! The first control figure (Fig.2:I) was a photograph of the corner of a building in which the walls were made of glass and were transparent. It was used by Gregory (1968) to illustrate the sort of real object that an ingoing M-L might represent. It contains clear perspective cues which indicate that the point at which the walls meet is nearer than any other point on the walls.

To illustrate Gregory's position more clearly I quote the legend attached to the picture in his 1968 article:-

"Theory of the M-L illusion favoured by the author suggests that the eye unconsciously interprets the arrow like figures as 3-dimensional structures resembling either the outside or inside corner of a physical structure." To illustrate the sort of real object that an outgoing M-L might represent, Gregory included another picture of the same corner taken from inside the building. It was thought that Gregory's theory requires that the results from an ingoing M-L figure in which the shaft and fin-ends were distanced should be very similar to the results from the picture of what will be referred to as the 'glass corner' in which the meeting point of the walls and other points on the walls were distanced. Should this hot be so then it would appear that the S is not interpreting the ingoing M-L as if it were the outside corner of a building.

The glass corner picture is much less ambiguous than the M-L. It contains clear perspective cues which indicate that it represents one particular 3-dimensional object. Gregory contends that this is also true of the M-L under reduction conditions. By using both figures in the same apparatus it is possible to test this prediction directly.

The second control figure (Fig. 2:2) was a figure illustrating Gibson's cylinder illusion and it too contains clear perspective cues as to the relative distances of the cylinders. These figures were included, apart from other reasons, as a test of the apparatus.

Fig. 2.1 - The 'glass corner'

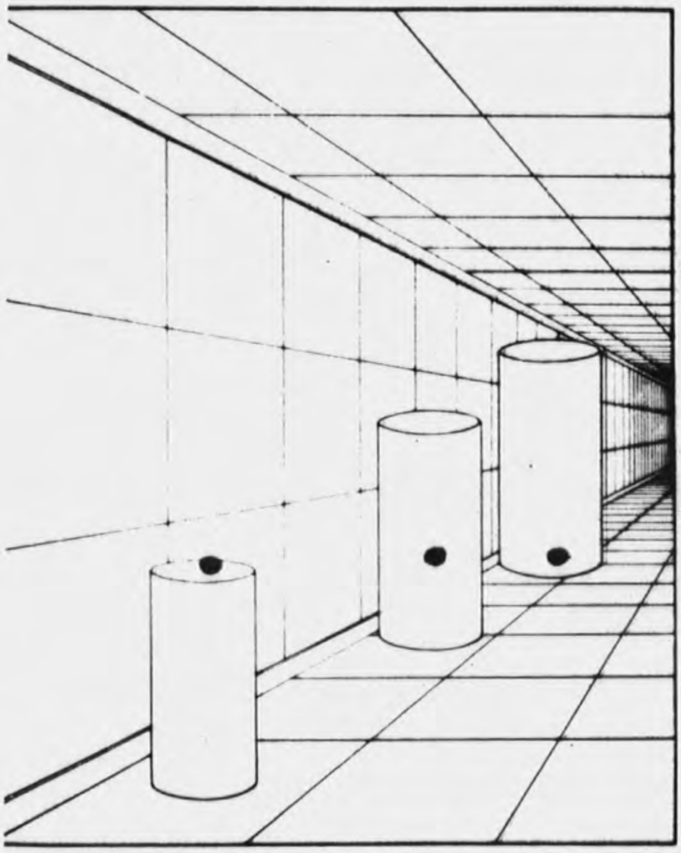


Fig. 2.2 - The 'cylinders' illusion

If Ss did not respond to these figures in the way one would expect given the obvious cues they contain, then it would be unreasonable to expect them to react as expected to the more subtle cues thought to be present in the experimental figures.

The 'glass corner' was included as a direct control for the M-L figure. The cylinders illusion was included as a control for the Ponzo figure (Fig.2:3) although the parallel is not nearly as close. The Ponzo was included not only to test Gregory's predictions but also in the hope that it might shed some further light on the results of Pike and Stacey (1968), as discussed in the last chapter. The ingoing M-L was also modified for the same purpose, in that a vertical line was placed to one side of the M-L (Fig. 2:4) The line was the same length as the shaft of the ingoing figure but, of course, the illusory distortion induced by the fins made the M-L look shorter.

A third experimental figurewas included also. This was a curve (Fig. 2:5) similar to, but not exactly like, the one used by Coren and Festinger (1967). Their results have already been mentioned in the Introduction (p.63). Since these results have a close bearing on Gregory's theory it was decided to try and replicate them as well. Unfortunately the constraints of the apparatus (described below) made it necessary to change the orientation in which the curve was presented to the S. Instead of being presented vertically it was presented horizontally. Gregory (1964) has pointed out that the Ponzo illusion, from which Coren and Festinger believe their results to derive, is unaffected by orientation. The Ponzo figure itself was also presented horizontally in this experiment.

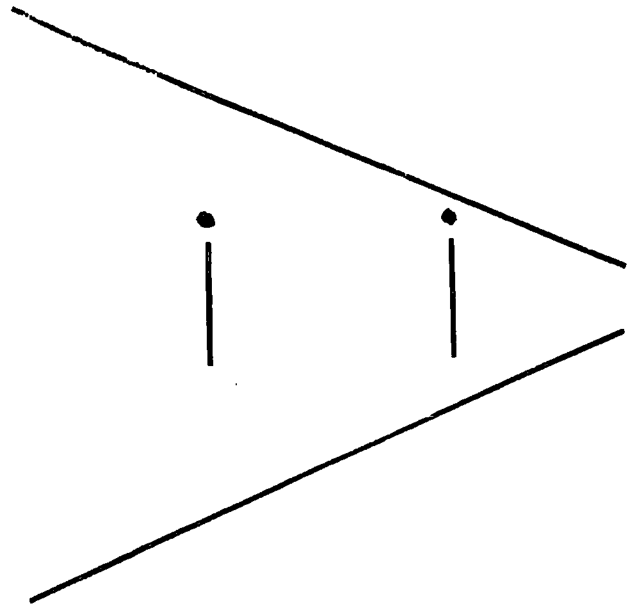


Fig. 2.3 - The Ponzo

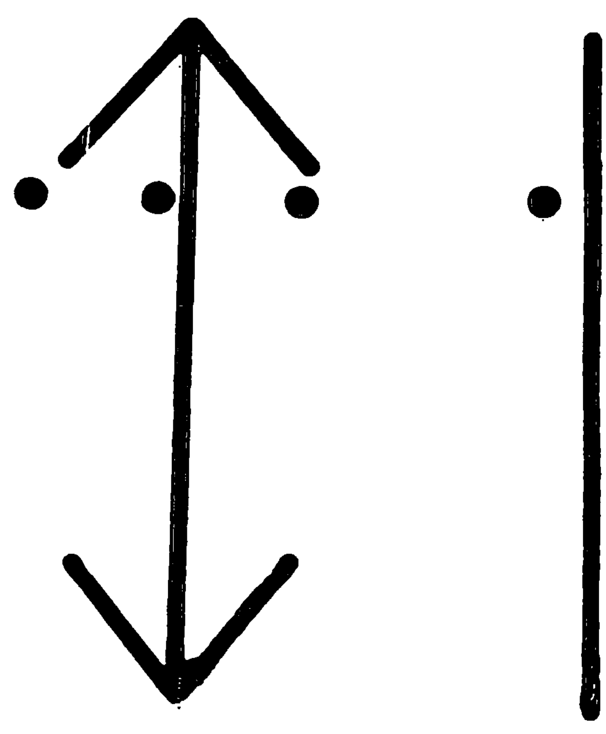


Fig. 2.4 - M-L and line

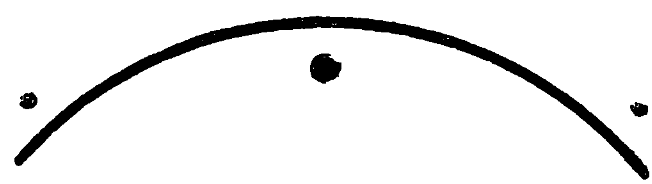


Fig. 2.5 - The curve

The results of Coren and Festinger indicated that their Ss saw the middle of the curve as further away than its wings.

<u>APPARATUS</u> The Curpax Synoptophore was found to be the ideal instrument (Fig. 2:6). Input to the eyes is completely independent thus precluding the possibility of 'ghosting'. Figures can be presented as back-illuminated slides (3.25" x 3.25") by planting them into the 'arms' of the apparatus. They are viewed through a half-silvered mirror which is included in the eye-piece, thus a figure presented in this way appears to 'float' somewhere in the Ss' normal visual field. The eye-pieces of this remarkable instrument also include a lens which refracts the light from the figure so that it is parallel when it enters the eye; thus the eye accommodates as if the figure were at 'infinity' i.e. it does not accommodate at all.

The S must look directly in front of him to see the figure i.e. his convergence is 0 degrees. Thus both accommodation and convergence indicate that the figure's true distance is 'infinity' i.e. beyond 6 metres. This is, of course, ambiguous in the sense that the figure could lie at <u>any</u> distance beyond 6 metres and there are no cues to tell the S that the figure is flat; we would expect Gregory's predicted effects to enjoy perfect conditions in which to manifest themselves.

It is perhaps easiest to consider the present arrangement as the reverse of Gregory's, since the S now sees the figure by means of the half-silvered mirror instead of the reference light. In our apparatus the reference light is set up directly before the S.

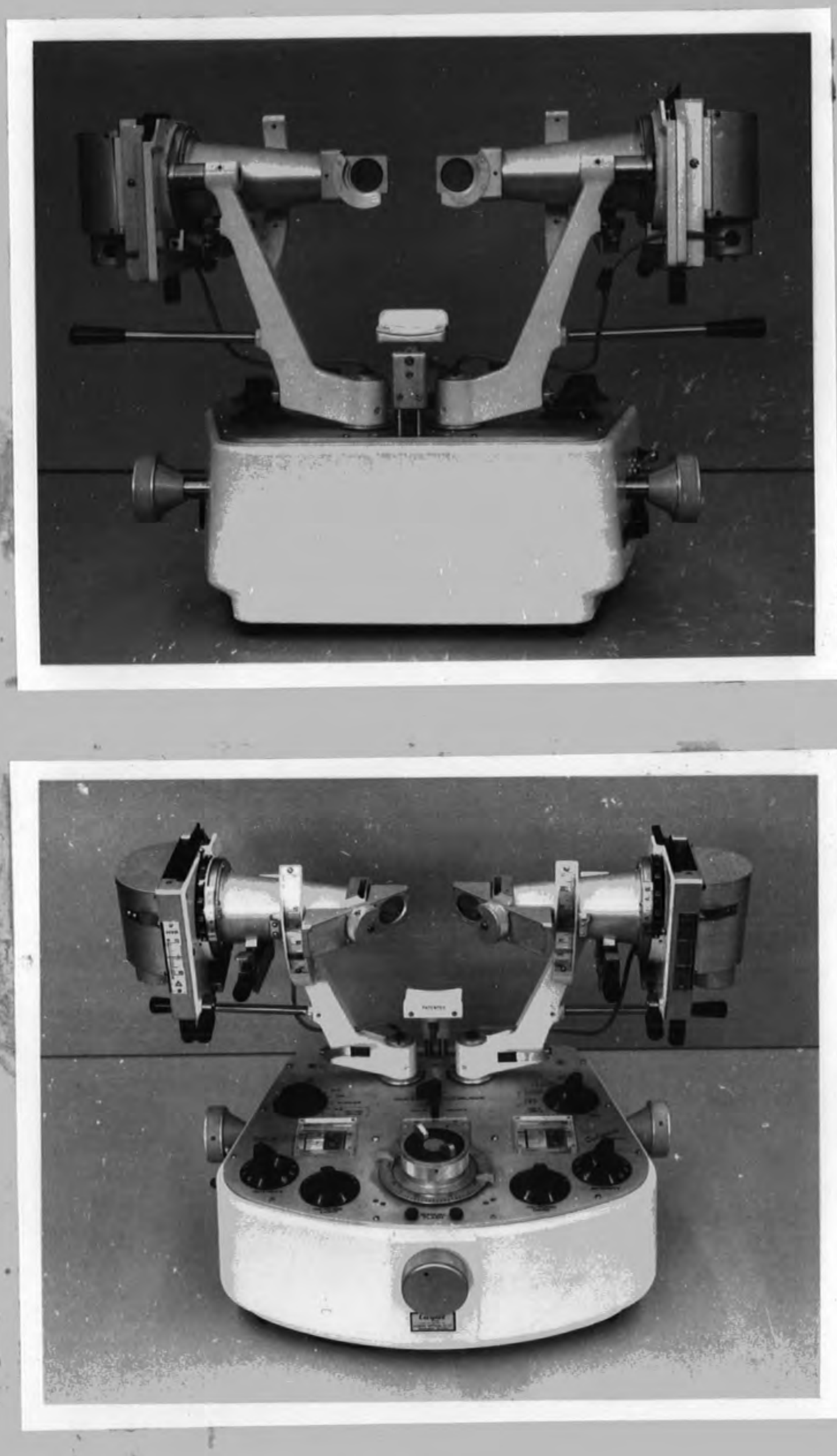


Fig. 2.6 - The Synpptophore

In all cases the figure was presented only to the S's right eye. The reference light was mounted on an optical bench 2 metres long. At its closest the light was 20 cms. away from the eye and at its furthest it was 220 cms. The Synoptophore was placed on the end of a long table in a room that could be totally blacked out. A scale was marked on the table top so that the distance of the light from the eye could easily be measured. Arcs were drawn on the table top IO cms. apart using the point where a line dropped perpendicularly from the centre of the eye met the table as the centre of the circle. Thus the distance of the light from the eye could easily be determined by reference to the scale, no matter where it might be. This was a great help when measuring in the dark. The reference light itself was mounted on an optical bench saddle. It was an ordinary pea-bulb powered by a six volt battery strapped to the saddle.

<u>FIGURES</u> The figures used were as illustrated. A dot marks the positions in which the reference light was seen.

Fig. I was an ingoing M-L arrow with an angle of 60 degrees between the fins and the shaft, i.e. the angle at which Gregory found the greatest depth effect. To the right of the M-L shaft was a line of the same length and thickness.

Fig. 2 was a photographic transparency of the corner of a building taken from the outside. This figure is referred to as the glass corner.

Fig. 3 was a Ponzo illusion figure presented on its side.

Fig. 4 was a photographic transparency of Gibson's 'cylinders illusion.'

Fig. 5 was a curve similar to the one used by Coren and Festinger (I967). It was presented horizontally.

Figs. 2 and 4 are the 'control' figures.

Figs. I,3 and 5 are the'experimental' figures.

SUBJECTS Six Ss were used, of whom four were male and two female. All were aged between I9 and 28 years. Two were post-graduates psychologists.

PROCEDURE It was hoped to make the present experiment as similar to Gregory's as possible so that the results would be comparable, but a number of changes were found to be necessary. The differences in the apparatus have already been mentioned. It was also necessary to devise our own procedure since Gregory does not make it clear exactly what his was. It seems that his Ss set their reference lights to the apparent distance of the shafts of the M-L figures and the fins. The only information Gregory gives is the following:- "by moving the lights so that they seem to coincide with the apparent distance of selected parts of the picture we can plot the visual space of the observer," (I968)', and, "the reference light can be moved around in 3 dimensions and so a 3-dimensional plot of visual space is obtained."(I966)[4]. No mention is made of placing the light near the part of the figure to be'distanced'. It was decided to design this experiment so that the light would be near the relevant part of the figure when the comparison was made, so as to make it easier.

As mentioned above it is believed that Gregory's Ss only distanced one fin-end. This is assumed because had more been done there must surely have been some mention of the fact since the results plotted on the published graph (1968)' would be some kind of average. Had, say, two fin-ends been distanced and the results found to be identical, Gregory would surely have mentioned it since this is what his theory would predict. In fact it would be embarassing if there were a depth distance between fin-ends.

In the present experiment both top fin-ends were distanced. Strictly speaking all four fin-ends should be done but it was thought that this would have prolonged the experiment beyond the endurance of most Ss.

Another point which Gregory does not make clear is how the reference light appeared to move to the S. In my opinion it is important that the light should maintain a constant position relative to the figure, no matter what its distance from the S. If it should appear to move across the figure, either vertically or horizontally, as its distance changes then a serious experimental artifact could occur e.g. if the light moves across the figure as its distance changes, say from one side of the M-L shaft to the other, then the S might make his settings at the point of intersection rather than at the point where they really seem equidistant. Ideally then the only indications that the S should have of the light changing its distance are stereoscopic cues and the change in size of the bulb.

In order to fulfill these conditions it was necessary to realign the optical bench carrying the reference light each time a different part of the figure was 'distanced' (Fig. 2:7). Marks were made on the bench top to indicate the approximate position of the optical bench for each different part of the figure, but the final adjustments were always made on the instructions of the S.

So that the light would appear to neither rise nor fall as it was moved, it was kept at eye-level at all times. If the part of the figure under consideration was at the wrong height, the whole figure was raised or lowered accordingly by means of the controls on the Synoptophore.

The S entered the experimental room with the lights on so that he could seat himself in front of the eye-pieces. He was asked to look through them and the lights were extinguished. After a few moments in which the S studied the figure, the lights were turned back on and the S was asked to look at the blackboard on his left. The experimenter then drew a rough sketch of the appropriate figure and said, "When you looked through the eye-pieces you saw a figure like this (pointing) and you will also have noticed a small light. I can move this light towards you and away from you and also from side to side, as you will see. I would like you to instruct me so that I may position this light so that it appears in the position marked by the crosses (draws crosses at the relevant points on the sketch). First of all I would like you to tell me how to move the light so that it appears in this position" (points to the relevant cross). Further instructions were given according to the position mentioned, e.g. "just to the left of the end of the left hand fin of the M-L figure." The room lights were then extinguished and the S asked to look through the


Fig. 2.7 - Diagram showing the different alignments of the optical bench in order to "distance" the right and left fin-ends.

The reference light was covered and moved to an extreme position on the optical bench i.e. as close, or as far as it would go. It was then uncovered and the S was asked to give instructions for its adjustment. It should be noted that adjustment was by the experimenter and not by the S himself. This was another difference between this experiment and those of Gregory and Coren and Festinger. When the S was satisfied that the light and the relevant part of the figure were equidistant, the light was covered and a dim torch used to read its distance from the eye off the scale on the bench top - a special pointer was attached to the saddle carrying the light for this purpose. When the experimenter had noted the distance, the light was moved to the opposite end of the optical bench from that at which the previous trial had begun, and uncovered.

Six trials were completed in this manner, each one starting at the opposite end of the bench from its predecessor. The starting end for the first trial was alternated for each S. After the sixth trial the optical bench was aligned for the next point on the figure in the manner already described and another six trials were done. In this way six readings were obtained from each point on the figure. This constituted the first 'Block' of readings. Three more such blocks were completed giving a total of 24 readings from each point. The order in which the points were taken within each block was randomised and different for each S.

It usually took one three hour session to obtain all the readings from all five figures (360) in all.

RESULTS A 4 x 2, 3 or 4 Factorial Analysis with six observations per cell was performed on each S's results for each figure, taking Points and Blocks as the main effects. Since a relatively large number of these analyses were performed i.e. 30, it was decided to look for significance beyond the .OI level in order to minimise the possibility of chance significance.

Gregory used several fin angles but he took only three readings at each point from his 20 Ss. The published graph (I968) was obtained by pooling these results. By taking 24 readings from each point enough data was available to allow individual analysis of each S's results. An analysis was also performed on the pooled data for each figure allowing a comparison between the two methods.

Control Figures Results from the control figures i.e. Figs. 2 and 4, were very much as expected. The glass corner was seen by five of the six Ss as 3-dimensional in the expected way i.e. the two side pillars of the corner were seen as equidistant and further than the centre pillar. The other S also saw the centre pillar as nearestbut he did not see the side pillars as equidistant.

The cylinders illusion was seen in the way predicted by three of the six Ss i.e. the 'small' cylinder was seen as nearest and the 'large' cylinder as furthest. Two of the other Ss ordered them correctly but the differences were not large enough for significance. One S did not order the cylinders as expected.

Some Ss showed a significant Blocks effect i.e. the distance at which they made their settings differed at different times. Three Ss showed this effect for the glass corner and one for the cylinders illusion. None of these Ss showed a significant Points

RESULTS The Glass Corner

MEANS (cms.)

| SUBJECT | LFT. FIN | SHAFT | RT. FIN | Significant Difference |
|---|---|---|---|---|
| Grundy | 112.5 | 82.1 | 108.9 | Lft. & Rt. > Centre |
| Thomson | 56.3 | 48.9 | 56.1 | NONE (Flat) |
| Templeton | 105.6 | 62.2 | 108.2 | Lft. & Rt. > Centre |
| Martin | 54.2 | 37.5 | 41.8 | Lft. & Rt. > Centre |
| Bateman | 78.9 | 66.1 | 83.9 | Lft. & Rt. > Centre |
| Cross | 116.0 | 51.7 | 107.9 | Lft. & Rt. > Centre |
| MEANS | 87.2 | 58.1 | 84.5 | |

OVERALL ANALYSIS OF VARIANCE

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | 5 | 6302.3 | |
| BLOCKS | 3 | 62.7 | 0.28 (N.S.) |
| POINTS | 2 | 6223.6 | 9.8 *** |
| BLOCKS X Ss | 15 | 220.5 | |
| POINTS X Ss | 10 | 633.8 | |
| POINTS X BLOCKS | 6 | 11.9 | 0.03 (N.S.) |
| POINTS X BLOCKS X Ss | 30 | 373.1 | |

DUNCAN TEST OF DIFFERENCES BETWEEN POINTS

| MEANS | 58.1 | 84.5 | 87.2 | SHORTEST SIG. RANGE |
|---|---|---|---|---|
| 58.1 | - | 26.4** | 29.1** | 21.4 |
| 84.5 | | - | 2.7(N.S.) | 22.4 |
| 87.2 | | | - | |

Lft. & Rt. > Centre

## The Cylinders Illusion

| SUBJECT | Lft. | Centre | Rt. | Significant Differences |
|---|---|---|---|---|
| | | MEANS (cms.) | | |
| Grundy | 64.0 | 90.4 | III.3 | Rt. > Centre > Lft. |
| Thomson | 58.3 | 7I.7 | 96.8 | Rt. > Centre > Lft. |
| Templeton | 45.I | 77.0 | 93.8 | Rt. > Centre > Lft. |
| Martin | 34.I | 84.6 | 7I.8 | Rt. > Centre > Lft. |
| Bateman | 67.3 | 72.4 | 77.7 | Rt. > Lft. |
| Cross | 43.6 | 7I.0 | 99.2 | Rt. > Centre > Lft. |
| MEANS | 52.I | 77.8 | 9I.8 | |

### OVERALL ANALYSIS OF VARIANCE

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | 5 | 826.0 | |
| BLOCKS | 3 | I08.2 | I.4I (N.S.) |
| POINTS | 2 | 9753.4 | I9.9 *** |
| BLOCKS X Ss | I5 | 76.9 | |
| POINTS X Ss | I0 | 49I.I | |
| POINTS X BLOCKS | 6 | I5.7 | 0.06 (N.S.) |
| POINTS X BLOCKS X Ss | 30 | 250.5 | |

### DUNCAN TEST OF DIFFERENCE BETWEEN MEANS

| MEANS | 52.I | 77.8 | 9I.8 | Shortest Sig. Range(.0I&.05) | |
|---|---|---|---|---|---|
| 52. I | - | 25.7** | 39.7** | 20.2 | I3.2 |
| 77.8 | | - | I4.0** | 2I.I | I3.9 |
| 9I.8 | | | - | | |

Rt. > Centre > Lft.

by Blocks interaction, i.e. they always saw the points in the same relative positions irrespective of distance.

The significances of differences between Point means were tested by Duncan New Multiple Range Tests (see Edwards, 1960, p. 136-140). Significance was sought beyond the .01 level for the same reason mentioned earlier, i.e. to reduce the chance significanee, given the large number of analyses done

The overall analyses for these two figures tended to reflect the trends found in the individual analyses. The glass corner side pillars were apparently equidistant with the centre pillar nearer. The cylinders were ordered as predicted. The differences between the 'small' cylinder and the other two were significant beyond the .01 level, but the difference between the middle and 'large' cylinders did not quite reach the .05 level.

(Significances beyond the .05 level were thought to be acceptable in overall analysis because far fewer overall analyses were performed, hence the risk of chance significance was that much less.)

Neither figure yielded either a significant overall Blocks effect or a significant overall Blocks by Points interaction.

<u>Experimental Figures</u> All possible results from these figures are covered by the four possibilities listed below:-

1. The figures are seen as <u>FLAT</u>, there being no significant differences between points. These individual analyses are marked FLAT.

2. The figures are seen as predicted by Gregory's theory. These will be marked P.G. (for pro-Gregory).

3. The figures were seen in a manner opposite to that predicted by Gregory. These will be marked A.G. (for anti-Gregory).
4. The analysis showed significant differences which did not fall into any of the above categories. These will be marked U.N.C. (for unclassified).

The results from the M-L are obviously at variance with Gregory's predictions. Only one S saw the figure as he predicts, while three had no significant differences; one saw it in exactly the opposite way and one S was 'unclassified'.

Three Ss saw the line and the shaft at the same apparent distance, two saw the line as further away and only one S saw the line as nearer (as predicted by the SDIH).

The overall analysis showed no significant differences between Point means.

All six Ss saw the two lines of the Ponzo illusion at the same apparent distance. The overall analysis reflects this.

Five Ss yielded no significant differences on the Curve. The other S was 'unclassified'. Once again there were no differences between Point means on the overall analysis.

As with the control figures a number of Ss showed a tendency to change the distance of their settings over time, as shown by a significant Blocks term in their individual analysis. No significant trend existed as evidenced by the non-significance of the term in the overall analyses. The number of individual Ss showing this effect were as follows:- three on the M-L; one on the Ponzo; one on the curve.

One S in one figure (Martin, M-L) yielded a significant Points by Blocks interaction. This means that the relative

## RESULTS The Ingoing M-L

MEANS (cms.)

| SUBJECT | LFT. FIN | SHAFT | RT. FIN | SIGNIFICANT DIFFERENCES | |
|---|---|---|---|---|---|
| Grundy | 102.0 | 82.1 | 101.9 | Lft & Rt > Shft | (PG) |
| Thomson | 49.0 | 53.2 | 47.6 | NONE (Flat) | |
| Templeton | 65.1 | 69.6 | 68.2 | NONE (Flat) | |
| Martin | 44.0 | 47.6 | 41.3 | NONE (Flat) | |
| Bateman | 70.7 | 89.7 | 76.6 | Shft > Lft | (UNC) |
| Cross | 65.4 | 73.0 | 62.4 | Shft > Lft & Rt | (AG) |
| MEANS | 66.0 | 69.2 | 66.3 | | |

## The Shaft vs. Line Comparison

MEANS (cms.)

| SUBJECT | SHAFT | LINE | SIG. OF DIFFERENCE |
|---|---|---|---|
| Grundy | 82.1 | 97.2 | Line > Shaft |
| Thomson | 53.2 | 50.3 | Not Sig. |
| Templeton | 69.6 | 68.9 | Not Sig. |
| Martin | 47.6 | 57.9 | Line > Shaft |
| Bateman | 89.7 | 89.5 | Not Sig. |
| Cross | 73.0 | 64.4 | Shaft > Line |
| MEANS | 69.2 | 71.5 | |

OVERALL ANALYSIS OF VARIANCE FOR INGOING M-L & LINE

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | 5 | 5425.6 | |
| BLOCKS | 3 | 34.2 | 0.2 (N.S.) |
| POINTS | 3 | I63.I | 0.9 (N.S.) |
| BLOCKS X Ss | I5 | I59.8 | |
| POINTS X Ss | I5 | I76.7 | |
| POINTS X BLOCKS | 9 | 35.0 | 0.2 (N.S.) |
| POINTS X BLOCKS X Ss | 45 | I49.I | |

DUNCAN TEST OF DIFFERENCES BETWEEN POINTS

| MEANS | 60.3 | 66.3 | 69.2 | 7I.5 | SHORTEST SIG. RANGE(.05) |
|---|---|---|---|---|---|
| 66.0 | - | 0.3 | 3.2 | 5.5 | 8.2 |
| 66.3 | | - | 2.9 | 5.2 | 8.5 |
| 69.2 | | | - | 2.3 | 8.8 |
| 7I.5 | | | | - | |

**RESULTS** **The Ponzo**

**MEANS (cms.)**

| **SUBJECT** | **APEX** | **BASE** | **Sig. of Difference** |
|---|---|---|---|
| Grundy | I04.I | I04.9 | Not Sig. |
| Thomson | 82.6 | 8I.6 | Not Sig. |
| Templeton | 67.7 | 66.0 | Not Sig. |
| Martin | 52.9 | 64.7 | Not Sig. |
| Bateman | 7I.8 | 69.4 | Not Sig. |
| Cross | 64.3 | 64.0 | Not Sig. |
| **MEANS** | 73.9 | 75.I | |

**OVERALL ANALYSIS OF VARIANCE**

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | 5 | 22I7.I | |
| BLOCKS | 3 | I04.5 | 0.8 (N.S.) |
| POINTS | I | I6.8 | 0.3 (N.S.) |
| BLOCKS X Ss | I5 | I33.I | |
| POINTS X Ss | 5 | 56.4 | |
| POINTS X BLOCKS | 3 | 20.2 | 0.I (N.S.) |
| POINTS X BLOCKS X Ss | I5 | 202.8 | |

**APEX & BASE** do not differ significantly

**RESULTS** **The Curve**

MEANS (cms.)

| SUBJECT | LFT. WING | CENTRE | RT. WING | Significant Diffreence |
|---|---|---|---|---|
| Grundy | 74.2 | 68.9 | 70.I | NONE (Flat) |
| Thomson | 35.2 | 35.2 | 37.2 | NONE (Flat) |
| Templeton | 60.9 | 56.4 | 58.7 | Lft > Centre (UNC) |
| Martin | 38.5 | 37.7 | 38.3 | NONE (Flat) |
| Bateman | 37.2 | 38.0 | 37.8 | NONE (Flat) |
| Cross | 53.0 | 54.5 | 56.8 | NONE (Flat) |
| MEANS | 49.8 | 48.4 | 49.8 | |

**OVERALL ANALYSIS OF VARIANCE**

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | 5 | 1857.2 | |
| BLOCKS | 2 | I50.6 | 0.3 (N.S.) |
| POINTS | 2 | II.7 | 0.I (N.S.) |
| BLOCKS X Ss | IO | 5I9.3 | |
| POINTS X Ss | IO | 84.9 | |
| POINTS X BLOCKS | 4 | 8.I | 0.2 (N.S.) |
| POINTS X BLOCKS X Ss | 20 | 55.I | |

**DUNCAN TEST OF DIFFERENCES BETWEEN POINTS**

| MEANS | 48.4 | 49.8 | 49.8 | SHORTEST SIG. RANGE |
|---|---|---|---|---|
| 48.4 | - | I.4(NS) | I.4(NS) | 6.8 |
| 49.8 | | - | 0.0(NS) | 7.I |
| 49.8 | | | - | |

No significant differences.

positions of his Points changed over Blocks, or, in other words, the way in which he saw the figure changed during the course of the experiment. A full discussion of the importance of this effect is postponed until experiment 2 where many more Ss showed it.

Before the start of the experiment it was thought that the first point at which the reference light was seen by the S i.e. near or far, might exert some influence on the distance of the settings.

Wallach and McKenna (1960) found that when Ss were asked to match the sizes of a square seen under reduction conditions, they tended to choose larger squares when the series of squares they had to choose from began with large squares, than when it began with small squares. However, Epstein and Landauer did not find this tendency in their experiment. In our experiment, such a 'position' effect would not be important in itself, but it could assume importance should it interact with the Points term - this would mean that the Ss saw the figure differently according to where they first saw the reference light. No S was found to have such an interaction and only one S showed a significant effect of Position. It was decided that it would be safe to ignore this factor in future experiments in the interest of speeding up the procedure. The Position factor was not included in the overall analysis.

<u>DISCUSSION</u> The results are directly contrary to the findings of Gregory on the M-L and Ponzo figures and Coren and Festinger on the curve. The overall analyses showed no significant differences in the directions predicted. It would seem quite clear from

this that the experimental figures do not contain the same sort of perspective cues as the control figures ~~and~~ under reduction conditions.

This complete failure to replicate the results of previous experiments is puzzling and begs the question of how these previous results were obtained - what factors were operating there that were not present here? A large part of the remaindér of this thesis will be devoted to answering this question.

The overall analysés tended generally to reflect the results of the individuals without being misleading. However, this is not entirely true for the M-L. Only half the individual Ss gave data indicating that they saw the figure very definitely in 3-dimensions - one being pro-Gregory and the other anti-Gregory. Because these two Ss give results of a directly opposite nature, they cancel each other out in the overall analysis, but even so to overlook them, as would happen if only the overall analysis is considered, would be to gain a false view of the findings.

Pike and Stacey's (1968) results indicated that half their Ss saw the figures as flat, which is just what we found. We must endorse their conclusion that, "only a minority of people spontaneously see luminous M-L figures 3-dimensionally.

The original suggestion made in the Introduction was that by removing the cues of accommodation and convergence the effects Gregory reports might actually be enhanced. Obviously this was not confirmed.

The prediction from the SDIH that under reduction conditions all objects are assumed to be equidistant was supported by the data.

In the Ponzo all six Ss saw the lines as apparently equidistant. Only three saw the shaft and the line in this way. These findings are generally against those of Epstein and Landauer (1969), Gogel (1969) and others. They would have expected the apparently longer line to have appeared nearer. However, after some thought it was decided that the results of this experiment could have been caused by an 'outside' influence.

As mentioned in the procedure, the glass corner and the M-L were always shown first. In the beginning these two figures were intended to comprise the whole experiment. A quick glance at the results revealed that they were not as decisive as had been hoped in the light of Gregory's results. Thus the Ponzo and cylinders figures were added in the hope that the situation might become clearer. Last of all the curve was added. Unfortunately the experimenter, believing that the Ss would not be needed again, discussed the purpose of the experiment with them on their completing the first two figures. It is thought that the views expressed by the experimenter in this discussion might have influenced the Ss in their responses to the other figures. The experimenter expressed some scepticism concerning Gregory's theory and, in retrospect, it was thought that this might have 'set' the Ss to give flat responses.

To test this possibility and to widen the number of Ss tested the whole experiment was repeated using another ten Ss who would not be told anything at all about the experiment's purpose.

Of course this 'set', if it had any influence at all, would only have had an effect on the results of the last three figures.

It was noted that Ss differed considerably in the distances at which they made their settings. This is as one would expect if the S chose the distance of his first setting arbitrarily as was suggested in the Introduction. If there are no cues to distance why should there be a tendency to favour one distance rather than another? However, this reasoning would seem to be contrary to Gogel's (1969) idea of a 'specific distance tendency'. This point will be returned to in later experiments. It might be noted that Gogel does admit the following:-

"It is not clear why D on the first presentation ..... was sometimes quite different between experiments with different groups of observers for the same values of O."

It was hypothesised in the Introduction that Ss ought to locate the figures somewhere beyond 6 metres. Gogel mentions the theories of Schober (1954) which would seem to suggest a different intepretation. Schober believes that the resting position for the accommodation of the eyes is located between the near and far points and he suggests a range of 50 to 200 cms. If Schober is correct this would remove one of the ambiguities from the experimental situation, ie that the figure is located beyond 6 metres yet the S cannot use the apparatus to indicate distances of more than 2 metres.

Perhaps the findings of Hofstetter should be mentioned here as well. It was suggested by Tait (1933) that accommodation could be stimulated by a S's awareness of the proximity of the stimulus. If this were true then the present experimental set up could be a case to point. Hofstetter investigated the possibility but concluded that, "the data provide no evidence for proximal accommodation."

## EXPERIMENT 2. - AN ATTEMPT TO REPLICATE AND EXPAND THE FINDINGS OF EXPERIMENT I USING 'NAIVE' Ss

INTRODUCTION The intention of this experiment was to repeat experiment I using Ss who could be termed 'naive'. In experiment I Ss had received a certain amount of information concerning the purpose of the experiment and it was thought that this might have set them to produce 'flat' results. The Ss used here were told nothing of the experiment's purpose and it was predicted that if 'set' was unimportant, then their results would follow a similar pattern to those of experiment I.

APPARATUS The apparatus was exactly as in experiment I.

FIGURES The figures were exactly as in experiment I apart from one addition. The main purpose of the experiment was to examine Gregory's previous findings thus it was decided to broaden the basis of comparison by adding an outgoing M-L to the figures already used. The new figure had an angle of 150 degrees between its shaft and fins i.e. the angle at which Gregory found the greatest depth effect (Fig.2:8)

The addition of this figure allows us to test Gregory's prediction that the position of the shaft in relation to the fins will reverse according to whether the figure is ingoing or outgoing i.e. the shaft should be seen as nearer than the fins if it is outgoing.

Four of the Ss used in experiment I were re-engaged to complete this figure. The other two were unobtainable.

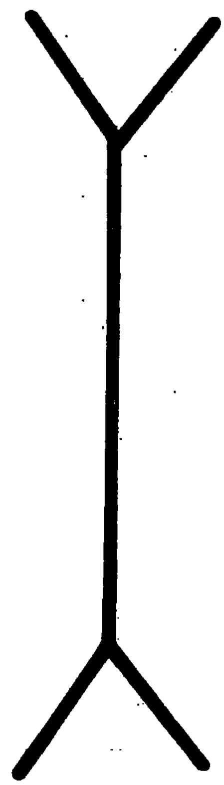


Fig. 2.8 - The outgoing M-L

SUBJECTS Ten Ss were used. Six were male and four female. All were aged between I9 and 2I years. Three of them were under-graduates in the Psychology department but questioning revealed that they had no knowledge of the theories involved. All were paid for their participation.

PROCEDURE The procedure was as for experiment I apart from a few minor changes. It was difficult and tedious when working in the dark to mark trials according to their starting position i.e. 'near' or 'far' and since the results of experiment I indicated that this factor was not of much significance it was decided not to bother recording it. This decision tended to speed up the experiment somewhat, to the benefit of S and the experimenter alike.

The order in which figures were presented was as for experiment I except that the outgoing M-L was added to the first group with the ingoing M-L and the glass corner. The order in which these three were presented was randomised.

RESULTS The results were computed in the same way as for experiment I.

Control Figures The results from the control figures were almost identical to those obtained in experiment I.

Individually, all Ss saw the centre pillar of the glass corner as nearer but three Ss saw the side pillars at significantly different distances. The overall analyses showed the centre pillar as nearer and the side pillars as equidistant.

RESULTS The Glass Corner

MEANS (cms.)

| SUBJECT | Lft. | Centre | Rt. | Significant Differences |
|---|---|---|---|---|
| Aldous | I00.4 | 77.4 | 98.7 | Lft. & RT. > Centre |
| Hockey | 85.3 | 63.2 | 98.5 | Rt. > Lft. > Centre |
| Holmes | II3.8 | 94.7 | II7.4 | Lft. & Rt. > Centre |
| Davies | I52.5 | II4.8 | I55.0 | Lft. & Rt. > Centre |
| James | I22.6 | 97.3 | I43.5 | Rt. > Lft. > Centre |
| Buckingham | I55.0 | I35.8 | I56.4 | Lft. & Rt. > Centre |
| Lee | 87.2 | 7I.8 | 89.6 | Lft. & Rt. > Centre |
| Curless | II8.8 | I02.2 | II8.I | Lft. & Rt. > Centre |
| Underwood | 72.3 | 52.9 | 82.0 | Rt. > Lft. > Centre |
| Hinchliffe | 92.3 | 85.4 | 89.0 | Lft. & Rt. > Centre |
| MEANS | II0.0 | 89.5 | II4.7 | |

OVERALL ANALYSIS OF VARIANCE

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | 9 | 8330.I | |
| BLOCKS | 3 | 8I.3 | 0.27 (N.S.) |
| POINTS | 2 | 7I69.2 | 38.4 *** |
| BLOCKS X Ss | 27 | 300.6 | |
| POINTS X Ss | I8 | I86.7 | |
| POINTS X BLOCKS | 6 | 42.0 | 0.I6 (N.S.) |
| POINTS X BLOCKS X Ss | 54 | 254.6 | |

DUNCAN TEST OF DIFFERENCE BETWEEN MEANS

| MEANS | 89.5 | II0.0 | II4.7 | Shortest Sig. Range (0.5 & 0.I) | |
|---|---|---|---|---|---|
| 89.5 | - | 20.5 | 25.2 | 6.4 | 8.8 |
| II0.0 | | - | 4.7 | 6.7 | 9.2 |
| II4.7 | | | - | | |

Rt. & Lft. > Centre

**RESULTS** **The Cylinders Illusion**

**MEANS (cms.)**

| SUBJECT | Lft. | Centre | Rt. | Significant Differences |
|---|---|---|---|---|
| Aldous | 56.5 | 67.2 | 7I.5 | Centre & Rt. > Lft. |
| Hockey | 54.8 | 59.5 | 63.2 | Rt. > Lft. |
| Holmes | 68.0 | 9I.7 | 97.4 | Centre & Rt. > Lft. |
| Davies | 94.3 | I07.0 | I32.4 | Rt. > Centre > Lft. |
| James | 68.8 | 76.8 | I0I.5 | Rt. > Centre > Lft. |
| Buckingham | II8.I | I35.2 | I45.0 | Rt. > Centre > Lft. |
| Lee | 60.6 | 66.8 | 7I.6 | Rt. > Lft. |
| Curless | 95.5 | I06.9 | II5.3 | Rt. & Centre > Lft. |
| Underwood | 62.9 | 8I.0 | 94.0 | Rt. > Centre > Lft. |
| Hinchliffe | 77.0 | 89.0 | 94.9 | Rt. & Centre > Lft. |
| **MEANS** | **75.7** | **88.I** | **98.7** | |

**OVERALL ANALYSIS OF VARIANCE**

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | 9 | 6392.8 | |
| BLOCKS | 3 | I8.9 | 0.37 (N.S.) |
| POINTS | 2 | 53I4.4 | 39.9*** |
| BLOCKS X Ss | 27 | 5I.6 | |
| POINTS X Ss | I8 | I33.3 | |
| POINTS X BLOCKS | 6 | 44.6 | 0.4I (N.S.) |
| POINTS X BLOCKS X Ss | 54 | I08.6 | |

**DUNCAN TEST OF DIFFERENCE BETWEEN MEANS**

| MEANS | 75.7 | 88.I | 98.7 | Shortest Significant Range (.0I) |
|---|---|---|---|---|
| 75.7 | - | I2.4 | 23.0 | 7.5 |
| 88.I | | - | I0.6 | 7.8 |
| 98.7 | | | - | |

**Rt. > Centre > Lft.**

All Ss ordered the cylinders as expected but only three Ss had all the differences significant. The overall analysis did show all the differences to be significant, however.

Seven Ss had significant Blocks effects in their individual analyses for the glass corner and two did for the cylinders illusion. This was a slight increase in the proportion of Ss showing this tendency over the first experiment i.e. from one-third to a half.

Three Ss from the glass corner and two from the cylinders illusion showed significant Points by Blocks interactions.

Apart from the Points effect none of the other terms in either overall analysis was significant.

Experimental Figures  Four Ss saw the ingoing M-L as flat i.e. with no significant differences; four Ss were pro-Gregory, three of these seeing the fin-ends as equidistant, and two Ss were 'unclassified'. None were anti-Gregory. These results were rather similar to those of experiment I as far as the proportion falling into each category is concerned, except perhaps in the case of the 'anti-Gregory' category since one S was so classified in experiment I but none were in this experiment. There was also a rise in the proportion of 'pro-Gregory' results from I7% in experiment I to 40% here. The other two categories remained relatively stable i.e. 'Flat', 50% to 40%; 'Unclassified', I7% to 20%.

The comparison between the line and the shaft produced only two Ss who did not see them as equidistant. One S saw the line as nearer, while the other saw it as further than the shaft.

Four Ss saw the outgoing M-L (Fig.2:8) as flat, four were anti-Gregory, three of these seeing the fin-ends as equidistant, and two Ss were 'unclassified'. None were pro-Gregory.

**RESULTS** **The Ingoing M-L**

**MEANS (cms.)**

| **SUBJECT** | **Lft.Fin** | **Shaft** | **Rt.Fin** | **Significant Differences** |
|---|---|---|---|---|
| Aldous | 77.I | 82.5 | 78.0 | None (Flat) |
| Hockey | 80.I | 49.7 | 79.2 | Lft. & Rt. > Shaft (P.G.) |
| Holmes | IOI.0 | IO5.6 | 90.5 | Lft. & Shaft > Rt. (UNC.) |
| Davies | I22.7 | 99.3 | I20.I | Lft. & Rt. > Shaft (P.G.) |
| James | 89.8 | 99.I | IOI.2 | Rt. > Lft. (UNC.) |
| Buckingham | I65.3 | I35.3 | I49.7 | Lft. & Rt. > Shaft (P.G.) |
| Lee | 65.2 | 7I.0 | 72.0 | None (Flat) |
| Curless | IOI.0 | IO5.4 | 98.0 | None (Flat) |
| Underwood | 69.4 | 60.8 | 76.I | Rt. > Lft. > Shaft (P.G.) |
| Hinchliffe | 93.I | 89.6 | 92.6 | None (Flat) |
| **MEANS** | **96.5** | **89.9** | **95.7** | |

**OVERALL ANALYSIS OF VARIANCE**

| | **D.F.** | **MEAN SQS.** | **F** |
|---|---|---|---|
| SUBJECTS | 9 | I0226.4 | |
| BLOCKS | 3 | 99.0 | 0.62 (N.S.) |
| POINTS | 3 | 424.6 | I.09 (N.S.) |
| BLOCKS X Ss | 27 | I59.4 | |
| POINTS X Ss | 27 | 390.2 | |
| POINTS X BLOCKS | 9 | 22.2 | 0.94 (N.S.) |
| POINTS X BLOCKS X Ss | 8I | 23.6 | |

**DUNCAN TEST OF DIFFERENCE BETWEEN MEANS**

| **Means** | **89.9** | **90.9** | **95.7** | **96.5** | **Shortest Sig. Range(.05)** |
|---|---|---|---|---|---|
| 89.9 | - | I.0 | 5.8 | 6.6 | 9.3 |
| 90.9 | | - | 4.8 | 5.6 | 9.8 |
| 95.7 | | | - | 0.8 | I0.I |
| 96.5 | | | | - | |

**None (Flat)**

## RESULTS The Line - Shaft Comparison

### MEANS (cms.)

| SUBJECT | Shaft | Line | Difference Significant? |
|---|---|---|---|
| Aldous | 82.5 | 77.2 | No |
| Hockey | 49.7 | 42.4 | Yes |
| Holmes | 105.5 | 116.9 | Yes |
| Davies | 99.1 | 102.5 | No |
| James | 99.1 | 104.4 | No |
| Buckingham | 135.3 | 135.6 | No |
| Lee | 71.0 | 75.5 | No |
| Curless | 105.4 | 107.4 | No |
| Underwood | 60.8 | 57.8 | No |
| Hinchliffe | 89.6 | 88.7 | No |
| MEANS | 89.9 | 90.9 | |

The Duncan Range test given below the Ingoing M-L results shows that the two means do not differ significantly.

RESULTS The Outgoing M-L

MEANS (cms.)

| SUBJECT | LFT. FIN | SHAFT | RT. FIN | Significant Differences | |
|---|---|---|---|---|---|
| Aldous | 72.5 | 77.3 | 76.2 | NONE (Flat) | |
| Hockey | 69.7 | 40.0 | 67.2 | Lft & Rt > Shft | (AG) |
| Holmes | 93.I | 98.9 | 99.9 | NONE (Flat) | |
| Davies | I03.7 | 93.9 | II3.2 | Rt > Lft > Shft | (AG) |
| James | I08.0 | 78.3 | 93.7 | Lft > Rt > Shft | (AG) |
| Buckingham | I27.0 | II5.I | I25.9 | Lft > Shft | (UNC) |
| Lee | 63.7 | 70.4 | 63.9 | NONE (Flat) | |
| Curless | II3.8 | 94.4 | I06.5 | Lft & Rt > Shft | (AG) |
| Underwood | 83.0 | 79.5 | 8I.0 | NONE (Flat) | |
| Hinchliffe | 93.7 | 83.4 | 86.7 | Lft > Shft | (UNC) |
| MEANS | 90.8 | 83.I | 9I.4 | | |

OVERALL ANALYSIS OF VARIANCE

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | 9 | 4485.0 | |
| BLOCKS | 3 | 49.3 | 0.I (N.S.) |
| POINTS | 2 | II05.5 | 4.9* |
| BLOCKS X Ss | 27 | 453.5 | |
| POINTS X Ss | I8 | 227.2 | |
| POINTS X BLOCKS | 6 | 69.4 | 0.2 (N.S.) |
| POINTS X BLOCKS X Ss | 54 | 377.8 | |

DUNCAN TEST OF DIFFERENCE BETWEEN MEANS

| MEANS | 83.I | 90.8 | 9I.4 | Shortest Sig. Range(.05 & .0I) | |
|---|---|---|---|---|---|
| 83.I | - | 7.7* | 8.3* | 7.I | 9.7 |
| 90.8 | | - | 0.6 | 7.4 | I0.I |
| 9I.4 | | | - | | |

Lft. & Rt. > Shaft (at .05 level only)

RESULTS — The Ponzo

MEANS (cms.)

| SUBJECT | APEX | BASE | Sig. of Difference |
|---|---|---|---|
| Aldous | 96.4 | 92.9 | Not Sig. |
| Hockey | 56.7 | 71.1 | Sig. |
| Holmes | 92.6 | 93.4 | Not Sig. |
| Davies | 103.8 | 119.9 | Not Sig. |
| James | 51.6 | 62.9 | Sig. |
| Buckingham | 124.4 | 117.6 | Not Sig. |
| Lee | 72.9 | 70.8 | Not Sig. |
| Curless | 110.8 | 112.3 | Not Sig. |
| Underwood | 66.1 | 66.4 | Not Sig. |
| Hinchliffe | 91.1 | 86.4 | Not Sig. |
| MEANS | 86.6 | 89.4 | |

OVERALL ANALYSIS OF VARIANCE

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | 9 | 3502.7 | |
| BLOCKS | 3 | 2737.2 | 1.0 (N.S.) |
| POINTS | 1 | 1465.9 | 0.7 (N.S.) |
| BLOCKS X Ss | 27 | 2514.9 | |
| POINTS X Ss | 9 | 2255.4 | |
| POINTS X BLOCKS | 3 | 3090.4 | 0.5 (N.S.) |
| POINTS X BLOCKS X Ss | 27 | 6512.8 | |

No significant difference

**RESULTS** **The Curve**

**MEANS (cms.)**

| SUBJECT | LFT. WING | CENTRE | RT. WING | Sig. Differences |
|---|---|---|---|---|
| Aldous | 88.2 | 80.3 | 84.5 | Lft > Cntre (UNC) |
| Hockey | 60.4 | 50.3 | 46.I | Lft > Rt & Cntre (UNC) |
| Holmes | 95.7 | 79.5 | 82.8 | Lft > Rt & Cntre (UNC) |
| Davies | 90.0 | 82.6 | 93.7 | NONE (Flat) |
| James | I20.7 | I26.I | I32.0 | Rt > Lft & Cntre (UNC) |
| Buckingham | I0I.0 | I04.3 | II7.0 | NONE (Flat) Cntre (UNC) |
| Lee | 69.3 | 68.6 | 67.0 | NONE (Flat) |
| Curless | 82.4 | 95.6 | 90.2 | Cntre > Lft (UNC) |
| Underwood | 68.0 | 77.2 | 8I.2 | Rt & Cntre > Lft (UNC) |
| Hinchliffe | 94.2 | 98.4 | I00.6 | NONE (Flat) |
| **MEANS** | 87.0 | 86.3 | 89.5 | |

**OVERALL ANALYSIS OF VARIANCE**

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | 9 | 5037.6 | |
| BLOCKS | 3 | 43.6 | 0.7 (N.S.) |
| POINTS | 2 | II4.I | 0.7 (N.S.) |
| BLOCKS X Ss | 27 | 62.3 | |
| POINTS X Ss | I8 | I55.7 | |
| POINTS X BLOCKS | 6 | I5.2 | 0.I (N.S.) |
| POINTS X BLOCKS X Ss | 54 | II2.5 | |

**DUNCAN TEST OF DIFFERENCES BETWEEN POINTS**

| MEANS | 86.3 | 87.0 | 89.5 | Shortest Sig. Range (.05) |
|---|---|---|---|---|
| 86.3 | - | 0.2 | 3.2 | 5.9 |
| 87.0 | | - | 2.5 | 6.2 |
| 89.5 | | | - | |

**NONE (Flat)**

The Ponzo (Fig. 2:3) was seen as flat by all but two Ss, both of whom saw the apparently longer line as nearer.

Only five Ss saw the Curve (Fig. 2:5) as flat. The other 5 were all 'unclassified'. All but one S saw this figure as flat in experiment I.

The number of Ss who showed a significant effect of time on distance, i.e. the Blocks factor, was similar in both experiments I and 2, i.e. 28% and 37%. The number of Ss showing this effect in experiment 2 was 6, 2, 2 and 5 for Figs. I, 3, 5 and 6 respectively.

Table 2:I shows the number of times each S had a significant Blocks factor in this experiment. It can be seen that all Ss showed the effect at least once with the exception of Hinchliffe. Only James (5 times) and Underwood (4 times) showed the effect in more than half the figures.

The number of Ss showing a significant Points by Blocks interaction was 4, 3, 2, 2, 3 and 3 for Figs. I, 2, 3, 4, 5 and 6 respectively. This represents a somewhat steeper rise from experiment I than was evident in the Blocks effect, i.e. from 3% to 28%. Considering the experimental figures only, the rise was from 5.5% to 30%. This increase was shown to be significant at the .05 level by a Mann and Whitney U test (Siegal, 1956). The data for this test was prepared by calculating the proportion of times each S had a significant interaction, e.g. if it were twice in the four experimental figures then the proportion was 0.5. After ranking these proportions U was found to be 10.5, which is significant at the 0.5 level on the basis of a two-tailed test.

For all the figures used in experiment I the overall analyses were exact replications of those from experiment I. No significant effects were found at all. For the outgoing M-L the Points factor was significant at the 0.5 level. A Multiple Range test was done and the two fin-ends were found to appear equidistant with the shaft nearer i.e. the exact opposite of Gregory's prediction.

TABLE 2:I

| SUBJECT | NO. OF. SIG. BLOCKS EFFECTS |
|---|---|
| Aldous | 2 |
| Hockey | 2 |
| Holmes | 3 |
| Davies | 2 |
| Lee | 2 |
| Curless | 3 |
| James | 5 |
| Buckingham | I |
| Underwood | 4 |
| Hinchliffe | 0 |

DISCUSSION The results of experiment 2 allow a number of interesting comparisons to be made. It is one of the primary predictions from Gregory's theory that the ingoing and outgoing M-Ls should be seen in opposite ways. The addition of an outgoing M-L to the ingoing one allows us to test this prediction. Table 2:2 shows how each S interpreted the two figures. It can be seen that only one S showed what might be described as a reversal. This S (Cross)

TABLE 2:2 - Classification Of Ss' 3-Dimensional Interpretations

Of Both M-L Figures

| SUBJECT | INGOING M-L | OUTGOING M-L |
|---|---|---|
| Aldous | Flat | Flat |
| Hockey | Shaft nearer(P.-G.) | Shaft nearer(A.-G.) |
| Holmes | Unclassified | Flat |
| Davies | Shaft nearer(P.-G.) | Shaft nearer(A.-G.) |
| James | Unclassified | Unclassified |
| Buckingham | Unclassified | Unclassified |
| Lee | Unclassified | Flat |
| Curless | Flat | Shaft nearer(A.-G.) |
| Hinchliffe | Flat | Unclassified |
| Grundy | Shaft nearer(P.-G.) | Shaft nearer(A.-G.) |
| Thomson | Flat | - |
| Templeton | Flat | - |
| Martin | Flat | Shaft nearer(A.-G.) |
| Cross | Shaft further(A.-G.) | Shaft nearer(A.-G.) |
| Bateman | Flat | Unclassified |
| Underwood | Shaft nearer(P.-G.) | Flat |

was anti-Gregory on both occasions. The complete failure of this prediction is further evidence against the suggestion that the 3-dimensional effects reported are linked with size-constancy or the illusory distortions.

Apart from the outgoing M-L (Fig.2 :8), our overall analyses have not shown any significant differences between Points at all, yet many Ss, particularly in this experiment, did have significant differences in their individual analyses. However, if one S had a significant difference in one direction, another S often had a significant difference in the opposite direction, thus one cancelled out the other to produce a non-significant overall effect. This raises the question of how consistent individual Ss were from one figure to another - was there a tendency to see all the figures in the same way e.g. pro-Gregory, flat, etc.?

In order to assess the S's consistency in this matter a special sort of measure was needed. We are interested here in relative differences of position rather than absolute distances, hence it would be inappropriate to calculate the correlation between the actual distances of the various points.A measure was required that would reflect the relationship between the three points of the figures. Once this measure was obtained, we could then correlate it with a similar measure derived from another figure. The answer was found by using orthogonal polynomials.

As an example let us take our first S, Aldous. His means for the outgoing figure were 72, 77 and 76 cms. (to the nearest whole number). We want a single score which represents the quadratic trend present in these numbers. The relevant orthogonal coefficients to test for such a trend are I,-2 and I. We multiply the first number (72) by the first coefficient (I), the second (77) by the second (-2) and the third (76) by the third (I). By adding the resultant scores we get an answer of -6. This is then divided by the total scores i.e. 72 + 77 + 76, to give -.003, thus eliminating any bias that might occur due to the different absolute values of groups of scores. The size of the answer represents the size of the trend; in this case rather small. The sign of the answer indicates its direction i.e. a negative sign indicates that the centre point is further, a positive sign that it is nearer.

By repeating this process for every S who completed both an ingoing and an outgoing figure we get a set of I4 scores for each figure. These can then be compared by means of a simple correlation.

Linear trends can be assessed in the same way using the orthogonal coefficients -I, 0 and +I. There was no reason to expect significant linear trends on these figures but they were worked out as a check. The comparisons between the ingoing and outgoing M-Ls for linear trend gave a correlation of +0.05, and for quadratic trend a correlation of +0.33. These yielded 't's of 0.2 and I.3, neither of which were significant.

Thus individual Ss differ greatly in their interpretations from one figure to another. You cannot predict with any certainty how a S will see one figure from his results on another figure, although there did appear to be a non-significant tendency to see the centre points of all figures as nearer.

It was hypothesised in the discussion that the 'sophisticated' Ss used might have been inadvertently set to see the figures as flat. This hypothesis can be tested by comparing the number of experimental figures seen in this way in each experiment. The relevant figures are the ingoing M-L, the Ponzo, and the Curve. The outgoing M-L was not used in experiment I and is ommitted.

In experiment I 70% (I4 out of I8) of the experimental figures were seen as flat. In experiment 2 only 53% (I6 out of 30) were seen in this way. The tables below show how the responses were divided between the figures.

**Fig.I**

| | Flat | Not-Flat | |
|---|---|---|---|
| Expt.I | 3 | 3 | 6 |
| Expt.2 | 4 | 6 | IO |
| | 7 | 9 | I6 |

**Fig.3**

| | Flat | Not-Flat | |
|---|---|---|---|
| Expt.I | 6 | 0 | 6 |
| Expt.2 | 8 | 2 | IO |
| | I4 | 2 | I6 |

**Fig.5**

| | Flat | Not-Flat | |
|---|---|---|---|
| Expt.I | 5 | I | 6 |
| Expt.2 | 4 | 6 | IO |
| | 9 | 7 | I6 |

Although none of the three figures taken separately are significant on a Fisher exact probability test, taken as a whole the results are at least suggestive. It does seem that fewer figures were seen as flat in experiment 2 and further research was planned accordingly. The suggestion that 'set' can affect the way that Ss perceive the figures might be particularly relevant to an explanation of how Gregory and Coren and Festinger got their results.

More Ss in experiment 2 showed a significant Points by Blocks interaction. This interaction indicates the extent to which the relationships between the points change over time. A significant interaction means that any 3-dimensional effect shown in the results was not stable throughout the experiment. In an extreme case this could mean that a S could begin an experiment seeing the shaft of a M-L figure as nearer than the fin-ends, but change during the experiment to seeing it as further than the fin-ends. The overall means might be almost identical, indicating that the figure was seen as flat. Only the significant interaction would tell us otherwise.

Only one S had a significant interaction in experiment I i.e. 3%, whereas I7 Ss showed the effect in experiment 2 i.e. 28.5%. This increase fits very well into the 'set' hypothesis mentioned just now. If the Ss in experiment I were set to see the figures as flat one would not expect this interpretation to change during the experiment, hence the low number of significant interactions. In experiment 2 the Ss were not 'set' and since the figures are ambiguous it is not surprising that they should change from one interpretation to another thus producing more significant interactions.

However, 5 of the I7 occasions on which this effect occurred were while Ss were viewing control figures. Our results from both experiments have indicated that everyone sees these figures in the same way and they are not ambiguous, therefore we would certainly not expect any significant interactions at all. The Block means f~~or~~t each of the five cases are presented in Table 2:3 and it can be seen that they do not contradict the theory. It has already been mentioned that the important thing is the relative positions of the points to each other rather than their actual distances. It has already been mentioned that a significant Points by Blocks interaction is usually associated with a change in relative positions in the course of the

TABLE 2:3 - Block Means for Ss who showed a Significant Points x Blocks Interaction

## THE GLASS CORNER (Fig. 2)

**Aldous** (Lft & Rt>Ctr)

| | Left | Centre | Right |
|---|---|---|---|
| | 101.4 | 78.3 | 95.3 |
| | 98.3 | 79.5 | 93.0 |
| | 96.8 | 75.5 | 101.3 |
| | 105.0 | 76.2 | 105.3 |
| MEANS | 100.4 | 77.4 | 98.8 |

**Hockey** (Rt>Ctr>Lft)

| | Left | Centre | Right |
|---|---|---|---|
| | 105.0 | 62.7 | 114.2 |
| | 86.2 | 61.8 | 106.2 |
| | 74.2 | 60.2 | 90.8 |
| | 75.7 | 68.3 | 82.7 |
| MEANS | 85.2 | 63.2 | 98.5 |

**Underwood** (Rt>Lft>Ctr)

| | Left | Centre | Right |
|---|---|---|---|
| | 75.8 | 55.0 | 91.8 |
| | 73.8 | 55.0 | 93.5 |
| | 69.5 | 51.2 | 71.2 |
| | 70.0 | 50.3 | 71.7 |
| MEANS | 72.3 | 52.9 | 82.0 |

## THE CYLINDERS (Fig. 4)

**James** (Rt>Ctr>Lft)

| | Left | Centre | Right |
|---|---|---|---|
| | 71.0 | 71.3 | 80.2 |
| | 64.2 | 73.2 | 89.3 |
| | 70.0 | 81.8 | 116.8 |
| | 70.0 | 81.0 | 119.8 |
| Means | 68.8 | 76.8 | 101.5 |

**Curless** (Rt & Ctr>Lft)

| | Left | Centre | Right |
|---|---|---|---|
| | 101.5 | 110.8 | 110.5 |
| | 103.0 | 112.5 | 115.2 |
| | 96.3 | 103.3 | 119.0 |
| | 81.0 | 101.7 | 116.3 |
| Means | 95.5 | 106.8 | 115.2 |

TABLE 2:3 (Cont.)

## THE INGOING M-L (Fig. I)

| | Aldous | | | Hockey | | |
|---|---|---|---|---|---|---|
| | Left | Centre | Right | Left | Centre | Right |
| | 86.2 | 82.3 | 66.8 | 72.0 | 50.5 | 72.2 |
| | 76.0 | 79.2 | 77.7 | 64.2 | 5I.8 | 69.3 |
| | 74.3 | 83.7 | 86.8 | 94.8 | 56.3 | 90.3 |
| | 74.3 | 84.8 | 80.8 | 89.5 | 40.0 | 85.0 |
| MEANS | 77.7 | 82.5 | 78.0 | 80.I | 45.7 | 79.2 |
| | | (Flat) | | | (Lft. & Rt. > Shaft) | |

| | Lee | | | Curless | | |
|---|---|---|---|---|---|---|
| | Left | Centre | Right | Left | Centre | Right |
| | 64.0 | 76.0 | 69.5 | 9I.3 | I05.3 | 95.3 |
| | 58.7 | 67.8 | 72.0 | 94.5 | I03.8 | 96.5 |
| | 72.0 | 70.5 | 72.5 | I08.2 | 99.8 | 99.0 |
| | 66.2 | 69.7 | 73.8 | II0.5 | II2.5 | I0I.2 |
| MEANS | 65.2 | 7I.0 | 72.0 | I0I.I | I05.4 | 98.0 |
| | | (Flat) | | | (Flat) | |

TABLE 2.3 (contd)

THE PONZO EXPTL (Fig 3)

Hockey (Flat)

| Base | Apex |
|---|---|
| 86.3 | 39.8 |
| 68.0 | 45.2 |
| 62.3 | 75.8 |
| 67.7 | 65.8 |
| 56.7 | 71.1 |

Underwood (Flat)

| Base | Apex |
|---|---|
| 61.5 | 72.2 |
| 56.3 | 73.7 |
| 76.2 | 90.5 |
| 71.7 | 92.3 |
| 66.1 | 66.4 |

THE CURVE (Fig 5)

Aldous (Lft > Centr)

| Left | Centre | Right |
|---|---|---|
| 86.3 | 90.7 | 86.7 |
| 97.7 | 74.3 | 81.8 |
| 84.2 | 76.0 | 84.5 |
| 84.5 | 80.3 | 85.0 |
| 88.2 | 80.3 | 84.5 |

Hockey (Lft > Rt & Cntr)

| Left | Centre | Right |
|---|---|---|
| 54.2 | 55.3 | 45.7 |
| 66.0 | 49.3 | 47.7 |
| 62.5 | 45.7 | 47.2 |
| 58.8 | 51.0 | 43.8 |
| 60.4 | 50.3 | 46.1 |

Davies (Flat)

| Left | Centre | Right |
|---|---|---|
| 86.8 | 88.8 | 93.0 |
| 84.7 | 75.5 | 88.3 |
| 91.7 | 84.3 | 99.2 |
| 97.0 | 81.7 | 94.2 |
| 90.0 | 82.6 | 93.7 |

TABLE 2.3 (contd)

THE OUTGOING M-L (Fig 6)

Holmes

| Left | Centre | Right |
|---|---|---|
| 96.2 | 94.7 | 102.3 |
| 96.3 | 119.5 | 98.8 |
| 92.8 | 94.5 | 94.2 |
| 87.0 | 86.7 | 104.3 |
| 93.1 | 98.9 | 99.9 |

(Flat)

James

| Left | Centre | Right |
|---|---|---|
| 66.5 | 70.8 | 63.7 |
| 67.1 | 67.7 | 64.9 |
| 150.5 | 66.2 | 123.8 |
| 147.9 | 108.6 | 122.3 |
| 108.0 | 78.3 | 93.7 |

(Lft > Rt > Shft)

Hinchliffe

| Left | Centre | Right |
|---|---|---|
| 97.2 | 84.0 | 79.3 |
| 90.8 | 80.5 | 79.8 |
| 94.0 | 80.3 | 98.7 |
| 93.0 | 88.7 | 89.2 |
| 93.7 | 83.4 | 86.7 |

(Lft > Shft)

experiment. However, it is possible to obtain a significant interaction by maintaining the relative positions of the various points but changing the distance between them. This is what happened in all 5 cases involving control figures.

The three Ss who had a significant interaction for the Glass corner all saw the centre pillar as nearer. It was the distance by which they saw it as nearer which fluctuated. A similar pattern is discernible with the Cylinders illusion. In only one instance is the ordering different from expected. The offending case occurs in the first set of Blocks for Curless where the distances are IOI.5, IIO.8 and IIO.5 - the last distance should have been furthest. It is noticeable in both these figures that the gaps between points tended to widen as the experiment progressed. This could be interpreted as the S learning to use the cues more effectively as time goes on.

An examination of the data from the experimental figures (Table 2:3) reveals a different pattern entirely. In all cases but one the significant interaction is due to a change in the relative positions of the points. The odd result is that of Hockey (ingoing M-L), which is also the only experimental result to resemble those of the Control figure in that the shaft is seen as nearest with the fin-ends equidistant.

We may conclude, then, that the significant Control figure interactions do not affect the theory outlined so far and can be ignored. Whereas the Control figures with significant interactions were seen in a consistent way i.e. relative positions remained constant, the Experimental figures were not.

Turning our attention solely to the experimental figures with significant interactions it is pertinent to ask if a significant interaction is linked with any particular 3-dimensional effect. Of the I2 figures, II were either flat (5) or 'unclassified' (6). The twelfth figure was Hockey (ingoing M-L), already mentioned. That the bulk of the data should fall into these two categories is not surprising and it would suggest that our hypothetical example (p.137) might not be so far from the truth.

Since in these cases a significant interaction means that the final means do not truly reflect how the S saw the figure, they should be withdrawn from the data. This means that fewer people than was at first indicated actually saw the figures in a flat or unclassified way. This leads us to revise the tables shown on p.136 in the following manner:-

**Fig.I**

| | Flat | Not-Flat | |
|---|---|---|---|
| Expt.I | 3 | 3 | 6 |
| Expt.2 | I | 5 | 6 |
| | 4 | 8 | I2 |

**Fig.3**

| | Flat | Not-Flat | |
|---|---|---|---|
| Expt.I | 6 | 0 | 6 |
| Expt.2 | 7 | I | 8 |
| | I3 | I | I4 |

**Fig.5**

| | Flat | Not-Flat | |
|---|---|---|---|
| Expt.I | 5 | I | 6 |
| Expt.2 | 3 | 4 | 7 |
| | 8 | 5 | I3 |

These new tables show the same trend as the earlier ones except that the numbers involved are now even smaller. There is no reason to amend our previous tentative conclusion that the S's set might be responsible for the larger number of 'flat' figures in experiment I. In this respect it should be mentioned that the 'setting' would have only have occurred after the first two figures had been 'distanced' since they were always in the first group to be done and Ss were not told about the experiment until after this.

The ingoing M-L shows a far greater proportion of non-flat figures than either of the other experimental figures in experiment I.

It was hypothesised that the Ponzo (Fig.2:3) was seen as flat in experiment I because Ss were set to see it as flat. With the absence of any set it was expected that the SDIH would be confirmed in that the apparently longer line would be seen as nearer. This prediction is directly contrary to Gregory's since he believes that the horizontal bars will be interpreted as 'sleepers' on a railway line and that consequently the apparently longer one would appear further. The results do not help either hypothesis since no significant difference was found on the overall analysis, although the two Ss who did show a significant effect in their individual results were both in the direction indicated by the size-distance invariance hypothesis i.e. apparently longest, nearest. The difference between the means was also in this direction but it was nowhere near large enough for significance.

Similar reasoning was applied to the line-shaft comparison. It was expected that the 'flat' results of experiment I would not be replicated but that the apparently longer line would be seen as nearer, as predicted by the SDIH. This, as with the Ponzo, was not the case. The difference between the means on the overall analysis was just I cm. and while two Ss showed significant effects in their individual analyses, they were in opposite directions.

An obvious question to ask is whether these figures actually produced the illusory effect expected. It has already been noted (p.13 ) that Brown and Houssiadas (I965) used a Ponzo figure which produced no illusion in I5 out of 25 Ss. Even at its greatest, this figure does not produce the huge effects noted in the M-L. Fisher (I967) mentions IO% and this would seem to be a good average. The line-shaft comparison would also produce quite a small effect even under ideal conditions since it is only half the usual M-L effect - and the smallest half at that (Pollack and Zetland, 1964).

Unfortunately, Ss were not asked to comment on their impressions of the relative lengths of the lines and in the light of our results it was decided that this should be standard practice in future experiments. It was also decided to test whether or not an illusion was in fact present. The M-L and line was the figure used because the results from this comparison were the most equivocal.

Eleven slides were prepared from the actual figure used in the experiment. The M-L figure on each of these slides had a shaft 34 mm. in length, but the line-length varied from 27 mm. to 41 mm. in equal steps. The slides were given to 10 naive Ss who had not taken part in the experiment and they were told to select the slide in which, "both the line and the shaft appeared to be the same length." They were allowed as much time as they wished to sort through the slides, going back to ones they had already seen if they wished. The results were as follows:-

| SUBJECT | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | AVERAGE |
|---|---|---|---|---|---|---|---|---|---|---|---|
| LINE LENGTH | 31 | 32 | 32 | 33½ | 30 | 32 | 31 | 30 | 32 | 33½ | 31.7 |
| % ILLUSIONS | 10 | 6 | 6 | 1½ | 13 | 6 | 10 | 13 | 6 | 1½ | 7.25 |

The difference was significant on a binomial test at the .001 level. Thus an illusion of 7% would seem to be indicated. This is very much the sort of figure that might be expected.

In regard to the results of Pike and Stacey (1968), the obvious conclusion would seem to be that their Ss were responding to the <u>over-all</u> length of the figures rather than to the apparent lengths of the shafts, despite their controlling for this (Fig. 3:5, p.160) However, a later experiment by Stacey and Pike (1970) obtained similar results to their 1968 study using figures in which overall length could not have been a contaminating factor.

Three Ss out of twenty did respond in the way found by Pike and Stacey and predicted by Gogel and Epstein and Landauer, i.e. set the apparently longer line as nearer. One S reacted in exactly the opposite way, whereas in the Pike and Stacey study six Ss reacted in the opposite way. Compare the relative distribution of Ss into the possible categories.

| | Pike and Stacey | Green |
|---|---|---|
| App. longer line nearer | 50% | 15% |
| App. longer line further | 20% | 5% |
| Equidistant | 30% | 80% |

Epstein and Landauer have already been quoted as saying that the S, using visual angle as his yardstick, can perceive either a difference in size or a difference in distance. It would seem that our experimental set up is such as to induce Ss to see the former. The next experiment casts some light on why this should be.

## PART 3 - EXPERIMENTS CONCERNING THE SIZE-DISTANCE INVARIANCE HYPOTHESIS (SDIH) AND THE APPARENT FRONTO-PARALLEL PLANE (AFPP)

## EXPERIMENT 3 - A more direct replication of Pike and Stacey and of Coren and Festinger, and to test Ss' response to a non-perspective figure.

INTRODUCTION The results of the first two experiments do not confirm results reported by Gregory and by Coren and Festinger. All the so-called experimental figures produced no significant differences between points on overall analysis (apart from the outgoing M-L in experiment 2 whose results were anti-Gregory). Yet it was shown that control figures which contained many obvious cues were seen as 3-dimensional in the same way by everyone. This result is unsurprising in itself and these figures were included mainly to demonstrate that such cues could be used to produce the expected depth differences in our apparatus. It was obvious that our experimental figures were not at all similar to the control figures. Although the overall analyses showed no significant difference, individuals showed many sorts of significant difference which tended to cancel each other out. There was also a tendency, not present in the control figures, for some Ss to change their perception of the figures during the experiment, as evinced by the significant Points by Blocks interactions. This should not be too surprising because the figures were chosen specially because they were ambiguous. However, in all the experimental figures, apart from the ingoing M-L of experiment I, there was a tendency to see the centre point of the figure as nearest e.g. the curve in experiment I; the curve, the ingoing and outgoing M-L in experiment 2. Only in the case of the outgoing M-L was this tendency significant.

It was decided that Ss' reactions to a new figure should be tested - one that had not previously been shown to give any obvious cues for 3-dimensionality, in fact a figure which should appear flat. If a tendency does exist to see the centre point of any figure as nearer, rather than just for figures noted for appearing 3-dimensional, then it should appear in such a figure unadulterated by other effects. If such a tendency does not exist the figure should appear flat, at least on the overall analysis.

The figure chosen comprised three crosses, the centre one of which was in the middle of the S's visual field (Fig.3:I). All the crosses were at eye-level. The task was to use the lights to distance each cross.

Experiments I and 2 failed to confirm Pike and Stacey's (1968) finding that a phenomenal difference in length can produce a depth difference. The reasons for this are not clear hence an attempt was made to repeat their results using figures more like the ones they used. The figures previously used i.e. the Ponzo and the line-ingoing shaft comparison were uncontaminated by overall length effects but, on the other hand, the illusory distortion produced was not nearly so great. Ingoing and an outgoing M-L with shafts the same length were photographed side by side and a transparency was prepared (Fig.3:2). The Ss' task was to 'distance' the two shafts.

In case it should be thought that the results of experiments I and 2 are being treated with undue caution, it should be mentioned that some Ss seemed to be very confused by the experiment. When asked to comment on their perception of the figures many Ss were

Fig. 3.1 - The crosses

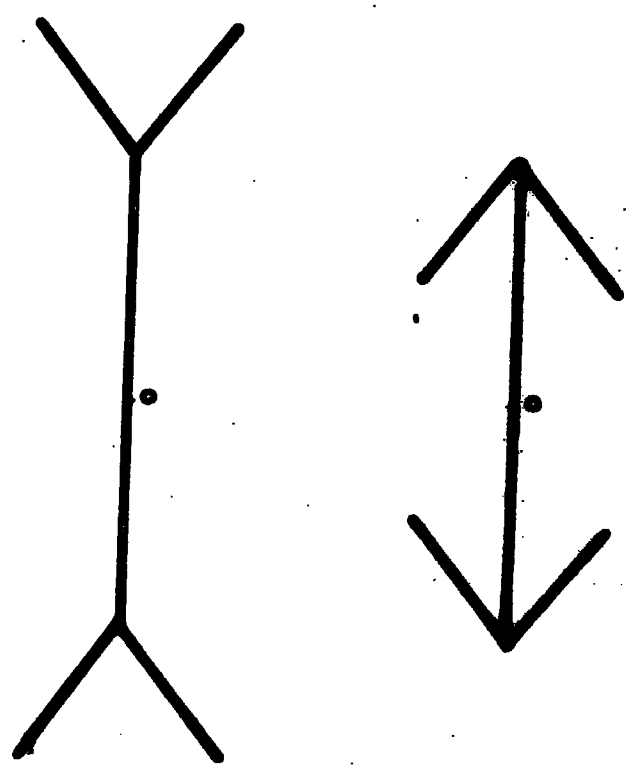


Fig. 3.2 - The double M-L

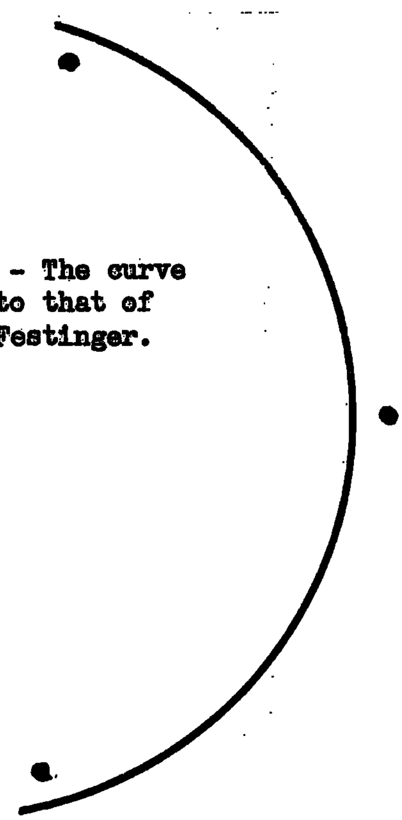


Fig. 3.3 - The curve similar to that of Coren & Festinger.

unable to give a coherent report at all. One S (Cross) was viewing the ingoing M-L, having previously viewed the glass corner, when she exclaimed, "This is the same as the last one, isn't it?" In the glass corner she had in fact set the centre point nearer than the side points, yet she continued, even after her exclamation, to set the ingoing M-L in exactly the opposite way. In other words she had perceived the similarity between the two figures but she was not reacting to them in the same way. It may be argued that it is what the S does that is important, not what he says - indeed that is the main reason for taking objective measures rather than subjective reports, but such a conflict is disturbing.

How a S uses differences in visual angle to assess size and/or distance and the relationship of his decisions to the SDIH seems to be determined by parameters which have so far proved elusive. Hence the present repetition of experiments with very few changes.

The third figure was a curve of exactly similar proportions to the one used by Coren and Festinger (Fig.3:3). It was also presented in the same orientation that they used i.e. vertically rather than horizontally as previously. This necessitated some changes in the apparatus which are described below. It was hoped that the results from this figure might give a clue as to why previous results had been so different from those reported by Coren and Festinger.

<u>APPARATUS</u> The Synoptophore was used, the figures again being inserted into its right arm. Two or three reference lights were used instead of the single light of experiments I and 2. Previously it had been necessary to realign the optical bench carrying the light each time the S came to consider a different part of the figure i.e.

every 6 trials. This was both tedious and time consuming. By using a different light for each part of the figure we wished to 'distance' this was avoided. Each light could now be lined up at the start of a session and it would remain on the correct line until the end. The impetus for this innovation came from the change in the curvės orientation. With the curve presented in its present manner it was no longer possible to maintain the lights at eye-level at all times. Thus, if the lights were to maintain their position relative to the figure as they moved back and forth, it was necessary that the runway or rail on which they run was at a slant. The optical bench on which the lights ran in experiments I and 2 was too heavy so an alternative had to be found.

The solution adopted was to use Dural bars 0.5" square as the track along which the lights ran. The lights themselves were mounted on brass saddles about an inch long which straddled the bars. The bars were I40 cms. long and could be adjusted to any desired position by means of the clamps and stands illustrated.* Bars of 0.5" section were used because any larger dimension would have meant that they would have fouled each other as they neared the S's eye. This was the reason for using one light in experiments I and 2; the optical bench, being some 3" across was too wide to allow any other benches to occupy the required position.

For the M-L and the Crosses figures the lights could be at eye-level as before (although, of course, two or three lights were used as described) but for the curve, only the light to be aligned next to the centre of the curve was at eye-level. The light next to the top of

* Fig. 3.4

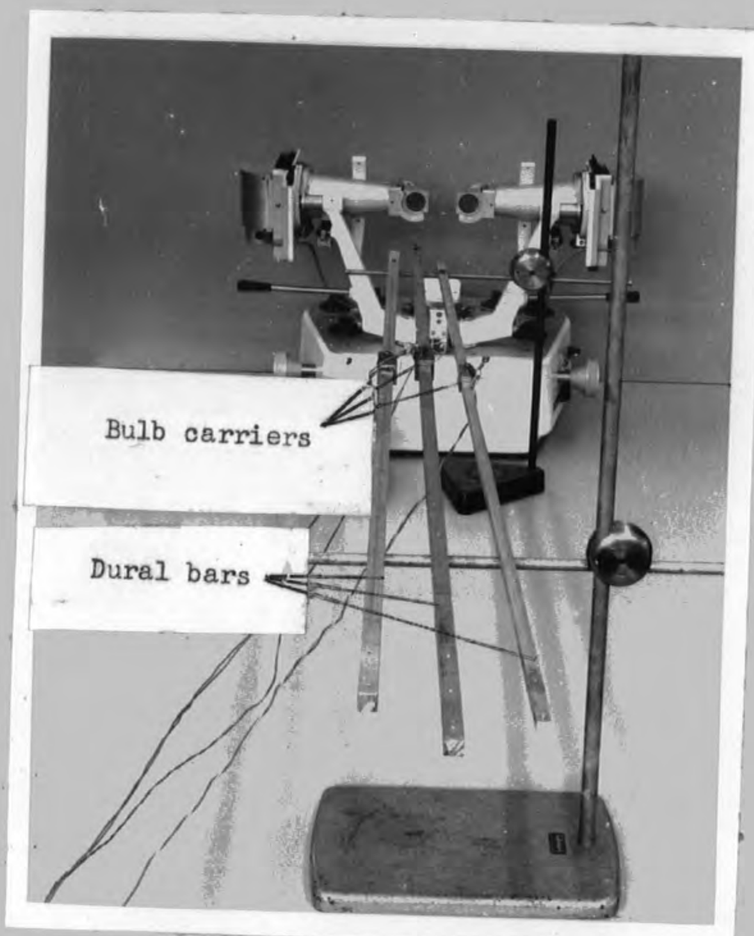



Fig. 3.4 - The apparatus.

the curve was above eye-level, and the bottom light was below it. Obviously, the further away the light is, the higher or lower it must be to maintain its position relative to the figure; thus the bar on which the 'top' light is mounted must slope up from the eye while the bar on which the 'lower' light is mounted must slope down from the eye.

The lights were wired to a rotary switch and could be illuminated in turn, only one being on at one time. The battery which had powered the light used in experiments I and 2 was replaced by a Radford Labpack. Some care was taken to adjust the lights to the same brightness i.e. they were adjusted until they appeared to be of equal brightness. to the experimenter sitting in the S's seat.

PROCEDURE The procedure was very similar to that used previously but since some changes were made, it is given in full.

The S was brought into the room and seated in front of the Synoptophore. The room light was turned out leaving the room in complete darkness and the S was asked to look through the eye-pieces and inspect the figure. The room light was then turned back on again, and the S was asked to look at the blackboard, on which were drawings of the three figures. The experimenter indicated to the S the parts of the figure near which he wished the reference lights to appear. The room light was turned off again and one of the reference lights was turned on. The S was asked to say in which direction the light should be moved in order for it to be near the part of the figure the S had indicated. For this initial setting the light was positioned at the far end of its bar - as far away from the S as possible. The experimenter moved the bar according to the S's instructions until the light appeared to be in the right place. The light was then slid down to the near end of the bar and the S was asked if it had moved, and if so, in which direction. The light was moved from one end of the bar to the other until it had moved the whole length of its travel without straying from the appointed position. The experimenter then said, "You may have noticed that when the light is very near to you, it appears to be closer than the figure i.e. the figure appears to lie somewhere behind the light, while when it goes as far away from you as it will go, it appears to be behind the figure. Is this so?" All Ss agreed with this. The experimenter then continued, "What I want you to do is to tell me at what point between these two extremes the light appears to be at the same distance as the part of the figure it is near." The S was questioned until the experimenter was satisfied that he understood what was required. Particular emphasis was laid on the fact that the S must concentrate on the particular part of the figure indicated and that it was this that he had to relate the light to.

The S made three settings with each light in turn. Only the light that was being set was lit. 24 readings were taken for each S for each point. The procedure was to take thsee readings from one point and then move on to take three readings from the other points. When this had been done, another set of three readings was taken in the same way, and so on until the whole 24 were obtained. The order in which the readings and the figures were taken was randomised. A felt-tipped pen was used to mark each setting on the bar.

When the S was satisfied that the light was set correctly, it was covered and moved to the opposite end of the bar from that at which the previous trial had started. This method was more rapid than that used in experiments I and 2, during which Ss had complained of boredom, but it did not allow the recording of the various 'blocks' from which the readings came. This was unfortunate in that light of the number of significant interactions discovered in experiment 2, but this experiment was begun before these results had been fully analysed, and at a time when Ss were showing a reluctance to be used for such long periods.

When all readings had been taken for a particular figure the distance from the S's eye to each mark was carefully measured. It took between 30 and 40 minutes for a S to complete a figure.

SUBJECTS Twelve Ss (six of each sex) were used for the curve. Only eleven of these (6 women, 5 men) completed the Crosses and the double M-L figures. All were paid and all had served in experiment 2, except for four Ss who had served in experiment I.

RESULTS The results from the double M-L were clear cut. The outgoing apparently longer shaft was seen as significantly nearer by IO of the II Ss, 8 being significant at the .OOI level and 2 at the .OI level. The difference of the odd man out was in the same direction as the others but it was not significant. The overall analysis was significant at the .OOI level.

**RESULTS** **The Double M-L**

**MEANS (cms.)**

| SUBJECT | Ingoing | Outgoing | Difference |
|---|---|---|---|
| Aldous | 9I.9 | 84.7 | Sig. |
| Heckey | 58.6 | 53.0 | Not Sig. |
| James | I00.0 | 84.0 | Sig. |
| Lee | 66.2 | 60.5 | Sig. |
| Curless | I0I.8 | 77.I | Sig. |
| Underwood | 48.I | 4I.5 | Sig. |
| Hinchliffe | 86.7 | 74.5 | Sig. |
| Grundy | 76.9 | 55.9 | Sig. |
| Martin | I03.4 | 79.6 | Sig. |
| Bateman | 82.9 | 69.8 | Sig. |
| Cross | II3.I | 92.7 | Sig. |
| **MEANS** | 84.0 | 70.8 | |

**OVERALL ANALYSIS OF VARIANCE**

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | I0 | III0.4 | |
| POINTS | I | 637.I | 40.5 ** |
| POINTS X Ss | I0 | 27.42 | |

The individual analyses for the crosses figure contained the varied trend noted with the earlier figures. Six Ss had differences which would have counted as 'unclassified' had they been recorded for one of the perspective experimental figures. Four Ss saw the centre cross as nearer with the two side crosses as equidistant. One S had no significant differences.

Four Ss saw the bottom of the curve as furthest with the centre and top being equidistant. Four Ss saw the centre as nearest, two of whom saw the top and bottom as equidistant. One S had no significant differences and the remaining two Ss had differences which were 'unclassified'. The overall analysis had the Points factor significant at the .05 level and a Duncan Range test showed that the bottom appeared further than the top and centre i.e. as with the first four Ss mentioned.

It will be noted that the Ss for this experiment are drawn from both the 'sophisticated' experiment I Ss and the 'naive' experiment 2 Ss. Doubts about the advisability of including the 4 sophisticated Ss were overruled on the grounds that several months had passed since the completion of experiment I and different figures were involved. The experiment I Ss were told that the purpose of this experiment was to investigate 'something different' However, their results were checked carefully for any sign of difference between them and the naive Ss. Such a difference only seemed to occur in one S, Grundy. She was the only S who saw the centre cross as furthest and was also the only S to see the curve as flat. However, it is difficult to see how her previous experience could have affected the Cross results although her original 'flat' set could have caused the Curve results. Even if her data is excluded from the results, the conclusions are unaffected.

**RESULTS**   **The Crosses**

**MEANS (cms.)**

| SUBJECT | Lft.X | Centre X | Rt.X | Significant Diffs. |
|---|---|---|---|---|
| Aldous | 97.7 | 88.3 | 79.2 | Lft. > Centre > Rt. |
| Hockey | 64.6 | 87.2 | 96.I | Rt. > Centre & Lft. |
| James | I00.9 | 96.4 | 96.4 | None |
| Lee | 90.6 | 77.6 | 72.5 | Lft. > Centre > Rt. |
| Underwood | 92.4 | 82.3 | 92.I | Rt. & Lft. > Centre |
| Hinchcliffe | 86.0 | 74.4 | 86.4 | Rt. & Lft. > Centre |
| Curless | 92.4 | 97.6 | I0I.4 | Rt. & Centre > Lft. |
| Grundy | I00.0 | I20.0 | I08.9 | Centre > Rt. > Lft. |
| Martin | I02.5 | 63.7 | 97.5 | Rt. & Lft. > Centre |
| Bateman | 80.7 | 78.5 | 73.5 | Lft. > Rt. |
| Cross | II8.0 | 9I.5 | I2I.4 | Rt. & Lft. > Centre |
| **MEANS** | **93.2** | **87.0** | **93.2** | |

| OVERALL ANALYSIS OF VARIANCE | D.F. | MEANS SQS. | F |
|---|---|---|---|
| SUBJECTS | I0 | 380.4 | |
| POINTS | 2 | I40.50 | I.I4 (N.S.) |
| POINTS X Ss | 20 | I23.0 | |

**DUNCAN TEST OF DIFFERENCE BETWEEN MEANS**

| MEANS | 87.0 | 93.2 | 93.2 | Shortest Sig. Range (.05) |
|---|---|---|---|---|
| 87.0 | -- | 6.2** | 6.2** | 9.9 |
| 93.2 | | -- | 0.0 | I0.3 |
| 93.2 | | | -- | |

**Lft. & Rt. > Centre**

**RESULTS** **The Curve**

MEANS (cms.)

| SUBJECT | LFT. WING | CENTRE | RT. WING | Significant Differences |
|---|---|---|---|---|
| Aldous | 88.2 | 80.3 | 84.5 | Lft > Cntre (UNC) |
| Hockey | 60.4 | 50.3 | 46.I | Lft > Rt & Cntre (UNC) |
| Holmes | 95.7 | 79.5 | 82.8 | Lft > Rt & Cntre (UNC) |
| Davies | 90.0 | 82.6 | 93.7 | NONE (Flat) |
| James | I20.7 | I26.I | I32.0 | NONE (Flat) |
| Buckingham | IOI.0 | I04.3 | II7.0 | Rt > Lft & Cntre (UNC) |
| Lee | 69.3 | 68.6 | 67.0 | NONE (Flat) |
| Curless | 82.4 | 95.6 | 90.2 | Cntre > Lft (UNC) |
| Underwood | 68.0 | 77.2 | 8I.2 | Rt & Cntre > Lft (UNC) |
| Hinchliffe | 94.2 | 98.4 | I00.6 | NONE (Flat) |
| **MEANS** | **87.0** | **86.3** | **89.5** | |

**OVERALL ANALYSIS OF VARIANCE**

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | 9 | 5037.6 | |
| BLOCKS | 3 | 43.6 | 0.7 (N.S.) |
| POINTS | 2 | II4.I | 0.7 (N.S.) |
| BLOCKS X Ss | 27 | 62.3 | |
| POINTS X Ss | I8 | I55.7 | |
| POINTS X BLOCKS | 6 | I5.2 | 0.I (N.S.) |
| POINTS X BLOCKS X Ss | 54 | II2.5 | |

**DUNCAN TEST OF DIFFERENCES BETWEEN MEANS**

| MEANS | 86.3 | 87.0 | 89.5 | Shortest Sig. Range (.05) |
|---|---|---|---|---|
| 86.3 | - | 0.7 | 3.2 | 5.9 |
| 87.0 | | - | 2.5 | 6.2 |

**NONE (Flat)**

<u>DISCUSSION</u> The results from the double M-L figure replicate those of Pike and Stacey's (1968) study. Why should this have occurred here and not in the sections of experiments 1 and 2 in which the line was compared to the shaft of the ingoing M-L? One difference between the experiments was the overall length of the figures compared. In experiments 1 and 2 both the line and the M-L shaft were the same actual length while the outgoing M-L used here, and by Pike and Stacey, is longer than the ingoing M-L, if you include the fins. In fact Pike and Stacey do mention the possibility that Ss might be responding to overall length rather than concentrating on just the shafts, even though clear instructions were given and they used 'square' figures (Fig.3:5). In view of their precautions it would seem unlikely that this would have happened. There is also reason to believe that it did not occur in this experiment.

If the results are pooled, as is justified considering the similarity of different S's responses, we find that the two shafts appeared at average distances of 70.8 and 84.0 cms. By simple trigonometry we find the average % distortion to be 18.6%. What sort of figure would we expect if the Ss were responding to <u>overall</u> length? The actual overall lengths were 3.4 and 4.4 cms; one expressed as a percentage of the other gives an answer of 29.4 for our percentage distortion. Clearly this is far more than we actually have; 18.6% is, in fact, just the sort of effect we would expect if Ss were responding to the shafts only, as they were instructed to do. The only way to decide exactly how much illusion the figure used actually produced on our Ss would be to measure it.

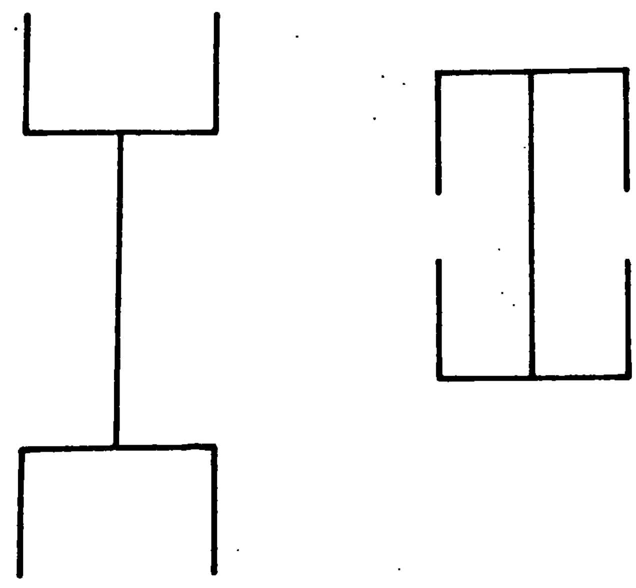


Fig. 3.5 - Pike & Stacey's square M-Ls.

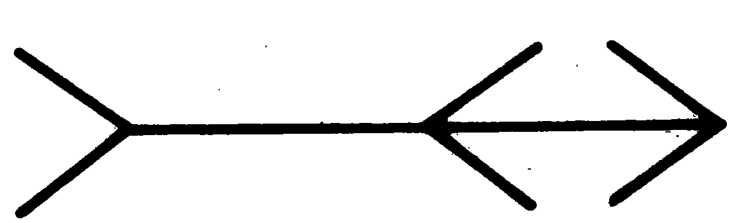


Fig. 3.6 - Dewar's 'combined' M-L.

Unfortunately the necessity for this was not forseen and so the best we can do is to refer to other studies.

Tsai (1967) found a 16% illusion, Judd (1905), 17% and Kohler and Fishback (1950), 21%. From Gregory's graphs it would appear that his effect was in the region of 25%. However, it should also be mentioned that one of Kohler and Fishback's Ss showed an initial effect of 32%. However, much depends on the angle of the fins, their length and the separation between them (Erlbacher and Sekular, 1969; Dewar, 1967t). Dewar used a number of different fin angles and lengths, one combination of which was very similar to those used here.

Dewar's figures were presented as part of a single horizontal line (Fig. 3:6) rather than side by side and vertical, as were ours, but it is not thought that this is important. Our figures were also much smaller than his. Scaling our figures up, they would resemble very closely the figure which had fins of 4 cms. in length and an angle 'between obliques' of 60 degrees. (The angles we have referred to have been between the shaft and the oblique, see p.99). Dewar's table (p.710) shows that this figure produced an illusion of 8.39 mm. which is 18.7 in percentage terms.

In other words the observed distortion approximates much more closely to what we would expect if the Ss were responding to the shafts alone, than to what we expect if they were responding to the overall length.

Another difference between this experiment and experiments 1 and 2 is in the number of points the S had to distance. In the case of the earlier experiments the S had to distance the shaft and the line and also two fin-ends. Here he had only to consider the shafts.

It is possible that these extra settings, which were not relevant to this particular question, might have interfered in some way. Part of experiment 4 is devoted to testing this possibility.

The Crosses figure was not thought to contain any perspective information; therefore any 3-dimensional effects obtained with this figure could not be ascribed to perspective. Since Gregory's theory would attribute the 3-dimensional effects obtained with the other experimental figures used so far to be due to the perspective information thought to be contained in these figures, it would seem to call for different results from this figure. In fact this prediction is not confirmed and the results from the Crosses figure are very similar to those from the earlier experimental figures. Had the Crosses' results come from an ingoing M-L, then we would have concluded that 4 of the II Ss were pro-Gregery since they saw the centre point as nearest with the two side points as equidistant. In the ingoing M-L used in experiment 2 only 3 Ss out of IO were so classified! Thus a figure which would not be expected to appear 3-dimensional in any paticular way, produces just as many cases of what would be called pro-Gregory results as a figure in which every S is expected to see it in this way.

The results from this figure continue the non-significant tendency to see the centre point as nearest. In actual fact, any point which lies on the primary visual axis i.e. in the centre of the field of vision when the S looks straight ahead, will be slightly nearer than any point at right-angles to this axis (Fig.3:7) Thus the part of our figures which lies on the primary visual axis will be nearer than any other part of the figure.

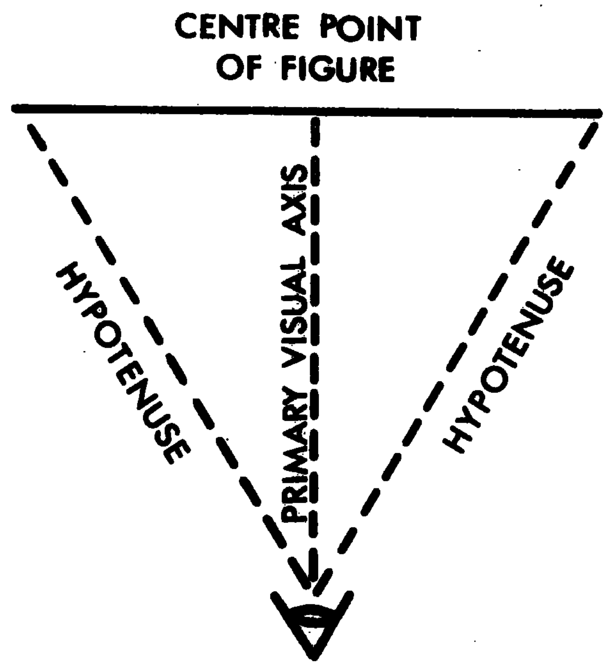



Fig. 3.7 - Diagram showing that the point where the primary visual axis intersects the figure is the point nearest the eye.

However, the actual distances involved were very small and are listed in Table 3:I. The method of calculating these distances is as follows. In all cases the distance from the eye to the nearest point on each figure was I8 cms. The distance between the furthest points on each figure was measured and divided by two to give the second side of a right-angled triangle (the first side being I8cms.) The hypotenuse could then be calculated using Pythagoras. The difference in distance between the centre and the most extreme points was obtained by subtracting I8 cms. from the hypotenuse.

For the Ponzo and the double M-L the calculation had no significance since the points compared were of equal distance from the intersection of the primary visual axis.

| TABLE 3:I | | DISTANCE DIFFERENCE | | CORRECTION TO |
|---|---|---|---|---|
| FIGURE | WIDTH | (HYPOTENUSE - I8cms.) | % | OVERALL ANALYSIS |
| Ingoing M-L | 3.0cms. | 0.06cms. | 0.35 | 0.3cms. (see below) |
| Curve (Expt.I) | 3.0cms. | 0.06cms. | 0.35 | 0.2cms. |
| Curve (Expt.2) | 3.5cms. | 0.07cms. | 0.4I | 0.3cms. |
| Outgoing M-L | 2.0cms. | 0.04cms. | 0.23 | 0.2cms. |
| Crosses | 4.5cms. | 0.09cms. | 0.52 | 0.4cms. |

In no case would the indicated correction to the overall analysis have altered any of the analyses. The differences are so small as to be completely swamped by other effects. However, they are mentioned because of the noted tendency to see the centre points as neaer in the overall analyses.

As far as the curve was concerned, we at last obtained a result, that of Grundy, in which the Centre point was seen as further as found by Coren and Festinger; however, it has already been noted that this result is slightly suspect. Ss fell into two main groups, 4 seeing the bottom as furthest and 4 seeing the centre as nearest i.e. an anti- Coren and Festinger result. The former group produced the overall analysis showing the bottom as furthest. This is a somewhat surprising result since, if anything, one would expect the top point to appear furthest since objects high up in the visual field do tend to be further than those lower down. However, this result should be treated with caution since most Ss did not see the bottom as further. A wider sample could well produce a different result. This is a clear example of an overall analysis not reflecting the trends shown by the individuals.

In experiment 2, the question was asked, can we predict how a S will see one figure from his results on another figure? The answer given was no. The method of comparison used in experiment 2 was used again here, only our results now allowed us to make many more comparisons. The details (including those already quoted in experiment 2) are set out in Table 3:2. Not one of the I2 correlations was significant at even the .05 level. These results confirm the conclusion drawn in experiment 2 that how a S sees one

TABLE 3:2

| | LINEAR | | | QUADRATIC | | |
|---|---|---|---|---|---|---|
| COMPARISON | r | df | t | r | df | t |
| Crosses vs Ingoing M-L | -0.45 | 9 | I.7 | -0.46 | 9 | I.8 |
| Crosses vs Outgoing M-L | -0.50 | 9 | 2.0 | +0.24 | 9 | 0.8 |
| Crosses vs Curve (Expt.2) | +0.I2 | 5 | 0.3 | -0.3I | 5 | 0.8 |
| Ingoing M-L vs Curve (Expt.2) | +0.I4 | 8 | 0.4 | +0.I8 | 8 | 0.5 |
| Ingoing M-L vs Outgoing M-L | +0.05 | I2 | 0.2 | +0.33 | I2 | I.3 |
| Curve vs Outgoing M-L | -0.28 | 8 | 0.8 | -0.I2 | 8 | 0.3 |

figure is no prediction of how he will see another, although we can say that he is likely to see the central part of any figure as nearer.

Bearing these results in mind it is now possible to set out an explanation of our Ss' behaviour which can be tested in subsequent experiments. We have shown that the experimental and control figures fall into different categories as far as the S is concerned. It seems that the division occurs because of differences in the 'structure' of the figures. This term should not be confused with 'set' as used earlier. They refer to different concepts, e.g. a S may be 'set towards a particular structure'.

First let us consider the Control figures. They have what might be termed a 'public structure' in that Ss know how to react to them as soon as they see them and all Ss react the same way on the basis of past experience. The task is simple and the response mode allows the S to use the perspective information provided.

The situation is different when our S sees an experimental figure. He is not immediately aware of any distance cues and yet he is required to indicate distances, thus he must structure the figure in his own 'private' way.

If this explanation is correct we should not be surprised at the number of 'unclassified' responses. They are probably the result of Ss attributing arbitrary distances to each point and remaining consistent to these distances throughout the experiment. This is not to say that a S's private structure will always be arbitrary - if the figure does appear truly 3-dimensional to him then it will obviously not be arbitrary.

We may conclude then, that Gregory is not justified in believing his illusion figures to contain the perspective cues he described. Why then did he and Coren and Festinger get the results they did? It is tentatively suggested that perhaps the figures used did have a 'public structure' for the Ss used. Perhaps this structure was given to the figures by the experimenter's instructions. The results of the experiments reported here have suggested that knowledge of the experimenter's belief can 'set' Ss to see them as flat. Once this knowledge is removed the S produces more 'unclassified' differences. Perhaps Gregory's Ss knew of his theories and used this information to 'structure' the figures accordingly. We know that the figures <u>can</u> be seen in the way Gregory suggests - his Ss just selected the indicated alternative, rather than one of the others.

The literature contains many examples of experiments in which extraneous factors have been shown to change a S's perceptual response. The plausibility of it happening here is investigated more fully in later experiments.

## EXPERIMENT 4 - Further Testing Of The Prediction From The Size-Distance Invariance Hypothesis (SDIH) And An Investigation Into The Shape Of The Apparent Fronto-Parallel Plane (AFPP)

INTRODUCTION   Experiment 3 confirmed the prediction from the SDIH that the shaft of the outgoing M-L should appear to be nearer than the shaft of the ingoing M-L because it appears to be longer. This finding was at variance with that of experiments I and 2, in which the ingoing shaft and a vertical line appeared to be equidistant. One difference between the experiments was in the number of points the S was required to 'distance'. In experiment 3 the S's whole attention was concentrated on the two shafts, whereas in experiments I and 2, the fin-ends had to be considered as well. It was thought that these additional points might have interacted with the 'shaft points' in such a way as to negate the effect obtained in experiment 3.

There are no a priori grounds for such a prediction but it is quite easy to hypothesise as to how it might happen. For example, in the earlier experiments, Ss may have tended to see the two vertical lines and the two fin-ends as two distinct groups, and made their settings by putting points in the samegroup at the same distance. When the S does not have to bother about the fin-ends, he would be more likely to notice differences between the lines rather than similarities - hence on this occasion he might use the apparent length difference as a basis for his settings. Although Holway and Boring (I94I) have shown that Ss will readily respond to differences in visual angle, Epstein and Landauer (1969) have suggested that they do not invariably do so. They may well choose the response which seems appropriate to the situation.

In experiment 4, Ss were shown a double M-L figure similar to that used in experiment 3. They were asked to distance not only the shafts but four of the fin-ends as well, i.e. six points in all (Fig. 3:8). If the results replicated those of experiment 3 then this would be strong evidence for concluding that the number of points is not a relevant factor. We would have to look elsewhere for an explanation of the differences in our results.

In five of the six experimental figures in which three points or more were presented at eye-level across the visual field, the point nearest the centre of the visual field has been seen as nearest when the data was pooled. This was also true of the curve used in experiment 3 which was presented vertically. In none of these cases was the effect large enough for significance at even a low level. It was pointed out in the discussion of experiment 3 that the most central point would actually be the nearest, although the difference would be marginal. If, in fact, this was the reason for the preponderance of near results from central points, then it was thought that using six points would allow the effect to show itself more clearly. It was decided to test for a quadratic trend if the results looked as if they warranted it.

In the discussion of experiment 3 a theory embodying 'private' and 'public' structures was outlined. It does not seem that our experimental figures have a 'public' structure, i.e. Ss do not see them in the same way. Yet the preponderance of results indicating

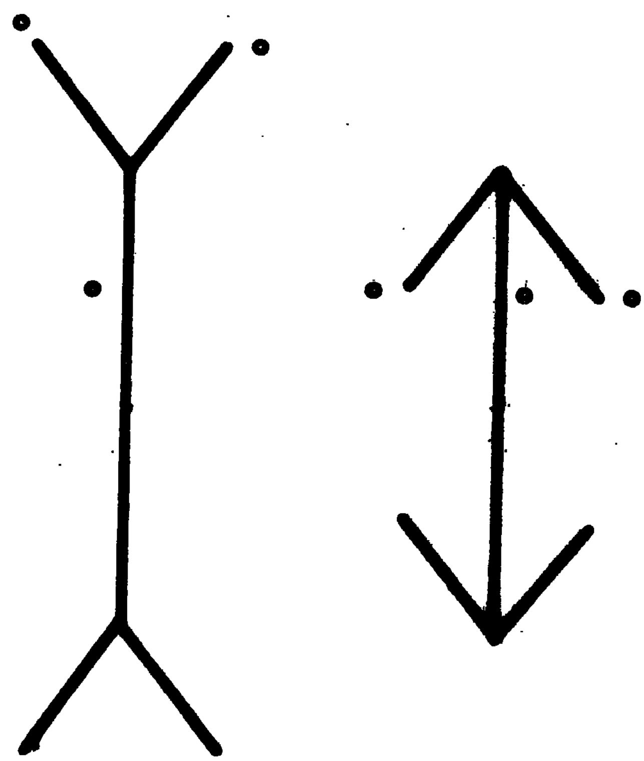


Fig.3.8 - The 'double' M-L. Dots indicate where the lights were positioned.

that the centre is seen as nearest might indicate that this is the way that most people interpret the figures even though it is not nearly such a unanimous effect as was found with the control figures. In this case, such a public structure would be 'veridical' in the sense that the centre point is nearest, although it would exaggerate the actual difference.

Previously the centre point of the figure was coinicident with the centre of the S's visual field. This would no longer be true since two separate figures were viewed in combination here and the centre of the S's visual field would lie between them.

A precedent for our results can be found in Helmholtz (1925). "Hang three fine black silk threads from three pegs which are fastened several inches apart in a horizontal beam some distance above the head; the threads being stretched by weights and all three of them at first being in the same vertical plane. Then stand directly in front of them, so that the central thread lies in the median plane of your face at arm's length away, the plane of the threads being perpendicular to this plane. At some distance behind these threads there should be a background all of uniform colour without any conspicuous points on it. Now look carefully and see whether the threads really do seem to be all in one plane. It will be found that the central one apparently lies in front of the other two." (p.318). Helmholtz found this effect in all five Ss used. This experiment is very similar to ours in many respects, although there are obvious differences, e.g. if the three threads are taken to resemble our 'points', ours are seen with only one eye, whereas Helmholtz's are viewed with two.

Helmholtz believed that stereoscopic acuity alone was not good enough to tell us whether or not the threads were in the same plane. "Now, in order to be able to tell whether the differences perceived in this part of projection belong to a plane or to a curved surface, we should have to be able to estimate the distance of an object very accurately by means of the convergence of the visual axes." (p.319). He believed that information "in the vertical dimension" was also neccessary. With this in mind he repeated the experiment after hanging beads on the threads at 4 cms. intervals and found the illusion greatly reduced. In fact, the beads do provide the information neccessary for a S to make the correct perception, providing he assumes that the distance between the beads is equal on all three threads. Consider the diagrams in Fig. 3:9. The crosses represent the beads. All the S has to do is to compare the angle subtended between the beads on each thread. If the angle is the same then the threads lie in the same plane.

Helmholtz also found that when the threads were "brought near any object at all containing a sufficient number of conspicuous points" the illusion disappeared entirely. Presumably what happens is that we know the shape of the object and judge the threads in relation to it - quite an easy judgement when the object and the threads are close together.

However, it would be difficult to adapt Helmholtz's largely binocular explanation to fit our results, in which Ss were viewing monocularly. Another explanation seems more plausible.

Many experimenters have attempted to determine the apparent fronto-parallel plane (AFPP). Hering (1942) postulated that with

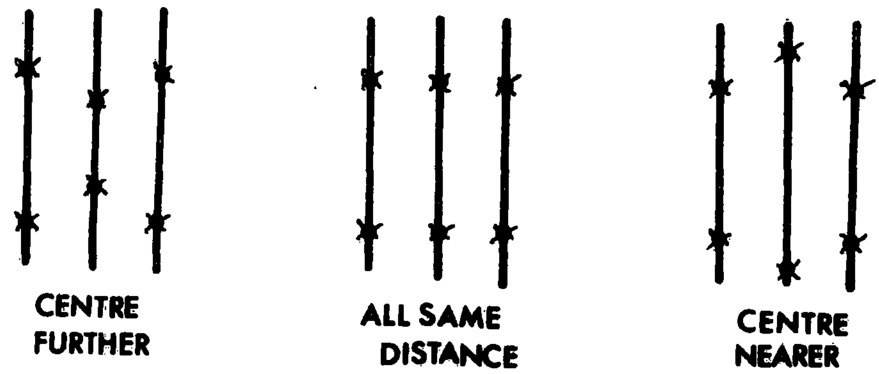



Fig. 3.9 - Diagram showing how beads can give cues to the relative distances of the threads.

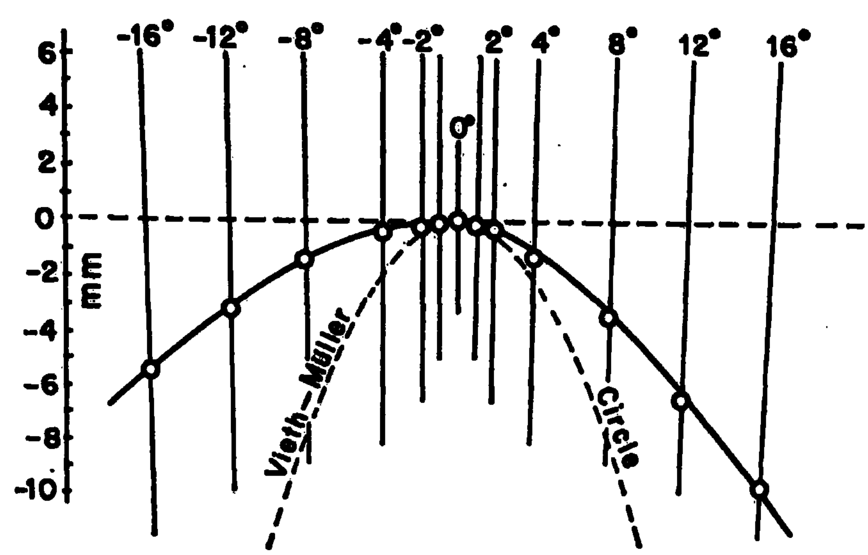



Fig. 3.10 - The AFPP. Data from Ogle (1950).

a steady fixation of the eyes, a series of points in space lie on the horopter when so adjusted that they and the fixation point all appear the same distance from the observer, that is so that they appear to lie in a plane parallel to the frontal plane of the head. Fig. 3:10 illustrates a typical set of data for the apparent fronto-parallel plane taken from Ogle (1950). It can be seen that the locus of points on the plane is not parallel to the frontal plane of the head; it is concave towards the S. This means that a thread not at the fixation point but on a plane that was parallel to the frontal plane of the head would lie behind the AFPP i.e. it would appear further away than the fixation point. This is just what Helmholtz reports.

There are a number of difficulties in applying this sort of explanation to our data. It would seem to be a fairly straightforward matter to say that when a S fixates the shaft of a M-L, then the fin-ends, being on the actual FPP, would lie behind the AFPP. Yet the S can only indicate this by using the reference lights. If he set his lights as far behind the AFPP as the fin-ends appeared to be, then he would be setting them at the same distance as the fin-ends, i.e. on the actual FPP. This clearly would not give the observed result.

It should also be noted that for the fin-ends to appear behind the AFPP the S must fixate the shaft at all times. If he should change his fixation point and stare at the fin-ends themselves, as Ss were instructed to do, then the shaft would be behind the AFPP.

Also the distance of the settings is crucial. Hillebrand (1893) concluded from his experiments that the AFPP would decrease in curvature with increasing viewing distance. At some particular distance (the "abathic" distance - Liebermann, 1910) the AFPP should coincide with the actual FPP. For greater distances than this the AFPP reverses and becomes convex towards the observer. Data from Ogle (1950) indicates that the AFPP is still very convex at distances around 80 cms., which is the sort of distance most of our Ss were using, but that it had reversed by 6 metres. Foley (1966)' found no tendency for reversal even at distances as far as 4.2 metres.

Tshermak (1924) has shown that the shape of the AFPP changes according to the colour of the threads and background used.

There are further complications when the eyes are fixed on a point which requires asymmetric convergence. (Miller and Ogle, 1964; Richards, 1968; Ogle, 1962)'.

Another factor to be considered is the size of the difference between the actual FPP and the AFPP. The angular difference between the two outermost points given the width of the figures and the distance at which they were presented, was never more than 4 degrees in our experiments. It can be seen from reference to Fig. 3:10 that the difference between the actual and apparent FFPs is very small for such a small difference of angle.

Despite these difficulties, it was still thought possible that the AFPP was responsible for the tendency to see the points in the centre of the visual field as nearer. It is considered reasonable to assume that a S gets his first overall impression of a figure by fixating its centre point. In our experimental situation this would mean that the outer parts of the figure lay behind the AFPP. If the S wished to indicate that he perceived the figure in this way by means of setting the lights, all he would have to do would be to make sure that he set the peripheral lights a little further away than the centre lights. If he concentrated on this task he might not notice that when he fixated a 'peripheral' point, the other points were behind the AFPP since he is no longer seeking an overall impression.

How would we expect the locus of points from the double M-L figure to be affected by the AFPP? In this figure the centre point of each M-L i.e. the shaft, is no longer in the centre of the visual field. It has been mentioned that the situation is complicated by asymmetric convergence. Data from Ogle (1962) indicates that under these conditions the figures should appear tilted (Fig. 3:11). If the S is converged on a M-L shaft to the right of the centre of the visual field then the left fin-end of this M-L would appear to lie in front of the AFPP but the right fin-end would lie behind it. The situation would be reversed for a M-L shaft lying to the left of the centre of the visual field. This leads to the prediction that the locus of the six points should be something like that illustrated in Fig. 3:12. The left-hand M-L was always the outgoing figure in this experiment . If Ss are influenced in their judgements by the AFPP and the figure's relation to it, we would expect the left-hand fin-end of this figure to appear nearest, with the right-hand fin-end appearing furthest. The right-hand M-L was always the ingoing figure. We would expect the left-hand fin-end of this figure to be furthest and the right-hand fin-end nearest.

It was mentioned in experiment 3 that the nearest point to the S's seeing eye was the point directly in front of it, i.e. the point where the primary visual axis strikes the figure. All other points

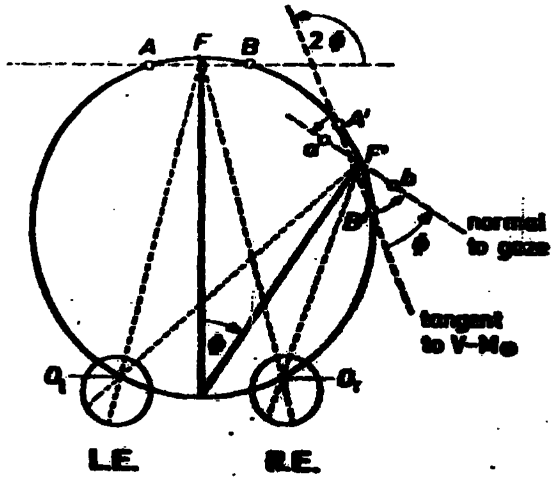



**Fig. 2. The problem of the position of the apparent normal plane under asymmetric convergence: Positions *AB* each stimulate corresponding retinal points, as they lie on the Vieth-Müller circle. Under asymmetric convergence, however, the new locations of the corresponding retinal points seem to shift from the expected locations of *A'B'* to *ab***

Fig. 3.11 - From Ogle (1962).

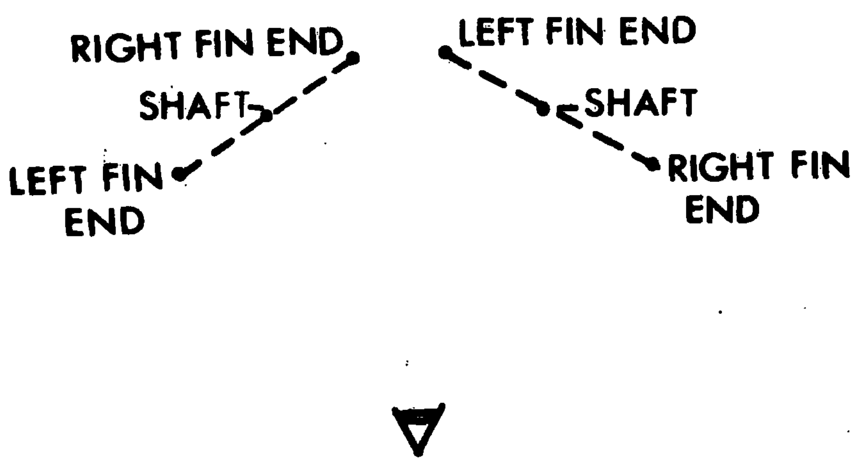



Fig. 3.12 - Prediction of how the various parts of the figures should appear tilted based on the information given in Fig. 3.11.

are slightly further away, increasingly sso as one goes further away from this point. If Ss are using this as the basis for their judgements we would expect the locus of the six lights of the double M-L to describe a curve convex towards the S, i.e. the M-Ls would show linear trends in the opposite direction from those predicted for the AFPP thesis. It was thought unwise to predict a perfect curve lest the different apparent shaft lengths should cause the two M-Ls to be seen at very different distances.

In addition to the double M-L, Ss were asked to distance another figure. When Ss had set the six lights in position for the double M-L, this figure was removed and replaced by a figure consisting of a single spot of light which appeared in the centre of the S's visual field, i.e. half-way between the positions previously occupied by the shafts of the M-L figures. The S's task was to set all six lights to the same distance as the spot while fixating the spot at all times. It was hoped that the locus of the six lights would correspond to the S's AFPP. The Crosses figure in experiment 3 has been presented as one containing no perspective cues. This figure is even more neutral.

However, it differs from the more conventional attempts to map the AFPP in one important respect. The fixation point, i.e. the spot, is viewed monocularly under conditions of complete reduction. Thus its distance is ambiguous and subject to fluctuation over time. This figure provides the minimum of a framework and is thought to be the ultimate in 'unstructured' figures - using the definition of structure given in experiment 3. If the distance of the spot is ambiguous and does appear to fluctuate we would expect most Ss to show either significant Blocks effects or significant Blocks by Points interactions, or both.

Chapter I was concerned with assessing the possibility that the proprioceptive cues available in Gregory's apparatus might have been used by Ss. The evidence suggested that this was not so, but it was thought that they may have acted to reduce the observed 3-dimensional effects. Our previous experiments have shown this to be unlikely. It was decided to modify the apparatus to allow Ss to accommodate to the figure's actual distance. It was expected that the results would confirm the prediction that accommodation was not an effective cue in this situation. The introduction of this cue should remove one of the ambiguities of the experimental situation in that Ss could now set the lights at a distance compatible with the distance information available, in theory at least. Of course, if Schober (1954) is right, then they were doing this anyway.

<u>APPARATUS</u> The Synoptophore was modified in the following way. The end of the right-hand reduction tube was removed, i.e. that part into which the figure was inserted. Beyond the end of this now open reduction tube was placed a Pandora's Box, with its front surface removed. Thus by looking through the eye-pieces of the Synoptophore the S now saw via the half-silvered mirror, the figure displayed in the Pandora's Box (Fig. 3:13).

The optical system of the right-hand eye-piece was dismantled and the lens removed. The system was put back together without the lens but was the same apart from this. The removal of the lens allowed the eye to accommodate for the actual distance of the figure presented in the Pandora's Box (66 cms. in this case).

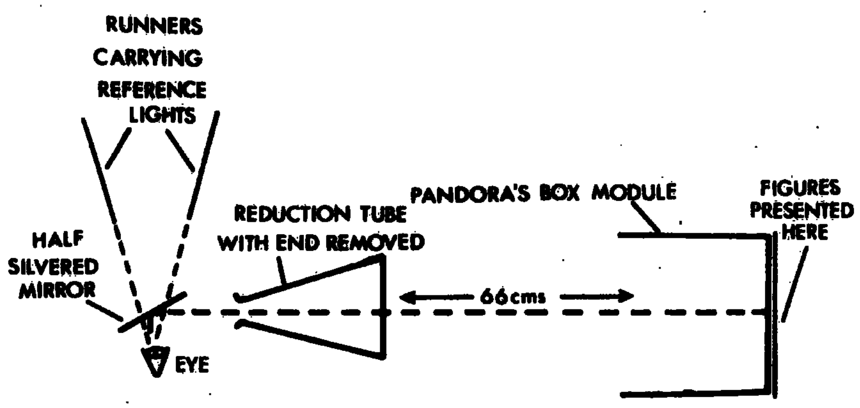



Fig. 3.13 - The modified apparatus.

The figures were photographic transparencies mounted on a glass plate and back-illuminated.

The figures have been described briefly already. The 'spot' appeared as a circular patch of green light, 0.5 cms. in diameter. It was sited so as to fall in the centre of the S's visual field. Both the ingoing and outgoing M-Ls had shafts of 9 cms. in length. The outgoing figure always appeared on the left of the ingoing figure and the centre of the S's visual field was always half-way between the two shafts.

To recapitulate, the S saw the lights, viewed with both eyes, and the figure, viewed with only his right eye. The actual distance of the figure was 66 cms. but he had only the accommodation cue to tell him this.

<u>**PROCEDURE**</u> The S was brought into the room and seated in front of the Synoptophore. The room lights were extinguished and the S requested to look through the eye-pieces at the M-L figure. The six reference lights were illuminated in turn and positioned relative to different parts of the figure as described in experiment I. Lights were positioned near the top fin-ends of both the ingoing and outgoing figures and next to both shafts. The lights near the shafts and the fin-ends of the ingoing figure were all at eye-level but the lights near the fin-ends of the outgoing figure were of necessity somewhat higher.

The lights ran on Dural bars as described in experiment 2.

8 Ss completed the spot figure (3 male, 5 female) but only 7 of these (2 male, 5 female) completed the M-L. All Ss were between

the ages of I8 and 26 years and all were paid a small sum for their participation. They had all participated in previous experiments, 4 of them in experiment I. These Ss were the same 4 who participated in experiment 3, their results (with the possible exception of Grundy) were no different from the other 'naive' Ss. It was felt that any 'set' imparted during experiment I would have been dispersed during the long interval (a year) between experiments. To increase this possibility these Ss were specifically told that the purpose of the experiment was "different from the first one you did". Nevertheless their results were scrutinised for any differences from the other Ss.

Although the M-L figure was always used to position the lights, which remained in the same place for both figures, it was not always the first figure to be 'distanced' - 3 Ss 'distanced' the spot first. This meant that for these three Ss and the S who only completed the spot figure, the M-L figure was removed,after the position of the lights had been set, and replaced by the spot.

As all the Ss had served in previous experiments they needed little instruction as to their task. For the M-L figure they were told to set the lights at the same distance as the part of the figure indicated; for the spot figure, the instructions were to set all the lights at the same distance as the spot.

Ss did two Blocks of I2 trials for each light, making 24 settings in all. Having completed the first Block, i.e. I2 settings for each light, the S was asked to wait in another room while his settings were measured. This usually took about five minutes. He was then recalled for his second Block. The order in which the lights were 'distanced' was random and different for each Block. Great care was taken during the measuring that the position of the bars was not disturbed. Ss, on their return, were asked to report any change in the position of the lights.

Readings were marked on the bars with a felt-tipped pen, as in experiments 2 and 3.

RESULTS First let us consider the M-L results. In order to compare the results with those of the previous experiments, the ingoing and outgoing M-Ls are considered as separate units.

For the ingoing M-L, four Ss were 'unclassified', two were 'flat' and one S was anti-Gregory. For the outgoing M-L, three Ss were 'unclassified', two were 'flat' and one S was pro-Gregory. Obviously the two sets of results are very similar, not only to each other, but also to those of previous experiments.

The overall analyses showed no significant Points differences within figures. A test for quadratic trend was not significant. The shafts were the nearest points for both figures when the data was pooled.

All three points for the ingoing figure were nearer than the nearest point of the outgoing figure. However, the difference between the two figures considered as wholes was not significant (F=2.97 for I and 30 d.f.), when tested by the method of orthogonal comparisons.

Three Ss (Martin, Cross and Bateman) yielded a significant effect of Blocks in their individual analyses. All three made

**RESULTS** **The Ingoing M-L**

MEANS (cms.)

| SUBJECT | LFT. FIN | SHAFT | RT. FIN | Significant Difference | |
|---|---|---|---|---|---|
| Martin | 67.2 | 79.5 | 78.8 | Shft & Rt > Lft | (UNC) |
| Cross | 98.7 | 92.3 | 105.6 | Rt > Shft | (UNC) |
| Bateman | 92.8 | 91.5 | 106.1 | Rt > Shft & Lft | (UNC) |
| Lee | 72.8 | 66.2 | 75.2 | NONE (Flat) | |
| Grundy | 64.7 | 69.0 | 67.0 | NONE (Flat) | |
| Hockey | 114.7 | 100.2 | 95.5 | Lft> Shft & Rt | (UNC) |
| Curless | 65.2 | 74.9 | 66.1 | Shft> Lft & Rt | (A.G.) |
| MEANS | 82.3 | 81.9 | 84.9 | | |

**The Outgoing M-L**

| | | | | | |
|---|---|---|---|---|---|
| Martin | 70.2 | 80.2 | 71.6 | Shft > Lft & Rt | (P.G.) |
| Cross | 92.5 | 86.7 | 97.3 | Rt > Shft | (UNC) |
| Bateman | 95.6 | 97.6 | 105.3 | Rt > Shft & Lft | (UNC) |
| Lee | 76.2 | 72.9 | 77.8 | NONE (Flat) | |
| Grundy | 93.5 | 79.7 | 77.5 | Lft > Rt & Shft | (UNC) |
| Hockey | 136.2 | 132.1 | 124.7 | NONE (Flat) | |
| Curless | 76.9 | 74.1 | 78.1 | NONE (Flat) | |
| MEANS | 91.6 | 89.0 | 90.3 | | |

**RESULTS** **The Ingoing & Outgoing Shafts Compared**

MEANS (cms.)

| SUBJECTS | IN. SHFT. | OUT. SHFT. | Sig. of Difference |
|---|---|---|---|
| Martin | 79.5 | 80.2 | Not Sig. |
| Cross | 92.3 | 86.7 | Not Sig. |
| Bateman | 91.5 | 97.6 | Not Sig. |
| Lee | 66.2 | 72.9 | Not Sig. |
| Grundy | 69.0 | 79.7 | Outgoing > Ingoing |
| Hockey | 100.2 | 132.1 | Outgoing > Ingoing |
| Curless | 74.9 | 74.1 | Not Sig. |
| **MEANS** | 81.9 | 89.0 | |

**OVERALL ANALYSIS OF VARIANCE FOR INGOING- OUTGOING FIGURE**

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | 6 | 2130.7 | |
| BLOCKS | 1 | 96.4 | 1.8 (N.S.) |
| POINTS | 5 | 242.0 | 0.6 (N.S.) |
| BLOCKS X Ss | 6 | 52.9 | |
| POINTS X Ss | 30 | 417.9 | |
| POINTS X BLOCKS | 5 | 9.5 | 0.02 (N.S.) |
| POINTS X BLOCKS X Ss | 30 | 487.8 | |

**DUNCAN TEST OF DIFFERENCES BETWEEN POINTS**

| MEANS | 81.9 | 82.3 | 84.9 | 89.0 | 90.3 | 91.6 | Shortest Sig. Range(.05) |
|---|---|---|---|---|---|---|---|
| 81.9 | - | 0.4 | 3.0 | 7.1 | 7.4 | 9.7 | 15.8 |
| 82.3 | | - | 2.6 | 5.7 | 8.0 | 9.3 | 16.6 |
| 84.9 | | | - | 4.1 | 5.4 | 6.7 | 17.1 |
| 89.0 | | | | - | 1.3 | 2.6 | 17.5 |
| 90.3 | | | | | - | 1.3 | 17.8 |
| 91.6 | | | | | | - | |

RESULTS The Spot

MEANS (cms.) - Left to Right

| SUBJECT | ONE | TWO | THREE | FOUR | FIVE | SIX |
|---|---|---|---|---|---|---|
| Hinchliffe | 84.4 | 92.7 | 84.0 | 71.8 | 68.7 | 60.9 |
| Bateman | 92.4 | 93.4 | 102.7 | 83.9 | 88.3 | 80.2 |
| Grundy | 94.5 | 100.6 | 109.1 | 82.1 | 114.8 | 76.5 |
| Lee | 80.2 | 78.5 | 79.7 | 75.0 | 79.7 | 68.0 |
| Curless | 99.0 | 103.7 | 113.5 | 101.1 | 104.8 | 100.7 |
| Hockey | 106.8 | 108.2 | 102.0 | 92.0 | 82.2 | 86.4 |
| Martin | 91.7 | 100.9 | 90.3 | 79.6 | 122.9 | 73.2 |
| Cross | 101.5 | 109.1 | 107.9 | 93.4 | 114.5 | 99.7 |
| MEANS | 93.8 | 98.4 | 97.4 | 84.9 | 97.0 | 80.7 |

OVERALL ANALYSIS OF VARIANCE

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | 7 | 1300.6 | |
| BLOCKS | 1 | 32.6 | 0.3 (N.S.) |
| POINTS | 5 | 872.2 | 6.2*** |
| BLOCKS X Ss | 7 | 94.8 | |
| POINTS X Ss | 35 | 141.9 | |
| POINTS X BLOCKS | 5 | 53.8 | 0.2 (N.S.) |
| POINTS X BLOCKS X Ss | 35 | 221.6 | |

DUNCAN TEST OF DIFFERENCES BETWEEN POINTS

| MEANS | 80.7 | 84.9 | 93.8 | 97.0 | 97.4 | 98.4 | Shortest Sig. Range(.05 & .01) | |
|---|---|---|---|---|---|---|---|---|
| 80.7 | - | 4.2 | 13.1** | 16.3** | 16.7** | 17.7** | 8.6 | 11.5 |
| 84.9 | | - | 8.9* | 12.1** | 12.5** | 13.5** | 8.9 | 11.9 |
| 93.8 | | | - | 3.2 | 3.6 | 4.6 | 9.3 | 12.2 |
| 97.0 | | | | - | 0.4 | 1.4 | 9.5 | 12.5 |
| 97.4 | | | | | - | 1.0 | 9.6 | 12.7 |

TEST FOR QUADRATIC TREND F=14.0*** for I and 35 df.

their settings in the first Block further than in the second Block. This trend was not significant in the overall analysis.

Four Ss (Cross, Grundy, Heckey and Lee) yielded significant Blocks by Points interactions. The overall analysis did not.

The mean of all the settings for this figure was 86.7 cms. - more than 20 cms. further than the figure's actual distance.

The individual analyses for the spot showed each S to have a different pattern of differences between points (see Appendix for further details). The overall analysis showed points I, 2, 3 and 5 to be set significantly nearer than points 4 and 6. Quadratic trend was found to be significant beyond the .00I level.

Four Ss yielded significant Blocks effects in their individual analyses, three of whom (Bateman, Curless and Cross) made their first Block settings furthest. The fourth S (Hockey) made her second Block settings furthest.

All Ss but one (Lee) yielded significant Points by Blocks interactions, six of them beyond the .00I level. Neither the Blocks nor the Points by Blocks interaction effects were significant on the overall analysis.

<u>DISCUSSION</u> In the Introduction, two hypotheses were put forward to explain the tendency of our Ss to see the most central point of any figure as nearer than any other point. One hypothesis was based on the fact that the most central point of any figure would actually be nearer the viewing eye than any other point, although the

difference would be marginal. For the double M-L this hypothesis would predict that the locus of the six points would describe a curve convex towards the observer.

The other hypothesis was based on the fact that the AFPP is not the same as the actual FPP. With fixation of a point on the primary visual axis this hypothesis would predict similar results to the first one, but with asymmetric convergence it would predict linear trends within each M-L.

The results do not support either of these hypotheses. As in previous experiments the shafts of the M-L were seen as nearer than the fin-ends when the results were pooled, although the differences were not large enough for significance. In the double M-L it seems reasonable to assume that each M-L is seen as a separate object or gestalt - none of our previous figures contained more than one gestalt. Thus we are left with the conclusion that the centre point of any gestalt will be seen as nearer than its extremeties no matter where it appears in the S's visual field. Why this should be so is difficult to explain and it is a result which has virtually no precedents in the literature.

A large number of experiments have been conducted in which Ss have been asked to align binocularly seen lights in apparently straight lines under conditions similar to those prevalent in this experiment. These studies by Luneburg (1947, 1950), Blank (1953, 1958, 1959), Foley (1964, 1966, 1967, 1968) and others have been concerned with determining the geometry appropriate to describe perceived visual space. These studies have established that the requisite geometry is non-Euclidean, since the locus of a series of lights in an apparently straight line is not in fact straight but curved, eg the AFPP. One of these experiments (Foley, 1964) reports a result similar to that reported here. In one part of the experiment Ss were presented with two fixed lights and were required to adjust four other lights to lie on a straight line between these two fixed points. Foley comments that, "the fixed points often lie a short distance behind the curve suggested by the other points." He found this result difficult to explain - "the explanation of why the fixed points often lie behind the curve suggested by the other points is unknown. Helmholtz noted a similar phenomenon in his AFPP experiment ..... He attributed this effect to fatigue of the eyes. The present result suggests that the phenomenon is associated with attention."

The result seems to be peculiar to this experiment and is not mentioned in any of Foley's other studies. It does not appear to occur when the extreme lights are themselves adjustable, (Foley, 1968). It is difficult to see how 'attentional' factors could have produced the results and Foley does not elaborate.

Although this slight tendency to see central points of gestalts as nearer cannot be properly explained, it seems safe to assume that it is not related in any way to size-constancy.

Experiment 3 showed that using the double M-L figure used here, 10 Ss out of 11 saw the apparently longer outgoing shaft as nearer. None of the Ss saw the shafts in this way here. 5 Ss saw them with no significant difference and 2 saw the ingoing shaft as nearer. It would seem that when Ss are asked to distance the fin-ends as well as the shafts they do not behave in the same way. It was conjectured in the Introduction that this might be because they saw the four fin-ends as a separate group from the two shafts, and set members of the same group at similar distances. No such tendency was apparent. Ss seemed to see the three points on the ingoing M-L as one group and the other three points on the outgoing M-L as another, i.e. each M-L appeared as a separate gestalt. In this case it might be expected that the relative distance of each group would be determined by the apparent length of the shaft - as in experiment 3. This did not happen and there was a tendency (non-significant) for the ingoing M-L to be seen as nearer than the outgoing M-L - the reverse of the tendency shown in experiment 3.

The prediction that the locus of the overall means from the 'spot' would describe a curve concave towards the S was confirmed. The quadratic trend was significant beyond the .00I level. The curve was not particularly smooth but this is not surprising when one considers the fluctuation there was in settings. All but one S had a significant Points by Blocks interaction which indicates that the apparent distance of the 'spot' changed over time, as predicted.

In previous experiments significant interactions have been linked with lack of structure. As mentioned in the Introduction, the 'spot' would seem to be our most unstructured figure yet and several Ss reported an auto-kinetic effect when viewing it. Hence these results fit our thesis that lack of structure produces significant interactions very well.

The double M-L figure would appear at first sight to produce more interactions than expected, since it might be thought no more 'unstructured' than the figures used in, say, experiment 2. However, the double M-L required Ss to 'distance' more points, and the more points there are, the greater the likelihood that small changes in distance will produce a significant interaction. It is instructive to note that the only figure in experiment 2 which had four points (instead of three) i.e. the ingoing M-L and line, also had the greatest proportion of interactions.

The Table below shows the relationship between lack of structure and significant interactions.

The effect of accommodation would seem to be minimal in that neither figure was set at anything near its true distance of 66 cms. This was as expected.

| EXPT. | TYPE OF FIGURE | % SIGNIFICANT INTERACTIONS |
|---|---|---|
| I & 2 | Control (Highly structured) | I5*(0) |
| I | Exptl., but with sophisticated Ss (Structured) | 6 |
| 2 | Exptl., but with naive Ss (Unstructured) | 27.5 |
| 4 | Complex Exptl. with naive Ss (Unstructured) | 55 |
| 4 | Spot (Highly unstructured) | 87.5 |

* It was explained in the discussion of experiment 2 that the interactions shown by these Ss were caused by changes of relative distance rather than relative positions and hence should be ignored for our present purpose.

## EXPERIMENT 5 - To Replicate Experiment 4 Using An Improved Apparatus And Method

**INTRODUCTION** The four experiments presented so far have all allowed the S to see only one reference light at any one time. A group of readings would be taken from one point, then that light would be turned off and another light near another point would be turned on. Hence the settings obtained for different points were taken at different times, although the difference would only be about a minute or so. Our results have shown that some Ss have significant Blocks effects i.e. that the distance they attributed to the figure tended to change from one time to another, thus it is possible that an experimental artifact might occur.

Suppose, for example, that a S sees a given figure as flat and at a particular distance. If the distance at which he sees the figure changes after he has 'distanced' a few points, then, although he always sees the figure as flat, the results would probably show a difference between those points distanced before the change and those distanced after. Thus his results would be interpreted as showing that he saw the figure in 3-dimensions when, in fact, he did not.

If the distance at which a S sees the figure changes continually then a significant Points by Blocks interaction could occur even though the S's perception of the figure has remained unchanged. A significant Blocks effect could also occur but this would depend on exactly when the changes in distance occurred.

The results from previous experiments have suggested that most Ss do see the figure in 3 dimensions, i.e. they showed significant differences between points. This experiment was an attempt to replicate experiment 4 but with the possibility of any spurious differences removed. There is no a priori reason to suppose that many or indeed any of the observed effects were due to this, but since the possibility exists, it is obviously safest to test it.

The original idea was to make our experiments as similar to those of Gregory as possible so that direct comparisons would be possible between the results. Both Gregory and Coren and Festinger allowed their Ss to see only one reference light at a time (see Introduction to experiment I), and that is why this method was adopted here. At this stage, however, there would seem to be little point in continuing to adhere to Gregory's methods when our previous results have all differed from his, thus making further comparison irrelevant. We are at present interested in an effect not found by Gregory and new methods would seem more appropriate to our investigation.

As has been stated previously, we are more interested in the relative positions of the Points than their absolute distances. Hence it would seem appropriate that all the points should be distanced simultaneously. We would then have a direct record of the relative positions rather than having to infer it from absolute distances obtained at different times - which is what we have been doing. If the results from this experiment are similar to those obtained previously, then we may assume that the incidence of artifacts of the sort suggested is low and that our previous results may be accepted without modification.

It was decided to use the double M-L figure from experiment 4 since this would allow a direct comparison of the results from the two experiments. Different Ss were needed in case the ones already used were affected by their previous experience with the figure. Our previous results have shown that different Ss tend to interpret these figures in different ways so the use of different Ss might be expected to cause some slight difference between these results and those of experiment 4.

One desirable aspect of Gregory's and Coren and Festinger's experimental techniques was to allow their Ss to adjust the reference light themselves rather than to give instructions for its movement to the experimenter. To allow the Ss to do this with our experimental set up posed quite a messy mechanical problem which was solved by fitting electric motors on the ends of the Dural bars on which the lights ran and using them to drive a loop of thread to which the lights were attached. A full description is given below in the Apparatus section.

<u>APPARATUS</u> The main change in the apparatus was the addition of a facility which allowed the sliders on which the reference lights were mounted to be moved by an electric motor controlled by the S. The electric motors were six Mullard synchromesh motors (AU 5050) with gear-boxes (AU 300/80 BW). The Dural bars used previously were retained and the motors and gear-boxes were secured to one end. The drive shaft was fixed into a metal capstan shaped very much like a cotton reel. At the other end of

the bar a free spinning disc was fitted. It had a groove round its edge such that a length of thread could be looped around it. The thread was also looped around the capstan at the 'motor' end of the bar and then its two ends were tied to the slider carrying the bulb so that a continuous loop was formed. Thus when the motor was activated the capstan was turned and the reference light and slider were drawn along the bar. It was quite important that its progress should be smooth and this was accomplished by adding a small spring to the loop. This, in effect, took up such jerks as there were. The motor was reversible, hence the light could be moved in either direction.

The gear-boxes were chosen so that they turned the shaft at the rate of 50 cycles per second. This speed, allied to the diameter of the cotton reel i.e. 2.5 cms., meant that the light was moved along the bar at exactly 10 cms. per second. This speed was judged to be about right in that it was not so fast as to preclude an exact setting, nor was it so slow as to waste time.

The motors were wired to six Moulded Rocker Contact switches obtained from A. F. Bulgin & Co. (Type S 792 change over, centre off, sides biased to off) which were fitted into a console which could easily be moved about. Fig. 3:14 shows the new apparatus.

Previously the bulbs had been wired in such a way that only one bulb at a time could be lit. This was changed by wiring each bulb to its own switch so that any combination was possible.

The S viewed the figure and the lights through the eye-pieces of ths Synoptophore. The right eye alone saw the figure whereas both eyes saw the lights. The figure was the same double M-L used in experiment 4 and the manner of its presentation was

Console housing the switches that operate the motors.

Motors.

Light switches.

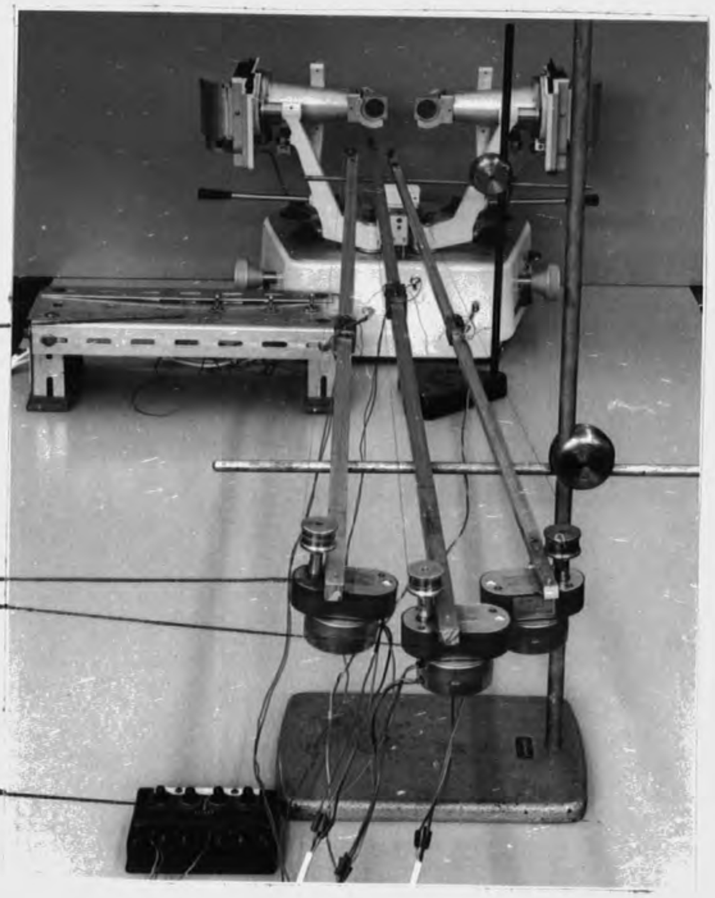


Fig. 3.14 - The new apparatus.

also as in experiment 4 i.e. in a Pandora's Box placed at the end of the right hand reduction tube at a distance of 66.0 cms. Ss were allowed the cue of accommodation.

<u>SUBJECTS</u> Seven Ss, six of whom were female, were used. All were aged I9 or 20 years and none were students of Psychology. None had been used as a S before. They were paid for their participation and they were told nothing about the purpose of the experiment except that it would be painless.

<u>PROCEDURE</u> The changes in the apparatus necessitated some changes in the procedure, but for the most part it was kept as similar to that of experiment 4 as possible.

The S came into the room and had his first look at the figure in just the same way as previously. The points near which the lights should be positioned were indicated by a sketch on the blackboard as before. The S was then introduced to the switch console which was so arranged that the most left hand switch operated the most left hand light and so on. Pulling the switch towards you moved the light towards you and vice-versa. One of the lights was switched on and the room lights were switched off. The S was asked to move the light towards him and to say if it was moving relative to the figure. The lights were then positioned one by one in the usual way. The instructions were exactly the same as in experiment 4, except for the following addition:-

"All the lights will be visible at the same time. You may adjust them in any order you wish and you may go back and change any setting you have made. When you are satisfied with the positions of all six lights, tell me." The six lights were then turned on and the first trial begun. When the S said that he was satisfied with their positions he was asked if he was quite certain. If he was, a cloth was dropped over the eye-pieces and the positions of the lights were marked on the bars with a felt pen. The lights were then moved en-masse to the opposite end of the bar from that at which the previous trial had been started.

Each S completed 12 trials in this manner. When all 12 trials were completed, they were measured. It would have been useful to have known which settings of a particular light were made on which trial but this information was not available since the only way of collecting it would have been to measure each trial immediately it was finished. This would have greatly lengthened the experiment and carried some risk of disturbing the positions of the bars.(Ss had been difficult to obtain and they were faithfully promised that the experiment would only take about 40 minutes. As it was, it still took longer than this for most Ss).

The end of the bar at which the lights were seen by the S on the first trial was alternated from S to S and each trial began at the opposite end from the last.

**RESULTS** The individual analyses were obtained by doing a one-way analysis of variance and using the within Points factor as the error term. Duncan Multiple Range tests were used to assess the differences between respective means.

Two Ss saw the ingoing figure in the way predicted by Gregory. Four were 'unclassified' and one S had no significant differences. The outgoing M-L was seen with no significant differences by five Ss, and two were anti-Gregory.

The overall analysis was a two-way analysis of variance with Points as the main effect. It was not significant and a Duncan Multiple Range test showed no mean to differ significantly from any other beyond the .OI level. Despite this, it was noticeable that many Ss had set the shaft as nearer than the fin-ends. This trend overall was not significant by analysis of variance but if there were really no difference between the shafts and fin-ends, then one would expect the shafts to be set nearest only one-third of the time. In other words, if all points were seen as equidistant, then deviations from equidistance would only occur by chance and each point would be set nearest the same proportion of the time - in this case, one-third.

While four Ss set the shaft nearest for the ingoing M-L, no less then six did so for the outgoing M-L. This means that the proportions of times that the shaft was set nearest were: 0.57 and 0.86 respectively rather than the predicted 0.33. The probabilities of discrepancies so large occurring by chance can be calculated from the binomial distribution and are as follows:-

For the outgoing M-L - 0.006

For the ingoing M-L - 0.I47

Thus it would seem that there is a tendency to see the shaft as nearest and that it is particularly marked in the case of the outgoing M-L.

A comparison between the distances at which the two shafts were set found two Ss with differences significant beyond the .OI level. One of these saw the ingoing M-L as nearer while the other saw it as further than the outgoing M-L. The overall comparisons was not significant.

<u>DISCUSSION</u> This experiment was carried out to see if changes in procedure and apparatus had any effect on the way in which Ss perceived the figures. If the results are very similar to those of experiment 4 then we may conclude that they do not.

The pattern of the overall Point means was virtually identical in both experiments. In both cases the shafts of the M-Ls were closer (overall) than their respective fin-ends although in neither case was the difference large enough for significance. Individually, more Ss set the shafts as nearer than the fin-ends than would be expected by chance, although this tendency was slightly more noticeable here.

There were some small differences in the proportion of Ss falling into the various categories on individual analysis. For the ingoing M-L in experiment 4, one S was anti-Gregory and none were anti-Gregory in this experiment. For the outgoing M-L in

**RESULTS** **The Ingoing M-L**

**MEANS (cms.)**

| SUBJECT | LFT. FIN | SHAFT | RT. FIN | Significant Differences | |
|---|---|---|---|---|---|
| Wise | 76.9 | 47.0 | 82.9 | Lft & Rt > Shft | (PG) |
| McKee | 68.4 | 59.8 | 73.5 | Rt > Shft | (UNC) |
| France | 69.2 | 68.0 | 76.2 | NONE (Flat) | |
| Ratsey | 63.I | 77.0 | 77.0 | Shft & Rt > Lft | (UNC) |
| Eastaugh | 93.2 | 70.8 | II5.8 | Rt > Lft > Shft | (PG) |
| Stevenson | 67.3 | 8I.8 | 87.6 | Rt > Lft | (UNC) |
| Ward | 70.2 | 70.2 | 58.2 | Lft & Shft > Rt | (UNC) |
| MEANS | 72.6 | 66.4 | 8I.6 | | |

**The Outgoing M-L**

| | | | | | |
|---|---|---|---|---|---|
| Wise | 92.2 | 85.3 | 89.2 | NONE (Flat) | |
| McKee | 66.2 | 64.5 | 65.2 | NONE (Flat) | |
| France | 73.0 | 62.3 | 82.2 | Rt > Lft > Shft | (AG) |
| Ratsey | 69.0 | 68.5 | 69.2 | NONE (Flat) | |
| Eastaugh | 67.7 | 52.I | 76.2 | Shft > Lft & Rt | (AG) |
| Stevenson | 93.7 | 93.4 | 83.5 | NONE (Flat) | |
| Ward | 75.6 | 7I.3 | 73.7 | NONE (Flat) | |
| MEANS | 76.8 | 7I.0 | 77.0 | | |

**RESULTS** **The Ingoing & Outgoing Shafts Compared**

MEANS (cms.)

| SUBJECT | ING. SHFT. | OUT. SHFT. | Sig. of Differences |
|---|---|---|---|
| Wise | 47.0 | 85.3 | Out > Ing |
| McKee | 59.8 | 64.5 | Not Sig. |
| France | 68.0 | 62.3 | Not Sig. |
| Ratsey | 77.0 | 68.5 | Ing > Out |
| Eastaugh | 70.8 | 52.I | Not Sig. |
| Stevenson | 8I.8 | 93.4 | Not Sig. |
| Ward | 70.2 | 7I.3 | Not Sig. |
| MEANS | 66.4 | 7I.0 | |

**OVERALL ANALYSIS OF VARIANCE**

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| POINTS | 5 | 200.3 | I.4 (N.S.) |
| SUBJECTS | 6 | 24I.3 | |
| POINTS X Ss | 30 | I4I.6 | |

**DUNCAN TEST OF DIFFERENCES BETWEEN POINTS**

| MEANS | 66.4 | 7I.0 | 72.6 | 76.8 | 77.0 | 8I.6 | Shortest Sig. Range(.05 & .0I) | |
|---|---|---|---|---|---|---|---|---|
| 66.4 | - | 3.6 | 5.2 | I0.4 | I0.6 | I5.2* | I3.0 | I7.5 |
| 7I.0 | | - | I.6 | 5.8 | 6.0 | I0.6 | I3.6 | I8.2 |
| 72.6 | | | - | 4.2 | 4.4 | 9.0 | I4.I | I8.7 |
| 76.8 | | | | - | 0.2 | 4.8 | I4.4 | I9.I |
| 77.0 | | | | | - | 4.6 | I4.6 | I9.3 |

**No Significant Differences (Flat)**

**TEST FOR QUADRATIC TREND** F=0.8(N.S.) for I & 30 df.

experiment 4, 3 Ss had no significant differences and 3 were 'unclassified'. In this experiment, 8 Ss had no significant differences and none were 'unclassified'. These differences are probably due to the use of different Ss in the two experiments since this has been shown to be a common source of variation in our experiments. The similarity in the trends shown by the over-all Points means would suggest that these differences are of minor importance.

In the introduction to experiment 4 it was hypothesised that the tendency to see the centre points of different figures as nearer than the peripheral points was due to their usually being nearest to the centre of the S's field of vision, and thus actually being a fraction nearer. The results from experiment 4 did not confirm this hypothesis. Instead they suggested that the centre of any figure will always appear nearest irrespective of its position in the visual field. This suggestion would seem to be supported by the results of this experiment. This finding is unexpected and difficult to fit into any theoretical framework, as mentioned previously.

We may conclude that, since the results of experiment 4 and experiment 5 are so very similar, our improvements in apparatus and procedure have had no effect and we may safely say that the results of previous experiments using the old techniques need not be questioned.

PART 4 - THE EFFECT OF INSTRUCTION

USING GREGORY'S APPARATUS

## EXPERIMENT 6 - A Return To The Use Of Polaroid

INTRODUCTION In the introduction to experiment I, it was noted that Gregory's apparatus allowed his Ss a number of cues which could conceivably have been used to discover the figure's real distance and its flatness. It would appear from Gregory's results that these cues were not used since his Ss saw the figures in three dimensions. However, the distance differences obtained were rather small and it was thought that by making the Ss' environment completely cueless we might prduce differences of a size more compatible with Gregory's theory. In fact, although larger differences were obtained in many cases, they were not in the predicted directions. From our results the suggestion was made that perhaps Gregory had unwittingly 'set' his Ss in some way so that they 'structured' their perceptions in accordance with the dictates of his theory. If this was so, and if Ss can make use of the sort of cues available in Gregory's experimental set up, then it would be expected that completely naive i.e. 'unstructured' Ss would perceive the figures as flat in such a set up.

Gregory used a Pandora's Box module to present his figures and he relied on cross-polaroid glass to ensure that they were seen by only one eye. The nature of the apparatus is such that rather than presenting the figure directly before the 'seeing' eye, it is presented directly in front of the point half-way between the eyes, i.e. before the ego-centre or Cyclopean eye (Howard and Templeton, 1960; p.17). This means that the 'seeing' eye must converge in order to fixate the figure, and as was pointed out in the introduction to experiment I, the non-seeing eye will follow this lead and converge more or less the same amount (Woodworth, 1938; Grant, 1942; Biersdorf, 1966). Hence the figure's actual location is at the point where the lines of sight from the two eyes intersect (Fig. 4:I). If the non-seeing eye does not converge enough this point will be further away than it should be and this is what Grant believed to be happening in his experiments. In our experiments so far we have used the Synoptophore which requires that the figures be presented before the seeing eye rather than before the ego-centre. In this case there is no convergence in the non-seeing eye because of the accommodation correction (or lack of it). Most of the studies so far referred to have used this method.

Of course, if the 'monocular convergence' which occurs in Gregory's apparatus is to serve as a cue, the assumption must be made by the S that the figure lies directly before the Cyclopean eye. If the figure actually lies somewhere else along the line of sight of the seeing eye, as it quite well could, then his distance judgements will be inaccurate.

The vast majority of the evidence presented so far would suggest that the addition of monocular convergence would make no difference whatsoever to our results. The results from experiments 4 and 5 would suggest that the cue of accommodation, which is intimately linked to convergence, is of no use at all in this situation. Yet there are some aspects of Gregory's results which are puzzling if this is so.

Although he presents no statistics, Gregory writes as if he is convinced that the distance differences he obtained between his

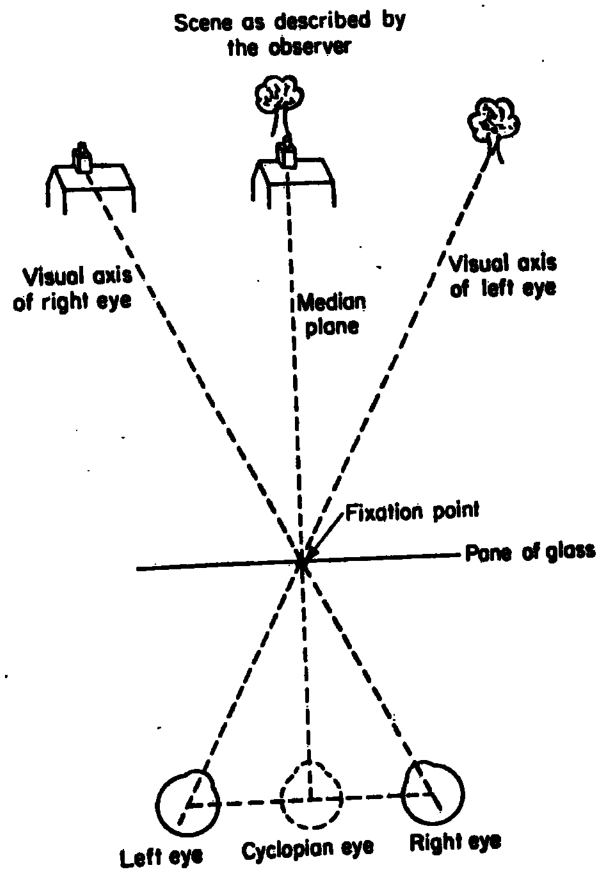



Fig. 4.1 - From Howard & Templeton (1960).

points were significant. The largest of these seems to be less than two cms. In none of our analyses has a difference anything like as small as this proved to be significant. This means that if such a difference were to be significant, his Ss must have shown far greater consistency in their distance settings than did ours.

The only cue available to Gregory's Ss that has so far not been available to our Ss is that of 'monocular convergence'. This would not seem to be an effective cue, yet for some reason Gregory's Ss seem to be much more consistent than do ours. Our results seem to indicate that consistency is linked to certainty, and yet Gregory's Ss should not be any more certain than ours.

It was decided to experiment with an apparatus very similar to Gregory's in an attempt to explain the apparent paradox outlined above. In theory, we would not expect the results to differ at all from those already obtained.

APPARATUS The apparatus was very similar to that of Gregory but there were a number of detail changes. Gregory presented his figures at a distance of 50 cms. Because of the half-silvered mirror running diagonally across the box, it was impossible to move a reference light any nearer than 40 cms. to the S, if one was 'distancing'a point on the extreme right of the figure. It was felt that 10 cms. (the difference between Gregory's near position and the true distance of the figure) was too small a range for adjustment. To overcome this difficulty, it was decided to join together two Pandora's Box modules. The front box contained the half-silvered mirror while the second box served merely to extend the distance between the mirror and the figure. Thus the distance of the figure from the S was now 82 cms. while the nearest setting of the light was still 40 cms.

The figure was a double M-L of the sort used in experiments 4 and 5. It was identical to the figure used in these experiments except that it was larger. The increase in distance meant that the old figure subtended too small a visual angle so that the Dural bars on which the lights ran tended to foul one another at their nearest positions. A further snag was that the electroluminescent panels supplied with a Pandora's Box is only IO cms. high by I5 cms. wide. Changing its orientation so that it is I5 cms. high by IOcms. wide allows a 50% increase in the lengths of the arrows.

A sheet of polaroid with a claimed efficiency of 99.7% was mounted in front of the figure and a pair of 'glasses' were made up of the same material to fit into the viewer. These were so arranged that the S saw the figure with his right eye only. The images of the lights were, of course, reflected into both eyes by way of the half-silvered mirror. With the use of this high quality polaroid there was no tendency towards the 'ghosting' that was mentioned in experiment I.

The lights were electrically operated by the S as in experiment 5, only they were now mounted at right angles to the S's line of sight and seen through the half-silvered mirror, rather than directly in front of him as had previously been the case when using the Synoptophore(Fig. 4:2 - full picture of the apparatus).

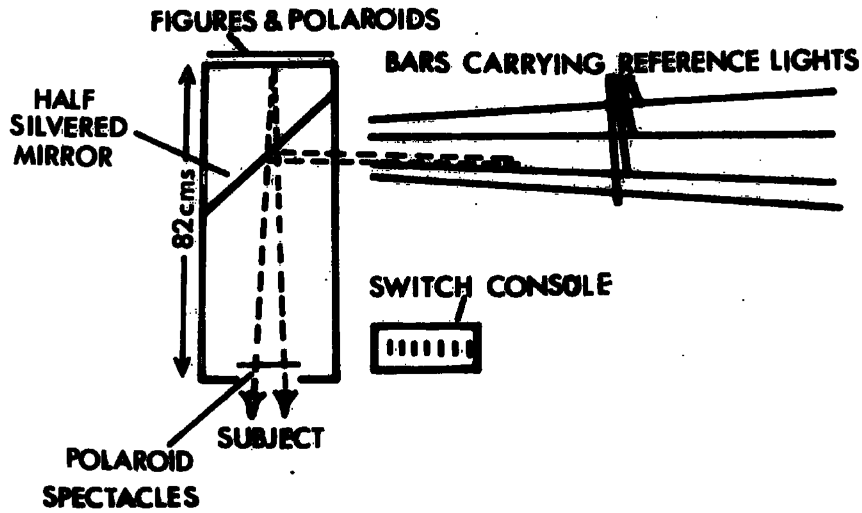



**Fig. 4.2 - Modified apparatus, showing the position of the switch consul next to the Ss right hand.**

**PROCEDURE** The procedure was exactly the same as that for experiment 5 except that Ss now looked through the viewer of the Pandora's Box rather than through the eye-pieces of the Synoptophore. Also a screen was used to obscure the Ss' view of the Dural bars etc. so that he would not be able to watch the experimenter marking his settings. The Ss' view through the viewer was also obscured while marking was in progress. This was done by dropping a cloth over the half-silvered mirror.

There were also some differences in the marking procedure and in the number of trials completed by some Ss. It was mentioned in experiment 5 that it would be desirable to know on which trial a particular setting was made. In this experiment this was accomplished by using different colours. This procedure also allowed us to determine whether a trial began 'near' or 'far'. As mentioned before this increased the length of the experiment and some Ss did not complete all I2 trials - in fact, some Ss did only 6. How many trials each S completed is given in the Appendix. Five Ss did I2, one S did II, one S did 9, two Ss did 8, and three Ss did 6.

Two Ss were unable to make settings when the light was first seen near to them. They saw the light double and it had to be moved back until it 'fused' and then moved forward again. These two Ss both completed I2 trials, all of which started from the back.

**SUBJECTS** I2 Ss were recruited and paid for their participation. 6 were male and 6 female; all were aged betwee I9 and 22 years. None were students of Psychology and they were told nothing concerning the purpose of the experiment. None had served as a S in any previous experiment.

**RESULTS** The individual analyses were carried out in the same manner as in previous experiments. The full individual analyses may be found in the Appendix. Differences between points were assessed by a Duncan Multiple Range test and as before significance was sought beyond the .OI level.

The two Ss who were unable to make settings from the near position were excluded from the overall analysis because their error terms were significantly larger than those of the other Ss (256.6 / II7.2; F=2.2*** for 90 and 420 d.f.).

The individual results all showed no significant differences with the exception of Cripps on the outgoing M-L who had one significant difference ('unclassified').

Of the ten Ss for whom it was possible, four showed a significant effect for 'Position', i.e. starting position, two beyond the .OOI level, one beyond the .OI level and one beyond the .05 level. All four of these Ss set the lights starting from the furthest position on the bar, further away than those starting from the nearest position.

No S showed a significant Points by Position interaction.

The overall analysis was done as for previous experiments.

RESULTS The Ingoing M-L

MEANS (cms.)

| SUBJECTS | LFT. FIN | SHAFT | RT. FIN. | Significant Differences |
|---|---|---|---|---|
| White | 82.I | 84.7 | 75.7 | NONE (Flat) |
| Cook | 76.8 | 76.4 | 76.5 | NONE (Flat) |
| Challenor | 70.5 | 78.8 | 78.3 | NONE (Flat) |
| Cripps | 77.5 | 78.7 | 73.3 | NONE (Flat) |
| Lewis | 89.0 | 91.9 | 94.0 | NONE (Flat) |
| Davis | 71.5 | 73.4 | 73.6 | NONE (Flat) |
| Brett | 89.9 | 78.0 | 85.1 | NONE (Flat) |
| Dicey | 80.4 | 79.5 | 79.3 | NONE (Flat) |
| Stamp | I09.5 | III.8 | II0.7 | NONE (Flat) |
| Bird | 89.7 | 76.7 | 79.3 | NONE (Flat) |
| MEANS | 83.7 | 83.0 | 82.6 | |
| Auffret | 67.5 | 72.I | 64.8 | NONE (Flat) |
| Gussoy | 93.0 | 96.6 | 97.I | NONE (Flat) |
| TOT. MEANS | 83.I | 83.2 | 82.3 | |

**RESULTS** **The Outgoing M-L**

MEANS (cms.)

| SUBJECT | LFT. FIN | SHAFT | RT. FIN. | Significant Differences |
|---|---|---|---|---|
| White | 75.6 | 79.5 | 76.8 | NONE (Flat) |
| Cook | 77.7 | 77.5 | 77.5 | NONE (Flat) |
| Challenor | 78.0 | 86.8 | 80.5 | NONE (Flat) |
| Cripps | 80.8 | 74.6 | 77.4 | Lft > Shft (UNC) |
| Lewis | 86.8 | 85.2 | 76.9 | NONE (Flat) |
| Davis | 74.4 | 75.2 | 74.6 | NONE (Flat) |
| Brett | 84.5 | 83.4 | 84.9 | NONE (Flat) |
| Dicey | 79.7 | 77.8 | 77.7 | NONE (Flat) |
| Stamp | II0.7 | II0.9 | III.2 | NONE (Flat) |
| Bird | 82.4 | 78.4 | 77.0 | NONE (Flat) |
| MEANS | 83.I | 82.9 | 8I.5 | |
| Auffret | 74.I | 79.5 | 70.7 | NONE (Flat) |
| Gussoy | I03.0 | I00.0 | 83.4 | NONE (Flat) |
| TOT. MEANS | 84.0 | 84.I | 80.7 | |

## OVERALL ANALYSIS OF VARIANCE

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | 9 | I339.6 | |
| BLOCKS | I | 587.I | II.7 ** |
| POINTS | 5 | II.2 | 0.4 (N.S.) |
| BLOCKS X Ss | 9 | 50.2 | |
| POINTS X Ss | 45 | 26.0 | |
| POINTS X BLOCKS | 5 | 9.6 | 0.2 (N.S.) |
| POINTS X BLOCKS X Ss | 45 | 59.6 | |

## DUNCAN TEST OF DIFFERENCES BETWEEN MEANS

| MEANS | 8I.5 | 82.6 | 82.9 | 83.0 | 83.I | 83.7 | S. Sig. Range (.05) |
|---|---|---|---|---|---|---|---|
| 8I.5 | - | I.I | I.4 | I.5 | I.6 | 2.2 | 3.2 |
| 82.6 | | - | 0.3 | 0.4 | 0.5 | I.I | 3.4 |
| 82.9 | | | - | 0.I | 0.2 | 0.8 | 3.5 |
| 83.0 | | | | - | 0.I | 0.7 | 3.6 |
| 83.I | | | | | - | 0.6 | 3.6 |

NONE (Flat)

There were two main effects i.e. Points and Position. The significance of the Points and Position effects was assessed by using their respective interactions with Subjects as the error term. The significance of the Points by Position interaction was assessed by using the Points by Position by Subjects interaction as the error term. The Position effect was significant beyond the .01 level. None of the other differences were significant.

In experiment 5 it had been noticed that there was a slight tendency for the shafts of the ingoing and outgoing M-Ls to be set nearest more often than would be expected by chance. This tendency was not present in this experiment. By chance the shafts would be expected to be set as nearest in 4 cases out of 12. For both the ingoing and outgoing M-Ls they were actually set as nearest 3 times out of 12.

The overall mean for all points was 83.1 cms. The actual distance of the figure was 82.0 cms.

**DISCUSSION** In the introduction it was said that the 'monocular convergence' cue was not expected to provide the S with much extra information and that it was expected that the results of this experiment would replicate those of previous experiments. This prediction has not been substantiated. Are we to revise our opinion of the 'monocular convergence' cue, or might there be some other reason for the change?

It would seem that our Ss no longer consider the figures ambiguous. Not only did they perceive them as flat, but they also seem to have correctly perceived their distance. The average distance of all the settings was only I.I cms. further than the actual distance of the figures. If the settings at all six points are averaged for each S, we find that six out of the ten Ss included in the analysis of variance average between 77 and 8I cms. This is much closer grouping than occurred in any of our previous experiments.

This leaves us with two main questions to be answered.

I. What cues enabled the Ss to perceive the situation veridically?

2. Assuming the same cues were operating in Gregory's experiment, why did his Ss see the figures 3-dimensionally and not flat, as did our Ss?

On reflection it seems that extra cues may have come from three other sources, if we discount the effectiveness of 'monocular convergence'. All three of these sources are illegitimate in that the S is not supposed to have them. Firstly, the polaroids could have been faulty and allowed Ss to see ghost images in their non-seeing eyes, as outlined on p. 99. In the pilot study in which this ghosting was noticed, the polaroid used was of an inferior quality. With the very high quality polaroid used in this study it is thought unlikely that any ghosting occurred. The experimenter spent some time testing for this possibility using himself as a S and found no trace of any ghosts at all. However, in the light of our results, it remains a possibility.

A more likely source of cues is thought to be found in the construction of the visor in which the polaroid spectacles are located. It is possible, with small head movements, for a S to look over or under the spectacles and thus gain an unimpeded binocular view of the figure. The S's head is not restrained in any way but if he rests his forehead on the curved piece of plastic provided the S would have to look through the glasses. The S is instructed to do this but, in the dark, it is impossible to see whether he is doing it or not. Gregory makes no mention of using a bite bar or any other form of head restraint and his diagrams of the apparatus (e.g. 1966)[3] indicate that it is exactly similar to ours. Thus if our Ss could have done it, so could his.

It would not be necessary for Ss to 'cheat' in this way all the time to obtain the veridical results we have. One or two glimpses would probably be enough to reveal the figure's true shape and distance. It is thought possible that many Ss might have obtained these glimpses during the course of the experiment.

The third possible source from which Ss might have gained information is from their knowledge of the apparatus. When the S enters the room he sees the apparatus and he might well deduce the distance of the figures from this. This need not occur with the Synoptophore since it is not at all obvious that the figures are viewed through half-silvered mirrors. Clearly the S could not deduce the form of the figures by viewing the apparatus from the outside, but this might add confidence to judgements derived from any glimpses under the glasses that he gets.

It was mentioned that Gregory obtained distance differences of about two cms. and seemed convinced of their significance. From the Duncan Multiple Range test (p. 215) it can be seen that in this experiment differences of between three and four cms. would be necessary even for significance at the .05 level. However, it should be remembered that Gregory's figures were at only 50 cms. while ours were at 82 cms. The greater the distance the less accurate and consistent one would expect settings to be.

This brings me to the second question. Why do our results differ from Gregory's? It is proposed that the difference in the results is due to differences in the set of the Ss. Our Ss were told nothing concerning the purpose of the experiment since our early results seemed to suggest that an expression of scepticism on the probability of 3-dimensional responses by the experimenter seemed to have inhibited Ss from making such responses (see experiments I and 2). Suppose, however, that the experimenter had suggested with conviction that everyone would see the figures in 3-dimensions? It seems possible that such a suggestion might have lead some Ss to structure their perceptions accordingly. Since Gregory obviously holds such a conviction it is thought possible that he might have communicated it, albeit unwittingly, to his Ss. Experiment 7 will be devoted to an attempt to induce Gregory-type results by exposing the Ss to what might be termed pro-Gregory propaganda.

There is a weakness in the above argument in that if our Ss perveived the figure as flat, then why did Gregory's Ss not do the same if they had the same cues? We must assume that their 'set' was strong enough to overcome the information provided by their glimpses under the glasses - if, in fact, this is how they obtained their information. This is thought to be a feasible assumption in that the situation would not be <u>absolutely</u> clear cut. For instance, it would be difficult to induce Ss to see a straight line as curved under full cue conditions (although Asch's experiments, soon to be reported, would suggest that it is possible). In the dark, using luminous figures, there might be a better chance.

All this is, of course, conjecture which experiment 7, it is hoped, will either confirm or dispel.

So far our experiments have shown that when sufficient cues are present for Ss to perceive the figure relatively accurately, or to relate it to their past experience (e.g. the control figures in experiments I and 2) then they all see the figure in the same way. We have called this the 'public structure'. If the number of cues available is not sufficient for this, then each S interprets the figure in his own way - he imposes a 'private structure' on it. This private structure will often vary over time for the same figure. We have shown that Gregory's apparatus allows enough cues of one sort or another to allow Ss to perceive the figures as flat. Experiment 7 is an attempt to change this public structure and to replace it with another. If this is possible then a full explanation of many of the questions this thesis set out to answer might well be possible.

In experiment I it was decided that the starting position of the reference light had no influence on where a setting was made and subsequent experiments ignored this factor. It seems that this judgement was premature for in this experiment the Position effect was significant beyond the .OOI level. Four Ss had a significant effect on the individual analyses and the direction of their trend was reflected in the overall analysis i.e. when the light was first seen at its furthest position, it was set further away than when it was first set at its near position. It should be remembered that this analysis did not contain the two Ss who could not make settings from the near position at all. However, this finding does not require us to amend any of our thoeries. It is comforting to note that the Points by Position interaction was not significant.

All our results have tended to differ from those of Gregory but these would seem to be the most contrary of all. In what is almost an exact replica of Gregory's experimental situation, we have found Ss to see the figures as flat.

## AN INTRODUCTION TO EXPT. 7

## THE EFFECT OF 'SET', INSTRUCTION AND EXPERIMENTER BIAS ON ON EXPERIMENTAL RESULTS

The experimental results so far have led to the hypothesis that, in the absence of suitable cues, Ss tend to interpret our experimental figures in accordance with their own 'private' structures. If certain cues are made available, either in the form of perspective or distance information, these 'private structures' are replaced by 'public structures' common to all Ss. It has also been suggested that a 'public structure' might be imposed by manipulating the Ss' prior knowledge. This experiment is an attempt to test the latter suggestion.

Much has been writen by social psychologists on the concept of 'set' and 'structure', and many of their experiments are directly relevant to the questions being considered here. Their motives, of course, tend to be rather different in that they are usually interested in the effects on the individual of group pressures in ambiguous situations.

Perhaps the closest approximation to our experimental situation was achieved by Sherif (1952), when he made use of the autokinetic illusion. He was seeking an 'objectively unstable' situation in which he could measure individual responses and then compare these with responses obtained in a group situation. He was seeking situations "that would permit themselves to be structured in several ways depending on the character of the subjectively established reference points." He could easily have used our 'no cue' situation to satisfy this definition but the autokinetic situation is easier to set up.

The autokinetic illusion occurs when a S is presented with a small spot of light in an otherwise completely dark room. After some seconds the spot appears to move about in an erratic fashion. The effect is thought to be due to the lack of any other object in the visual field with which a stable relationship can be established - "in a completely dark room a single spot of light cannot be located definitely, because there is nothing in reference to which you can locate it." (Sherif, 1952).

Firstly Sherif exposed his Ss to the effect on their own. They were required to tell the experimenter when the light appeared to move. A few seconds after this the experimenter turned the light off and asked how far it had moved. Ss were asked to make their estimates "as accurate as possible".

The results were summarised by Sherif as follows:-

"The results unequivocally indicate that when individuals perceive movements which lack any other standard of comparison, they subjectively establish a range which is peculiar to the individual, that may differ from the range and point (a standard or norm) established by other individuals." An almost identical summary could be written to describe our results in the 'no cue situation'. The parallel should perhaps not be taken too far, since Sherif's Ss tended to stick to their norms on subsequent occasions whereas our Ss tended to change their 'private structures' from experiment to experiment.

The next part of Sherif's experiment was to put several Ss into the experimental situation at the same time. Ss were asked to tell the experimenter how far the light had moved just as before only now each S would know what estimates the other Ss were making as well. Ss were allowed to inform the experimenter of their estimates in any order they wished. A marked tendency was noticed for 'group norms' to be established, i.e. each individual would be influenced by the other Ss in such a way as to produce like estimates from them all. This occurred even when Ss with completely different norms (as established in the 'lone' situation) were included in the same group.

The implication of this experiment is that in a very uncertain situation a S will be very amenable to suggestion as to how it should be interpreted.

Further evidence on this point comes from the series of experiments conducted by Asch (1956). He used a group situation similar to that of Sherif except that all the 'Ss' but one would be 'confederates' of the experimenter. All the Ss were seated in the same room and were presented with a standard line and three 'experimental' lines. All of these would be appreciably longer or shorter than the standard except for one which would be exactly the same length. The task was to select the line that matched the standard. The 'guinea-pig' S would be manoeuvred into a seat where he would not be asked for his judgement until nearly all the other Ss had given theirs. All the confederate Ss would consistently pick a line that was obviously of different length to the standard. The point of the experiment would be to observe the guinea-pig's reaction.

About one-third of the reports made by the Ss in the Asch situation are not correct but are in accordance with the majority judgement. Ss can be categorised as largely independent or as largely conforming, although most people tend to conform on some trials but be independent on others. In a control situation, i.e. without a group present, Ss almost invariably made the correct choice. Hence all the false results are caused by what might be termed 'a pressure to conform'.

The situation used by Asch was a rather slow way of collecting data and it was modified by Crutchfield (Krech, Crutchfield and Ballackey, 1962) so that several Ss could be tested at the same time. The method was to seat Ss side by side in individual booths. Each booth had a panel with a row of numbered switches which the S used to signal his judgements. Also displayed on the panel were signal lights which indicated the responses of the other Ss. In actual fact the signal lights do not indicate the other Ss' responses but are controlled by the experimenter. Moreover each S is told that he will be the last to respond. The items presented also covered a much wider range and Ss were found to be just as likely to succumb to group pressure when asked if "free speech were a privalege rather than a right", as they were with the Asch lines.

Sherif's experiment shows Ss clustering their responses together when a situation is uncertain. The experiments of Asch and Crutchfield show Ss conforming to group pressure in situations where uncertainty should be non-existent. Ss who'stuck out' against the majority underwent obvious discomfort and Bogdonoff et al. (1961) obtained physiological evidence of arousal in the central nervous system in such situations. For those who yielded to the majority the arousal decreased.

Allport (1962) found that Ss tended to conform in their responses even when they were doing completely independent tasks. He says, "A frame of reference is afforded by a situation of a reacting or an interacting group in which the individual participates as a member, and this frame of reference provides an 'anchorage' for the individuals' participations......Such an anchorage can be assumed to be a kind of collective standard or 'norm' that arises amongst the individuals as a result of their interaction."

Brown (1970) believes that these experiments show "that the impulse to agree is coincident with the creation of interpersonal bonds. Even when the task in no sense requires agreement this is so, seven when agreement means the assertion of manifest falsehoods."

The above is provided as a background against which the coming experiment can be viewed.

It is not suggested that either Gregory or Coren and Festinger attempted to prime their Ss beforehand but it is thought that Ss might well have known about the theories that were being investigated. In Gregory's case it is believed that his Ss were students. If this is so it is quite likely that they would be acquainted with the details of his theory, particularly if he lectured to them. In the light of the experiments just reported, the pressure on the Ss to conform would indeed be formidable, if this were so. However, even if no such obvious connection existed, other, more subtle, cues could well have been picked up and used by the S, unless specific steps are taken to ensure that this does not happen. An example of this would be experiment I in this series. In a situation of great uncertainty even the merest hint might be seized upon as a means of resolving the ambiguity.

It is obviously impossible to recreate the situation exactly as it was for Gregory's experiments. What was attempted was to produce a situation in which prior knowledge would be shown to influence the results in the way predicted. The inference would then be that this could be a possible artifact in previous results rather than a definitive statement on the matter.

Brown is quoted above as believing that the impulse to agree is "coincident with the creation of interpersonal bonds." This was thought to be an important point. In this experiment Ss were given an explanation of Gregory's theories and it was subtly indicated that the experimenter was personally hoping to confirm them, although at face value the instructions were neutral. An experimental situation identical to that of experiment 6 was used so that results from this experiment could be compared with those of the 'uninstructed' Ss of that experiment. Of course, the set up used in experiment 6 was not as uncertain as the 'no cue' situation prevalent in the earlier experiments and hence it would be proportionately more difficult to produce the required 'set'. We would be replacing one public structure by another rather than replacing a private structure with a public structure. However, the experiment 6 set up was chosen because it was most similar to Gregory's.

Our experimental method is intended to influence the S in two ways. Firstly by the formation of a 'group' with the experimenter so that the S will be induced to conform to the experimenter's norm, and secondly by using the experimental instructions to induce an appropriate 'set'. The experiments mentioned so far have all been concerned with establishing the rationale for the first method of influence. It remains to be shown that instructions can be used to produce the effect suggested. Perhaps the best example of this is provided by the work of Carlson (1960, 1962).

Attempts to investigate the size-distance invariance hypothesis had produced conflicting results as to the way distance was judged. Purdy and Gibson (1955) and Tarda (1956) found that Ss tended to overestimate the further half of any distance, while the results of Gilinski (1951) and Smith (1958) suggested the opposite. Carlson maintained that the increasing overestimation was an artifact of the 'objective' instructions and that experiments which used 'look' or 'apparent' instructions did not show this effect. In his own series of experiments Carlson showed that different instructions did produce different results. "(Size-constancy) is the neutral point from which size-judgement starts and substantial deviations from this value can be produced in either direction by variations in experimental conditions." A follow up study by Epstein (1963)[1&2] expanded Carlson's method by using more distances and getting Ss to estimate distance as well as size. The Epstein experiment is summarised below.

Ss viewed binocularly down a lighted corridor in which a standard triangle was placed at either 10, 20, 30, 60 or 120 feet. The S's task was to adjust a variable triangle which was five feet away in accordance with instructions so that it 'matched' the standard. 200 Ss were used and each one made judgements for one distance under one set of instructions only. By using several distances rather than just one (as did Carlson) Epstein hoped to discover how size judgements changed with changes in distance under the various instructions. The instructions given by Epstein were identical to those given by Carlson. Four different types were used. They were as follows:- (Apparent size)

"Adjust the variable so that it looks equal to the standard in apparent visual size. It may also be equal in actual physical size at that point or it may not - we are not concerned about that. Try to adjust the variable so that it appears equal to you visually, whether you think it is equal in actual size or not."

(Objective size):-

"Adjust the variable so that it is, as best you can judge, equal in actual physical size to the standard, so that if you were to measure them both with a ruler they would measure the same size. They may also look equal to you in apparent visual size or they may not - we are not concerned about that. Try to adjust the variable so that it is the same actual size as the standard whether it appears equal to you visually or not."

(Perspective size):-

"As you look down a road or a set of railway tracks, the edges of the road or rails appear to come together in the distance. It is that degree of apparent convergence that we want you to try to duplicate here. Set the near triangle so that if the two triangles were lined up, lines extended from the corners of the near triangle through the corresponding corners of the far triangle would seem to come together in the distance. Make this setting so that the apparent convergence of these lines would be the same as the apparent convergence of railroad tracks or the edges of a sidewalk or road which extends away from you."

Some paraphrasing of this instruction was usually required.

(Projective size):-

The S was instructed to adjust the variable triangle so that its subtended angle would be equal to the angle subtended by the standard triangle. The experimenter demonstrated that the image of a small extent close to the eye can encompass the visual angle of a more distance large object and illustrated the geometry of the problem with pencil sketches. Any statement to the effect that object size should be proportionate to distance was carefully avoided.

Distance judgements were obtained by using a special ruler. On it were two markers. One of these was fixed near the end of the ruler and was used to represent the variable triangle. The other marker was movable and was set by the S so that the relationship between the two markers and the end of the ruler appeared the same as the relationship between the two trangles and himself.

It was found that both size and distance judgements differed significantly according to which instructions were used. It was also found that in no case did the size and distance judgements coincide as was required by the size-distance invariance hypothesis, nor was the relationship between them constant. However, more important from our point of view, is the fact that although the wording is different, the 'objective' and 'perspective' instructions are logically identical. They both call for the same judgement. The same may be said for the 'apparent' and 'projective' instructions. And yet in neither case did the S respond in the same way.

Rosenthal (1963, 1964) has developed in some detail the hypothesis that the experimenter's orientation towards the results of his research may partially determine those results. As Rosenthal says, "In any science, experimenters have some orientation towards the outcome or results of their research. Rarely is this orientation one of truly dispassionate disinterest." (1964). Clearcut examples of experimenter bias are very difficult to pin down but Rosenthal does quote a number of studies which taken as a whole make an impressive body of evidence.

For example, Rice (1929) analysed the responses of applicants for charity. These applicants were interviewed by a group of 12 skilled interviewers and were allocated to an interviewer in a random manner. The applicants were found to ascribe their dependent status to factors predictable from a knowledge of the interviewers' 'outcome orientations'. Thus, one of the interviewers who was a staunch prohibitionist obtained three times as many responses blaming alcohol as did another interviewer regarded as a socialist, who in turn obtained half again as many responses blaming industrial factors as did the prohibitionist. Rice concluded that the outcome orientation or bias of the interviewer was somehow communicated to the applicant who then replied as expected. This is exactly what is being suggested to happen in Gregory's experiment.

Whyte (1943) and Back (1951) have shown how group expectations can affect someone's performance in a game, with those 'expected' to do well almost always doing so. Although this is some way from the Gregory situation, it still suggests that the experimenter can markedly influence expectation and attitude.

Zangwill (1937) has illustrated the importance of attitude induced by instructions in an excellent experiment. Ss were shown a series of ink blots and were asked to say what they looked like. One group was told that the blots in the first series would suggest animals, while those in the next series would suggest mountains. One of the blots was rather more ambiguous than the others and this blot was included in both series. After seeing both series Ss were asked to state if any blot had appeared in both series.

A control group also viewed both series but they were not told that the blots would represent anything. Only 10% of this group failed to pick out the blot that had been shown twice, whereas 63.3% of the 'instructed' group failed.

This experiment shows the strength that an experimenter induced set can acquire. Having designated the blot as an 'animal' in one case, it was not recognised as a 'mountain' when it was shown again.

Mackavey (1970) has done an experiment very similar to this using a Ponzo type figure. 36 Ss were shown the figure and to half it was identified as flat, i.e. it was referred to as a pennent; to the other half it was presented without any instructions. All Ss were required to draw a line within the figure equal in length to a standard line. A significant size-matching error was exhibited by each group with a significantly smaller error being shown by the 'flat' group. A replication using 71 Ss confirmed this result.

Story (1959) and Willems (1967) have produced similar results with F.A.E.s (Figural after effects). Story shows that Ss can be set to see 'I3' as 'B'.

The experiments quoted above all suggest that under certain conditions the experimenter or his instructions can cause a S to respond in one way rather than another. It remains to be shown that this could happen in the situation existing in Gregory's experiment. The next two experiments attempt to test this possibility.

## EXPERIMENT 7 - To Assess The Possible Influence Of 'Set' Induced By Instruction In A Gregory-Type Situation

**INTRODUCTION** The preceding review has mentioned numerous studies in which experimenter induced set has been instrumental in getting a S to produce a certain response. This experiment attempts to 'set' Ss by instruction to produce the sort of results predicted by Gregory in an experimental situation where naive Ss had not produced these results (as in experiment 6).

The inference from these experiments is that the earlier investigators had failed to appreciate the nuances present in their instructions and that these had caused the differences in results. Gibson seems to have anticipated the situation in his "Reply to Prof. Boring" (1952). He states that, "the effect of stimulus reduction on object perception is to substitute for the normal perceptual process of size determination an attitudinal process......So far from being the basis, it is a kind of alternative to ordinary perception."

**APPARATUS** The apparatus was exactly the same as in experiment 6.

**PROCEDURE** Ss were brought into the room and asked to sit down. A drawing of the double M-L figure was made on the blackboard and Ss were asked if they had seen it before. Almost all Ss replied that they had and that "it was something to do with one arrow being longer than the other." The E then delivered the following:-

"The shafts of these two figures are actually the same length although this one (points to outgoing arrow) looks longer. One of the theories which tries to explain this distortion proposes that it is caused by the eye reacting to inappropriate perspective cues. The idea is that these figures are interpreted as if they are 3-dimensional when in fact they are not. If you look up into the corner of the room you will see that the configuration made where the walls and ceiling meet is similar to that represented in 2-dimensional form by this figure (points to the outgoing arrow), whereas the other figure (points to the ingoing arrow) resembles the corner of a building. (The S is now shown the pictures on p. 148 of Eye and Brain). Now if you look at this diagram lower down the blackboard you will see what happens when two objects subtend the same angle at the eye. The object judged to be further is seen as larger. Do you understand that? (This point was clarified if the S expressed any doubts). This is what is thought to happen with these figures. If this figure is interpreted as the inside of a room (points to outgoing figure) then the shaft is seen as further than the ends of the fins. Given that the angle that it subtends at the eye remains constant, which it does, if it is seen as further it is seen as bigger. And vice-versa with the other figure. Do you understand that? (Again further clarification was given if required). Obviously these figures do not appear 3-dimensional to you now because you can see that they are drawn on a flat blackboard, however, the theory predicts that if the background is removed, and you see them without definite cues which tell you that they are flat, then you will not see them as flat but 3-dimensional in the way explained i.e. the shaft of this outgoing arrow will appear further from you than the rest of the figure while the shaft of this ingoing figure will appear nearer than the rest of the figure. What we are trying to do here is to present these figures to you without their background to see if they appear to you in this manner."

After this the procedure was just as for experiment 6, except for a further reminder administered just before the S began his settings. He was told:- "Now remember, if you see the figures in the way the theory suggests, then you should set the light next to the shaft nearer in this case (points to the ingoing M-L) and further in this case (points to the outgoing M-L). You may not see the figures in this way. You may see them as flat or as 3-dimensional in some other way than that suggested by the theory. This does not matter. Bear in mind what I have said to you and just set the lights in the way that seems to you to be the most suitable."

There was one difference in the procedure apart from this. In experiment 6 no record was kept of which trials were made in what order. In this experiment this was achieved by marking the trials on the Dural bars in different coloured inks, of much greater variety than previously.

In this experiment, 9 Ss did IO trials, 2 Ss did 6 and one S did 8.

One S was unable to make settings when the light was first seen as near to him. This S completed IO trials, all of which began with the light in the far position.

**SUBJECTS** I2 Ss were recruited and paid for their participation. 7 were male and 5 female. All were aged between I9 and 22 years. None were students of Psychology and none had served as a S in any previous experiment. They were told nothing concerning the purpose of the experiment except for what is contained in the procedure.

One of the male Ss (Newstubb) was blind in his right eye. The apparatus was adapted so that he could view the figure with his left eye. His results are presented separately.

**RESULTS** The individual analyses were done exactly as in experiment 6 and can be found in the Appendix.

The S who was unable to make settings from the near position (Crawford) was excluded from the overall analysis, as was the case in experiment 6. The one-eyed S was also excluded.

The individual analyses for the ingoing figure showed only one S to have any significant differences. This S produced a pro-Gregory result. For the outgoing figure, 3 Ss had significant differences (including Crawford). Two of these were anti-Gregory and one S was 'unclassified'.

Of the IO Ss included in the overall analysis, 8 showed a significant Position effect, 6 beyond the .OOI level and 2 beyond .05 level. 6 of these made further settings from the far position than the near. For the other two Ss the reverse was true.

No S showed a significant Points by Position interaction.

**RESULTS** **The Ingoing M-L**

MEANS (cms.)

| SUBJECTS | LFT. FIN | SHAFT | RT. FIN | Significant Differences |
|---|---|---|---|---|
| Marvin | 93.2 | 66.8 | 98.9 | Lft & Rt > Shft (PG) |
| Dobson | 79.7 | 77.2 | 79.0 | NONE (Flat) |
| Carter | 66.8 | 75.8 | 62.6 | NONE (Flat) |
| Dunlevy | 8I.2 | 82.9 | 82.0 | NONE (Flat) |
| Jones | I00.7 | 85.2 | 98.5 | NONE (Flat) |
| Brereton | 79.2 | 74.6 | 8I.2 | NONE (Flat) |
| Dean | 89.5 | 88.6 | 87.I | NONE (Flat) |
| McGregor | 88.9 | 82.6 | 84.8 | NONE (Flat) |
| Newman | 74.7 | 68.2 | 72.8 | NONE (Flat) |
| Tighe | 58.0 | 55.6 | 62.3 | NONE (Flat) |
| MEANS | 8I.2 | 75.7 | 80.9 | |
| Crawford | 83.8 | 8I.6 | 79.0 | NONE (Flat) |
| TOT. MEANS | 8I.4 | 76.3 | 80.7 | |
| | | | | |
| Newstubb | I0I.7 | 95.4 | 80.7 | NONE(Flat) |

**RESULTS** **The Outgoing M-L**

**MEANS (cms.)**

| SUBJECT | LFT. FIN | SHAFT | RT. FIN | Significant Differences | |
|---|---|---|---|---|---|
| Marvin | 102.9 | 62.5 | 90.8 | Lft & Rt > Shft | (AG) |
| Dobson | 81.2 | 78.0 | 81.0 | NONE (Flat) | |
| Carter | 72.8 | 73.2 | 58.6 | NONE (Flat) | |
| Dunlevy | 76.7 | 79.1 | 75.5 | NONE (Flat) | |
| Jones | 90.7 | 86.3 | 89.3 | NONE (Flat) | |
| Brereton | 87.8 | 69.1 | 79.8 | Lft > Shft | (UNC) |
| Dean | 88.1 | 82.9 | 87.0 | NONE (Flat) | |
| McGregor | 82.0 | 78.5 | 79.0 | NONE (Flat) | |
| Newman | 75.5 | 68.3 | 72.3 | NONE (Flat) | |
| Tighe | 59.3 | 58.2 | 60.9 | NONE (Flat) | |
| MEANS | 81.7 | 73.6 | 77.4 | | |
| Crawford | 99.3 | 75.3 | 91.1 | Lft & Rt > Shft | (AG) |
| TOT. MEANS | 83.3 | 73.8 | 78.7 | | |
| | | | | | |
| Newstubb | 90.5 | 102.6 | 89.8 | NONE (Flat) | |

OVERALL ANALYSIS OF VARIANCE

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | 9 | 1134.6 | |
| BLOCKS | 1 | 2720.4 | 4.8 (N.S.) |
| POINTS | 5 | 228.9 | 2.9 * |
| BLOCKS X Ss | 9 | 567.6 | |
| POINTS X Ss | 45 | 78.0 | |
| POINTS X BLOCKS | 5 | 71.9 | 0.3 (N.S.) |
| POINTS X BLOCKS X Ss | 45 | 278.4 | |

DUNCAN TEST OF DIFFERENCES BETWEEN MEANS

| MEANS | 73.6 | 75.7 | 77.4 | 80.9 | 81.2 | 81.7 | S. Sig. Range(.05 & .01) | |
|---|---|---|---|---|---|---|---|---|
| 73.6 | - | 2.1 | 3.8 | 7.3* | 7.6** | 8.1** | 5.6 | 7.6 |
| 75.7 | | - | 1.7 | 5.2 | 5.5 | 6.0* | 5.9 | 7.9 |
| 77.4 | | | - | 3.5 | 3.8 | 4.3 | 6.1 | 8.1 |
| 80.9 | | | | - | 0.3 | 0.8 | 6.3 | 8.3 |
| 81.2 | | | | | - | 0.5 | 6.3 | 8.4 |

ORTHOGANAL COMPARISON - SHAFTS vs FINS

F=10.9** for 1 & 45 df.

SHAFTS nearer than FINS

INGOING FIGURE = NONE (Flat)

OUTGOING FIGURE - Lft > Shft

The overall analysis was done exactly as for experiment 6. The Points effect was significant beyond the .05 level. None of the other effects were significant. The Position effect was undoubtedly prevented from attaining significance only by the presence of the two Ss (both significant beyond the .00I level) who set their 'near' settings furthest. With 8 Ss out of IO showing a significant effect it would be wrong to say that Position had no effect.

It was noted that the tendency for the shafts to be set nearer than the fins, which had appeared in earlier experiments, (but not experiment 6) had reappeared. This trend was shown to be significant beyond the .0I level when the fins and the shafts were compared by orthogonal comparisons.

There was no tendency to see the shafts at different distances. No S had this comparison significant in the individual analyses and it was not significant overall.

As in previous experiments, a count was made of the number of times the shaft was set nearest. All Ss, except Newstubb, were included in this count. For the ingoing figure the shaft was nearest 8 times out of II. For the outgoing shaft it was nearest 9 times out of II. By chance one would expect it to be nearest only one-third of the time. The probabilities of these numbers occuring by chance were as follows:-

<u>Ingoing M-L</u>: $II!/8!3! \times (I/3)^8 \times (2/3)^3 = 0.007^{**}$

<u>Outgoing M-L</u>: $II!/9!2! \times (I/3)^9 \times (2/3)^2 = 0.00I^{***}$

In this experiment this same method of analysis could be applied to the individual analyses because we know which reading was taken on which trial. The number of times the shaft appeared nearer than its respective fin-ends was counted for each individual and expressed as a chance probability in the same way as above. Table 4: I lists these probabilities. For both the ingoing and outgoing M-Ls five Ss out of eleven had probabilities of 0.I6 or less of such results as they achieved occuring by chance.

This method would seem to indicate that a large proportion of Ss as individuals are setting the shafts as nearer while overall the trend would appear significant for both figures. This conclusion is somewhat at odds with that indicated by the overall analysis of variance. Although the Points effect is significant here beyond the .05 level the significant differences are not those expected from the above analysis. Considering the ingoing and outgoing M-Ls as separate units the only significant difference is for the outgoing M-L where the left fin-end is set further than the shaft. However, if an orthogonal comparison is made between the two shafts and the four fin-ends the shafts are significantly nearer beyond the .00I level.

The within subject terms of experiments 6 and 7 were very similar, ie II7.2 as against I22.3, and the resultant F of I.04 for 420 and 420 d.f. was not significant.

TABLE 4:1 Number of 'Shaft nearest' results expressed as chance probabilities.

| SUBJECT | INGOING | OUTGOING |
|---|---|---|
| Tighe | 0.I4 | 0.46 |
| Marvin | 0.00002*** | 0.00002*** |
| Dobson | 0.0I6* | 0.0I6* |
| Carter | 0.09 | 0.09 |
| Dunlevy | 0.46 | 0.2 |
| Jones | 0.0I6* | 0.08 |
| Brereton | 0.003** | 0.06 |
| Dean | 0.26 | 0.003** |
| McGregor | 0.I7 | 0.I7 |
| Newman | 0.08 | 0.0I6* |
| Crawford | 0.26 | 0.00002*** |
| Newstubb | 0.0I6* | 0.2 |

DISCUSSION Strictly speaking, the results did not confirm the prediction made in the introduction. It was expected that the 'instruction' these Ss had received would have produced results in agreement with those required by Gregory's theory. This would have meant Ss seeing the shaft of the ingoing figure as nearer than the fin-ends and the shaft of the outgoing figure as further than the fin-ends. In fact, Ss tended to either see the figures as flat or to place both shafts nearer than the fin-ends. This is very similar to the results obtained in the earlier 'no cue' experiments but is markedly different from the results of experiment 6.

Experiments 6 and 7 were identical apart from the 'instrution' given to the Ss, hence it is reasonable to ascribe any differences in the results to this factor. The main difference is the existence of many more 3-dimensional results in this experiment, although they were not quite as predicted. It would seem then, that for about half our Ss, the public structure established in experiment 6 was replaced by a public structure induced by instruction. For the other half, the experiment 6 - type structure endured. Why did our instructions not produce the predicted effect in its entirety?

It is thought possible that the S was given too much information to take in within too short a time, which may have resulted in some confusion. Whether this was the case or not, it would seem that if the S is going to see these figures as 3-dimensional then these results and those of our previous experiments suggest that the shafts will be seen as nearest for both figures. This experiment did not resemble that of Gregory in one respect which might be important (with hindsight). He presented his figures separately rather than both together. It seems possible that if we had done this, giving the appropriate instruction before each figure, we would have achieved results exactly as predicted.

Be this as it may, these results do show that Ss' perception can be affected in the way suggested and unless specific steps are taken to prevent the influence of these factors, they remain a possible cause of any 3-dimensional effects achieved in this sort of situation.

Apart from these differences, the results of the two experiments are practically identical. The overall means for Points are very similar - 83.I cms. in experiment 6, 79.0 cms. here. The Position effect was significant in experiment 6 but not here. The reason for this is that, although a comparable number of Ss showed the effect in both experiments on an individual basis (including those who could not make settings from the near position) i.e. 6 out of I2 against 9 out of II, two of those in this experiment were in the opposite direction to the others.

The results of the one-eyed S are most interesting. He was not included in the overall analysis because we cannot be completely sure that his disability would not affect his performance, e.g. he would be unable to use stereoscopic vision to set the lights. As it turned out his results were very similar to those of the other Ss except in one respect. Although the differences were not significant in either case, he was the only S to show the reversal effect which Gregory's theory predicts. He saw the shaft of the ingoing figure as nearer than the fin-ends but saw the shaft of the outgoing figure as further. If all Ss had shown the same trend our results would have confirmed the predictions exactly. Why should this S have differed from the others? The S was very concerned that his disability might prevent his doing the experiment, and he was obviously far less confident of his ability to judge distances than the others. Thus the situation was one of greater uncertainty for him than for the other Ss, hence one would expect him to be more open to the kind of pressures applied.

It was decided that since our predictions had not been confirmed in full, another experiment would be done using a simpler situation in which the demands made on the S, in terms of information contained in the instructions would be less severe.

## EXPERIMENT 8 - Instructed Versus Uninstructed Ss On The Ponzo Illusion

INTRODUCTION Experiment 7 attempted to influence the way in which Ss perceived M-L figures by giving them certain instructions. An effect was obtained but it was not as clear cut as had been hoped. It was thought that more success might have been achieved if Ss had not been given quite so much information to assimilate at once. It was felt that the S might have been confused and unsure of exactly what was expected of him.

It might be significant that Gregory never required his Ss to distance more than two points on any one figure i.e the shaft and one fin-end of a M-L, the two horizontal lines of the Ponzo.

In his Ponzo experiment, Gregory found that the depth distances between the horizontal lines closely matched the illusory distortion. The upper, apparently longer line, was always seen as further than the lower, apparently shorter line. It is this illusion, more than any other, that most obviously suggests a perspective explanation, and it is sometimes referred to as the 'railway line' illusion. Indeed Gregory has published photographs (Fig. 4:3) in which two horizontal bars of equal length are superimposed on a set of railway lines receding into the distance. The illusory effect is illustrated most dramatically and the upper line certainly appears further away than the lower line. Since the converging lines of the conventional Ponzo produce the same effect as a photograph of receding railway lines, and since the latter seems obviously to be a perspective induced effect, it seems a short step to assume that the former is also perspective induced.

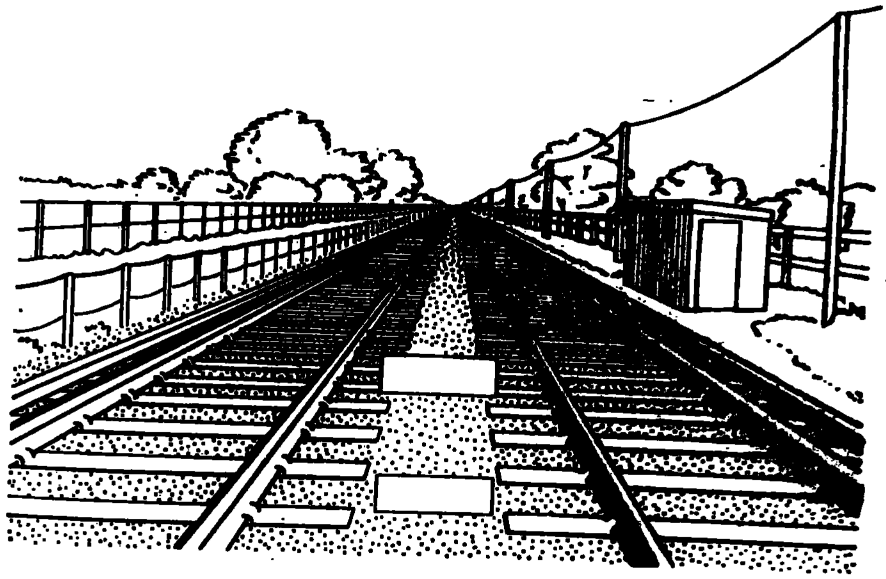


Fig. 4.3 - The 'railway lines' illusion.

However, the results of experiments 1 and 2 reported in this thesis failed to confirm those of Gregory. It was decided to use two groups of Ss, one group would be 'naive' while the other group would receive instruction on Gregory's theory and would be shown the railway lines picture. It was thought that Ss would more easily grasp what response the theory required in this case than in the case of the M-L. It was predicted that the 'instructed' group would provide results similar to those of Gregory, while the 'naive' group would provide results similar to those of experiments 1 and 2.

<u>APPARATUS</u> The apparatus was exactly as for experiment 7 except for the figures used.

The naive group saw only the conventional Ponzo figure (Fig. 4:4). The 'instructed' group also saw the conventional Ponzo, but before they did so they were asked to view the railway lines figure (Fig. 4:3). Both figures were presented in the Pandora's Box module in the same way as in previous experiments.

Two lights only were required, instead of the six used in experiment 7. The other four switches were dismantled.

<u>PROCEDURE</u> The procedure for control group Ss was as follows. They were admitted to the lighted room and asked to sit down in front of the eye-piece. They were then asked to look into the visor. The room light was turned off and the E said the following.....

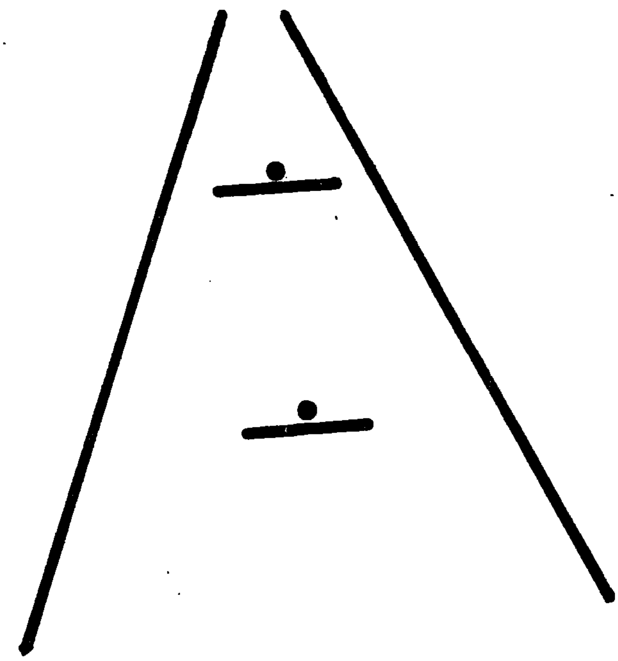


Fig. 4.4 - The Ponzo. Note the different orientation from Fig. 2.3.

"You will now be able to see a figure comprising two lines joining each other at an angle and two horizontal lines. You will also be able to see two small lights, one just above the lower horizontal line. You can move these lights by pressing a switch on t the desk, here. (The experimenter then brought one of the lights to its nearest position). The light I have just moved should now appear to be closer to you than the figure, is that so? The other light probably appears to be further away than the figure, is that so? (The other light would be at its furthest position). Your task is to set these lights to the same apparent distance as the horizontal lines they are nearest to. If one of the lights appears too near and the other too far, there must be some distance in between at which the lights will appear at the same distance away as the horizontal lines. I just want you to set the top light at the same distance as the top horizontal line and the lower light at the same distance as the lower line. Do you understand? (Further explanation was given if neccessary, which was not often). Here are the switches - push them to send the lights further away, pull them to bring them closer."

The S then operated the switches. To start with both lights were set in an extreme position (as far or as near as they would go). When the S was satisfied that both lights were properly positioned the cloth drapeswas dropped so that his view of the lights was obscured. The experimenter then marked the settings with a felt-tipped pen using a small torch and returned the lights to the opposite end of the bar to that at which the preceding trial had begun. The cloth drape was raised and the S told to continue. Before the S left he was asked which line looked longest.

The procedure for the experimental group differed in several respects. Instead of seeing the normal Ponzo illusion when they first looked into Pandora's Box they saw the railway lines figure illustrated. The experimenter's instructions began....."You will now be able to see a picture of railway lines receding into the distance with two cut out bars between the tracks. Which bar looks longer? You will also be able to see two small lights...." From this point on the instructions were the same as for the control group.

The S made four settings for each bar on this figure and was then asked to leave the room while these responses were measured, the runners cleaned and the ordinary Ponzo figure substituted for the railway lines figure. The S then re-entered the room and took up his seat. The room light was extinguished and the experimenter said, "You should now be able to see a figure similar to the one you have just seen in many ways. It comprises two lines joining each other and two horizontal lines. The two small lights should appear in the same positions relative to the horizontal lines as they did to the bars in the other figure. Is that so? I would like you to adjust the lights to the same apparent distance as the horizontal lines in the same manner as you did before i.e. the top light to the same distance as the top line and vice-versa. The purpose of this experiment is to see if you see the two figures in the same way."

All Ss completed I2 trials on each line except for one S in the 'uninstructed' group who, by an oversight, only completed I0. As in the previous experiment, a record was kept of which settings were made on which trial so that the differences in distance between any two lights viewed at the same time was known.

SUBJECTS Eight Ss were used in the 'uninstructed' group and IO different ones in the 'instructed' group. All were female and aged between I8 and 20 years. All were paid a small fee and none were students of Psychology.

The experiment took about 30 minutes altogether.

RESULTS The results were analysed in a slightly different way from before. An analysis of variance was performed on the individual data and overall in a similar way to that used in previous experiments. However, knowing which 'pairs' of readings went together allowed the use of a more comprehensive analysis and the format described in Winer (I966, p.302) as a 'two factor analysis with repeated measures' was adapted for our data.

For the 'instructed' group individual results, seven Ss saw the upper line as further than the lower line, with two Ss seeing them as equidistant. The tenth S, Liddle, can also be counted as seeing the upper line as furthest although no data was obtained. All her settings for the upper line were as far away as the light would go and all her settings for the lower line were as near as the light would go.

For the 'uninstructed' group (individual results) there were six Ss who saw the upper line as further away than the lower line, two who had no significant differences and one who saw the lower line as further.

The overall analyses for both groups were very similar. Both showed no significant difference but in both cases the Points effect was nearly significant at the .05 level. A significant Points effect would have meant that the lines were set at different distances. The prediction made in the introduction was that the two groups would differ from each other and this was tested by a t test for uncorrelated means.

A single score was obtained for each S by subtracting his upper line mean from his lower line mean. T was found to be significant beyond the .01 level.

It is also possible to compare these two sets of scores with zero. A significant t would then indicate that the upper and lower lines were set at different distances, i.e. exactly what a significant F for the Points effect would indicate in the overall analyses. By using a one-tailed test for Ss in the instructed group, on the grounds that a difference was predicted, a significant result was obtained (.05). The two-tailed test for the uninstructed group was also marginally significant beyond the .05 level.

Ss in the instructed group were required to make four settings on the 'railway line' figure. All Ss but one consistently set the upper line as further than the lower, as expected.

RESULTS Uninstructed Group

MEANS (cms.)

| SUBJECT | APEX | BASE | Significant Differences |
| --- | --- | --- | --- |
| Brock | 47.6 | 66.5 | Base > Apex |
| Taylor | 80.2 | 80.0 | Not Sig. |
| Emmerson | 74.2 | 84.2 | Base > Apex |
| Lyon | 73.I | 72.8 | Not Sig. |
| Price | 50.5 | 54.8 | Not Sig. |
| Dixon | 67.3 | I04.6 | Base > Apex |
| Dunn | 73.I | 88.4 | Not Sig. |
| Kettley | 95.0 | 92.7 | Not Sig. |
| MEANS | 70.I | 80.5 | |

Instructed Group

| | | | |
| --- | --- | --- | --- |
| Brothers | 97.8 | 90.7 | Not Sig. |
| Bird | 78.3 | 75.7 | Not Sig. |
| Morton | 88.3 | 72.I | Apex > Base |
| Franks | 66.5 | 60.4 | Apex > Base |
| Thunder | 69.0 | 65.4 | Apex > Base |
| May | 7I.3 | 78.2 | Base > Apex |
| Darling | 7I.0 | 52.5 | Apex > Base |
| Chips | 66.3 | 62.7 | Apex > Base |
| Hunt | 95.8 | 90.2 | Apex > Base |
| MEANS | 78.3 | 72.0 | |

Also Liddle Apex > Base (no data obtainable - see text)

## OVERALL ANALYSES OF VARIANCE

### The Uninstructed Group

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | 7 | 31012.9 | |
| BLOCKS | 1 | 3872.0 | 1.0 (N.S.) |
| POINTS | 1 | 28680.3 | 4.7 (N.S.) |
| BLOCKS X Ss | 7 | 3816.8 | |
| POINTS X Ss | 7 | 6157.6 | |
| POINTS X BLOCKS | 1 | 3784.2 | 0.3 (N.S.) |
| POINTS X BLOCKS X Ss | 7 | 12831.6 | |

### The Instructed Group

| | D.F. | MEAN SQS. | F |
|---|---|---|---|
| SUBJECTS | 8 | 27079.8 | |
| BLOCKS | 1 | 576.0 | 0.0 (N.S.) |
| POINTS | 1 | 20164.0 | 4.6 (N.S.) |
| BLOCKS X Ss | 8 | 20003.3 | |
| POINTS X Ss | 8 | 4419.9 | |
| POINTS X BLOCKS | 1 | 44.0 | 0.0 (N.S.) |
| POINTS BLOCKS X Ss | 8 | 6859.6 | |

## OVERALL T-TEST

| | Instr. | Uninstr. |
|---|---|---|
| | 3.6 | -10.0 |
| | -6.9 | 0.3 |
| | 17.5 | -18.9 |
| | 5.6 | 0.2 |
| | 7.1 | -4.3 |
| | 2.6 | -37.3 |
| | 16.2 | -15.3 |
| | 6.1 | 2.3 |
| | 3.6 | |
| $\bar{X}$ = | 6.1 | -10.4 |
| $\Sigma X$ = | 55.4 | -83.0 |
| $\Sigma X^2$ = | 768.0 | 2106.5 |
| $\Sigma X^2 - (\Sigma X)^2/n$ = | 427.0 | 1245.4 |

$S_{\bar{x}_1-\bar{x}_2} = 5.13$ $\quad\quad$ $t = 6.1-(-)10.4/5.13 = $ 3.22** for 16 df.

## TESTING GROUPS AGAINST ZERO

**Instr.** $\quad s = 7.3 \quad\quad s\bar{x} = 2.57$

$t = \bar{X}-M/s\bar{x} = 6.1/2.57 = $ 2.37* fot 8 df.

**Uninstr.** $\quad s = 13.34 \quad\quad s\bar{x} = 5.03$

$t = \bar{X}-M/s\bar{x} = 10.4/5.03 = $ 2.07* for 7 df on a one tail test.

DISCUSSION The main prediction for this experiment was that the instructed and uninstructed groups would differ. This prediction was fulfilled, and therefore our instructions can be said to have changed the Ss' relative positioning of the lines.

The individual analyses were interesting. Three Ss in the uninstructed group saw the lower line as further while the others saw them as equidistant. Seeing the lower line as further is the expected result if the upper line actually was longer i.e. Ss using visual angle in the manner suggested by Epstein and Landauer (1969) and others. However, if this is so why did the Ss in previous experiments not respond in a similar way to the phenomenal size differences between the ingoing and outgoing M-L figures? Also we may ask why similar results were not obtained from the Ss of experiment 2 who viewed a Ponzo figure similar to the one used in this experiment.

A possible answer to these questions is suggested by an effect reported by Bugelski (1967). He found that if the two lights were placed one above the other at eye-level and at the same distance and were viewed in darkness, the upper light appeared closer i.e. it had to be moved further away for it to appear at the same distance as the lower light. The separation between the lights had to be about 2 degrees for an optimum effect. In this experiment the two lights used were directly above each other and at the overall mean setting distance of 75 cms. the distance between them was exactly 3 cms., this giving a visual angle of 2.25 degrees.

Bugelski found a difference in the distances at which the lights had to be set to appear equidistant of about 5 inches when the lights were at 20 feet. A comparable effect in our experiment would produce a difference of some 2 cms. which is quite a lot less than the observed overall difference of 10.4 cms. However, it is strongly suspected that Bugelski's effect was contributory to our results. It should be remembered that the Ponzo used in experiment 2 was presented on its side and therefore would not be expected to produce Bugelski's effect since the reference lights were not one above the other. The F for the Points effect in the overall analysis in this experiment was very small.

There is another reason for suspecting that Bugelski's effect may have been responsible for the observed difference from equidistance. The results of experiment 6 suggest that with this apparatus Ss would see almost any figure as flat. It was suggested that this may be because they were using 'illicit' cues. We have already described Bugelski's effect as resulting in the upper light having to be moved further away for it to appear at the same distance as the lower one. Apart from Bugelski's effect the experimenter can think of no other reason why the results of the uninstructed group in this experiment should not have been as 'flat' as those of experiment 7.

The results for the instructed group found six Ss who saw the upper line as furthest i.e. as predicted by Gregory's railway line hypothesis. Two of the other Ss set the lines at the same distance while the other S saw the upper line as nearer. One of the two 'equidistant' Ss was the only S not to experience the illusion when viewing the railway line figure under reduction conditions.

Our findings seem to indicate that results of the sort published by Gregory can only be obtained if Ss are influenced in some way. Left to themselves our Ss set the lines at the same distance or exhibited a tendency to set them in the opposite direction to that predicted by Gregory.

Our results conflict somewhat with those of Gogel and Epstein and Landauer. Their results indicate that Ss use visual angle as a basis for judging distance. Pike and Stacey found their Ss to react to the shaft of an outgoing M-L as if it really did subtend a larger visual angle than an ingoing shaft of similar length i.e. they set the outgoing shaft nearer. Our Ss showed virtually no tendency to respond to phenomenal differences in this way and it is difficult to pin down any differences between the experiments which might be responsible. However, these investigators noted that not every S responded in the same way. For some reason the balance seems to have been shifted in our experiments. Whereas the majority of Ss in Pike and Stacey's experiments see the apparently longer line as nearer with a minority seeing them as equidistant, the majority of Ss in our situation see the lines as equidistant with only a few seeing the apparently longer line as nearer.

In a later experiment to the one already described, Stacey and Pike (I968) found II out of I9 Ss to respond in the way they expected to an outgoing M-L while I4 out of I9 did to an ingoing M-L. In a follow up study (I970) they report similar results using figures in which there is no possibility of overall length being a contaminating factor.

Although the differences are puzzling it is as well to bear in mind the remarks of Ogle (1967):-

"Anyone who is aware how pliable our spatial visual perceptions are under the influence of various conditions of observation and under the influence of past experience, taken into account consciously or unconsciously, should not be surprised at the multiplicity of results of observations on different objects and with different observers."

Our results would seem to indicate that the inverted V does not induce perspective at all under normal conditions i.e. that the Ponzo is not a perspective illusion. This might well be too extreme a conclusion. An interesting experiment by Leibowitz, Brislin, Perlmutter and Hennessy (1969) is relevant to this point.

They found that the conventional Ponzo 'typically' produced an illusion of about 10%, but this effect was doubled if the lines were superimposed on a photograph of a field i.e. a photograph containing many perspective cues but no converging lines. The addition of converging lines (a railway line photograph) produced an illusion of 30%. Twenty observers from Guam also did the experiment and although they too experienced a 10% illusion with the conventional figure they did not get any increase in illusion with the photographs. Leibowitz et al. conclude:-

"Monocular depth cues which are hypothesised to be operative in the Ponzo illusion and in size-constancy are acquired through experience. If this is so differential experience with such cues should influences magnitudes of the illusion. The Guam students were of the same age and educational level as the (other) subjects, but they had spent their entire lives on the island of Guam where the terrain is markedly different.....There are no railroads on the island, vistas on land are short due to hilly terrain covered by tropical plant growth and such individuals do not normally view the kind of environments typified by the photographs used in this study." However, if the Guam islanders cannot use the perspective present in the photographs, surely one would not expect them to use any perspective cues present with the conventional figure?

These results would seem to indicate that the Ponzo is not a perspective illusion, although this is not the conclusion of Leibowitz et al. Further discussion of this topic will be found in experiment 9.

With regard to the effects of instruction and set perhaps our findings are best summed up by this quote from Carlson (1962):-

"Purported effects of motivational or personological factors on (perception).....would be more adequately formulated as judgemental response biases than as effects upon the perceptual process itself."

# PART 5 - VISUAL ACUITY AND PHENOMENAL LENGTH DIFFERENCES

## EXPERIMENT 9 - Visual Acuity In Relation To The Ponzo Illusion

**INTRODUCTION** In the Ponzo figure the two horizontal lines are equal in length and yet the top line appears to be longer than the lower line (Fig. 4:4). The magnitude of this illusion is usually in the region of 10%. Fisher (1967) attempted to find out whether performance in a visual detection task was determined by retinal or phenomenal characteristics of a figure, i.e. normally the larger an object is the easier it is to detect, therefore one might expect the upper line in the Ponzo illusion to be detected more easily. However, visual acuity, as Fisher says, "is usually specified in terms of the threshold of angular resolution of the eye with respect to a given spatial pattern," - if this is so, there should be no difference in the ease with which the two lines are detected since they both subtend the same retinal angle.

This is an issue which might be used as a test as to whether or not the Ponzo is a perspective illusion. The results of our previous experiments 1, 2 and 8, suggest that it is not.

The perspective explanation is that the upper line appears longer because our size-constancy mechanism works inappropriately due to false assumptions concerning distance. Two objects subtending the same retinal angle should be seen equally well no matter what our assumptions as to their distance. Thus the perspective explanation would predict no difference in the detectability of the two lines.

If the upper line looks longer because some kind of neural inhibition has occurred, then one would expect it to be more easily detectable. Should this be the case, the perspective theory is not invalidated but it would need further assumptions adding to explain the results.

Bartley (I94I) has shown that the emergence of observable contour as a function of brightness difference between two adjacent visual fields depends upon the proximity, orientation and sharpness of other contours present in the total visual display. Since this discovery, many theories have been advanced based on inhibition and facilitation of some sort, e.g. Ganz (I964, I966), Pollack and Chaplin (I964), Kohler and Wallach (I944). Should such a theory prove viable, as seems likely, then the need to make assumptions involving constancy and perspective is removed.

The design of Fisher's experiment left much to be desired, however, so it was decided to repeat it in a modified form with adequate controls. Fisher's idea was to divide both of the horizontal lines into five sections each. He prepared eleven stimulus cards each with the converging lines drawn in ("an angular bracket of 60 degrees") and added one section of horizontal line to each card, except the eleventh which remained 'empty'. These completed figures were then presented tachistoscopically for I/I25 secs. and the subject's task was to say whether or not a section (or 'block' as Fisher called each section) was present. Each of the ten 'block' cards was presented four times and the 'empty' card was presented forty times, giving 80 trials in all for each subject. 33 subjects were used and presentation was randomised, the order being different for each subject. The results showed that a correct detection was more likely if the block was part of the top line, i.e. the 'longest' line. Fisher summed up that, "it appears justifiable to conclude that the apparent features of illusory spatial patterns rather than the actual physical characteristics determine the probability with which near-threshold stimuli can be detected.

Unfortunately, there were a number of artifacts inherent in the design which might have given these results. Firstly, there should have been a fixation point midway between the two horizontal lines. In Fisher's study we must assume that the subject was free to look where he pleased since we are told nothing to the contrary. There are two reasons why this might have resulted in the subject finding the upper line blocks easier to detect:-

I. If we assume that a subject looking through a tachistoscope stares at the centre of the screen, and also that the figure Fisher illustrates (Fig.2, p.554) is typical of those used in the experiment, then the subject would be staring at a point fractionally below the upper line blocks (Fig.5:I) since this would be the centre of Fisher's figure. This would mean that the upper line blocks would be nearer the fovea than the lower line blocks and consequently easier to detect.

2. The orientation of the figure seems always to have been the same, hence the subject always knew where the apex of the angle would be. It is a well-known artistic device to use converging lines to 'lead' the eye towards a picture's'centre of interest'. This phenomenon might have induced subjects to look higher up the screen than the centre and once again this would result in the upper line blocks being detected more easily.

The design set out below avoids these pitfalls.

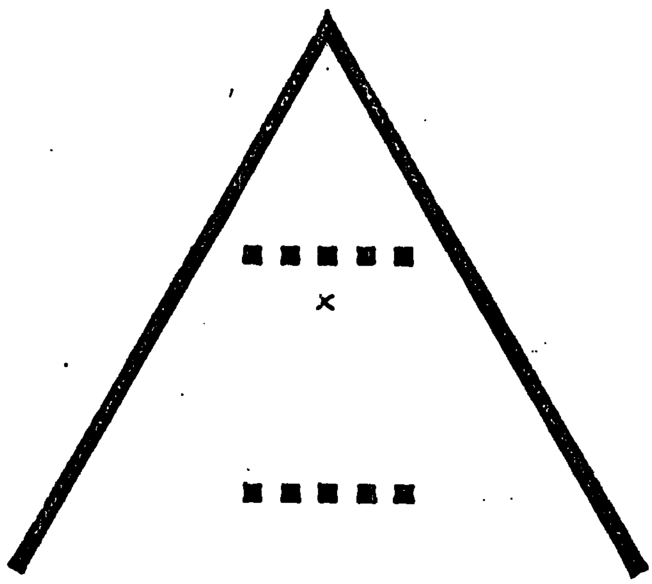


Fig. 5.1 - Fisher's (1967 ) experimental figure with its centre marked. Note its proximity to the 'Apex' blocks.

**APPARATUS** Fisher's eleven figures were modified so that their centre points lay exactly between the two rows of blocks. This was done quite easily by lengthening the converging lines by the appropriate amount. The figures were made by carefully sticking black adhesive tape I/I6" wide onto sheets of plate glass IO" x 8" so that when the glass sheets were stacked one on top of the other, the 60 degree angles and the centre points were superimposed. A randomised sequence was drawn up in which the 'empty' figure appeared ten times and each of the ten block figures was presented four times. Each of the ten block figures was presented twice the right way up ( $\Lambda$ ) and twice inverted ( V ). The 'empty' figure was presented five times in each orientation. Thus the subject never knew where to look for the apex of the figure. Once this sequence was decided the figures were photographed in the appropriate order and a loop film prepared. Hence the order was the same for each subject. The film was shown four times to each S, making 400 trials in all.

A sort of box was constructed for the subject to sit in with a screen built into one side. The screen was circular with a radius of 2' and was made of two layers of grease-proof paper. The subject sat about 2' behind the screen, while the projector was 5' in front of it. This arrangement had the advantage of isolating the subject from the experimenter while also cutting off any stray light from the projector. This same stray light enabled the experimenter to see to write down the results since this was the only light source in the room. In the centre of the screen was painted a luminous dot which served as a fixation point. The subject was asked to concentrate on this throughout the experiment. This fixation point was kept at eye-level for all subjects by adjusting the height of the chair on which they sat.

A camera shutter was fitted over the projector lens and this provided the means for controlling both exposure time and figure illumination. The loop film was moved through the projector, one frame at a time and each frame was exposed on the screen by firing the camera shutter. The projector was carefully set so that the centre point of each figure was exactly superimposed over the fixation point on the screen and this was carefully checked before and after each session.

<u>SUBJECTS</u> Ten subjects were used, all of whom were either students or university technicians, with an average age of 20 years. Two were female. All were paid for their assistance. None were psychologists.

<u>PROCEDURE</u> In Fisher's experiment the subject had to state whether or not a block had appeared. In this experiment he had to say <u>where</u> it appeared. A response of either 'above' or 'below' was required for each of the 400 trials and he was told to guess if he was unsure. This means, of course, that the subject had to respond even when the figure was 'empty'. All subjects were told, in fact, that there would be a block in every figure and such was the speed of exposure and illumination that no subject reported that a block was not present, although few were surprised when told at the end of the experiment that ten of the figures had been empty.

The purpose of the 'empty' trials was to see if the subject exhibited any 'preference guessing.' There are two directions in which a bias might be expected. In an 'empty' trial the subject might decide that the block is more likely to be in the apex of the angle since the contour density is highest in that region i.e. it looks more 'filled' than the rest of the figure. On the other hand, there might also be a tendency to respond 'below' irrespective of the orientation of the figure on the grounds that our everyday experience will have taught us that we are more likely to find any object that we are looking for on the ground, i.e. below eye-level, rather than in the air.

It should be remembered that this design requires that the subject makes some errors, but not so many as to indicate that he is operating at the chance level. To achieve this preferred error level the figure was exposed for I/IOO secs. for all subjects, but the level of illumination was adjusted for each individual. Subjects were allowed about ten minutes to dark adapt and were then given a number of preliminary trials on the basis of which the illumination level was set. At first the level was very high and the position of the block was easily distinguished, but progressively it was reduced until mistakes were made. When the level was such that the subject was consistently making about one error every four responses the experiment proper was begun. Once the experiment had started the level of illumination was not altered again. Usually one run through the loop film was enough to determine what the setting should be. However, it should be noted that during these preliminary trials the subject was not shown any 'empty' figures -

these were shown only during the experiment itself. Fisher simply states that his figures were exposed for I/I25 secs. However, with the apparatus used here, had the level of illumination been kept constant, individual differences would have been such that some subjects would have made no errors at all while others would have been responding on the chance level.

At no time was the subject given any knowledge of results. Before the start of each trial the experimenter said, "ready" and then triggered the camera shutter. The subject was encouraged to respond fairly quickly. Trials took place at a rate of about one every 7-IO secs. The actual order in which the figures were presented is given in the Appendix along with the raw data (p.308-311).

<u>RESULTS</u> It is assumed that when a subject does not see the block in a trial where a block is, in fact, present, he will exhibit the same guessing behaviour as he shows when he responds in trials where no block was present at all, i.e. the 'empty' trials. Now correct guesses may be regarded as artificially inflating a subject's score. The data from the 'empty' trials shows that subject's guesses favoured the apex of the angle 69% of the time. Therefore he will make more correct 'apex' guesses than 'base' guesses and the apex score will then be artificially inflated as compared with the base score. Some sort of correction is needed for this bias if we are to discover the true picture. The data also shows a bias to guess 'below' rather than 'above' (62.5% - 37.5%), but this can be ignored since both the apex and base of the figure appear in

the below position the same number of times - hence the bias will affect both equally. Incidentally, the above-below results provide reassuring evidence that the assumption was justified that a subject exhibits the same guessing behaviour when a block is present as when one is not - subjects responded 'below' to the same proportion of 'empty' trials as they did to 'block' trials (62.5% to 62.3%), see Table 5:2.

Taken at face value the results in Table 5:I indicate that the position of a block appearing in the apex of the angle is correctly ascertained 90% of the time, while the position of the block appearing in the lower part (base) of the figure was correctly ascertained only 58% of the time. However, as mentioned above, these figures do not give a true picture of the relative detectability of the two sets of blocks because of the bias to guess 'Apex' when a subject is unsure. We correct for this by applying the following formula:-

$$\text{Pcor (Ap/Ap)} = \frac{\text{P (Ap/Ap)} - \text{P (Ap/E)}}{\text{I} - \text{P (Ap/E)}}$$

where,

Pcor (Ap/Ap) = Corrected probability of S correctly identifying an Apex trial.

P (Ap/Ap) = Probability of S correctly identifying an Apex trial.

P (Ap/E) = Probability of S responding Apex to an Empty trial.

From Table 5:I, we get:-

P (Ap/Ap) = 0.89

P (Ap/E) = 0.6925

Therefore:-

$$\text{Pcor (Ap/Ap)} = \frac{0.89 - 0.6925}{\text{I} - 0.6925} = 0.639$$

TABLE 5:I

| Ss | No. Apex Responses(%) | No. Base Responses(%) | %Apex Correct | %Base Correct | EMPTY TRIALS %Apex | %Base |
|---|---|---|---|---|---|---|
| I | 72.5 | 27.5 | 92.5 | 47.5 | 87.5 | I2.5 |
| 2 | 65.0 | 35.0 | 96.2 | 67.3 | 87.5 | I2.5 |
| 3 | 55.0 | 45.0 | 87.5 | 77.5 | 42.5 | 57.5 |
| 4 | 69.4 | 30.6 | 90.0 | 5I.2 | 75.0 | 25.0 |
| 5 | 60.6 | 39.4 | 8I.2 | 60.0 | 50.0 | 50.0 |
| 6 | 8I.9 | I8.I | 92.5 | 28.7 | 80.0 | 20.0 |
| 7 | 66.2 | 33.8 | 93.7 | 6I.2 | 80.0 | 20.0 |
| 8 | 69.4 | 30.6 | 93.7 | 55.0 | 77.5 | 22.5 |
| 9 | 68.5 | 3I.5 | 82.5 | 46.2 | 75.0 | 25.0 |
| IO | 45.6 | 54.4 | 78.7 | 87.5 | 37.5 | 62.5 |
| MEANS | 65.4 | 34.6 | 89.9 | 58.2 | 69.3 | 30.7 |

TABLE 5:2

| Ss | No. Above Responses(%) | No. Below Responses(%) | %Above Correct | %Below Correct | EMPTY TRIALS %Above | %Below |
|---|---|---|---|---|---|---|
| I | 25.6 | 74.4 | 42.5 | 97.5 | 47.5 | 52.5 |
| 2 | 36.2 | 63.8 | 67.5 | 95.0 | 47.5 | 52.5 |
| 3 | 38.7 | 6I.3 | 7I.2 | 93.7 | 57.5 | 42.5 |
| 4 | 40.6 | 59.4 | 6I.2 | 80.0 | 45.0 | 55.0 |
| 5 | 39.4 | 60.6 | 60.0 | 8I.2 | 25.0 | 75.0 |
| 6 | 36.9 | 63.I | 37.5 | 73.7 | 30.0 | 70.0 |
| 7 | 42.5 | 57.5 | 7I.2 | 83.7 | 32.5 | 67.5 |
| 8 | 45.6 | 54.4 | 70.0 | 78.7 | 32.5 | 67.5 |
| 9 | 25.6 | 74.4 | 40.0 | 88.7 | 25.0 | 75.0 |
| IO | 45.6 | 54.4 | 78.7 | 87.5 | 32.5 | 67.5 |
| MEANS | 37.7 | 62.3 | 60.0 | 86.0 | 37.5 | 62.5 |

Therefore, we may say that in the experiment subjects correctly discriminated the position of the Apex blocks in 63.9% of the trials.

Adapting the formula for base trials, we get:-

$$\text{Pcor (Ba/Ba)} = \frac{\text{P (Ba/Ba)} - \text{P (Ba/E)}}{\text{I} - \text{P (Ba/E)}}$$

From Table 5:I, we get,

$$\text{P (Ba/Ba)} = 0.582$$

$$\text{P (Ba/E)} = 0.3075$$

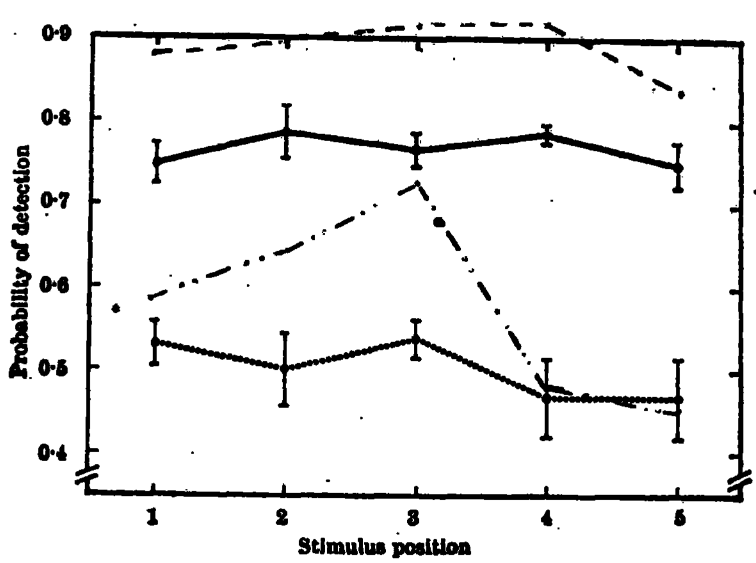
Therefore:-

$$\text{Pcor (Ba/Ba)} = \frac{0.582 - 0.3075}{\text{I} - 0.3075} = 0.396$$

Therefore we may say that in the experiment subjects correctly discriminated the position of the Base blocks in 39.6% of the trials.

Fisher did not give any definite figures for the probabilities of detecting his stimuli, instead he published a graph showing the probability of detecting each individual block (Fig. 5:2). In order to compare our data with his, a similar graph has been prepared. Table 5:3 shows the probability of a correct detection for each block. These, of course, need correcting for the 'apex' guessing bias. This correction is effected by multiplying the apex blocks by 0.639/0.89 = 0.718, and the base blocks by 0.396/0.582 = 0.68.

The graph (Fig. 5:2) shows that the results of the present study largely confirm those obtained by Fisher. The fact that the actual probabilities of detection differ is irrelevant since these are a function of illumination and exposure time; it is the relative differences that are important and these are more or less the same in both cases.




The probabilities of detecting each of the ten stimuli.

Fig. 5.2 -

Fisher's Apex results ————

Fisher's Base results · · · · · · · ·

Green's Apex results (uncorrected) — — — —

Green's Base results (uncorrected) — ·· — ·· —

Both sets of Apex results are very flat whereas both sets of Base results show a tendency to 'peak' in the middle.

TABLE 5:3

| | | Prob. of Correct Detection (Uncor.) | Prob. of Correct Detection (Cor.) |
|---|---|---|---|
| | I | 0.8875 | 0.637 |
| | 2 | 0.9 | 0.646 |
| APEX | 3 | 0.9125 | 0.655 |
| BLOCKS | 4 | 0.9125 | 0.655 |
| | 5 | 0.8312 | 0.597 |
| | MEANS | 0.89 | 0.639 |
| | I | 0.5875 | 0.4 |
| | 2 | 0.6437 | 0.438 |
| BASE | 3 | 0.725 | 0.493 |
| BLOCKS | 4 | 0.4812 | 0.327 |
| | 5 | 0.4687 | 0.319 |
| | MEANS | 0.582 | 0.396 |

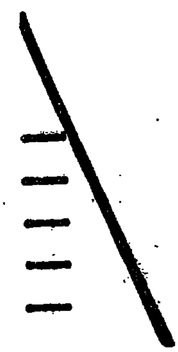
DISCUSSION Before any further comparisons are made between the two sets of data we must consider what effects the difference in procedure might have had. For example, our subjects were required to locate the block either above or below the fixation point, whereas Fisher's subjects only had to indicate its presence or absence. Fisher's procedure was changed after a pilot study in which it became obvious that when a subject was sure he had seen a block, he was also sure of its location, i.e. there were no occasions on which a subject was sure a block was present and yet located it, say, above when it had in fact appeared below. Thus this method allowed us to investigate guessing behaviour and the biases mentioned above were uncovered. However, it is reasonable to ask whether these biases were actually operating in Fisher's experiment, i.e. is it not reasonable to assume that the simpler form of response would by nature preclude the operation of these biases, since a more complex response was required of the subject to reveal them in the first place? I do not believe this to be the case. Using the terminology of signal detection theory (Swets, Tanner and Birdsall, 1961) the 'empty' trials seem to have provided evidence that the Apex response criterion is more likely to be exceeded by random noise alone than is the Base response criterion. Now, although Fisher gives no data on this point, I think it is safe to assume that there were a number of trials on which his subjects will have responded 'Yes' to an 'empty' figure, i.e. random noise alone had been high enough to exceed their response criterion. As in our experiment such an occurrence could be taken as an indication of the number of correct guesses that a S made.

Of course, without detailed knowledge of Fisher's results or further experiments, it would be rash to predict that the biases were equally as effective in Fisher's study as in ours. Another point worth noting is that in Fisher's design, the two biases were opposed and would largely cancel each other out.

In fact the present study can be thought of as containing Fisher's experiment since each subject was shown 80 figures whose orientation was $\Lambda$ i.e. as in Fisher's experiment. We can abstract the results from these figures and treat them as a separate experiment. Thus Table 5:4 shows the percentage correct responses given by each subject to the 40 Apex exposures and to the 40 Base exposures. It is obvious from Table 5:4 that the gap between the two is much narrower than it is in the study as a whole (uncorrected), i.e. 82 - 75 as opposed to 89 - 58. This is because the 'below' bias is only allowed to work in favour of the Base exposures hence swelling the number of Base false positives. However, although these results may be thought of as providing a parallel with Fisher's experiment, the gap here is much narrower than the one he found, i.e. approximately 77 - 50 (estimated from the graph). There are two possible explanations of why this should be so. Firstly, Fisher omitted to control a number of factors which would have assisted in widening the gap. These are mentioned on p.264 and include the lack of a fixation point. Secondly, there are so many differences between the two experiments which could have unforseen effects, e.g. the results in Table 5:4 are extracted from a larger experiment and were interspersed with other trials; Fisher's were not. Different sorts of responses were required and so on. In view of these differences it would be unwise to attribute the narrowing

TABLE 5:4

| Ss | ORIENTATION 'A' %Apex Correct | %Base Correct | ORIENTATION 'V' %Apex Correct | %Base Correct |
|---|---|---|---|---|
| 1 | 85.0 | 82.5 | 100.0 | 12.5 |
| 2 | 92.5 | 90.0 | 100.0 | 42.5 |
| 3 | 75.0 | 87.5 | 100.0 | 67.5 |
| 4 | 85.0 | 65.0 | 95.0 | 37.5 |
| 5 | 77.5 | 77.5 | 85.0 | 42.5 |
| 6 | 87.5 | 50.0 | 97.5 | 7.5 |
| 7 | 90.0 | 70.0 | 97.5 | 52.5 |
| 8 | 87.5 | 57.5 | 100.0 | 12.5 |
| 9 | 67.5 | 80.0 | 97.5 | 87.5 |
| 10 | 70.0 | 87.5 | 87.5 | 52.5 |
| MEANS | 81.7 | 74.7 | 96.0 | 41.5 |



The distortion-gradient induced by a single oblique line.

Fig. 5.3 - From Fisher (1970)

of the gap solely to the first reason but it seems reasonable to assume that it may have played a large part.

From the above it is plain that Fisher was somewhat lucky in obtaining the results he did. Had he used the opposite orientation his results would have been even more extreme than those shown in columns 3 and 4 of Table 5:4.

Perhaps the results are best viewed in the light of a further paper by Fisher (1970) in which he demonstrates fairly conclusively that the distortion in the Ponzo figure occurs only at the ends of the lines (Fig. 5:3). This finding indicates two possible theoretical positions:-

I. That the distortion occurs only at the ends of the lines and all other parts are unaffected.

2. That the distortion occurs in all parts of the lines and the centre points are the only points not affected.

There is a further complication, however. Are both the upper and lower lines equally affected? There are a number of possibilities suggested by this question:-

A. The upper line appears longer than it should but the lower line is unaffected.

B. The lower line appears shorter than it should but the upper line is unaffected.

C. Both are distorted equally.

This gives us six possibilities (Figs. 5:4 to 9).

In actual fact our results indicate that there is no difference in the discriminability of the blocks on the top row but that there is for the blocks on the bottom row - this curve distinctly peaks in the centre. Not only this, but all points on the lower curve

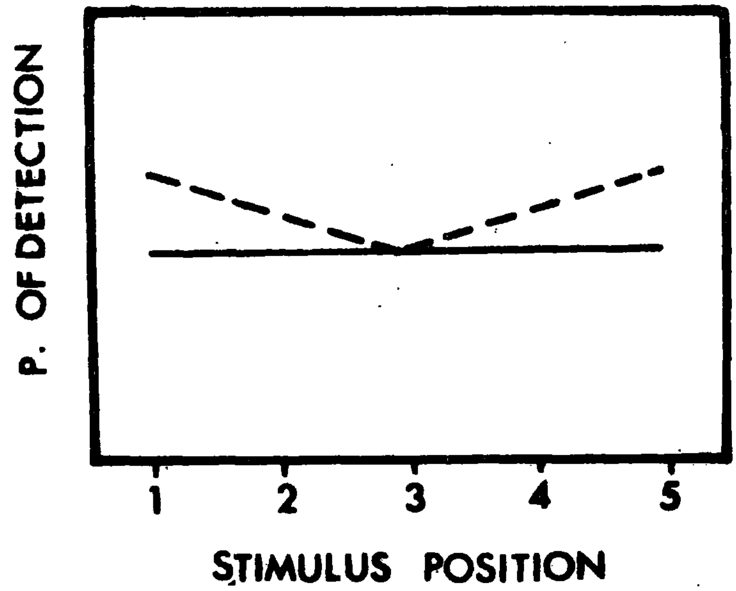



Fig. 5.4 - Distortion occurring over the whole of the 'Apex' line, except the middle.

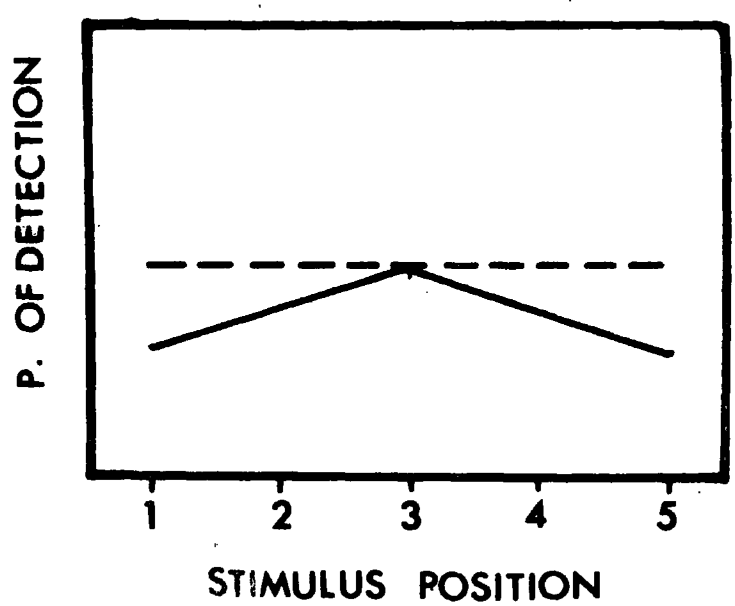



Fig. 5.5 - Distortion occurring over the whole of the 'Base' line, except the middle.

are well below any point on the upper curve.

The suggestion of the second hypothesis (2) would seem to be upheld in that all parts of the line are affected, not just the ends. It was thought that the centre of the line would be unaffected because the forces acting on it from both sides would tend to neutralise each other. This assumption seems to be unwarranted since, although the centre block is apparently more easily discriminable than any other 'lower' block, it is much less easily discriminable than any 'upper' block. Of the second set of hypotheses the evidence would tend to support B. It would appear that the upper line is seen veridically, while the lower line is seen as shorter than it actually is, i.e. the lower line is 'inhibited'.

The results would seem to favour some sort of inhibition theory. Such a theory would find assumptions involving constancy and perspective unnecessary. It would appear that the Ponzo is not a perspective illusion after all.

There is another point on which I would like to take issue with Fisher. He states, "Taken as a whole, detection of stimuli in the upper row, in comparison with the lower, is facilitated by a factor of the order of 10%. This approximates to the extent of attenuation of the lower line in relation to the upper in this particular version of the Ponzo figure." I find it difficult to determine exactly where this figure of 10% comes from. From his graph, the probability of detection for the upper row is about 0.77, and for the lower row about 0.5 (i.e. chance in Fisher's experiment). This gives a figure more in the order of 50% than 10%, i.e. (77 - 50)/50 x 100 = 54%. In my study, the figure would be about 55% uncorrected and 60% corrected. Whichever way Fisher obtained this figure, I would contend that the comparison is worthless anyway. So much depends on the exposure time and the illumination ~~we~~ make the task too difficult and both the upper and lower probabilities of detection would be at the chance level, while if the task was very easy, both probabilities could be above 0.9. In addition, just because one line looks about 10% longer than the other, it does not follow that its probability of detection should be 10% higher - the function relating the two could take almost any shape.

What is needed if we are going to make a comparison like Fisher's is to take a line that is actually 10% longer and see if its threshold is equivalent to that of the apparently longer line.

# PART 6 - SUMMARY AND CONCLUSIONS

# SUMMARY AND CONCLUSIONS

The original aim of this thesis was to inquire more closely into Gregory's theory that certain visual illusions are the results of inappropriate action by a constancy-scaling mechanism. The most original parts of the theory, i.e. the 'typical view' hypothesis and the concepts of primary and secondary scaling were found to be open to many grave theoretical objections.

The 'typical view' hypoyhesis requires that we should all see certain illusion figures as 3-dimensional in a particular way when they are presented in reduced cue conditions. It was found to be based on anthropological evidence which Gregory had seriously misinterpreted. The independence of primary and secondary scaling was found to be difficult to justify and impossible to demonstrate.

Although theoretically weak, the theory is supported by Gregory's own experiments. The first three experiments presented here were attempts to replicate his findings. An improved apparatus was used to eliminate possible proprioceptive cues present in Gregory's experiments. It had been noted that the depth effects he reported were not large enough to account for the whole of the observed illusion and it was thought that these cues might have been acting to reduce the 3-dimensional effects. Instead of our experiments producing larger effects in the same direction as Gregony's, different Ss seemed to see the figures in a variety of ways . This was in marked contrast to the results obtained with two 'control' figures which contained clear perspective cues. All Ss seemed able to interpret these cues in the expected way, thus suggesting that the 'experimental' figures did not contain similar cues.

It was hypothesised that the 'flat' results obtained in experiment I were caused by the experimenter inadvertently inducing the Ss to adopt a 'flat' perceptual set. An extension of this idea was adopted in experiments 7 and 8 when attempts were made to induce Ss to adopt a pro-Gregory perceptual set. Limited success was achieved in experiment 7 with a complex 'double' M-L figure. The more simple Ponzo figure used in experiment 8 produced more conclusive results.

A distinction was drawn between 'public' and 'private' structuring. A figure was said to induce a public structure when a strong tendency existed for all Ss to interpret it in the same way, e.g. as in our control figures. However, when figures did not contain enough information to reduce their ambiguity then Ss tended to adopt their own private structures which differed from S to S and from time to time. The more ambiguous a figure was, the less able were Ss to decide which interpretation they should adopt and the more changeable their structuring became - as evidenced by the number of Ss showing significant Blocks by Points interactions.

There were thought to be two ways in which a public structure could be induced:-

I. By introducing cues which favoured a particular interpretation - as with the control figures.

2. By 'setting' the Ss with instructions of some sort - as in experiment 8. It was thought that this method was the only way to replicate Gregory's results.

As already mentioned, left to themselves different Ss tended to view the illusion figures in different ways. However, when their results were pooled a consistent tendency emerged, which was not apparent from the individual results, for the most central part of the Gestalt to be seen as nearer. This tendency was noted for several different figures. Two hypotheses were put forward to explain these results:-

I. That the most central part of the figure was perceived as nearer because it was actually nearer, although how the S discovered this was not specified.

2. The effect was due to the AFPP not coinciding with the actual FPP.

Predictions from both theories were inconsistent with the results of experiments 4 and 5. Precedents for our findings were found in the work of Helmholtz and Foley.

Attempts were made throughout our series of experiments to confirm the findings of Pike and Stacey that an apparently longer line will be seen as nearer than an apparently shorter line. Only in experiment 3 was this tendency confirmed and this was the only experiment in which Ss were allowed to concentrate solely on the relative distances of the M-L shafts. The reasons for the difference between the bulk of our results and those of Pike and Stacey remain obscure although the literature contains many examples of conflicting data in this area.

The illusion which most readily lends itself to a perspective interpretation is the Ponzo. However, our 'unset' Ss did not show any tendency to see the upper line as more distant than the lower line as any perspective theory would require. This suggests that the illusion may not be a perspective illusion at all.

This suggestion was strengthened by the results of experiment 9. The perspective theory states that the upper line is seen as larger because it is seen as further while subtending the same visual angle. If this is so then there is no reason to think that the upper line should be more easily detectable than the lower line - yet this is in fact the case.

Taken as a whole the results of this thesis suggest that any perspective theory will prove inadequate although the possibility that perspective might be a minor component in some illusions is conceded. If we abandon perspective explanations, what alternatives are there? The result of experiment 9 suggests that a theory is required that predicts a change in threshold as well as a change in size. A number of such theories do exist, the best known of which is probably that of Ganz. The emphasis of these theories is on the effect of the positions of the various lines in the illusion figures rather than on ascribing the illusions to the operation of some other mechanism, e.g. size-constancy.

Ganz has been criticised by Coren (1970) on the grounds that illusions still occur on occasions where Ganz would not expect them, e.g. the replacement of the distorted lines with dots in the Wundt-Hering illusion.

A similar approach is that of Blakemore and Sutton (1969). Their Ss observed a grating made up of wide bars for some time before switching their fixation to a grating made up of narrow bars. The narrow bars then appeared narrower than they were, while the wide bars appeared wider than they were. Blakemore and Sutton believe this illusion to result from the adaptation of size-selective neurones of the type already found in monkeys and cats. If such neurones exist it might be possible to construct a workable theory of the illusions based on their inhibition or facilitation.

It seems regrettable that a theory as elegant and appealing as Gregory's should not survive close scrutiny.

A P P E N D I X

INDIVIDUAL ANALYSES FOR EXPT. 1

INDIVIDUAL ANALYSES FOR EXPERIMENT 1

M-L CONTROL

| Effects | | Grundy | Thomson | Templeton | Martin | Bateman | Cross |
|---|---|---|---|---|---|---|---|
| Position (df 1) | MS | 43.5 | 180.5 | 292.0 | 5.0 | 485.7 | 144.5 |
| | F | 0.2 | 0.7 | 0.9 | 0.2 | 2.5 | 0.6 |
| Blocks (df 3) | MS | 1401.8 | 1312.3 | 4219.9 | 33.5 | 224.0 | 65.8 |
| | F | 7.3*** | 5.3** | 12.3*** | 0.5 | 1.2 | 0.3 |
| Points (df 2) | MS | 7157.7 | 442.5 | 16033.3 | 1801.7 | 2010.5 | 29448.6 |
| | F | 37.5*** | 1.8 | 46.9*** | 25.7 | 10.5*** | 125.0*** |
| Pos x Blks (df 3) | MS | 12.1 | 33.6 | 27.8 | 3.7 | 55.5 | 18.2 |
| | F | 0.1 | 0.1 | 0.1 | 0.1 | 0.3 | 0.1 |
| Pos x Pnts (df 2) | MS | 15.5 | 6.8 | 150.5 | 14.1 | 35.4 | 1.8 |
| | F | 0.1 | 0.0 | 0.4 | 0.2 | 0.2 | 0.0 |
| Prts x Blks (df 6) | MS | 75.5 | 116.4 | 821.1 | 37.2 | 80.4 | 251.2 |
| | F | 0.4 | 0.5 | 2.4* | 0.5 | 0.4 | 1.1 |
| Prts x Blks (df 6) | MS | 12.0 | 28.6 | 33.6 | 1.5 | 28.3 | 27.0 |
| | F | 0.1 | 0.1 | 0.1 | 0.0 | 0.2 | 0.1 |
| Error (df 48) | MS | 191.0 | 247.6 | 342.1 | 70.2 | 190.7 | 235.7 |

The df shown in brackets after each 'effect' applies to all Ss

M-II Exptl (Ingoing)

| Effects | | SUBJECTS Grundy | Thomson | Templeton | Martin | Bateman | Cross |
|---|---|---|---|---|---|---|---|
| Position (df 1) | MS | 55.5 | 160.2 | 36.3 | 30.4 | 1232.7 | 1305.4 |
| | F | 0.2 | 0.8 | 0.3 | 0.3 | 4.3* | 18.6*** |
| Blocks (df 3) | MS | 1527.5 | 2413.9 | 198.9 | 631.5 | 199.5 | 38.1 |
| | F | 5.3** | 11.7*** | 1.7 | 6.7* | 0.7 | 0.5 |
| Points (df 3) | MS | 2119.7 | 134.9 | 86.4 | 1275.0 | 2191.2 | 491.6 |
| | F | 7.4*** | 0.7 | 0.7 | 13.6*** | 7.6*** | 7.0 |
| Pos x Blks (df 3) | MS | 17.1 | 17.6 | 7.0 | 6.6 | 62.9 | 34.5 |
| | F | 0.1 | 0.1 | 0.1 | 0.1 | 0.2 | 0.5 |
| Pos x Prts (df 9) | MS | 25.4 | 30.4 | 16.6 | 16.9 | 129.7 | 18.6 |
| | F | 0.1 | 0.2 | 0.1 | 0.2 | 0.4 | 0.2 |
| Blks x Prts (df 3) | MS | 191.9 | 140.3 | 38.5 | 476.8 | 108.1 | 93.7 |
| | F | 0.7 | 0.7 | 0.3 | 5.1*** | 0.4 | 1.3 |
| P x P x B (df 9) | MS | 23.1 | 39.9 | 7.8 | 4.0 | 31.3 | 13.5 |
| | F | 0.1 | 0.2 | 0.1 | 0.0 | 0.1 | 0.2 |
| Error (df 64) | MS | 288.2 | 207.7 | 117.7 | 93.7 | 286.9 | 70.3 |

The df shown in brackets after each 'effect' applies to all Ss

PONZO EXPTL

| Effects | | Grundy | Thomson | Templeton | Martin | Bateman | Cross |
|---|---|---|---|---|---|---|---|
| | | **S U B J E C T S** | | | | | |
| Position (df 1) | MS | 0.2 | 25.5 | 22.7 | 30.1 | 150.5 | 385.3 |
| | F | 0.0 | 0.1 | 0.1 | 0.0 | 2.0 | 3.5 |
| Blocks (df 3) | MS | 676.9 | 2161.9 | 105.7 | 822.8 | 332.9 | 517.1 |
| | F | 3.2* | 4.5* | 0.6 | 0.4 | 4.5* | 4.7** |
| Points (df 1) | MS | 7.5 | 13.0 | 38.5 | 1656.8 | 67.7 | 1.3 |
| | F | 0.0 | 0.0 | 0.2 | 0.8 | 0.9 | 0.0 |
| Pos x Blks (df 3) | MS | 26.4 | 150.0 | 18.6 | 20.1 | 8.4 | 3.7 |
| | F | 0.1 | 0.3 | 0.1 | 0.0 | 0.1 | 0.0 |
| Pos x Pnts (df 1) | MS | 15.2 | 35.0 | 9.2 | 60.7 | 1.7 | 1.3 |
| | F | 0.1 | 0.1 | 0.1 | 0.0 | 0.0 | 0.0 |
| Blks x Pnts (df 3) | MS | 34.5 | 23.1 | 10.6 | 761.8 | 9.3 | 62.2 |
| | F | 0.2 | 0.1 | 0.1 | 0.4 | 0.1 | 0.6 |
| P x P x B (df 3) | MS | 65.6 | 2.9 | 28.5 | 12.2 | 9.1 | 20.2 |
| | F | 0.3 | 0.0 | 0.2 | 0.0 | 0.1 | 0.2 |
| Error (df 32) | MS | 213.1 | 482.8 | 189.7 | 1964.8 | 74.9 | 111.1 |

The df shown in brackets after each 'effect' applies to all Ss.

PONZO CONTROL

| Effects | | Grundy | Thomson | Templeton | Martin | Bateman | Cross |
|---|---|---|---|---|---|---|---|
| | | **S U B J E C T S** | | | | | |
| Position (df 1) | MS | 102.7 | 138.9 | 156.1 | 1241.7 | 98.0 | 320.9 |
| | F | 0.6 | 0.3 | 0.7 | 0.5 | 0.7 | 4.3* |
| Blocks (df 3) | MS | 176.9 | 745.8 | 97.9 | 1080.8 | 81.4 | 769.3 |
| | F | 1.1 | 1.5 | 0.4 | 0.5 | 0.6 | 10.3*** |
| Points (df 2) | MS | 13476.8 | 9131.2 | 14640.9 | 16547.8 | 646.0 | 18510.3 |
| | F | 82.0*** | 17.8*** | 62.1*** | 7.1** | 4.4* | 247.2*** |
| Pos x Blks (df 3) | MS | 18.4 | 62.0 | 50.5 | 197.8 | 4.1 | 5.5 |
| | F | 0.1 | 0.1 | 0.2 | 0.1 | 0.0 | 0.1 |
| Pos x Pnts (df 2) | MS | 4.2 | 39.4 | 4.1 | 404.8 | 23.8 | 41.5 |
| | F | 0.0 | 0.1 | 0.0 | 0.2 | 0.2 | 0.6 |
| Blks x Pnts (df 6) | MS | 105.2 | 358.5 | 126.0 | 286.9 | 11.3 | 210.9 |
| | F | 0.6 | 0.7 | 0.5 | 0.1 | 0.1 | 2.8 |
| P x P x B (df 6) | MS | 11.4 | 47.6 | 3.5 | 86.0 | 17.2 | 19.5 |
| | F | 0.1 | 0.1 | 0.0 | 0.0 | 0.1 | 0.3 |
| Error (df 48) | MS | 164.3 | 511.9 | 235.8 | 2323.3 | 148.1 | 74.9 |

The df shown in brackets after each 'effect' applies to all Ss.

**The Curve**

| Effects | | SUBJECTS | | | | | |
|---|---|---|---|---|---|---|---|
| | | Grundy | Thomson | Templeton | Martin | Bateman | Cross |
| Position (df 1) | MS | 9.0 | 54.0 | 427.8 | 29.6 | 73.5 | 332.5 |
| | F | 0.1 | 0.9 | 1.5 | 0.3 | 1.3 | 0.1 |
| Blocks (df 2) | MS | 55.4 | 102.7 | 1888.7 | 337.0 | 39.4 | 2.9 |
| | F | 0.5 | 1.7 | 6.5** | 3.2 | 0.7 | 0.1 |
| Points (df 2) | MS | 134.1 | 18.1 | 91.2 | 3.4 | 3.4 | 65.4 |
| | F | 1.1 | 0.3 | 0.3 | 0.0 | 0.1 | 1.3 |
| Pos x Blks (df 2) | MS | 19.0 | 1.6 | 3.2 | 9.0 | 13.6 | 2.6 |
| | F | 0.2 | 0.0 | 0.0 | 0.1 | 0.2 | 0.1 |
| Pos x Pnts (df 2) | MS | 19.9 | 1.6 | 69.8 | 7.9 | 10.2 | 6.7 |
| | F | 0.2 | 0.0 | 0.2 | 0.1 | 0.2 | 0.1 |
| Blks x Pnts (df 4) | MS | 36.1 | 102.1 | 73.7 | 36.7 | 26.7 | 21.4 |
| | F | 0.3 | 1.7 | 0.3 | 0.4 | 0.5 | 0.4 |
| P x P x B (df 4) | MS | 15.9 | 5.1 | 19.6 | 8.1 | 6.9 | 18.3 |
| | F | 0.1 | 0.1 | 0.1 | 0.1 | 0.1 | 0.4 |
| Error (df 36) | MS | 122.2 | 61.8 | 292.7 | 105.2 | 58.2 | 51.0 |

The df shown in brackets after each 'effect' applies to all Ss.

INDIVIDUAL ANALYSES FOR EXPT. 2

INDIVIDUAL ANALYSES EXPT 2

M-L Control

| | Points | | Blocks | | Pnts x Blks | | Error |
|---|---|---|---|---|---|---|---|
| SUBJECTS | MS | F | MS | F | MS | F | MS |
| Aldous | 3954.1 | 208.9*** | 94.9 | 5.0** | 96.7 | 5.1*** | 18.8 |
| Hockey | 8724.4 | 62.8*** | 1757.7 | 12.7*** | 504.4 | 3.6** | 139.0 |
| Holmes | 6219.6 | 42.4*** | 209.6 | 4.8** | 195.0 | 1.3 | 146.6 |
| Davies | 14065.6 | 97.7*** | 1053.4 | 7.3*** | 374.2 | 2.6* | 143.9 |
| James | 15801.1 | 79.5*** | 9122.8 | 45.9*** | 253.2 | 1.3 | 198.8 |
| Buckingham | 4877.0 | 21.7*** | 268.0 | 1.2 | 240.0 | 1.1 | 225.1 |
| Lee | 2829.3 | 33.1*** | 2428.4 | 28.4*** | 157.4 | 1.8 | 85.5 |
| Curless | 3572.0 | 25.2*** | 653.1 | 4.6** | 373.5 | 2.6* | 141.8 |
| Underwood | 6021.7 | 143.5 | 838.8 | 20.0*** | 155.4 | 3.7** | 42.0 |
| Hinchcliffe | 668.8 | 5.9** | 142.1 | 1.2 | 187.5 | 1.7 | 113.5 |

DF for all Ss were 2 for POINTS, 3 for BLOCKS 6 for PNTS x BLKS and 60 for ERROR

Ingoing M-L

| | Points | | Blocks | | Pnts x Blks | | Error |
|---|---|---|---|---|---|---|---|
| SUBJECTS | MS | F | MS | F | MS | F | MS |
| Aldous | 19.5 | 0.2 | 60.4 | 0.7 | 293.1 | 3.4** | 87.1 |
| Hockey | 9704.7 | 93.1*** | 562.2 | 5.4** | 534.7 | 5.1*** | 104.2 |
| Holmes | 2823.7 | 17.5*** | 985.5 | 6.1** | 133.1 | 0.8 | 161.5 |
| Davies | 4153.2 | 34.7*** | 3022.6 | 25.2*** | 175.5 | 1.5 | 119.9 |
| James | 734.1 | 4.0* | 2846.0 | 15.6*** | 470.0 | 2.6 | 182.0 |
| Buckingham | 5726.4 | 20.7*** | 122.3 | 0.4 | 283.6 | 1.0 | 277.1 |
| Lee | 478.8 | 6.0*** | 249.5 | 3.1* | 342.7 | 4.3*** | 79.8 |
| Curless | 414.1 | 3.5* | 1073.0 | 8.9*** | 432.4 | 3.6** | 120.1 |
| Underwood | 1909.5 | 41.1*** | 284.4 | 6.1*** | 94.8 | 2.0 | 46.4 |
| Hinchcliffe | 263.4 | 2.4 | 89.5 | 0.8 | 106.7 | 1.0 | 109.4 |

DF for all Ss were 3 for POINTS, 3 for BLOCKS, 9 for PNTS x BLKS and 80 for ERROR

PONZO EXPTL

| | Points | | Blocks | | Pnts x Blks | | Error |
|---|---|---|---|---|---|---|---|
| SUBJECTS | MS | F | MS | F | MS | F | MS |
| Aldous | 609.2 | 2.5 | 610.2 | 2.5 | 91.2 | 0.4 | 239.1 |
| Hockey | 1740.0 | 16.2*** | 226.5 | 2.1 | 2635.0 | 24.5*** | 107.5 |
| Holmes | 105.0 | 0.5 | 223.7 | 1.1 | 338.2 | 1.6 | 210.4 |
| Davies | 1507.5 | 5.4* | 631.1 | 2.3 | 304.3 | 1.1 | 277.7 |
| James | 1485.2 | 9.9** | 60.4 | 0.4 | 272.2 | 1.8 | 149.4 |
| Buckingham | 1452.0 | 5.0* | 1654.8 | 5.7** | 526.0 | 1.8 | 288.3 |
| Lee | 225.3 | 2.4 | 739.6 | 7.6** | 93.9 | 1.0 | 94.7 |
| Curless | 96.3 | 0.4 | 1010.3 | 4.3* | 345.3 | 1.5 | 236.1 |
| Underwood | 72.5 | 0.5 | 26.3 | 0.2 | 629.7 | 4.4** | 144.3 |
| Hinchcliffe | 760.0 | 5.9* | 349.6 | 2.7 | 356.6 | 2.8 | 127.6 |

DF for all Ss were 1 for POINTS, 3 for BLOCKS, 3 for PNTS x BLKS and 40 for ERROR

PONZO CONTROL

| | Points | | Blocks | | Pnts x Blks | | Error |
|---|---|---|---|---|---|---|---|
| SUBJECTS | MS | F | MS | F | MS | F | MS |
| Aldous | 1341.2 | 19.3*** | 393.9 | 5.7** | 58.9 | 0.9 | 69.5 |
| Hockey | 432.8 | 5.9** | 10.4 | 0.1 | 214.1 | 2.9* | 73.1 |
| Holmes | 5432.3 | 43.6*** | 477.6 | 3.8* | 831.2 | 2.7* | 124.1 |
| Davies | 9561.5 | 50.4*** | 58.8 | 0.3 | 538.7 | 2.8* | 189.8 |
| James | 7455.4 | 64.8*** | 1098.2 | 9.6*** | 727.2 | 6.3*** | 115.0 |
| Buckingham | 4367.1 | 16.2*** | 299.4 | 1.1 | 261.7 | 1.0 | 270.2 |
| Lee | 755.4 | 9.6*** | 119.7 | 1.5 | 75.2 | 1.0 | 78.4 |
| Curless | 2399.4 | 16.4*** | 490.5 | 3.4* | 493.0 | 3.4** | 146.5 |
| Underwood | 5813.2 | 43.3*** | 133.8 | 1.0 | 233.1 | 1.7 | 134.4 |
| Hinchcliffe | 1876.2 | 16.4*** | 231.8 | 2.0 | 134.8 | 1.2 | 114.2 |

DF for all Ss were 2 for POINTS, 3 for BLOCKS, 6 for PNTS x BLKS and 60 for ERROR

Curve

| SUBJECTS | POINTS MS | POINTS F | BLOCKS MS | BLOCKS F | PNTS x BLKS MS | PNTS x BLKS F | ERROR MS |
|---|---|---|---|---|---|---|---|
| Aldous | 716.7 | 6.8** | 327.0 | 3.1* | 336.4 | 3.2** | 105.7 |
| Hockey | 1417.3 | 26.9*** | 79.5 | 1.5 | 213.3 | 4.0** | 52.7 |
| Holmes | 2278.0 | 20.4*** | 69.4 | 0.6 | 90.8 | 0.8 | 111.7 |
| Davies | 631.6 | 5.5** | 40.4 | 0.4 | 378.8 | 3.3** | 115.2 |
| James | 588.9 | 2.6 | 1481.0 | 6.5*** | 677.6 | 3.0* | 229.6 |
| Buckingham | 2149.3 | 10.0*** | 655.1 | 3.0* | 446.2 | 2.1 | 215.6 |
| Lee | 68.3 | 0.8 | 199.0 | 2.3 | 85.6 | 1.0 | 87.5 |
| Curless | 602.9 | 3.2* | 110.7 | 0.6 | 163.0 | 0.9 | 192.6 |
| Underwood | 1053.7 | 7.8*** | 1180.6 | 8.7*** | 215.2 | 1.6 | 135.7 |
| Hinchliffe | 375.0 | 2.7 | 111.8 | 0.8 | 117.7 | 0.9 | 139.1 |

DF for all Ss were 2 for POINTS, 3 for BLOCKS, 6 for PNTS x BLKS and 60 for ERROR

Outgoing M-L

| SUBJECTS | POINTS MS | POINTS F | BLOCKS MS | BLOCKS F | PNTS x BLKS MS | PNTS x BLKS F | ERROR MS |
|---|---|---|---|---|---|---|---|
| Hockey | 7135.4 | 75.2*** | 883.3 | 9.3*** | 102.5 | 1.1 | 95.0 |
| Aldous | 49.5 | 0.6 | 170.1 | 1.9 | 137.7 | 1.6 | 88.3 |
| Holmes | 287.3 | 1.9 | 866.7 | 5.8** | 620.0 | 4.1** | 150.0 |
| Davies | 3161.5 | 25.3*** | 63.0 | 0.5 | 201.7 | 1.6 | 124.8 |
| James | 3416.4 | 15.4*** | 19454.3 | 87.4*** | 1484.1 | 6.7*** | 222.6 |
| Buckingham | 1970.6 | 9.0*** | 227.7 | 1.1 | 215.5 | 1.0 | 217.7 |
| Lee | 102.9 | 1.3 | 285.5 | 3.5* | 43.6 | 0.5 | 81.0 |
| Curless | 3488.8 | 26.9*** | 546.6 | 4.2** | 198.5 | 1.5 | 129.7 |
| Underwood | 228.0 | 3.0 | 5124.6 | 66.6*** | 139.8 | 1.8 | 77.0 |
| Hinchliffe | 1094.0 | 8.9*** | 129.1 | 1.1 | 446.1 | 3.6** | 123.3 |

DF for all the above Ss were 2 for POINTS, 3 for BLOCKS, 6 for PNTS x BLKS and 60 for ERROR

Also tested were the following (see text):-
OUTGOING M-L (continued)

| SUBJECTS | POINTS DF | POINTS MS | POINTS F | ERROR DF | ERROR MS |
|---|---|---|---|---|---|
| Bateman | 2 | 268.0 | 4.0* | 69 | 66.1 |
| Cross | 2 | 414.0 | 12.5*** | 69 | 33.2 |
| Martin | 2 | 34642.0 | 477.0*** | 69 | 72.5 |
| Grundy | 2 | 567.0 | 9.9*** | 69 | 57.3 |

INDIVIDUAL ANALYSES FOR EXPT. 3

INDIVIDUAL ANALYSES FOR EXPT 3

THE DOUBLE M-L:

| SUBJECTS | POINTS DF | POINTS MS | F | ERROR DF | ERROR MS |
|---|---|---|---|---|---|
| Aldous | 1 | 616.3 | 11.5** | 46 | 53.7 |
| Hockey | 1 | 374 | 2.7 | 46 | 136.9 |
| James | 1 | 3072 | 16.2*** | 46 | 189.3 |
| Lee | 1 | 391 | 11.5** | 46 | 34.1 |
| Curless | 1 | 7326 | 20.6*** | 46 | 35.6 |
| Underwood | 1 | 520 | 16.1*** | 46 | 32.4 |
| Hinchliffe | 1 | 1789 | 42.4*** | 46 | 42.2 |
| Grundy | 1 | 5292 | 25.8*** | 46 | 20.5 |
| Martin | 1 | 6793 | 90.6*** | 46 | 90.6 |
| Bateman | 1 | 2054 | 47.6*** | 46 | 47.6 |
| Cross | 1 | 5023 | 31.7*** | 46 | 31.7 |

Curve

| SUBJECTS | POINTS DF | POINTS MS | F | ERROR DF | ERROR MS |
|---|---|---|---|---|---|
| Aldous | 2 | 2054 | 54.4*** | 69 | 37.7 |
| Hockey | 2 | 2474 | 149.3*** | 69 | 165.7 |
| James | 2 | 1618 | 25.1*** | 69 | 64.4 |
| Buckingham | 2 | 4189 | 53.2*** | 69 | 118.9 |
| Lee | 2 | 2830 | 40.2*** | 69 | 70.4 |
| Curless | 2 | 4189 | 53.2*** | 69 | 118.9 |
| Underwood | 2 | 2166 | 41.4*** | 69 | 52.4 |
| Hinchliffe | 2 | 269.6 | 2.8 | 69 | 94.8 |
| Grundy | 2 | 11.5 | 0.1 | 69 | 88.5 |
| Martin | 2 | 3281 | 13.9*** | 69 | 235.8 |
| Bateman | 2 | 512 | 15.3*** | 69 | 33.4 |
| Cross | 2 | 6480 | 34.6*** | 69 | 18.7 |

Crosses

| SUBJECTS | POINTS DF | POINTS MS | F | ERROR DF | ERROR MS |
|---|---|---|---|---|---|
| Aldous | 2 | 2054 | 54.4*** | 69 | 37.7 |
| Hockey | 2 | 6309 | 15.4*** | 69 | 410.5 |
| James | 2 | 165 | 1.4 | 69 | 116.5 |
| Lee | 2 | 2090 | 40.4*** | 69 | 51.7 |
| Curless | 2 | 494 | 7.8*** | 69 | 62.9 |
| Underwood | 2 | 787 | 14.2*** | 69 | 55.4 |
| Hinchliffe | 2 | 1121 | 40.0*** | 69 | 28.0 |
| Grundy | 2 | 2399 | 24.7*** | 69 | 97.3 |
| Martin | 2 | 10662 | 37.3*** | 69 | 285.8 |
| Bateman | 2 | 326 | 4.6* | 69 | 70.4 |
| Cross | 2 | 4549 | 81.8*** | 69 | 55.6 |

INDIVIDUAL ANALYSES FOR EXPTS. 4 TO 8

INDIVIDUAL ANALYSES FOR EXPERIMENT 4

THE DOUBLE M-L

| | BLOCKS | | POINTS | | BLKS x PNTS | | ERROR |
|---|---|---|---|---|---|---|---|
| SUBJECTS | MS | F | MS | F | MS | F | MS |
| Martin | 1213.3 | 11.3*** | 755.0 | 7.1*** | 46.5 | 0.4 | 107.0 |
| Cross | 2055.1 | 30.6*** | 1013.9 | 15.1*** | 602.8 | 9.0*** | 67.1 |
| Bateman | 774.6 | 10.1*** | 924.8 | 12.1*** | 133.5 | 1.7 | 76.4 |
| Lee | 258.7 | 2.3 | 397.0 | 3.6** | 464.5 | 4.1** | 111.9 |
| Grundy | 324.0 | 5.0* | 2771.0 | 43.1*** | 1294.5 | 20.1*** | 64.3 |
| Hockey | 212.6 | 1.0 | 6748.8 | 32.6*** | 1053.9 | 5.1** | 207.0 |
| Curless | 136.1 | 3.6 | 734.0 | 19.7*** | 60.3 | 1.6 | 37.3 |

DF for all Ss were 1 for BLOCKS, 5 for POINTS, 5 for BLKS x PNTS and 132 for ERROR

THE SPOT

| | BLOCKS | | POINTS | | BLKS x PNTS | | ERROR |
|---|---|---|---|---|---|---|---|
| SUBJECTS | MS | F | MS | F | MS | F | MS |
| Martin | 173.3 | 1.7 | 7365.8 | 73.2*** | 479.1 | 4.8*** | 100.6 |
| Cross | 702.2 | 9.2** | 1384.6 | 18.2*** | 353.5 | 4.7*** | 76.0 |
| Bateman | 845.8 | 17.3*** | 1514.8 | 30.9*** | 266.7 | 5.4*** | 49.0 |
| Lee | 220.0 | 3.0 | 539.5 | 7.4*** | 50.8 | 0.7 | 72.5 |
| Grundy | 79.5 | 1.9 | 5387.1 | 127.6*** | 242.6 | 5.7*** | 42.2 |
| Hockey | 890.0 | 6.8* | 2890.7 | 21.9*** | 608.0 | 4.6*** | 131.7 |
| Curless | 427.0 | 14.9*** | 116.3 | 4.1** | 98.1 | 3.4** | 28.7 |
| Hinchcliffe | 4680.8 | 79.1*** | 3398.6 | 57.4*** | 2988.9 | 50.5*** | 59.2 |

DF for all Ss were 1 for BLOCKS, 5 for POINTS, 5 for PNTS x BLKS and 132 for ERROR

INDIVIDUAL ANALYSES FOR EXPERIMENT 5

| | BLOCKS | | POINTS | | BLKS x PNTS | | ERROR |
|---|---|---|---|---|---|---|---|
| SUBJECTS | MS | F | MS | F | MS | F | MS |
| Ward | 70.0 | 3.5 | 448.9 | 22.5*** | 99.3 | 5.0*** | 20.0 |
| Wise | 120.2 | 1.4 | 3269.3 | 38.0*** | 38.3 | 0.5 | 86.0 |
| McKee | 960.6 | 18.4*** | 246.0 | 4.7 | 105.9 | 2.0 | 52.3 |
| Stephenson | 16.0 | 0.1 | 1141.0 | 9.8*** | 1213.2 | 10.4*** | 116.2 |
| Baustaugh | 3.1 | 0.1 | 6574.6 | 56.6*** | 276.0 | 2.4 | 116.2 |
| Ratsey | 221.5 | 17.2*** | 353.3 | 27.6*** | 185.3 | 14.5*** | 12.8 |
| France | 767.0 | 13.8*** | 577.6 | 10.4*** | 163.1 | 2.9 | 55.7 |

DF for all Ss were 1 for BLOCKS, 5 for POINTS, 5 for BLKS x PNTS and 71 for ERROR

INDIVIDUAL ANALYSES FOR EXPT 6

| | POSITION | | POINTS | | POS x POINTS | | ERROR | |
|---|---|---|---|---|---|---|---|---|
| SUBJECTS | MS | F | MS | F | MS | F | DF | MS |
| White | 90 | 3.1 | 81.0 | 2.8* | 60.0 | 2.1 | 24 | 29.3 |
| Cook | 2 | 3.4 | 2.3 | 3.8* | 0.0 | 0.0 | 24 | 58.0 |
| Challenor | 2 | 0.0 | 141.0 | 0.5 | 39.0 | 0.1 | 24 | 290.0 |
| Cripps | 69 | 3.2 | 68.0 | 3.2* | 15.0 | 0.7 | 42 | 21.3 |
| Lewis | 36 | 0.1 | 569.0 | 1.8 | 8.0 | 0.0 | 54 | 323.0 |
| Davis | 272 | 4.0 | 13.8 | 0.2 | 30.0 | 0.6 | 36 | 68.8 |
| Brett | 1260 | 14.2*** | 116.0 | 1.3 | 26.0 | 0.3 | 36 | 88.5 |
| Dicey | 105 | 5.8* | 13.8 | 0.8 | 1.8 | 0.1 | 60 | 18.3 |
| Stamp | 1810 | 46.3*** | 4.8 | 0.3 | 42.6 | 1.1 | 60 | 39.1 |
| Bird | 1821 | 7.8** | 292.0 | 1.5 | 40.0 | 0.2 | 60 | 234.6 |
| Auffret | - | - | 241.0 | 1.2 | - | - | 48 | 194.0 |
| Gussoy | - | - | 558.5 | 1.7 | - | - | 42 | 327.9 |

DF for all Ss were (where applicable) 1 for POSITION, 5 for POINTS and 5 for POS x PNTS

INDIVIDUAL ANALYSES FOR EXPT 7

| | POSITION | | POINTS | | POS x POINTS | | ERROR | |
|---|---|---|---|---|---|---|---|---|
| SUBJECTS | MS | F | MS | F | MS | F | DF | MS |
| Tighe | 1411 | 41.6*** | 55.4 | 1.6 | 46.0 | 1.4 | 48 | 33.9 |
| Dobson | 99 | 2.5 | 25.6 | 0.7 | 18.6 | 0.5 | 48 | 39.2 |
| Carter | 4612 | 27.9*** | 453.0 | 2.7* | 279.0 | 1.6 | 48 | 165.4 |
| Dunlevy | 11207 | 46.3*** | 92.0 | 0.9 | 111.0 | 1.1 | 48 | 99.3 |
| Jones | 5776 | 24.5*** | 247.0 | 1.1 | 108.0 | 0.5 | 24 | 235.5 |
| Brereton | 6553 | 41.1*** | 385.0 | 2.4 | 220.0 | 1.4 | 48 | 159.6 |
| Dean | 667 | 5.5* | 53.0 | 0.4 | 2.4 | 0.0 | 48 | 121.2 |
| McGregor | 3267 | 17.3*** | 119.0 | 0.6 | 72.0 | 0.3 | 36 | 188.9 |
| Newman | 165 | 5.2* | 58.0 | 1.7 | 29.0 | 0.9 | 24 | 31.9 |
| Marvin | 54 | 0.3 | 2895.0 | 16.4*** | 140.0 | 0.8 | 48 | 176.1 |
| Newstubb | 75 | 0.4 | 540.0 | 3.1* | 112.0 | 0.6 | 48 | 175.9 |
| Crawford | - | - | 365.0 | 4.8*** | - | - | 54 | 75.7 |

DF for all Ss were (where applicable) 1 for POSITION, 5 for POINTS and 5 for POS x PNTS

INDIVIDUAL ANALYSES FOR EXPT 8

Uninstructed Group

| | Blks | | Blks Error | | Points | | Blks x Pnts | | Error | |
|---|---|---|---|---|---|---|---|---|---|---|
| Subject | MS | DF | MS | F | MS | F | MS | F | DF | MS |
| Brock | 2487 | 8 | 20.4 | $120.9^{***}$ | 1786 | $31.9^{***}$ | 2904 | $51.9^{***}$ | 8 | 56.0 |
| Taylor | 901 | 10 | 501.6 | 1.8 | 1 | 0.3 | 1 | 0.3 | 10 | 2.9 |
| Emmerson | 468 | 10 | 123.0 | 3.8 | 600 | $7.1^{*}$ | 37 | 0.4 | 10 | 84.7 |
| Lyon | 7 | 10 | 1.6 | 4.4 | 0 | 0.0 | 2 | 1.1 | 10 | 1.9 |
| Price | 715 | 10 | 324.1 | 2.2 | 109 | 0.2 | 100 | 0.2 | 10 | 497.3 |
| Dixon | 198 | 10 | 160.1 | 1.2 | 8325 | $48.5^{***}$ | 10 | 0.1 | 10 | 171.6 |
| Dunn | 1 | 10 | 758.2 | 0.0 | 1411 | 3.3 | 95 | 0.2 | 10 | 424.8 |
| Kettley | 737 | 10 | 187.7 | 3.9 | 31 | 3.9 | 22 | 2.8 | 10 | 7.9 |

DF for all Ss were 1 for BLOCKS, 1 for POINTS and 1 for BLKS x PNTS

Instructed Group

| | Blks | | Blks Error | | Points | | Blks x Pnts | | Error | |
|---|---|---|---|---|---|---|---|---|---|---|
| Subject | MS | DF | MS | F | MS | F | MS | F | DF | MS |
| Brothers | 408 | 10 | 341.4 | 1.2 | 308 | 2.8 | 136 | 1.2 | 10 | 112.5 |
| Bird | 315 | 10 | 180.2 | 1.8 | 40 | 1.8 | 7 | 0.3 | 10 | 21.8 |
| Morton | 165 | 10 | 183.2 | 0.9 | 1584 | $37.7^{***}$ | 13 | 0.3 | 10 | 42.0 |
| Franks | 9 | 10 | 67.0 | 0.1 | 222 | $11.5^{**}$ | 3 | 0.1 | 10 | 19.3 |
| Thunder | 260 | 10 | 768.0 | 0.3 | 235 | $25.3^{***}$ | 5 | 0.5 | 10 | 9.3 |
| May | 20 | 10 | 78.3 | 0.3 | 641 | $15.8^{**}$ | 81 | 2.0 | 10 | 40.7 |
| Darling | 126 | 10 | 991.5 | 0.1 | 5797 | $140.0^{***}$ | 210 | $5.1^{*}$ | 10 | 41.4 |
| Chips | 610 | 10 | 246.3 | 2.5 | 247 | $44.9^{***}$ | 4 | 0.7 | 10 | 5.5 |
| Hunt | 852 | 10 | 149.3 | $5.7^{*}$ | 187 | $14.5^{**}$ | 31 | 2.4 | 10 | 12.9 |

DF for all Ss were 1 for BLOCKS, 1 for POINTS and 1 for BLKS x PNTS

RAW DATA FROM EXPT. 9

NUMBER OF 'ABOVE' RESPONSES TO 'EMPTY' TRIALS (MAX POSSIBLE = 4)

| TRIAL NO | | 7 | 12 | 15 | 17 | 22 | 26 | 33 | 37 | 40 | 46 | TOTAL |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| ORIENTATION | | Λ | V | V | Λ | V | Λ | V | Λ | Λ | V | |
| | 1 | 3 | 0 | 0 | 3 | 0 | 4 | 1 | 3 | 4 | 1 | 19 |
| S | 2 | 4 | 0 | 0 | 2 | 0 | 4 | 0 | 3 | 4 | 2 | 19 |
| U | 3 | 1 | 1 | 4 | 3 | 3 | 4 | 2 | 1 | 1 | 3 | 23 |
| B | 4 | 3 | 0 | 0 | 4 | 1 | 3 | 1 | 1 | 3 | 2 | 18 |
| J | 5 | 1 | 1 | 0 | 1 | 1 | 2 | 1 | 0 | 1 | 2 | 10 |
| E | 6 | 2 | 0 | 0 | 2 | 0 | 3 | 0 | 2 | 2 | 0 | 12 |
| C | 7 | 4 | 0 | 0 | 2 | 0 | 3 | 0 | 3 | 1 | 0 | 13 |
| T | 8 | 2 | 0 | 0 | 2 | 0 | 2 | 1 | 4 | 2 | 0 | 13 |
| S | 9 | 2 | 0 | 0 | 2 | 0 | 2 | 0 | 2 | 2 | 0 | 10 |
| | 10 | 2 | 3 | 2 | 0 | 1 | 1 | 2 | 1 | 0 | 1 | 13 |
| TOTAL | | 24 | 5 | 6 | 21 | 6 | 28 | 8 | 21 | 20 | 11 | 150 |

NUMBER OF 'BELOW' RESPONSES TO 'EMPTY' TRIALS (MAX POSSIBLE = 4)

| TRIAL NO | | 7 | 12 | 15 | 17 | 22 | 26 | 33 | 37 | 40 | 46 | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| ORIENTATION | | Λ | V | V | Λ | V | Λ | V | Λ | Λ | V | TOTAL |
| | 1 | 1 | 4 | 4 | 1 | 4 | 0 | 3 | 1 | 0 | 3 | 21 |
| S | 2 | 0 | 4 | 4 | 2 | 4 | 0 | 4 | 1 | 0 | 2 | 21 |
| U | 3 | 3 | 3 | 0 | 1 | 1 | 0 | 2 | 3 | 3 | 1 | 17 |
| B | 4 | 1 | 4 | 4 | 0 | 3 | 1 | 3 | 3 | 1 | 2 | 22 |
| J | 5 | 3 | 3 | 4 | 3 | 3 | 2 | 3 | 4 | 3 | 2 | 30 |
| E | 6 | 2 | 4 | 4 | 2 | 4 | 1 | 4 | 1 | 2 | 4 | 28 |
| C | 7 | 0 | 4 | 4 | 2 | 4 | 1 | 4 | 1 | 3 | 4 | 27 |
| T | 8 | 2 | 4 | 4 | 2 | 4 | 2 | 3 | 0 | 2 | 4 | 27 |
| S | 9 | 2 | 4 | 4 | 2 | 4 | 2 | 4 | 2 | 2 | 4 | 30 |
| | 10 | 2 | 1 | 2 | 4 | 3 | 3 | 2 | 3 | 4 | 3 | 27 |
| TOTAL | | 16 | 35 | 34 | 19 | 34 | 12 | 32 | 19 | 20 | 29 | 250 |

NUMBER OF 'APEX' RESPONSES TO 'EMPTY' TRIALS (MAX POSSIBLE = 4)

| TRIAL NO | | 7 | 12 | 15 | 17 | 22 | 26 | 33 | 37 | 40 | 46 | TOTAL |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| ORIENTATION | | Λ | V | V | Λ | V | Λ | V | Λ | Λ | V | |
| | 1 | 3 | 4 | 4 | 3 | 4 | 4 | 3 | 3 | 4 | 3 | 35 |
| S | 2 | 4 | 4 | 4 | 2 | 4 | 4 | 4 | 3 | 4 | 2 | 35 |
| U | 3 | 1 | 3 | 0 | 3 | 1 | 4 | 2 | 1 | 1 | 1 | 17 |
| B | 4 | 3 | 4 | 4 | 4 | 3 | 3 | 3 | 1 | 3 | 2 | 30 |
| J | 5 | 1 | 3 | 4 | 1 | 3 | 2 | 3 | 0 | 1 | 2 | 20 |
| E | 6 | 2 | 4 | 4 | 2 | 4 | 3 | 4 | 3 | 2 | 4 | 32 |
| C | 7 | 4 | 4 | 4 | 2 | 4 | 3 | 4 | 2 | 1 | 4 | 32 |
| T | 8 | 2 | 4 | 4 | 2 | 4 | 2 | 3 | 4 | 2 | 4 | 31 |
| S | 9 | 2 | 4 | 4 | 2 | 4 | 2 | 4 | 2 | 2 | 4 | 30 |
| | 10 | 2 | 1 | 2 | 0 | 3 | 1 | 2 | 1 | 0 | 3 | 15 |
| TOTAL | | 24 | 35 | 34 | 21 | 34 | 28 | 32 | 20 | 20 | 29 | 277 |

NUMBER OF 'BASE' RESPONSES TO 'EMPTY' TRIALS (MAX POSSIBLE = 4)

| TRIAL NO | | 7 | 12 | 15 | 17 | 22 | 26 | 33 | 37 | 40 | 46 | TOTAL |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| ORIENTATION | | Λ | V | V | Λ | V | Λ | V | Λ | Λ | V | |
| | 1 | 1 | 0 | 0 | 1 | 0 | 0 | 1 | 1 | 0 | 1 | 5 |
| S | 2 | 0 | 0 | 0 | 2 | 0 | 0 | 0 | 1 | 0 | 2 | 5 |
| U | 3 | 3 | 1 | 4 | 1 | 3 | 0 | 2 | 3 | 3 | 3 | 23 |
| B | 4 | 1 | 0 | 0 | 0 | 1 | 1 | 1 | 3 | 1 | 2 | 10 |
| J | 5 | 3 | 1 | 0 | 3 | 1 | 2 | 1 | 4 | 3 | 2 | 20 |
| E | 6 | 2 | 0 | 0 | 2 | 0 | 1 | 0 | 1 | 2 | 0 | 8 |
| C | 7 | 0 | 0 | 0 | 2 | 0 | 1 | 0 | 2 | 3 | 0 | 8 |
| T | 8 | 2 | 0 | 0 | 2 | 0 | 2 | 1 | 0 | 2 | 0 | 9 |
| S | 9 | 2 | 0 | 0 | 2 | 0 | 2 | 0 | 2 | 2 | 0 | 10 |
| | 10 | 2 | 3 | 2 | 4 | 1 | 3 | 2 | 3 | 4 | 1 | 25 |
| TOTAL | | 16 | 5 | 6 | 19 | 6 | 12 | 8 | 20 | 20 | 11 | 123 |

The key below gives the order in which the figures were presented and also the number of errors each subject made to each figure (maximum was four). Note that the blocks were numbered as follows:-

6 7 8 9 10
1 2 3 4 5

and

1 2 3 4 5
6 7 8 9 10

| TRIAL | ORIENTATION | BLOCK NO | CORRECT RESPONSE | NUMBER OF ERRORS (MAXIMUM FOUR) | | | | | | | | | | TOTAL |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | |
| 1 | V | 5 | BELOW | | | | | | | | | | | 0 |
| 2 | V | 1 | B | | | | | 1 | | | | | 1 | 2 |
| 3 | Λ | 2 | ABOVE | 1 | | 1 | | 2 | 1 | | 1 | 1 | | 7 |
| 4 | Λ | 1 | A | 1 | 1 | 1 | 1 | | 1 | | 1 | | 1 | 7 |
| 5 | V | 4 | B | | | | | 1 | 1 | | | | | 2 |
| 6 | Λ | 10 | B | 2 | 1 | 1 | 2 | 2 | 3 | 3 | 4 | 2 | 1 | 21 |
| 7 | Λ | BLANK | - | | | | | | | | | | | - |
| 8 | V | 5 | B | | | | | 1 | | | | | | 1 |
| 9 | V | 4 | B | | | | | 1 | | 1 | | | 1 | 3 |
| 10 | V | 6 | A | 3 | 4 | 4 | 4 | 4 | 4 | 4 | 4 | 4 | | 35 |
| 11 | V | 1 | B | | | | 1 | 2 | | | | | 1 | 4 |
| 12 | V | BLANK | - | | | | | | | | | | | - |
| 13 | Λ | 3 | A | | | 1 | | 2 | 1 | 1 | 1 | | 1 | 7 |
| 14 | Λ | 7 | B | 1 | 1 | | 2 | 2 | 3 | 1 | 3 | 2 | | 15 |
| 15 | V | BLANK | - | | | | | | | | | | | - |
| 16 | V | 9 | A | 4 | 4 | | 4 | 3 | 4 | 3 | 4 | 4 | 1 | 31 |
| 17 | Λ | BLANK | - | | | | | | | | | | | - |
| 18 | V | 10 | A | 4 | 3 | | 2 | 4 | 4 | 4 | 4 | 4 | 3 | 32 |
| 19 | Λ | 8 | B | 2 | | 1 | | | 2 | 2 | 3 | 1 | | 11 |
| 20 | V | 9 | A | 4 | 3 | 1 | 1 | 1 | 4 | 2 | 2 | 4 | | 22 |
| 21 | Λ | 6 | B | | | | 2 | 1 | 1 | 1 | 2 | | | 7 |
| 22 | V | BLANK | - | | | | | | | | | | | - |
| 23 | V | 7 | A | 4 | 3 | | 4 | 4 | 4 | 1 | | 3 | | 23 |
| 24 | Λ | 5 | A | | | 2 | 1 | 2 | | 1 | 2 | 2 | 3 | 13 |
| 25 | V | 2 | B | | | | 1 | | | | | 1 | 2 | 4 |
| 26 | Λ | BLANK | - | | | | | | | | | | | - |
| 27 | Λ | 3 | A | 1 | 1 | | 1 | | | 1 | | 1 | 2 | 7 |
| 28 | V | 8 | A | 3 | 2 | 2 | 2 | 3 | 4 | 2 | 1 | 2 | | 21 |
| 29 | Λ | 9 | B | | 1 | 2 | 2 | 2 | 3 | 2 | 2 | 2 | | 16 |
| 30 | V | 3 | B | | | | | | | | | | | 0 |
| 31 | Λ | 9 | B | 1 | 1 | | 2 | 1 | 3 | 1 | 3 | 1 | 1 | 14 |
| 32 | Λ | 5 | A | 1 | 1 | 2 | 2 | 1 | 1 | 1 | | 1 | 3 | 13 |
| 33 | V | BLANK | - | | | | | | | | | | | - |
| 34 | V | 10 | A | 4 | 2 | 1 | 4 | 3 | 4 | 2 | 2 | 4 | 1 | 27 |
| 35 | Λ | 4 | A | | | | | | | | | 2 | 1 | 3 |
| 36 | Λ | 10 | B | | | 1 | 2 | 1 | | | | | 1 | 5 |
| 37 | Λ | BLANK | - | | | | | | | | | | | - |
| 38 | Λ | 6 | B | | | | 1 | | | | | | | 1 |
| 39 | Λ | 1 | A | 1 | | 2 | | 1 | | | | 1 | | 5 |
| 40 | Λ | BLANK | - | | | | | | | | | | | - |
| 41 | V | 7 | A | 3 | | 1 | | 1 | 2 | | | 2 | | 9 |
| 42 | Λ | 8 | B | | | | | | 1 | | | | | 1 |
| 43 | V | 6 | A | 4 | 2 | 3 | 3 | | 4 | 1 | 2 | 4 | | 23 |
| 44 | Λ | 4 | A | | | | 1 | 1 | | | | 3 | 1 | 6 |
| 45 | V | 8 | A | 2 | | 1 | 1 | | 3 | | | 4 | | 11 |
| 46 | V | BLANK | - | | | | | | | | | | | - |
| 47 | Λ | 2 | A | 1 | | 1 | | | 1 | | | 2 | | 5 |
| 48 | V | 2 | B | | | | | | | | | | | 0 |
| 49 | V | 3 | B | | | | | | | | | | | 0 |
| 50 | Λ | 7 | B | 1 | | | 1 | | 4 | 2 | | | 2 | 10 |
| | | TOTALS | | 48 | 30 | 28 | 47 | 47 | 63 | 36 | 41 | 57 | 27 | 424 |

The total number of responses from all ten subjects, excluding blank trials was 1600. 424 errors were made hence the correct response was given to 73·5% of the trials.

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