CROSS-MODAL MATCHING IN FIRST SCHOOL CHILDREN

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Thesis submitted in fulfilment of degree of D.Phil.,

Oxford University, November 1985

ACKNOWLEDGEMENTS

This thesis has been made possible by the contributions of many people. I should particularly like to express my gratitude to the following:

My supervisor, Dr Susanna Millar, for valued discussion and advice, patient reading of successive drafts of this thesis, and for making it possible for me to begin my research at all.

Dr J. Bithell and Dr I.G. Vlachonikolis for advice on statistical matters.

The Medical Research Council, for supporting me and my research for three years.

The Oxfordshire County Education Officer, and the staff and pupils of the following schools in the Oxford area, for their co-operation which made the research possible: Church Cowley St. James C.E. First, Cowley St. Christopher C.E. First, East Oxford County First, Harebell County First, Larkrise County First, Orchard Meadow County First, Our Lady's R.C. First, Overmede County First, Rose Hill County First, St. Barnabas C.E. First, St. Francis C.E. First, St. John Fisher R.C. First, SS. Mary and John C.E. First, SS. Philip and James C.E. First and Speedwell County First.

The University of Surrey, for providing facilities for the final production of this thesis.

My wife, Kristine, for invaluable encouragement and support, and for helping in the considerable task of typing and proof-reading this thesis, and with other friends in Oxford, for helping to transport the apparatus to schools, and for generally providing an enjoyable setting in which to work.

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D.Phil, Michaelmas Term 1985

ABSTRACT

This research examines how cross-modal and within-modal matching differ. Three broad classes of difference are considered, that cross-modal matching requires (a) "translation" between modality-specific stores or (b) "transformation" of information when different information is available through each modality or (c) transformation, whatever the information available through each modality, owing to differences in encoding strategy.

Visual-kinaesthetic matching of the end-point of lever movements has been investigated. It is argued that adult cross-modal performance depends on information and strategy, not directly on modality. Results with children have been less clear, experiments were therefore undertaken, with subjects aged 6-9 years. The hypothesis was that childrens' performance also would be determined by available information, and strategy.

With information differences eliminated, modality conditions did not differ in within-subject variability, with up to 20 second unfilled retention intervals.

With visual information enhanced by background cues and emphasis of the body midline, visual matching was superior to kinaesthetic, and within-modal was superior to cross-modal matching. These differences disappeared with practice, together with coding relative to the midline in the cross-modal conditions. Midline-relative coding was the norm with the midline emphasised, and without background cues.

With or without variability differences, VV did not differ from KK in bias, but KV resulted in greater overshooting, VK greater undershooting. The most likely explanation is resistance to movement when perception is kinaesthetic, causing overestimation of distance moved.

Consideration of the information normally available to subjects, generated the hypothesis that temporal and spatial parameters should interact more with kinaesthetic than with visual perception. This was supported, since movement velocity biased only kinaesthetic judgements.

It is concluded that matching performance depends on the information encoded and used as the basis of matching, which depends on strategy; strategy depends on information (a) available during stimulus presentation, (b) normally available in each modality, (c) which it is anticipated will be available during response.

CONTENTS

CHAPTER or SECTION	PAGE	
	7	LONG ABSTRACT
1	17	THEORETICAL BACKGROUND
[1.1]	17	The Subject Matter
[1.2]	27	The Questions
[1.3]	42	Review of Studies of Visual-Kinaesthetic Matching
[1.4]	66	A Comparison of Visual-Kinaesthetic (Length) Matching with Visual-Haptic (Shape) Matching
2	73	METHODOLOGY
[2.1]	73	Introduction
[2.2]	73	Subjects
[2.3]	76	Apparatus
[2.4]	85	Procedure
[2.5]	87	The Stimuli
[2.6]	89	Error Measures
[2.7]	103	Statistical Procedures
[2.8]	104	Presentation of Data
3	105	THE EXPERIMENTS, PART A
	107	Experiment A1a
	123	Experiment A1b
	140	Experiment A2a
	151	Experiment A2b
	158	Experiment A3
	165	Experiment A4
	173	Experiment A5

CHAPTER or SECTION	PAGE	
4	187	THE EXPERIMENTS, PART B
	196	Experiment B1
	203	Experiment B2
	211	Experiment B3
	220	Experiment B4
5	228	THE EXPERIMENTS, PART C
	232	Experiment C1
	237	Experiment C2
	241	Experiment C3
6	247	THE EXPERIMENTS, PART D
	250	Experiment D1
	259	Experiment D2
	269	Experiment D3
7	282	DISCUSSION
[7.1]	282	Introduction
[7.2]	284	Effects on Variability
[7.3]	311	Effects on Bias
[7.4]	324	Information and Strategy: A Model
[7.5]	330	Suggestions for Further Work
	336	REFERENCES
	352	APPENDICES
A	352	ANOVA Tables for Experiments in Chapter Three
B	370	ANOVA Tables for Experiments in Chapter Four
C	371	ANOVA Tables for Experiments in Chapter Five
D	375	ANOVA Tables for Experiments in Chapter Six
E	382	ANOVA and Mean Tables for Analysis of Absolute Errors
F	400	The Effect of Using an Arc Movement
G	409	Supplementary Analysis of Experiment B3

LONG ABSTRACT

CHAPTER ONE introduces the theoretical background to the experiments reported in later chapters.

The question is asked, in what way or ways does cross-modal differ from within-modal matching? Three broad classes of the possible differences are described:

(a) Storage and processing of information depend upon the modality through which the information is presented, so that cross-modal matching requires a "translation" between modality-specific stores.

(b) The information provided by a stimulus object or event commonly depends on the modality by which it is perceived, so that cross-modal matching will normally require a "transformation" of information, or selective use of encoded information, in order for inputs from different modalities to be compatible.

(c) Subjects adopt different encoding strategies with different presentation modalities, hence different information may be encoded, whether or not the same information is available through each modality. Consequently transformation of information, or a change of strategy, is required in cross-modal matching.

Assessment of these three classes of account is attempted, specifically for visual kinaesthetic matching of the end-point (position) of lever movements, by six- to nine-year-old children. In two preliminary experiments, adult subjects and length matching were also included. The overall hypothesis is tested, that information and strategy are important, but that processing and storage do not depend on input modality per se.

Theories of cross-modal matching are discussed in terms of the three broad classes noted above, and in light of data from the

literature on both adults and children. The research is concerned primarily with the broad classes, not with the theories of particular psychologists, but particular attention is given to Connolly and Jones' (1970) model, being the most precise available, as a useful starting point for experimentation and theoretical discussion.

Results of visual-kinaesthetic matching in adults have not been entirely consistent, and interpretation of them is confused by methodological problems. In general, they have shown that within-modal matching is more accurate than cross-modal, and that within-modal visual matching (VV) is more accurate than within-modal kinaesthetic matching (KK). In addition, when there is a difference between the two cross-modal conditions, kinaesthetic-visual matching (KV) is with one exception more accurate than visual-kinaesthetic matching (VK).

It is argued that the general pattern of differences between modality conditions, and exceptions to the general pattern, can be explained by reference to differences in the information available through each modality, differences in strategy, and misinterpretation of absolute error results. Hence, there is no need to hypothesise any role of modality-specific stores or processing.

It is also argued that effects of an unfilled retention interval can be explained in similar fashion. These effects appear to depend on modality conditions only to the extent that different information is encoded through different modalities.

Studies of visual-kinaesthetic matching by children did not permit the detailed analysis of task conditions which was possible for adults. Modality effects could be explained by reference to information and strategy, but it was not clear under what conditions performance is equally accurate in all modality conditions, or how task conditions affect performance. The research problem dealt with

in this thesis is essentially to begin to fill this gap in the literature.

As for developmental trends, it was predicted that there would be only an overall improvement with age, within the age-range studied, and between this range and adulthood: no specific improvement was predicted in any one modality condition, or in the within-modal conditions.

The final part of Chapter One is a discussion of different results which have been found for visual-haptic shape matching, and visual-kinaesthetic length matching. It is again argued that information, strategy and error measures account for the observed differences.

CHAPTER TWO describes the general method used in the experiments, and seeks to explain and justify this method.

The subjects were six to nine-year-old normal children, and in one experiment adults. All were right-handed and had no visual impairment. Equal numbers of males and females were tested, and any one subject took part in only one experiment.

The apparatus was a pair of pivoted levers, coupled to each other so that they moved in synchrony, and housed in a wooden box. One lever could be grasped by the subject by reaching through a curtain designed to exclude light without dragging on the subject's arm. A small dim light was mounted at the top of the lever, so that when presentation was visual, the subject looked into the apparatus through a light-excluding aperture, and saw only a small light moving in darkness.

Movements of the subject's lever were controlled by the experimenter in each modality condition, via the second lever. The subject passively followed the movements with his hand in the

kinaesthetic condition. The experimenter also controlled a shutter which was used to hide and display the lever visually. Lever positions were read off a protractor mounted behind the experimenter's lever.

The creation of a small dark space in this way is an alternative to the technique usually used with adults, namely extinguishing the room lights. Advantages and disadvantages associated with each alternative are discussed.

Error measures are discussed at length. Reasons are given for not analysing absolute errors, and for instead analysing variable errors based on only two replications per subject ("within-subject deviation") and a measure of between-subject variability. Constant errors measure bias to undershoot or overshoot. Within-subject deviation measures performance efficiency and strategy variation. Between-subject variability is a measure which aids discrimination between efficiency and strategy variation effects.

CHAPTER THREE presents experiments in which the information available was varied. In most published studies, the information available differs between modality conditions. Before explaining modality effects in terms of hypothetical differences (modality-specific stores and strategies), actual differences in information should be controlled for.

First, therefore, a task was designed, in which the same information was made available through each modality. The means by which this was achieved are derived from the review of the literature in Chapter One, and from Experiments A1a, A1b and A2a. Important aspects of the procedure are: (a) using exactly the same lever movements in each modality condition, so that both visual and kinaesthetic presentation were successive, rather than simultaneous,

and passive; (b) eliminating all visual external cues; (c) making the subject's midline an obvious reference point by starting each movement from this position; (d) specifying that the end-point of the movement was to be remembered, not the length, and making length an unreliable cue by changing the starting position of the movement between acquisition and response, and (e) maintaining the same head-arm-apparatus relation throughout each trial.

Under these conditions of equated information, there were no differences in variability between modality conditions. This was the case with an unfilled retention interval of 5, 15, or 20 seconds. The conclusion was reached that, as with adults, there is no need to hypothesise a role of modality-specific stores and processing in within- and cross-modal matching performance. In other words, there is no evidence in these experiments for any specifically cross-modal development between 6 years and adulthood.

In no modality condition was performance affected by an unfilled retention interval. The finding that adults performance is affected by a retention interval in all four modality conditions (Marteniuk and Rodney 1979) is explained by their subjects encoding length not position. Hence, the cue employed is more important than the modality in this respect.

In the process of experimentally designing the task referred to above, two important results were found. First, it appears that subjects in an earlier version of the task were not always able to correctly select the position or the length cue as required. Errors of this kind, termed "criterion errors", resulted in a large constant bias in some conditions.

Criterion errors were more frequent in KK than in VV (only these modality conditions were used in the experiments concerned) and more frequent in females than in males. Both these effects could be

explained by the subject being distracted.

Second, it was found that with visual presentation, subjects would code relative to the edges of the viewing aperture when these were visible. This appeared not to be advantageous, since errors decreased when this strategy was prevented. It is presumed that this is because the visible cues were not stable since the subject's head was not likely to be in the same orientation during acquisition and response. Susceptibility to these misleading cues was the main factor resulting in superior performance by adults.

Having established conditions under which modality conditions did not differ in variability, experiments could be performed to assess the effects of differences in information. Visual presentation was enhanced by providing stable external visual cues. Kinaesthetically presented stimuli were made more difficult to encode by not emphasising the body midline as a reference point.

As was predicted, VV performance was less variable than KK under these conditions, and within-modal matching was less variable than cross-modal matching. Thus, within-modal performance depends on the available information, rather than on modality as such, and cross-modal performance suffers when different information is made available through each modality.

However, the effect of modality was significant only in the first of the two sessions given to each subject. This disappearance of the modality effect was associated with a change in strategy: only in the first session was there evidence for coding relative to the midline. Thus, an interaction between information and strategy determined the differences among modality conditions.

In addition to the variability results noted here, there were important effects of bias, which are further examined in the next two chapters.

CHAPTER FOUR is concerned with the effect of modality on constant errors. In Chapter Three, the following pattern was found: VV did not differ from KK, VK resulted in greater bias to undershoot, KV resulted in greater bias to overshoot.

This pattern was found whether or not variability differed between modality conditions. Therefore, it was considered that an explanation in terms of translation, or transformation is not viable.

An alternative hypothesis is that four independent sources of bias arise in visual and kinaesthetic acquisition and response. This was considered unlikely, since the same pattern of modality differences is found in a variety of published experiments having widely differing methods. A specific form of this hypothesis, that the correct response position is anticipated or respond only when passed, according to modality condition, was proposed. It was rejected when the same pattern of differences was found when same-different judgements were used.

A third hypothesis was tested: that kinaesthetically perceived lengths are overestimated relative to visually perceived length. Psychophysical scaling differences supported this hypothesis, and length estimation provided more equivocal support. The most likely source of this difference is argued to be resistance to movement experienced when perception is kinaesthetic. This effect of resistance is pointed out to be itself an important consideration in experimental design.

CHAPTER FIVE examines coding relative to the body midline. It was consistently found in experiments in previous chapters, that undershooting was greater when the position to be reproduced was further from the midline. The length of the movement by which the standard position was approached was the same for each standard

position, hence the effect of standard position must be accounted for by position relative to the midline.

In this chapter, the effect of standard position is shown not to be an artifact of the task, resulting from the fact that the response movement crossed the midline only with the further position.

The effect of standard was absent in some experiments, in the VV condition. It is shown that this can be explained by an effect of strategy, resulting from subtle differences in wording or understanding of the instructions. The standard effect was found in VV when the subject was biased to continuously update a representation of the position of the lever, rather than code its position after the acquisition movement had terminated.

CHAPTER SIX presents experiments on a further aspect of strategy. Consideration of the information which is normally available to the subject, when working with each modality, generated the hypothesis that temporal and spatial parameters should interact more when perception is kinaesthetic than when it is visual. This hypothesis was tested by making the velocity of the movement used to present the standard position either faster or slower than that of the response movement. The hypothesis was supported, in that velocity was found to bias kinaesthetic, but not visual judgements of position: a fast acquisition movement resulted in undershooting errors, a slow acquisition movement resulted in overshooting.

While visual position judgements were never biased by the velocity the effect on kinaesthetic perception depended in a complex manner on age, response modality, standard position and practice. It is suggested that this can be accounted for in terms of the strategy employed by the subject: whether there is an attempt to take into account the change in velocity, or to ignore velocity altogether. The

results are seen as requiring modification of the explanation of the "tau effect" proposed by Jones and Huang (1982).

Modality conditions did not differ in variability with fast or intermediate presentation movements, except for an interaction between age and modality in Experiment D3. This interaction resulted from a trend for errors to be highest in 6-year-olds when acquisition was visual, but in 9-year-olds when acquisition was kinaesthetic. With a slower movement, kinaesthetic presentation resulted in higher variability.

CHAPTER SEVEN draws together conclusions and makes suggestions for further work. The individual main results have already been noted in this abstract. The experiments described contribute to an understanding of how particular aspects of information and strategy affect performance. The overall conclusion is that, for the task studied, differences between modality conditions depend on differences in information available, and on strategy, not on modality-specific stores and processing.

This appears to be the case from at least as young as six onwards. The research reported therefore both clarifies the factors affecting children's performance and supports the analysis of adult performance given in Chapter One. The particular aspects of information which have been shown to be important are discussed.

Conclusions are drawn as much from effects on bias as from effects on variability. Bias was in particular found to be an important indicator of strategy.

The implications of the research for theories of cross-modal matching are discussed. A model is proposed, in which performance depends interactively on information and strategy: the information which is encoded and employed as the basis for matching depends on

strategy; strategy in turn depends on the information which is available during stimulus presentation, the information which is normally available and reliable with the modality being used, and information which it is anticipated will be available during response.

Further work is suggested in three directions: further specification of the important task parameters; attempts to analyse more complex and adaptively important behaviours; and tests of the generality of the conclusions reached here, with other tasks and pairs of modalities.

It is hoped that a general theory of matching can be generated, which deals with the information received, not the channel by which it is received.

CHAPTER ONE: THEORETICAL BACKGROUND

[1.1] THE SUBJECT MATTER

The research which is reported in this thesis may broadly be classified as being concerned with the psychology of intersensory phenomena. A comprehensive review of this field of study will not be attempted. Rather, in this section, an attempt is made to place the research to be reported in context in the wider field.

The research is concerned with children's visual-kinaesthetic matching of positions in space. The age range of the child subjects was six to nine years. Previous research on visual-kinaesthetic matching in this age range, and on adults, has yielded contradictory results and conclusions. A review of work on adults (Section [1.3.2]) showed that the contradictory results could be explained by reference to (a) the amount and type of information provided by the experimenter, in each modality, and (b) strategies employed by the subject. Modality appears to be important only in determining what information is available and what strategies are used.

Research on children (Section [1.3.3]) was found to be less readily interpretable, there having been fewer studies, and consequently a less complete set of combinations of information and strategy variables. The research presented in this thesis was intended to begin to fill this gap in the literature. The overall purpose of the research was therefore to demonstrate the importance of the amount and type of information provided by the experimenter, and the strategies employed by the subject, compared with differences between sense modalities as such.

Constraints were imposed, in order to make the research problems manageable, by limiting the area within which experiments

were to be carried out. The remainder of this section is an attempt to specify this area, and to give some justification for selecting the specified area. The next section gives a more detailed account of the questions which were asked.

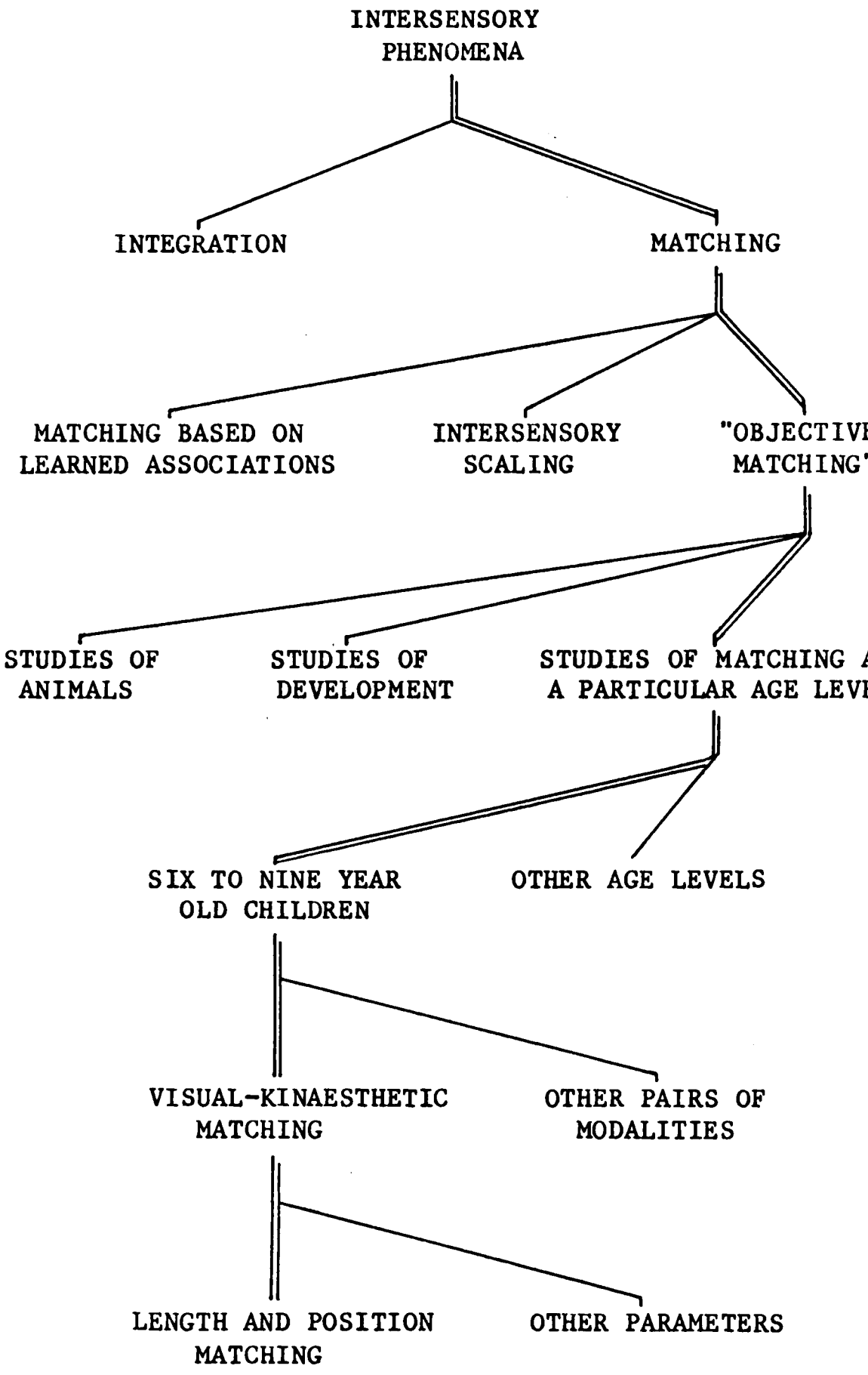
Figure [1.1.1] sets out the broad scheme of the discussion which follows. It will be clear that the order in which I have subdivided the field is largely arbitrary, and incomplete. This is because my purpose here is to place my own work in context, and not to provide a classification system for general use.

The first distinction which I should like to make is between cross-modal integration and cross-modal matching. The term "integration" is sometimes used to refer to an ontogenetic process by which initially separate sensory modalities become integrated. This is not the meaning with which I use the term here. I use the term "integration" to refer to the process of combining information from different modalities, to enable a deduction to be made, of the form "A and B implies C", or "A implies C, B confirms C".

Examples may help to clarify what is meant. When we make judgements of our orientation and movements in space, it is clear that we can combine vestibular, visual, tactual and auditory information (see Lackner 1981, for example). On a more anecdotal level, consider a pedestrian who is trying to cross a road. The safety-conscious individual will cross when no traffic can be seen approaching, and when no unseen traffic can be heard approaching: no sight of traffic implies it is safe, no sound of traffic confirms it is safe.

Matching is a logically simpler process, involving a decision of the form A = B. More precisely, matching is the process whereby a stimulus perceived by one sense modality is associated with a stimulus perceived by a second sense modality. Matching may be

Figure [1.1.1] The Subject-Matter in Context



The research reported in this thesis may be classified by following down the double line. See text for detailed account.

carried out by an organism at the same time as integration. For example, our wary pedestrian may associate a particular sound, of a lorry perhaps, with a particular sight. The two processes are, however, logically distinct and should be discussed and researched as such.

In order for cross-modal matching to be possible, clearly there must be perception in one modality which can be said to be in some way equivalent to perception in another modality. I have distinguished three types of matching on the basis of three types of equivalence.

First, there is "intersensory scaling". An example of this would be matching a light intensity to a sound intensity (e.g. Stevens and Marks 1965). The basis of matching is the subjective impression of intensity, and there is strictly no physical equivalence between the stimuli. Synaesthesia also entails subjectively based matching, and can be thought of as a type of approximate intersensory scaling. For example, a point on a scale of auditory frequency might be matched to a point on a scale of colour (frequency of electromagnetic radiation).

Although matching of this type is "subjective" in the sense that there is not a physically defined correct match, this does not necessarily imply a lack of consistency between individuals. Even so, the term "matching", is perhaps not entirely appropriate. The process involved is strictly one of describing stimuli as being analogous.

A second class of matching is matching based on learned associations. For example, the visual features of an apple can be associated with its odour. Few people who have had much contact with apples would fail to choose a picture of an apple, from a set of pictures of foods, if given apple odour as a cue. In general, an object or event can be matched between sense modalities when quite

different physical characteristics are perceived in each modality. A particularly important example of this is the association of the visual shape with the sound of letters. The shape and the sound are quite arbitrarily associated, yet they are associated, and can readily be matched once the association has been learned.

The third type of matching I have called "objective matching": this refers to cases where the same physical features of an object or event are detected through more than one modality. For example, the shape of an object can be perceived both visually and tactually. Matching of this type can be defined to have been performed correctly when the subject's judgement of a match corresponds to an objective, physically defined match. It is with this class of matching that my experiments have been concerned, because it provides an index of non-arbitrary relations between the sense modalities, and of supra-modal perception.

Research on objective matching may be classified according to who the subjects were. Experimental studies of humans fall into two broad categories on this basis, the categories corresponding to two major approaches to the study of child development. Some studies have examined the process of development of cross-modal abilities in children and infants. Others have examined performance at a particular age level. Studies of the second type have been limited to adults and infants. The research reported here is an attempt to investigate in detail the performance of children within a narrow age range. It is the writer's belief that such research is essential before developmental processes can properly be understood.

The age-range covered by previous studies of children, using comparable stimuli, is 4 to 11 years (Connolly and Jones 1970, Millar 1972, 1975). The range covered in the experiments reported in this thesis is 6 to 9 years. It was anticipated that some developmental

trends would be revealed within this age-range, but the main intended developmental comparison was with conclusions of previous studies of adults. Further discussion of age as a variable is given in Section [1.3.3], where developmental studies of visual-kinaesthetic matching are reviewed.

The particular pair of modalities with which I have been concerned is vision and kinaesthesis. Before giving reasons for this choice, it would be useful to briefly define the terms that I use to refer to different sense modalities. The four terms tactile, haptic, kinaesthetic and proprioceptive are widely used in the literature on matching, often interchangeably or with implicit meanings which differ between reports.

I reserve the terms "touch", "tactual", "tactile" to refer to perception of texture and form, using the skin, but not muscle or joint organs. Thus, if a shape is pressed against a subject's skin, the shape is said to be perceived tactually, by touch, or using the tactile sense.

If the subject feels around the shape with a finger, this is not referred to as tactile, but as haptic perception. Thus, "haptic" perception is perception to which the skin and muscle and/or joint organs contribute. If the subject perceives a texture, or a Braille letter, by moving a finger across the stimulus, this is regarded as being tactile perception. This is because although the muscles and joints are being used, they serve a motor, not a sensory function in this case.

I use the terms "kinaesthesis" and "kinaesthetic" to refer to perception, by the muscle and joint organs, of movement and force. "Proprioception" is used with the broader meaning of perception of position and movement of parts of the body in relation to each other, by any sense modality.

It is felt that these definitions according to the sense organ used are needed, because whereas four of the five classical senses are defined in this way (vision, hearing, taste and smell), the remaining sense (or senses) cannot claim such a precise historical definition. This has lead to confusion of issues. Walk and Pick (1981), for example, ask what is the sense organ for the proprioceptive sense (given that proprioception depends on more than one set of sense organs). This issue is only a problem if proprioception is considered to be a sense modality in the same way that vision and hearing are sense modalities, which plainly is not the case.

Part of the problem of defining sense modalities resides in the conflict between anatomical/physiological definitions and our experience of perception. I here define hearing, for example, as perception mediated by the basilar membrane in the ear. Experientially, hearing includes certain "illusions" in which non-auditory inputs give rise to sensations which are normally associated with auditory inputs. For example, visually perceived location can bias auditorily perceived location if there is a conflict between the two (e.g. Warren and Pick 1970).

In order to speak of matching or integration between sense modalities, the senses must be defined in such a way that they are at some stage separate systems. The minimum requirement of a definition is separation at the input stage. This matter is referred to again in Section [1.2.2].

Interest in visual-kinaesthetic matching was stimulated by two features of the literature. First, it is with this pair of modalities that cross-modal matching is most consistently found apparently to be inferior to within-modal matching (see Section [1.3]). As will be elaborated in the next section, the main hypothesis in the research

to be reported is that "modality" effects can be explained in terms of the information and strategy associated with each modality of presentation of stimuli. Visual-kinaesthetic matching, since it consistently results in a cross-modal deficit, provides a strong test of the hypothesis.

Second, there are discrepancies in the results of studies of visual-kinaesthetic matching, which are difficult to explain on existing models (see Section [1.3]). It is hypothesised here that these discrepancies can be explained if sufficient attention is given to the differences in information and strategy between experiments.

Returning to Figure [1.1.1], the final division is based on the parameters of the object or event which are to be matched. The parameters which can be perceived both by vision and by kinaesthesis can be reduced to distance, time, and functions of these. Examples of such functions are area, volume, velocity, acceleration, and patterns of these.

With kinaesthetic presentation, it is not possible to present temporal stimuli independently of spatial stimuli, except by static force, which cannot be perceived visually. For example, to present a distance or length, a movement is required. Even if the separation of the subject's hands is used to indicate the distance, the hands must be moved to the desired separation. Presentation of a distance therefore entails presentation of a velocity and of a duration.

Visually, independent presentation is possible, of for example, distance and duration. This is because different positions can be simultaneously perceived, without movement to or between locations. However, if a movement is used to present a distance or a duration, then the other of these two is necessarily also presented.

As a consequence, if a subject in an experiment is asked to reproduce a movement, it may not be clear to the experimenter which

parameter of the movement the subject is using in order to reproduce the movement. This ambiguity is discussed further in Chapter Six. For the present, the parameter which is matched will be defined in terms of the criterion for a correct response. For example, if the subject were required to match a movement of a particular length, this would be called length matching, even though the subject might be responding partly or wholly on the basis of the velocity and duration of the movement. This definition is based on the fact that the length could be accurately matched without reproduction of the original velocity and duration.

Allowing for the ambiguity noted above, the parameters involved in the research to be reported can be labelled "length" and "position". Length matching is a task in which the subject is required to match the distance between the start and end of a movement, regardless of the location of the start of the movement. In other words, the subject must move a specified distance from a starting position, wherever that position might be. Position matching is a task in which the subject is required to match a position in space, regardless of the movement by which the position is approached. In Section [1.3.2], research on these two parameters is discussed, and reasons are given for including both tasks in the first two experiments.

Length and position were chosen for study, in preference to other possible parameters, largely for practical reasons. There are technical difficulties in presenting velocity and acceleration, and the subject's response would similarly be difficult to record. This is an especially acute problem when the apparatus must be transported to and between schools. These difficulties could be overcome, but there would remain the problem of the difficulty that children have with concepts of velocity and, by extension, with acceleration

(Piaget 1946). Matching cannot be studied unless the subjects understand, and the experimenter knows they understand, which parameter of the stimulus is to be matched.

Cross-modal matching of durations has been studied using temporal patterns in visual-auditory matching (see reviews by Freides 1974, Jones 1981). Studies of visual-kinaesthetic matching have all, however, employed spatial or spatio-temporal stimuli. This is not altogether surprising in view of what has already been said about the difficulties of presenting durations kinaesthetically, in such a way that spatial information is not also given.

There is also a problem which is inherent in any study of duration matching. Matching of any duration greater than about one second could be performed by counting. There is good evidence (see Section [1.2]) that cross-modal matching can be performed without using language. This does not mean, however, that language is never a mediating process in matching tasks. However, it was my intention to study cross-modal matching, not counting ability. Regarding durations of less than a second, it is likely that within-modal inaccuracy and differences in afterimage duration would limit the usefulness of any experimental study.

Static force, or spatial parameters, can be used to present temporal patterns kinaesthetically. In both cases however, tactual perception could be important. Also, spatial variation would obviously make it difficult to specify to young subjects that the temporal pattern should be matched, while completely ignoring the spatial pattern. A static force cannot be perceived visually, (except indirectly, by inference) therefore if this option were taken, any apparently cross-modal effects could equally be interpreted as being cross-parameter effects.

To summarise, length and position were chosen for study.

Problems with the study of other parameters are considerable, although not necessarily insurmountable.

The area was chosen for study was thus quite restricted. The reason for this, as with the restriction of ages, was that the intended systematic analysis of the conditions and methodology would have been impossible if a broader area of subject-matter had been considered. Some discussion of other areas of research will be necessary from time to time, but such discussion is only included when it is relevant to the experiments to be reported.

[1.2] THE QUESTIONS

[1.2.1] INTRODUCTION

The previous section specified the subject matter, the present section introduces the questions I have attempted to answer, within the specified area of study.

My fundamental question is: in what ways do cross-modal and within-modal matching differ? The first stage of the process of answering this question is to consider how cross- and within-modal matching could differ: what are the possibilities? When the possibilities have been stated, experimental methods for assessing them can be elaborated.

As a springboard into the discussion, I should like to describe briefly some of the published developmental studies of visual-kinaesthetic matching. This literature is reviewed in more detail in Section [1.3]; a simplified description is given here, in order to illustrate some of the problems of interpretation that can arise in cross-modal studies.

Birch and Lefford (1963, 1967) demonstrated that visual-kinaesthetic matching performance improves with increasing age,

during childhood. One of their conclusions was that there is a developmental improvement in the "connections" between modalities.

Bryant (1968) criticised this conclusion on the grounds that Birch and Lefford did not include the necessary within-modal control conditions in their experiments. The developmental improvements could have resulted from improving within-modal discrimination. General assent has since been given to the need to include all four modality conditions (two cross-modal, two within-modal) in any matching experiment, in order to conclusively demonstrate specifically cross-modal effects.

This is just one problem that has been encountered. It was taken into account by Connolly and Jones (1970) in an experiment on visual-kinaesthetic matching. These workers found that performance improves with age at the same rate in within- and cross-modal matching. They therefore concluded that there is not a specifically cross-modal improvement with age. They did, however, find evidence for a specifically cross-modal difficulty at each age level (5, 8, 11 years and adult). This finding can be used to illustrate further problems of interpretation. First, it is necessary to describe the results in a little more detail.

Connolly and Jones (1970) found that variable errors (as defined in Section [2.6.3]) were highest in the VK* condition, followed by KV, KK, and VV, in descending order. Because VV performance was superior to KK, it was hypothesised that visual memory is superior to kinaesthetic memory. The hypothesis that there

* Here, and subsequently, modality conditions in an experiment are denoted by a pair of capital letters. The first letter indicates the modality in which the standard stimulus was presented, the second indicates the modality in which the comparison stimulus was presented. E.g. VV indicates visual within-modal matching, VK indicates matching a kinaesthetic comparison item to a visual standard. Written in full, "visual-kinaesthetic matching" refers to the experiment as a whole, rather than a specific condition.

is a translation process was proposed to account for the superiority of within- over cross-modal matching. KV was superior to VK, hence it was hypothesised that translation from one modality-specific memory store to another occurs at acquisition, rather than during response, so that in the KV condition, the superior visual memory is used.

Jones and Connolly (1970) found that the effect of a filled or unfilled delay, interposed between acquisition and response, differs between modality conditions, and depends on the response modality, but not on the acquisition modality. This finding supports their hypothesis that translation occurs during acquisition.

The Connolly and Jones model remains the most precisely worked out model of cross-modal matching. As such, it has been a valuable starting point for experimentation, since predictions can be derived from it, both for performance at any one age level and for development. In part because predictions are so clear, subsequent research has produced data which are incompatible with the model (Diewert & Stelmach 1977, Marteniuk and Rodney 1979, Millar 1972, 1975, Newell et al 1979), as discussed in Section [1.3].

But there are also conceptual problems with the model, which provide a valuable starting point for discussion of theories. As I see it, the fundamental problem with Connolly and Jones' model is that it is based on the assumption that differences between modality conditions imply differences in processing and storage of perceptual inputs, which result from differences between sense modalities per se. For example, the difference between VV and KK is assumed to be due to superior visual memory. However, the visual stimulus was a line, the kinaesthetic stimulus was an arm movement. In other words, the visual and the kinaesthetic presentation provided different information, and this could account for the differences between modality conditions. Further examples are provided in Section [1.3].

The main point I should like to make is this: in order to demonstrate a specific difficulty with cross-modal matching, within-modal matching must be used as a control condition; however, a difference between within- and cross-modal matching is not sufficient evidence, in itself, to conclude that there are specifically cross-modal difficulties. Having said what cannot be concluded, there is a need to state more precisely what can be concluded when differences between modality conditions are found. The possible types of conclusion may be divided into three broad classes, corresponding to three possible classes of theory of cross-modal matching.

The three are:

(a) memory and processing depend upon modality of acquisition and response per se,

(b) the information presented differs from one modality to another,

(c) strategy for selecting and encoding information, and performing a matching task differs between modality conditions.

The discussion which follows is concerned primarily with the nature of cross-modal matching at any given age level, rather than with developmental processes. These matters are not independent, but analysis can be simplified if they are initially considered separately.

[1.2.2] THEORIES BASED ON MODALITY-SPECIFIC STORES AND PROCESSING

The Connolly and Jones model provides an example of this first type of theory. It includes the directing of perceptual inputs into different channels and stores on the basis of input modality and expected output modality.

Pick (1970, 1974) proposed another variant: that information of a particular type is processed by the modality best adapted to the

particular type. Specifically, he proposed that spatial information is processed "visually", whatever the input modality. This leaves open the possibility that other types of information are processed in other modalities. For example, temporal information might always be translated to an auditory code. This would be consistent with the need for auditory inputs to be processed primarily in terms of temporal patterns, spatial patterns being inferred, and with evidence that temporal information is selectively extracted from auditory inputs (see reviews by Jones 1981, O'Connor and Hermelin 1981).

In both the above theories, memory stores and mechanisms of processing are labelled by modality. Another way of thinking about different stores and/or processing channels, is in terms of "selective access". For example, Pick's theory might be altered from a summary statement "spatial information is processed visually", to "visual inputs have selective access to channels in which spatial information is processed". This selective access might result from differences in central pathways, or from peripheral differences; visual inputs, for example, have an inherent two-dimensional spatial structure, given by the position of retinal stimulation.

The process referred to by Pick as translation to a visual-spatial code can be seen as non-visual input information gaining access to spatial processing by a route which is longer, and less efficient, than the route taken by visual inputs.

Modality-specific store theories, and selective access theory are alike in generating the prediction that within-modal matching performance should depend on the modality in which matching is performed. However, whereas modality-specific store theories require that there should be a cross-modal deficit in matching performance, this prediction does not necessarily follow from selective access theory. A cross-modal deficit would be predicted only if different

access routes transmitted different information. This would imply a strategic difference of the type discussed in Section [1.2.4].

Interaction between sense modalities is an example of a general property of nervous systems: convergence and divergence of information channels. For example, considering visual inputs alone, there is convergence of information from the two eyes, and divergence from each eye to several brain areas, including the primary visual cortex and the superior colliculus.

When considering human perceptual experience, we can reasonably speak of interaction between senses, because we commonly experience quite distinct sensations which we can ascribe (not necessarily correctly) to particular sense organs. At the level of neurophysiology and neuroanatomy, we are faced with a more complex question. We need to ask, at what stage in the convergence of inputs does the convergence represent interaction between what are experientially separate modalities? In other words, interaction between the senses must ultimately be seen as one element in a larger picture of convergence and divergence.

One implication which follows is that it is likely to be a considerable simplification to speak of one store for each modality, and one integrated store, although such a simplification might have short-term heuristic value. In the longer term, selective access formulations may be better able to cope with differences in matching performance between combinations of modalities, stimulus parameters and other experimental conditions in matching experiments.

[1.2.3] THEORIES BASED ON INFORMATION AVAILABLE

Theories of this second type emphasise the amount and type of information provided, rather than the sense through which it is perceived. The environment (including experimental procedures) is

such that different information is available to each sense. For example, as has already been pointed out, the visual presentation given by Connolly and Jones (1970) (also Jones and Connolly 1970, Freides 1975) provided different information to the kinaesthetic presentation. Clearly, the senses are also adapted to pick up particular types of information.

Matching can only be performed on the basis of compatible descriptions of an object or event. Matching performance therefore depends on the encoding of the same aspects of an object or event during acquisition and during response. An example will help to clarify this point. Consider a set of spheres varying in size and colour. Let us say that a sphere can be any combination of large or small, red or green. Visually, the colour and the size of the spheres can be perceived. Haptically, the size and weight of the spheres can be perceived. Therefore, only description of the spheres in terms of their size will permit cross-modal matching. Note, however, that if a subject initially perceives the relative weight of the spheres, an assumption of equal density would enable their relative size to be estimated.

The information provided about an object, through two modalities, can therefore be classified as compatible or incompatible (size is compatible information in the above example, colour is incompatible). The information may be further classified as directly perceived, or derived (as size was derived from weight in the above example).

Applying this analysis to visual-kinaesthetic matching, some more subtle examples of incompatible information can be noted. Visual judgements of length may involve lining up a stimulus against background cues. This information is not available during (blind) kinaesthetic presentation. If only background-relative coding were

used during visual presentation, then visual-kinaesthetic matching would not be possible at all.

If, as is more likely the case, both self- and external-reference are used visually, then visual-kinaesthetic matching requires the subject to either (a) ignore visual external references and match on the basis of self-reference only, or (b) transform self-referent to external-referent information (on the basis of memory of the visual field in relation to the body), and match on the basis of the derived external-referent information.

Two important predictions follow from the above analysis. First, because visual presentation (in the above example) provides more and better information, visual within-modal matching should be performed more accurately than kinaesthetic. Second, since there is a difference in amount and type of information between the two modalities, cross-modal matching involves either a transformation of information, or partial use of information. Accuracy should therefore be lower than in within-modal matching.

The same predictions would be made whenever there are reasons for expecting visual presentation to provide different, better information, compared to kinaesthetic presentation. For example, in some studies (Connolly & Jones 1970, Jones & Connolly 1970, Freides 1975, Raffel 1936) kinaesthetic presentation was successive (i.e. an arm movement), whereas visual presentation was simultaneous (i.e. a stationary length of some kind). Other differences in available information are considered in Section [1.3].

In general, it can be said that a difference in performance between cross- and within-modal matching does not require the conclusion that cross-modal matching involves translation between modality-specific stores. Transformation and/or selection of information can be alternative sufficient accounts.

It is difficult to point to a theorist who would hold to an account of cross-modal matching based simply on the information available. It seems that "hybrid" theories are the rule, and these are discussed in Section [1.2.5].

[1.2.4] THEORIES BASED ON STRATEGY VARIATION

The third type of theory can be seen as a natural extension of the second. Matching performance does not depend simply and directly on the information which is available to the subject. Rather, performance depends on the information which the subject perceives, encodes, and actually employs as the basis for matching.

Consider a position-matching task, as defined in Section [1.1]. The manner in which a position is encoded can vary in at least four ways. First, coding can be self-referent, (e.g. relative to the subject's body midline) or external-referent (e.g. relative to a distinctive background feature), when presentation is visual.

Second, whether position is encoded by self- or by external-reference, reference can operate in two distinct ways. One way is to note that the position lines up exactly with a feature which is assumed to have a stable location in space. The second way is to estimate the distance between the position and such a stable feature. These two options can of course be seen as being essentially the same, with the distance encoded being zero in the first case and not zero in the second case. In practice when the position is not exactly aligned with a stable feature, the subject must carry out the additional process of estimating a distance.

Third, when a distance has to be estimated, the subject can encode the distance as a distance, and/or as the velocity and duration of the movement between the reference feature and the position.

Fourth, whichever of the above procedures is used, coding can be verbal or non-verbal. Language is not a necessary component of the ability to perform cross-modal tasks. This is evidenced by demonstrations of cross-modal matching by pre-verbal infants (Bryant et al 1972) and of cross-modal transfer by apes (Davenport and Rogers 1970, Davenport et al 1973, Weiskrantz and Cowey 1975).

However, it is not certain what role language plays when linguistic capacities are present. Potentially, language can facilitate selection of a strategy, reduce the information needed to centrally represent a stimulus (thereby improving rehearsal), and provide mediation between sensory representations in different modalities.

It was not the purpose of the series of experiments reported in this thesis to examine the role of language. As far as possible, language was denied a role in the matching tasks, by requiring matching of parameters for which it is difficult for children to give exact verbal descriptions. Even adults gain limited benefit from applying a labelling strategy in movement reproduction tasks, and then only when labelling instructions are given, and a retention interval is filled with attention-demanding activity (Ho and Shea 1978, Shea 1977).

This leaves strategy selection as the main possible influence of language. Strategy is examined in the experiments, but the means by which strategies are selected is left as an open question.

A classification of "absolute" vs "relative" coding has not been included in the above list. Instead, the classifications have been based on questions such as "relative to what?", and "using which features of the stimulus?" Absolute and relative coding are discussed further in Section [1.4].

The above discussion has been concerned with strategy

variation in relation to the perception of a particular stimulus parameter. Another important aspect of strategy is the selection of the appropriate parameter as the basis of matching. Selection of position and length cues (see Section [1.1]) is an important example of this, which will be discussed in more detail in Sections [1.3.2] and [1.3.3].

Clearly, there is much scope for variation in encoding strategy between experiments, between conditions, between subjects, and between trials. Even if the same information is presented in each modality condition, different information may be encoded. For this reason, (a) within-modal matching performance might differ from modality to modality, and (b) cross-modal matching performance might suffer from incompatibility of information provided by the two modalities concerned. Here, then, is a third potentially sufficient account of differences between modality conditions.

I should make it clear that when I use the term "strategy", I do not wish to imply that the subject exercises conscious control over what is encoded (although this might occur). Neither do I mean that more than one strategy is necessarily ever used in any one modality. The crucial point is that, when the information available is identical (apart from modality of presentation), non-identical information may be encoded with different modalities of presentation.

When information is not simply "given" (as in the case of length and position matching), but must be obtained by the subject (as in the case of haptic exploration of a shape), then strategies of searching for information are important. Cross-modal matching then requires compatible information to be obtained through different modalities. See Abravanel (1981) for a recent comment on the role of search strategies.

[1.2.5] SPECIFIC THEORIES

In summary, the factors which can cause a difference between modality conditions are broadly divisible into three classes: strategy, information available, and "hardware" differences in efficiency of processing and storage. In any given instance, these three are not mutually exclusive, and in order to test an hypothesis about any one of them, the others must be controlled for.

The discussion and experiments which follow are, in the main, not concerned with testing the theories of particular psychologists. Rather, they are concerned with evaluating the three broad classes of theory elaborated above. It is useful, however, to give some indication as to how particular theories fit into the broad framework. As was mentioned above, theories tend to be "hybrids" in terms of this framework, owing something to more than one of my three classes. Theories are here considered mainly in terms of their implication for visual-kinaesthetic matching. Further discussion of theories, and a more general evaluation of them, are to be found in Freides (1974) and Millar (1981).

One of the most pervasive approaches to cross-modal matching is that of Gibson (1969, Chapter 11). It is important to have regard for the context in which Gibson originally put forward her often-quoted theory. My discussion so far has been concerned with reasons for differences between modality conditions. Gibson was mainly concerned with the more fundemental issue (arising from the more limited data available at the time), of how the performance of cross-modal tasks is possible at all. Her views may be summarised thus:

> "I have suggested that intermodal effects are the result of the pickup of invariant amodal stimulus information; that such information is of a high order in stimulation and is responded to increasingly in the course of development." (p.231)

In other words, Gibson was proposing that, beyond the immediate sensation, perception in any modality results in the pickup of invariant, amodal stimulus information (e.g. size, speed), which may also be picked up through an alternative modality. The proposition that information obtained through different modalities is equivalent, and directly perceived, was contrasted with theories requiring integration or mediation between fundamentally separate modalities.

Gibson's theory has its roots in the writings of Aristotle, and more recently, von Hornbostle (1927 / Ellis 1938), Hayek (1952), Michotte (Michotte et al 1964) and J.J.Gibson (1966). The contemporary alternative theories referred to by Gibson were the integration theories of Birch and co-workers (Birch and Lefford 1963) and Soviet motor copy theories (summarised by Pick 1964), the verbal mediation theory of Ettlinger (1960, see also Ettlinger 1967), and Piaget's theory of mediation by progressively constructed sensory-motor schemas (Piaget and Inhelder 1948).

The polarisation of theories into "unity" and "separateness" of the senses has been criticised (e.g. Millar 1981), but it remains a recognisable dividing force in discussion of intermodal capacities. The polarisation is evident in theories of cross-modal matching, to which I now turn.

During the past twenty years, a succession of "separateness" theories have been proposed, which are relevant to visual-kinaesthetic matching. Birch and Lefford (1963, 1967) developed a theory of cross-modal integration on the basis of a series of studies of cross-modal matching. The theory had two basic elements, that during development there is (a) a progression in motor control from dependence on proximoceptive stimulation to dependence on teloreceptive stimulation, and (b) increasing integration between

modalities, and increasing dependence on this integration.

In support of the theory, Birch and colleagues found cross-modal matching to improve with age, and to be deficient in many mentally impaired or subnormal groups (see Freides 1974 for review). However, as noted in Section [1.2.1], within-modal matching conditions were not included as controls in these experiments. Hence, it cannot be said with certainty that the observed performance difficulties resulted from cross-modal aspects of the tasks, or from poor within-modal discrimination (Bryant 1968).

Subsequent research has not supported the Birch and Lefford formulation as a general theory of cross-modal development, there being insufficient evidence for improvement with age specifically in cross-modal tasks (see Freides 1974). In the particular area of visual-kinaesthetic matching, the successors to Birch and Lefford can be seen to be Connolly and Jones (1970). In the area of cross-modal matching in general, Pick (1970, 1974) has proposed a separate-modalities theory. Both these theories were discussed in more detail in Section [1.2.1].

As far as "unity" theories are concerned, Gibson (1969) expressed a view on the matter of how cross-modal matching occurs, when it does occur. She left open the question of why, or under what conditions, cross-modal and within-modal matching performance might differ. Implicitly however, her view that cross-modal matching depends on detection of amodal stimulus properties means that performance must depend on the availability of the same amodal properties through each presentation modality, and on strategies for picking up this information. These two determinants of performance are essentially the ones described in the previous two sections.

The same two determinants can be important if a "separateness" theory is accepted. Marteniuk and Rodney (1979) implicitly made this

point in modifying Pick's theory. They suggested that information of a given type is translated to the modality which is most adept at processing that type of information (Pick 1970, 1974), but only if sufficient information is available to the adept modality. They derive this modification from Freides' (1974, 1975) conclusion that the complexity of (amount of information in) the stimuli is critical in determining the pattern of modality effects. Freides' theory is discussed further in Section [1.4].

Probably the purest example of a strategy theory is provided by Goodnow (1971). Goodnow stressed the importance of "differential sampling" in explaining differences between modality conditions in matching tasks. She also pointed out the role of "experience in working with a modality" as a determinant of differential sampling.

"Experience in working with a modality" is certainly important, although this may largely be due to experience with particular stimulus material (eg. Millar 1981, p.288ff). It seems improbable, however, that "differential sampling" should provide a complete account of cross-modal matching, for the simple reason that available information normally does differ between modalities. Millar (1975, 1981) favours an account based on strategy, which in turn depends on the available information.

O'Connor and Hermelin (1981) add a third factor to this interaction, by allowing the possibility of modality-specific coding:

> "Whether a stimulus-modality code or one based on mental images is used, or whether coding is based on the abstraction of "amodal" stimulus features, depends on the nature of the stimulation, the task's requirements, and the capacities and predispositions of the subject." (p.340)

The foregoing discussion is an indication of the diversity of theoretical thinking in the field of cross-modal matching. Any one theory may be applicable to some pairs of modalities, but not to

others. Given the number of widely or subtly differing theories, and the number of pairs of modalities, the search for a correct match between theories and pairs of modalities may be a long one. If it turns out that available information and strategy are important, then for each pair of modalities a description is necessary of the role of particular types of information and particular strategies.

In this thesis, a more limited research problem is approached. The experiments are limited to one pair of modalities, and an attempt is made to assess broad classes of theories, as elaborated above.

[1.3] REVIEW OF STUDIES OF VISUAL-KINAESTHETIC MATCHING

[1.3.1] INTRODUCTION

In the previous section, three types of account of cross-modal matching were described: modality-specific processing, information and strategy. In this section, published studies of visual-kinaesthetic matching are discussed in the context of the three types of account. Work on adults is reviewed first, then work on children.

[1.3.2] VISUAL-KINAESTHETIC MATCHING: ADULTS

The results of published studies are summarised in Tables [1.3.2], [1.3.3] and [1.3.4]. The performance measures reported differ from study to study. Absolute errors have been reported more frequently than have constant errors or variable errors. It will be argued (Section [2.6]), that the latter two measures are the more useful ones. Inconsistency in reporting results, and the popularity of absolute errors, combine to make comparison of studies difficult. Even so, it is clear that there is inconsistency in the results.

In theory, it should be possible to examine differences in method between experiments, explain the inconsistencies in results,

and thereby clarify the interpretation of any one experiment. In practice, this is an uncertain process, owing to the number of aspects of method on which the experiments differ. These aspects include simultaneous vs successive visual input, passive vs active kinaesthetic input, coronal vs sagittal plane movements, presence vs absence of visual background cues, length vs position matching, and varying retention interval conditions. Table [1.3.1] summarises differences in method. This list of differences is not necessarily exhaustive, but even these factors have not all been systematically varied, hence the effect of each is not easily assessed.

There are, then, problems with comparing experiments. In spite of this, an attempt can be made to generate a reasonable account of the results. The key question which I wish to answer in generating this account is, do the data demonstrate the existence of modality-specific stores having differing characteristics, with consequent necessity for translation when performing cross-modal matching? The absolute error results are examined first.

From Table [1.3.2], it can be seen that invariably there are differences in mean absolute error between modality conditions. Matching is generally more accurate in VV than in the other conditions. With the exception of Jones and Connolly (1970), absolute errors are significantly smaller in VV than in the cross-modal conditions, and significantly or non-significantly smaller than in KK. Again with the exception of the Jones and Connolly study, KK is superior to the cross-modal conditions. Where there is a difference between VK and KV (a cross-modal asymmetry), then with the exception of Diewert and Stelmach (1977), KV is superior to VK.

The results of Jones and Connolly, as noted above, probably show the most discrepancies with other studies, and therefore provide a useful starting point for discussion. This entails analysis of the

Table [1.3.1] Comparison of Experimental Method in Eight Studies of Visual-kinaesthetic Matching in Adults

STUDY	EXPT	Sim/Suc Vis	Sim/Suc Kin	Pass/Act (Kin)	Light (Vis)	L/P	Delay	Note
Connolly & Jones (1970)		Sim	Suc	Act	+	(L)	0	
Diewert & Stelmach (1977)		Suc	Suc	Pass	-	L+P	0	
Freides (1975)		Sim	Suc	Act	+	(L)	0	1
Jones (1973)		Suc	Suc	Both	-	L+P	0	2
Jones & Connolly (1970)		Sim	Suc	Act	+	(L)	0/10U/10F	
Legge (1965)		Sim*	Suc	Act	+	(P)	0	3
Marteniuk & Rodney (1979)	1	Suc	Suc	Act	-	L+P	0	
	2	Suc	Suc	Act	+	L+P	0/20U/20F	
Newell et al (1979)	1	Suc	Suc	Act	-	L+P	10	
	2	Suc	Suc	Act	-	1	10	
	3	Suc	Suc	Act	+	1	10	
	4	Suc	Suc	Act	+/-	1	10	

EXPT = Experiment number, where more than one is reported in a paper.

Sim/Suc = Simultaneous vs Successive presentation in each modality.
(* = both simultaneous and successive cues available, but simultaneous cues predominant.)

Pass/Act = Passive vs Active kinaesthetic presentation.
Jones (1973) used both modes, in different conditions.

Light = Visual background cues available (+) or not available (-) or available except during retention interval, (+/-).

L/P = Matching based on length or position cues or both.
Where one cue was definitely available, the other possibly available, the definite cue is shown in brackets.
(1 = Both cues available in within-modal conditions, only L available in cross-modal matching.)

Delay = Retention interval, in seconds. U = Unfilled, F = Filled.

Notes: 1,3 = The subject was allowed to adjust estimate until sure the response was correct.

2 = Within-modal matching was not included.

3 = Movements were in a sagittal plane, rather than a coronal plane as in the other experiments.

Table [1.3.2] Results of Visual-Kinaesthetic Length Matching Studies of Adults: Absolute Errors

STUDY	EXPT	DELAY	RESULT
Connolly & Jones (1970)		0	VV = KK < KV < VK
Diewert & Stelmach (1977)[1]		0	VV = KK < VK < KV
Freides (1975)		0	VV < KK < KV < VK
Jones & Connolly (1970)[2]		0	VV = KK = KV < VK
		10U	VV = KV < KK < VK
		10F	KK = KV = VV < VK
Jones (1973)		0	VKp = KVp < VKa
Marteniuk & Rodney (1979)	1	0 20U 20F	VV = KK < VK = KV
	2	0 20U 20F	VV < KV < VK
Newell et al (1979)	1	10	VV < KV = VK, KK < VK
	2	10	VV = KK < VK = KV
	3	10	VV < KK < KV < VK
	4	10	VV = KK < KV < VK

Notes:

EXPT: Experiment number, where more than one is reported in a paper.

DELAY: Retention interval in seconds, U = unfilled F = filled

[1] Results for longest movement length. There was no effect of modality for the shortest movement length.

[2] The differences shown are based on non-correlated t-tests performed on the means and standard deviations reported by Jones & Connolly.

Jones (1973) : p,a = passive,active kinaesthetic perception

effects of delay of response. Jones and Connolly compared matching under three retention interval conditions: immediate reproduction (the condition used by Connolly and Jones 1970), a 10 second unfilled interval, and a 10 second interval filled by the subject verbalising names in order to prevent rehearsal.

The experiment was carried out with the purpose of comparing the effect of interval conditions on different modality conditions, rather than comparing modality conditions within interval conditions. Presumably it is for this reason that statistics comparing modality conditions were not reported.

One feature of the model proposed by Connolly and Jones (1970) was that translation should occur, before the retention interval, from the acquisition modality to the response modality. From this model, Jones and Connolly predicted that the effect of interval conditions would depend only on response modality: effects on VV and KV should be similar, and different from effects on KK and VK. It was further predicted (following Connolly and Jones 1970, Posner 1967) that (a) kinaesthetically coded information should be subject to degradation over an unfilled interval, with no additional loss with interfering activity, but that (b) visually coded information should be lost only when rehearsal is prevented by interfering activity.

These predictions proved remarkably accurate. The effect of an unfilled delay was to reduce the accuracy of matching in KK and VK, with the result that KK became less accurate than KV matching. Attention-demanding activity during the interval had no additional effect on KK and VK, but reduced accuracy in VV and KV, so much so that accuracy in VV and KV was reduced to the level of KK.

The small number of subjects (12), is the most likely explanation for the absence of a difference between KV and VV in all three interval conditions, and between KV and KK in the immediate reproduction condition. These differences were found to be significant by Connolly and Jones (1970), using the same method with a larger number of subjects.

Accurate though the above predictions were, the Connolly and Jones model has largely been discredited, as Jones (1981) has agreed.

In the present context, the important aspect of the model is the notion that translation occurs immediately, from acquisition modality to response modality. This is contradicted by the results of Newell et al (1979), who found no difference in performance when the subject was informed of the response modality immediately after acquisition or immediately before response, rather than before acquisition. The time at which the response modality is cued should affect performance if translation normally occurs during acquisition. It is possible, however, that Newell et al's subjects adopted a different strategy because the response modality was cued at varying points in time.

The same reply cannot be offered against the results of Marcell and Allen (1975)*. These workers predicted from the Connolly and Jones model that visual interference tasks should affect VV and KV performance, while KK and KV should be subject to interference from kinaesthetic tasks. These predictions were not supported. The Connolly and Jones model is also incompatible with the results of Marteniuk and Rodney (1979), described below, and of Millar (1972, 1975), described in the next section.

Although Jones and Connolly's explanation for their results appears inadequate, their results remain, and must be explained. One plausible account is that the visual stimuli were encoded in terms of length and position, simultaneously perceived, relative to background cues, whereas kinaesthetic stimuli were encoded as movement lengths. A transformation between these two codes, during acquisition, could replace Connolly and Jones' translation as an explanation. The same transformations would not have been necessary in the Newell et al study, because both visual and kinaesthetic presentation were sequential. It is uncertain what stimulus presentations were used in each modality by Marcell and Allen.

* Cited by Jones (1981, p.125).

The above hypothesis can be examined in the light of two further studies of the effects of delay, reported by Marteniuk and Rodney (1979). In the first of their two experiments, four modality conditions were included (VV, VK, KV, KK), and three retention-interval conditions: immediate reproduction, 20 seconds unfilled interval and 20 seconds filled interval, during which the subject counted backwards in threes. The visual stimulus was a small luminous green line, moving in darkness.

Absolute errors were lower in VV and KK than in KV and VK, and lower with immediate reproduction than in the 20 second delay conditions. The modality x interval condition interaction was not significant. In other words, with reduced visual information (relative to Connolly and Jones 1970, Jones and Connolly 1970), the cross-modal asymmetry was abolished (i.e. VK = KV), and delay did not differentially affect conditions with kinaesthetic response.

In the second experiment, the same interval conditions were employed, but only three modality conditions, KK being excluded. Visual information was increased by providing dim illumination in the experimental room. Under these conditions, absolute errors were highest in VK, lowest in VV, and intermediate in KV, the three conditions being significantly different from each other. As in the first experiment, errors were increased equally by a filled or an unfilled interval, and the interaction between modality and interval conditions was not significant.

These results indicate that both the cross-modal asymmetry, and the modality x retention interval interaction found by Connolly and Jones / Jones and Connolly (1970) result not from differences between modalities as such, but from differences between the particular visual and kinaesthetic stimuli.

What are the relevant features of the stimuli? In the case of

the modality x retention interval condition interaction, the most obvious difference between Jones and Connolly (1970) and Marteniuk and Rodney (1979) is the use of simultaneous visual presentation in the former study, successive in the latter. Visual background cues alone, in Marteniuk and Rodney's second experiment, did not result in a modality x retention interval interaction, although the combination of background cues with simultaneous presentation may be important.

In the case of the cross-modal asymmetry, two aspects of the stimuli used have been shown to give rise to this phenomenon. First, Jones (1973) found that KV exceeds VK accuracy when the response movement in VK is active and the acquisition movement in KV is passive. When kinaesthetic perception was passive in each modality condition, VK and KV did not differ. Visual perception was necessarily passive, the passive : active comparison required in the VK(active K) condition may therefore be responsible for the observed cross-modal asymmetry. Diewert and Stelmach (1977) found that the usual asymmetry is even reversed when kinaesthetic perception is passive in each modality condition. It is not immediately clear why this result should have been found, but a possibility is noted in the discussion of variable error results, below.

Second, Marteniuk and Rodney (1979) and Newell et al (1979) found a cross-modal asymmetry only when visual background cues were available. Presumably, the presence of background cued caused subjects to encode the visually presented standard in VK in a form incompatible with the information available during kinaesthetic response. In KV, this difficulty would not arise, because the information available to kinaesthesis was also available to vision.

The most important finding to remain unexplained by the foregoing discussion is the general finding of a cross-modal deficit, i.e. that cross-modal matching is performed less accurately than

within-modal matching.

In some cases, the cross-modal deficit can be explained in terms of different information being made available through different modalities, as has already been noted. This is the case for Connolly and Jones (1970), Freides (1975), Jones and Connolly (1970) and Newell et al (1979) experiments 3 and 4. Hence, the modality-specific stores hypothesis, though sufficient, is not a necessary explanation of the results of these experiments.

Before discussing the cross-modal deficit in other cases, it is necessary to digress on the issue of differences between position and length coding. Given a fixed starting point, a simple movement can be reproduced by reproducing the end-point of the movement or the length of the movement, or both. Use of the length cue is commonly called "distance" or "extent" coding. While these terms are appropriate for the kinaesthetic matching studies from which they originate, they are not appropriate for the simultaneous stimulus presentations often used for visual presentation. The term "length" is therefore used here. Use of the end-point is commonly referred to as "location" coding. In order to avoid confusion with length (when the terms are abbreviated, the two having the same initial letter), the term "position" is used here.

A sizeable literature has built up in recent years, dealing with the inter-related effects of three variables on movement reproduction. The three variables are:

a) The availability and relative reliability of position and length cues.
b) Active vs passive presentation.
c) Criterion movement "preselected" (chosen by the subject, within a range defined by the experimenter) vs "constrained" (specified

by the experimenter by placing a stop in the path of the movement).

In cross-modal matching experiments only constrained movements have been used, therefore only studies dealing with this type of movement will be considered here. Literature on the difference between preselected and constrained movements is reviewed by Runnings and Diewert (1982) and Summers et al (1981).

There are two points I wish to make, regarding position and length cues. First, the two cues appear not to be used entirely independently, and second, to the extent that they are used independently, performance depends on which cue is used.

First, then, the interaction of position and length cues. Attempts to isolate the two cues have typically made use of a conflict paradigm. The starting point of the response movement is made different from the starting point of the acquisition movement, so that response on the basis of length and position will result in different movements. The subject is instructed to reproduce either length or position.

Whether position or length is the required (appropriate) cue, the alternative (inappropriate) cue biases the response (Kerr 1978, Laabs 1974, Walsh et al 1980, 1981). This biasing is important in interpreting experiments 2, 3 and 4 of Newell et al (1979). In these experiments, in the within-modal conditions, both position and length were appropriate cues, the same starting position being used for the acquisition and response movements.

In the cross-modal conditions, only the length cue was appropriate, but performance was strongly influenced by the inappropriate position cue. Constant errors therefore had high magnitude, with the result that absolute errors were also high in the

cross-modal conditions. The effect of this is that the cross-modal deficit in these experiments cannot definitely be attributed to a difference between modality conditions as such.

There remain three experiments which have demonstrated a cross-modal deficit in visual-kinaesthetic matching, and in which there has not yet been identified a difference in available information between modalities. These three are Diewert and Stelmach (1977), Marteniuk and Rodney (1979, Experiment 1) and Newell et al (1979, Experiment 1).

Three accounts can be offered for the cross-modal deficit in these experiments. First, in the latter two experiments, kinaesthetic presentation was active, whereas visual presentation was necessarily passive. Comparison of movements presented passively with movements presented actively has been found to increase errors in kinaesthetic matching (Jones 1973, Lee and Hirota 1980). The passive to active comparison would therefore also explain the higher errors in the cross-modal conditions of the two experiments concerned.

Second, recall that the results currently being discussed are absolute error results. The variable error results, discussed below, indicate caution in the interpretation of absolute errors. Such caution is supported by the discussion of error measures given in Section [2.6]. Tempting though it is to simply disregard the absolute error results, this would prohibit discussion of most visual-kinaesthetic matching research. It must be noted, however, that a cross-modal deficit in absolute error can result from constant bias effects which cannot be taken as evidence of translation between modality-specific stores.

If the absolute error results are to be taken at face value, there is a third interpretation of the cross-modal deficit in the three experiments being considered. This account is based on somewhat

patchy evidence, but is theoretically viable, and makes an important point regarding methodology.

In the three experiments concerned, both position and length cues were available. It is possible that the cue which was encoded was dependent on the modality of presentation. Evidence for this is to be found in research into differences in performance between position and length reproduction.

Diewert and Roy (1978) point out that kinaesthetic position information is retained over an unfilled interval, citing as evidence Diewert (1975), Keele and Ells (1972), Posner (1967) and Stelmach et al (1975). To this list can be added Laabs (1973), Marteniuk (1973), Runnings and Diewert (1982), Stelmach (1975) and Walsh et al (1981). The general pattern is weakened slightly by two experiments in which position reproduction was affected by an unfilled interval (Kerr 1978 and Moxley 1974).

In contrast, length reproduction accuracy is diminished following an unfilled interval (Diewert and Roy 1978, Kerr 1978, Laabs 1973, Moxley 1974, Posner 1967, Posner and Konick 1966) when a counting strategy is not adopted and a constrained presentation movement is used. Thus, when both position and length are available, if there is an effect of delay on kinaesthetic matching, the implication is that the length cue is the predominant one (Duffy et al 1975).

As noted above, Marteniuk and Rodney did find that matching, in all four modality conditions, was affected by an unfilled delay. Following the reasoning given in the previous paragraph, it would appear that their subjects selectively encoded the length cue. Such selective coding is known to occur, and to depend on the length of the movement (e.g. Gundry 1975) and task instructions (Hagman 1978, Hagman and Francis 1975).

Posner (1967) found that the position cue determines visual matching performance when both position and length are available. If this is a general phenomenon, then the cross-modal conditions in Marteniuk and Rodney (1979) would have involved matching visual position information to kinaesthetic length information. In other words, a transformation of information, necessitated by a strategic difference, could explain the cross-modal deficit.

The above account, if valid in the case of Marteniuk and Rodney, can readily be generalised to other experiments having a similar design, such as Diewert and Stelmach (1977) and Newell et al (1979). Experiment A4, reported in Chapter Three, provides some validation for the above analysis.

The foregoing discussion indicates that strategic selection of cues should be controlled for in experiments on cross-modal matching. The most obvious means by which to achieve this control would be to separately study length and position matching, taking steps to reduce to a minimum any interaction between cues. This task is taken up in Experiment Ala.

In summary, the absolute error results do not give conclusive support to the hypothesis that differences between modality conditions are due to retention of information in modality-specific stores, with consequent need for translation between stores during cross-modal matching. Differences in available information, and probable strategic differences, provide a sufficient account.

Examination of the variable error results (Table [1.3.4]) takes the discussion an important step further. Compared with absolute errors, the few sets of variable error results which are available present a fairly clear picture. Where there are differences in available information, modality conditions differ (Connolly and Jones 1970, Legge 1965, Marteniuk and Rodney 1979 Experiment 2).

Where there is no difference in available information, modality conditions do not differ (Marteniuk and Rodney 1979 Experiment 1).

Table [1.3.3] Results of Visual-Kinaesthetic Length Matching Studies of Adults: Constant Errors

	EXPT	Mean Constant Error/cm			
		VV	VK	KV	KK
Diewert & Stelmach (1977)		1.2	-0.7	2.1	1.4
Jones (1973)	p		-0.6	-0.6	
	a		0.2		
Legge (1965)		-1.0	-13.0	18.5	-2.1
Marteniuk & Rodney (1979)	1	0.2	-1.4	1.8	1.4
	2	0.4	-0.1	0.2	
Newell et al (1979)	1	-11.1	-4.3	-7.3	3.2
	2	-7.3	-60.0	-52.5	-3.0
	3	-3.5	-42.2	-25.7	-12.7
	4	1.5	-33.3	16.2	-0.3
				4.3	

Jones (1973): p,a = passive,active kinaesthetic perception

Newell et al (1979), Expt 4, KV : 4.3 = Result when room lights extinguished during response.

Table [1.3.4] Results of Visual-Kinaesthetic Length Matching Studies of Adults: Variable Errors

	EXPT	RESULT
Connolly and Jones (1970)		VV < KK < KV < VK
Legge (1965)		VV < KK < VK = KV
Marteniuk & Rodney (1979)*	1	VV = KK = VK = KV
	2	VV < KV < VK

* Significance of differences was not reported. Means (in centimetres) were:

	VV	VK	KV	KK
Experiment 1	1.18	1.22	1.22	1.12
2	0.48	1.24	0.76	

The difference between Marteniuk and Rodney's absolute error and variable error results reinforces the need to approach absolute error results with considerable caution (see Section [2.6]). In this study, differences in absolute errors can be seen to be due to differences in bias (measured by constant errors), not differences in performance efficiency (measured by variable errors). This, in turn, reinforces the conclusion that the absolute error results should not be taken to be indicative of effects of modality-specific stores and translation.

Also, the cross-modal asymmetry found by Diewert and Stelmach (1977) might be accounted for as an effect of bias, mean constant error being three times greater in KV than in VK. Constant error results are reviewed in Chapter Four, where experiments are reported which were intended to examine constant errors specifically. This discussion is postponed until then in order to incorporate results of experiments in Chapter Three.

In conclusion, an examination has been made of studies of adults' visual-kinaesthetic length matching performance. There is nothing in the absolute error results which demands the conclusion that differences between modality conditions are the result of modality-specific stores and/or processing. In the variable error results of Marteniuk and Rodney (1979), there is positive evidence against this notion.

[1.3.3] VISUAL-KINAESTHETIC MATCHING: CHILDREN

In comparison with studies of adult subjects, there have been few studies of children's visual-kinaesthetic matching ability, considering only studies of movement perception, as compared with haptic perception of length and position. If one excludes the studies which did not include the necessary within-modal control conditions

(see Bryant 1968), there are three relevant studies: Connolly and Jones (1970) and Millar (1972, 1975). The results of these studies are shown in Table [1.3.5].

Table [1.3.5] Results of Visual-kinaesthetic Length Matching Studies: Absolute Errors.

STUDY	EXPT	AGE	RESULT
Connolly & Jones (1970)		5,8,11,A	VV = KK < KV < VK*
Millar (1972)		4	VV = KV < KK = VK
		6,8	VV < KK = VK = KV
Millar (1975)	1	10	VV < KK < KV = VK
	2	4.5	VV = KK = VK = KV
		5.5	VV = KK < VK = KV
	3	4	VV = KV < KK = VK b
		4	VV < KV = VK < KK a

EXPT = Experiment, where more than one is reported in a paper

AGE: A = Adult

RESULT: * Variable error result was VV < KK < KV < VK

b,a = blocked,alternating presentation (see text)

The Connolly and Jones study was discussed in the previous section, in the context of the results found with adult subjects. Errors were higher in the younger groups, but followed the same pattern at each age level, as indicated in Table [1.3.5].

The study which is most directly comparable with Connolly and Jones (1970) is reported by Millar (1972). There are, however, a number of important differences in method between the two studies:

1) Millar employed three age groups, having mean ages 4, 6 and 8 years, compared with Connolly and Jones' 5, 8, 11 years and adult.

2) Connolly and Jones used a simultaneous visual presentation (the subject was shown a line, and the length was to be remembered), and successive kinaesthetic presentation (a stylus movement), with visual

background cues available. Millar used three presentation conditions, kinaesthetic presentation was always successive, visual presentation was either (a) successive, without specified visual background cues* or (b) successive with visual guidelines or (c) simultaneous without specified visual background cues.

3) The stimuli used by Connolly and Jones were all linear. Millar used arc movements of a lever except for simultaneous visual stimuli, which were lines on paper.

4) Connolly and Jones' kinaesthetic criterion movement was active. Millar reduced the control the subject had over the lever during the criterion movement, by means of apparatus which gave the experimenter remote control of the lever. The subject could only follow the movements which the lever made. In both experiments, the kinaesthetic response movement was fully active.

Millar found no difference in absolute errors between visual presentation conditions. This result is unexpected, given the differences between the stimuli involved. The explanation may be that the residual visual background cues available to the subject resulted in the use of a strategy of coding relative to background features in each condition. This interpretation is tested in Chapter Three.

In contrast with Connolly and Jones results**, the modality by age interaction effect was significant. This interaction did not,

* Visual background cues were reduced by housing a lever with an illuminated tip in the darkened interior of portable apparatus. The lever was viewed through an aperture in the apparatus, so that coding relative to the edges of the aperture may have been possible.

** Connolly and Jones, it will be recalled, found a main effect of age, but did not report a significant age x modality interaction. Jones (1981, p.124) states explicitly that the interaction was not significant. There was, however, a non-significant trend in this experiment for improvement with age to be greater in the cross-modal conditions than in the within-modal conditions. The implications of age x modality effects for their theory are discussed below.

however, indicate any differential improvement with age in the cross-modal conditions. Rather, the improvement with age was due mainly to conditions requiring kinaesthetic response, i.e. VK and KK. Errors were larger in these conditions than in VV and KV in the 4-year-old group. In the two older groups, errors were similar in VK, KV and KK, with only KV resulting in errors significantly higher than VV.

Recall that Connolly and Jones found a cross-modal asymmetry at all age levels, such that errors were higher in VK than in KV. Millar found this asymmetry in 4-year-olds, but this could at least partly be explained by the general tendency for errors to be higher with kinaesthetic response. Hence, the asymmetry was absent with older children, and in fact non-significantly reversed.

In a second analysis, Millar replaced subjects who showed signs of "impulsive ballistic movements" when responding kinaesthetically. This did not substantially alter the results in the 6- and 8-year-old groups, but in the 4-year-old group KK errors were selectively decreased so that the significant differences were:

VK > KK > VV and VK > VV = KV at this age level.

Hence, the cross-modal asymmetry at age four would appear to be in addition to the kinaesthetic response effect, but obscured by a floor effect in the initial analysis.

Millar explained the asymmetry in the following way. Three movement lengths were used, which were selected to be in a simple ratio to a total available length. This meant that subjects had available additional information which was not modality-specific, and which was not available to Connolly and Jones' subjects, who had to deal with five different lengths. Millar suggested that lack of this relational information was responsible for the cross-modal asymmetry in Connolly and Jones' study (where the information was simply not available) and in her 4-year-old subjects (who were too young to

utilise the additional information).

This explanation rests on the assumption that the amodal information is most crucial in the cross-modal conditions, and that without it, inputs are coded primarily in a visual code. If this assumption is correct, little kinaesthetically useful information would be coded in the acquisition phase of the VK condition, and would therefore not be available in the response phase, resulting in high errors in VK.

An alternative explanation can be proposed. It was suggested, in the previous section, that the cross-modal asymmetry in the Connolly and Jones study could be explained by their use of different kinaesthetic presentation in the two cross-modal conditions (active vs passive) and/or by the availability of visual background cues. These factors could account for the asymmetry in Millar's 4-year-old subjects, but additional assumptions are necessary to explain the absence of the asymmetry in the older subjects.

For example, Connolly and Jones' subjects may have been induced by the particular stimulus presentations used, to code mainly in terms of the length of the stimulus line or movement, rather than the end-position. Millar's older subjects, on the other hand, may have been able to benefit from making use of additional information provided by emphasis of the midline, due to use of an arc movement.

Whatever the correct explanation, Millar's results present problems for the Connolly and Jones model (Section [1.2]), which predicts (a) a cross-modal deficit, and (b) a cross-modal asymmetry whenever errors are higher in one within-modal condition than in the other. The asymmetry was found only in pre-school children (who were not included in the Connolly and Jones study).

Hence, evidence for a cross-modal deficit was weak, depending on an assumption of additivity of errors to predict that without a

cross-modal deficit VK and KV should have been superior to KK (see Section [1.4] for details). Hence, in this study there was little evidence of any need for translation between modality-specific stores, and any purely structural model would have difficulty in explaining changes in the pattern of the modality effect with minor changes in experimental method.

Overall, Millar found errors to be lowest in the VV condition. This may have been due to inherent superiority of the visual modality for this kind of task, or to input information and/or strategy differences. For example, external cues may have been used only in VV, and/or position information may have been used in VV but length in the other conditions.

Millar's results mean that explanations in terms of selective access (see Section [1.2.2]) remain viable. Such theories predict differences between within-modal matching conditions, but do not necessarily predict a cross-modal deficit or asymmetry. Experiments in Chapter Three provide tests of selective access explanations.

Although the point was not recognised by Connolly and Jones, the fact that they found no specifically cross-modal improvement with age creates a problem for integration/translation models of cross-modal matching, including their own. If there is a dependence on learned translations, then translation should presumably improve with age, unless the learning process is assumed to be complete in the youngest subjects tested. This problem is exacerbated by a further study by Millar (Millar 1975), in which tasks differing in difficulty were given to children aged 4.5 to 10 years.

The main results were as follows. Increasing task difficulty did not differentially affect cross-modal tasks, a result which is difficult to explain if translation is necessary. Neither did task difficulty selectively affect younger children. Whereas primary

school children showed a cross-modal deficit, pre-school children performed worst in KK, best in VV, and intermediate in the cross-modal conditions. In other words, cross-modal errors selectively increased with age (relative to within-modal errors) in this study.

This study will now be discussed in more detail. Three experiments were reported. In the first, 10-year-olds performed a recognition task involving short (10 degree) and long (30 degree) movements of a lever. These movements were incorporated in an easy task (D) and a difficult task (Q). In the D (Double) task, the two movements were presented, in sequence, in one of the two possible orders. They were then presented a second time, in the same or the alternative order, and the subject was required to judge whether the two presentations were the same or different. In the Q (Quadruple) task, four movements were presented in sequence, the shorter movement occurring once, in any one of the four possible positions in the sequence. Again, a same-different judgement was required as the response.

Errors were higher in the Q task, confirming that it was in fact more difficult. Cross-modal errors were higher than within-modal errors, and errors were higher in KK than in VV. The modality x task interaction was not significant.

There was a significant and interesting effect of the order in which the two tasks were given. In the cross-modal conditions, errors were lower if Q was given first, relative to when D was given first. This suggests an important effect of strategy, a less efficient strategy being adopted if the easier task was given first (e.g. remembering only the first movement of the pair).

By the age of ten, the subjects in this experiment may already have established a translation system (e.g. linguistic mediation). Younger children (4.5 and 5.5 years) were therefore tested in a

second experiment. The D task was compared with recognition of a single movement (S). Older children made fewer errors, errors were lower with the S task, and there was an age x modality interaction such that only the older children showed a cross-modal deficit. Cross-modal performance by the younger children was intermediate between VV (having the lowest errors) and KK, but the simple effect of modality was not significant. There was again an effect of the order of tasks, cross-modal performance in D being improved if preceded by the S task.

In the third experiment, 4-year-olds were required to recall either the short or the long movement, with trials on the two lengths either blocked or alternating. The blocked presentation resulted in smaller errors (absolute errors), but not selectively in the cross-modal conditions. The mean of VV and KK errors was not significantly different from the mean of VK and KV. With alternating presentation, KK errors were larger, so that within-modal errors actually exceeded cross modal errors.

In attempting to explain her results, Millar put forward an hypothesis based on strategy selection. She suggested that older children (and adults) have more strategies available, and more actively select strategies. In the cross-modal conditions, fewer strategies are appropriate and discrepancies between inputs require extra decisions (to ignore irrelevant discrepancies). The extra decisions might interfere with the more difficult task of strategy selection, to produce the cross-modal deficit. Younger children may use only one strategy whatever the task, a strategy which is more appropriate for one modality than the other. Hence younger children make more or larger errors in KK, but have no particular problem with cross-modal tasks.

An alternative hypothesis is that modality effects at each age

level resulted from different information being available or salient with different modalities of presentation. The main difficulty experienced by younger children may have been gathering information and coordinating responses when presentation was kinaesthetic. This would not be surprising, given the relative poverty of spatial cues provided by kinaesthesis.

Older children may have been better able to gather information kinaesthetically, but may consequently have gathered different information to that gathered visually, resulting in a difficulty in matching inputs from the two modalities.

There are clearly a number of unanswered questions regarding developmental trends in visual-kinaesthetic matching. The studies described above are, however, quite consistent regarding the age at which important changes occur.

Connolly and Jones (1970) found a significant improvement in performance in each modality condition, between the ages of 5 and 8, but only non-significant improvement from this age onwards. Millar (1972) found a similar improvement between 6 and 8 years, but in addition a change in the relative difficulty of modality conditions between 4 and 6, due mainly to selective improvement in kinaesthetic performance. Millar (1975) found a similar improvement in kinaesthetic performance between 4.5 and 5.5 years, but again only an overall improvement between 5.5 and 10 years.

These results indicate that the modality effect assumes a mature pattern between five and six years of age. The absence of any change in pattern after this age is particularly striking in light of assertions that the period from five to seven years is an important one for cognitive development (e.g. White 1965). Younger children show a specific difficulty with kinaesthetic performance, particularly kinaesthetic response. Only at five and over does any

cross-modal deficit arise, and this deficit is confounded with information and strategy effects.

In summary, the results of studies of visual-kinaesthetic matching in children indicate that modality effects depend on the task, available information, and possible strategies. There is no pattern of differences between modality conditions which is consistent across all task conditions and ages. It is not clear, however, under what conditions identical performance would be found in each modality condition. In Chapter Three, experiments are described which bear on this issue, with the hypothesis that performance will not differ when information and strategy do not differ.

In the previous section, it was concluded that there is no cross-modal deficit, or visual superiority in adults' performance, unless different information is encoded through each modality. The smaller number of experiments on children do not permit such a detailed analysis of task conditions, although there is ample evidence for effects of information and strategy.

It was predicted that with sufficient control of available information, the same conclusion could be reached for children, i.e. that modality conditions will not differ if available information and preferred strategy are made the same in each modality. Given that available information is controlled by the experimenter, if any interactions between age and modality are found, it was predicted that they should be accounted for by age changes in strategy.

[1.4] A COMPARISON OF VISUAL-KINAESTHETIC (LENGTH) MATCHING WITH VISUAL-HAPTIC (SHAPE) MATCHING

Experiments which have compared within- and cross-modal matching have yielded different results, depending on the pair of modalities used. In visual-kinaesthetic length-matching tasks, the general trend in the absolute error results, as discussed in the previous section, is for VV to be superior to KK, and for both these conditions to be superior to VK and KV. The general trend in visual-haptic shape-matching tasks, is for VV performance to be superior to the other three conditions, with no consistent pattern of differences between VH, HV and HH (Abravanel 1981, Jones 1981).

It has been concluded by some (Abravanel 1981, Bryant 1974, Freides 1974, 1975, Jones 1981) that these results indicate that there is (a) a specifically cross-modal deficit for visual-kinaesthetic matching, but (b) no specifically cross-modal deficit for visual-haptic matching. In Section [1.3], I argued against the first of these conclusions, and in Chapters Three to Six, experimental evidence is provided in support of the argument. This Section has two main points to make. First, I should like to note a difficulty with making the comparison between experiments on the two pairs of modalities. Second, I should like to discuss theories of why there should be a difference between visual-haptic and visual-kinaesthetic matching. This discussion is offered because it provides some insights into the role of available information and strategy in matching experiments.

First, then, what validity is there in making comparisons between length matching and shape matching? A problem arises because different methods of error measurement have been employed in the two types of matching experiment.

In the case of length matching, a continuous variable is recorded: absolute errors, variable errors or constant errors. Therefore, the error recorded on each trial, in each condition, can be seen as the summation of errors from different sources. For example, inaccuracy can arise in the acquisition process, during retention, in a translation process if one occurs, and in response processes. It is possible for effects of these different sources on bias to cancel each other out, but effects on variability are all of one sign, and therefore should simply summate.

The end result of this is that, in the absence of a specifically cross-modal source of error, variability in the cross-modal conditions should be intermediate between variability in the two within-modal conditions. Therefore, if the mean of the two cross-modal conditions is significantly greater than the mean of the within-modal conditions, this can be taken as evidence for a specifically cross-modal source of error.

In this context, "specifically cross-modal" does not necessarily imply translation between modality-specific stores or transformation of information. It is in principle possible, for example, that the encoding strategy employed during acquisition depends on the anticipated response modality. If this is the case, then the error resulting from visual acquisition, for example, may differ between VV and VK matching.

In the case of shape matching, the normal error measure is the number of erroneous matches. Errors are sometimes classified as false positives and false negatives, but this does not remove the basic difference between counting errors and measuring the magnitude of errors. Although errors can arise from different sources, and summate, the summation does not increase the measured error, because one error on a given trial has the same effect as ten errors: the

subject chooses the wrong shape, or incorrectly judges a match. Therefore, unambiguous conclusions cannot be drawn from the absence of a difference between conditions. In particular, if mean within-modal performance does not differ from mean cross-modal performance, this is not clear evidence that there is no specifically cross-modal source of error, even if VV < VH = HV = HH.

Despite the above problems, there is some value in exploring the theories which have been proposed to account for the difference between visual-kinaesthetic and visual-haptic matching results.

It is interesting that all such theories appear to assume that the difference is due to the different stimuli used (shape vs length) and not to the use of different sense modalities as defined in Section [1.1]. This is perhaps because it is more obvious why there should be differences between shape and length, than why there should be differences between kinaesthetic and haptic matching. Or perhaps it is because kinaesthetic and haptic perception have been assumed to involve the same sense modality.

In any case, I do not wish to seriously question the assumption. In fact I would extend it to the results for any single pair of modalities: it is one of the central arguments of this thesis, that the information perceived is more important than the modality by which it is perceived.

I will turn now to discussion of the theories which have been proposed to account for differences in the results of visual-haptic and visual-kinaesthetic matching experiments.

Freides (1974, 1975) worked on the hypothesis that the essential difference between shape and length matching is the complexity of the information involved. He took length (being unidimensional) to be simple, and shape (being two- or three-dimensional) to be complex information. He then argued that, the more

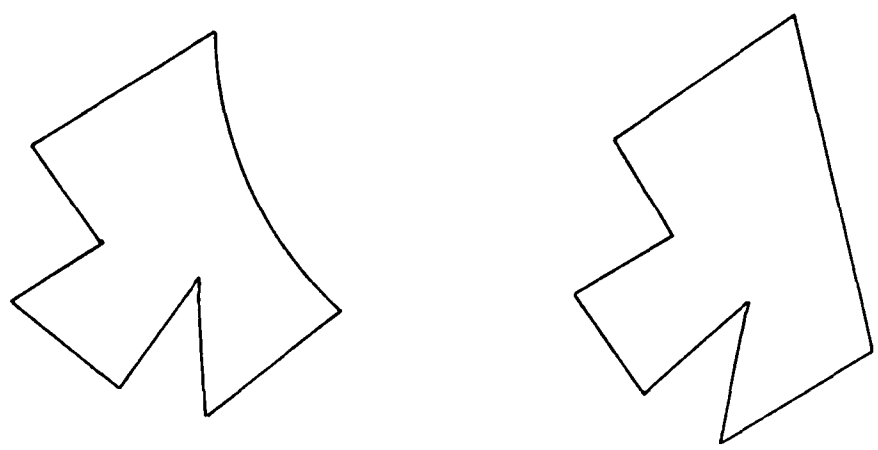
simple the information, the more performance should be susceptible to effects of "sensory and contextual features" such as modality of presentation, sex of subject and of experimenter, magnitude of length to be matched, and variations in experimental procedure.

Freides (1975) provided some evidence for this hypothesis, in an experiment in which shape and length matching were compared. Length matching performance was more affected than shape matching by modality and sex of experimenter, and was affected by magnitude of length to be matched.

I would not dispute that shape is a more complex variable than length. However Freides' theory must be questioned for two reasons. First, the complexity of the stimulus is not of direct relevance: the important factor is the complexity of the features which must be encoded in order to discriminate among alternatives. For example, the two shapes in Figure [1.4.1] are both complex, but they are simple to discriminate. The subject has only to note that only one shape has a curve. Feature-detection and labelling strategies may be at least as important as the complexity of the information.

Figure [1.4.1] Two complex shapes which are easy to discriminate

(See text for explanation)



The second problem with Freides' account is that shape and length matching differ in ways other than the complexity of the stimuli involved, a point acknowledged by Freides (1975). The theories described below provide elaboration on this point.

Bryant (1974) presented evidence that, in various areas of development, children show competence with relative judgements at an earlier age than with absolute judgements. Applying this generalisation to the difference between shape and length matching, Bryant proposed that shape comparisons require only relative judgements, whereas length matching requires absolute judgements.

"Absolute" in this context should presumably be taken to mean "not relative to any externally provided reference" - there remains the possibility of coding relative to internal scales of length, verbal or otherwise. Hence, length judgements are more "difficult" and result in poorer cross-modal performance, which is assumed to be more demanding than within-modal performance. This theory entails the proposition that, although length is simpler information, to encode it requires more advanced abilities.

It should be pointed out that the evidence upon which these two theories were based is from experiments in which the appropriate cue was length alone, or length plus position. My own experiments are concerned primarily with position matching. However, this position matching (except possibly in two experiments, A2b and A5), requires encoding of length (distance from a reference point).

A further hypothesis can be proposed, based on the different levels of numerical description required for shape and length matching. Length matching requires the use of an interval or ratio scale of measurement. Shape matching requires only nominal (e.g. "there is a curve"), or ordinal (e.g. "there are more bumps on this shape") measurement. This proposition can be seen to be similar to

Bryant's formulation, described above. Perhaps the level of numerical description required is the basis for differences in difficulty between absolute and relative judgements.

Language may be important in cross-modal matching, either in selecting appropriate information on which to judge a match, or in specifying stimulus features. Absolute quantities, or interval/ratio scales may in addition be less susceptible to verbal coding.

There are, then, several plausible ways in which it is possible to account for differences between visual-kinaesthetic length matching and visual-haptic shape matching. Implicit in each account is the notion that cross-modal matching is inherently more difficult than within-modal matching, so that increasing task difficulty will selectively affect cross-modal matching performance. This proposition is put in doubt by experiments reported by Millar (1975). Millar found a cross-modal deficit in visual-kinaesthetic matching with older but not with younger children (ages 10 and 5.5 vs 4 years). At no age level were cross-modal errors selectively increased by increasing the difficulty of the task. This study is described in more detail in Section [1.3.3].

This evidence supports a principal hypothesis of this thesis, that cross-modal matching is not in fact inherently more difficult (Section [1.2]). If this hypothesis is correct, what follows? One might expect to find that modalities differ in available information to a greater extent in length matching than in shape matching.

Evidence on this possibility is ambiguous. A cursory glance at the stimuli involved indicates that shape matching experiments involve differences in information which are analogous to those found in length matching experiments: visually, the entire shape can be presented instantaneously; and there is normally a visual background when presentation is visual. The background, however, is of less

obvious value in shape matching, compared to length matching, and the strategy of successive visual scanning (see Abravanel 1981) would reduce any advantage gained for vision by instantaneous presentation of the entire shape.

This ambiguity may not, however, be of great importance if the possible effects of information differences are considered. The visual-kinaesthetic matching results were accounted for, in the previous section, by pointing out that kinaesthetic presentation often provides information which is poorer than that provided by visual presentation, and different (see also Millar 1981, p.297ff).

In shape matching, it is not unreasonable that differences between modalities in available information and search strategy should determine the amount, rather than the type of information encoded. This would be the case if the same features of shapes are salient visually and haptically. The prediction which follows is that haptic perception will result in poorer performance in whatever condition it is used - HH, HV or VH. A cross-modal deficit would only be expected if visual and haptic presentation provide different information, so that there is a mismatch of information in the cross-modal conditions.

The general point which follows from the above discussion is that effects of available information, and of strategy, can be expected to depend on the modalities studied, the stimuli, and the error measure used. The present analysis will be supported if evidence can be produced to show that information and strategy are more important factors than modality in studies of cross-modal matching. The experiments which follow were performed with the aim of adding to the weight of the necessary evidence.

CHAPTER TWO: METHODOLOGY

[2.1] INTRODUCTION

This chapter has two aims. First, it gives a description of the methodology employed in the experiments described in Chapters Three to Six. Many features of the methodology are common to some or all of the experiments. In order to avoid excessive repetition, such features are described in this chapter, rather than in the report of each individual experiment.

Second, reasons are given for the particular methodology which was used. This is not done separately from a strictly descriptive account, rather there is a combined account, under headings of Subjects, Apparatus, Procedure, Stimuli, Error Measures, Statistical Procedures and Presentation of Data. The underlying criterion by which aspects of method were decided upon was that the basic method should result in the same information being made available through each modality of presentation. The reasons for this were set out in the previous chapter.

[2.2] SUBJECTS

In all the experiments, children took part as subjects. In addition, in one experiment (Experiment Ala), adult subjects were employed. Adult subjects are described in the report of Experiment Ala, the account in this section deals only with child subjects.

The subjects were children attending first schools in the Oxford area (see "Acknowledgements" for a list of the schools). The mean ages of groups of subjects are given in the report of each experiment, but three approximate age groupings were used throughout the series of experiments: 6 to 7.5, 7.5 to 8.5, and 8.5 to 10 years.

The total age range may be seen to be less than that used in the comparable published work (Connolly & Jones 1970; Millar 1972, 1975). These studies were discussed in Section [1.3.3]. One important conclusion was that visual-kinaesthetic modality effects change little between 6 and 11 years, there being only a general trend towards improving performance within this age-range.

It was decided to examine in detail the variables affecting performance within the 6 to 9 age range, in order to make comparisons with variables affecting adult performance. Within the age-range studied, it was predicted that previous results would be confirmed, i.e. that any improvement with age would be only an overall trend, not varying between modality conditions.

It was intended, at the start of the series of experiments, that later experiments would include a younger group of children, once the experimental method had been finalised. However, early in the series of experiments, it became clear that there were too many effects to be clarified within the initial age-range, for any extension of the range to be feasible. Interest therefore shifted from developmental trends to within-age-group effects, and it was decided not to extend the age range.

All subjects were right-handed, and used only their right hands in the performance of the experimental tasks. Left-handers were excluded because they would have been either using the non-preferred hand, or using the preferred hand to make movements of a quite different character. Also excluded were children with any known visual impairment, or injury to the right hand or arm.

The subjects attended schools which took pupils from mainly working and lower-middle class areas.

Equal numbers of males and females were tested in each condition of each experiment. When repeated measures were used, the

counterbalancing of the order of conditions was the same for each sex in each between-subjects condition (excepting Experiments B2 and B3). For an additional note on sex as a factor in experiments, the reader may refer to the introduction to Experiment A2b.

Table [2.2.1] The Number of Subjects Selected for each Experiment who were Replaced before Completing Testing

	NUMBER OF SUBJECTS REPLACED DUE TO:	
EXPERIMENT	ABSENCE FROM SCHOOL	FAILURE IN THE TASK
A1a	1	1
A1b	0	0
A2a	0	0
A2b	0	0
A3	6	1
A4	2	0
A5	1	0
B1	1	0
B2	0	0
B3	1	2
B4	0	0
C1	1	0
C2	2	0
C3	2	0
D1	2	0
D2	3	0
D3	3	7*

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* At one school in this experiment there was a high proportion of immigrant children. Failures were not as such in performance of the task, but rather failure to follow instructions such as when to look into the apparatus, release and grasp the lever etc., by children who had a limited knowledge of English.

In experiments in which age-groups were compared, one of two procedures was followed. Either all children were from the same school, or two schools were used, with an equal division of children into age-groups in each school. Whenever possible, all children in an

age-group were taken from the same school class. When this was not possible, children from each class used were divided as equally as possible among conditions.

The younger and older halves of each age-group, and ethnic groups, were also divided as equally as possible among conditions. This may have been unnecessarily cautious, but it was not on the whole difficult to achieve, and may have served to fractionally reduce within-cell variance.

Most of the experiments required subjects to take part in two sessions, on successive days. Inevitably, some subjects had to be replaced because they were absent from school when they were needed for their second session. Very occasionally, a subject was unable to perform the task, and was replaced (without informing the subject or the class teacher that this was the case).

Replacement subjects normally came from the same school class (always from the same school), age division within age-group, and ethnic group. Table [2.2.1] shows the number of subjects who were selected for each experiment, but did not complete testing.

Any one subject took part in only one experiment so as to avoid any possible effects of experience.

[2.3] APPARATUS

The same basic apparatus was used in all the experiments. In this section the basic apparatus, and modifications of it, are described. The basic form was itself developed from the apparatus used by Millar (1972, 1975). The lever, protractor and curtains were the main items modified before using Millar's partly dismantled apparatus.

The apparatus was designed to enable simple lever movements to be presented to subjects, in such a way that the movements could be

perceived either visually or kinaesthetically, but not by both modalities at the same time. For reasons which were dealt with at length in Chapter One, it was of particular importance that visual and kinaesthetic perception should each provide the same amount and type of information about the lever movements. In practical terms, this meant that the following requirements had to be met:

1) For visual presentation, only the top of the lever should be visible, not the remainder of the lever, or the background.

2) For kinaesthetic presentation, the lever should be easily grasped by the subject, without being visible.

3) The experimenter should have control, from second to second, over whether or not the lever is visible, so that the presentation modality can be changed rapidly in cross-modal matching tasks.

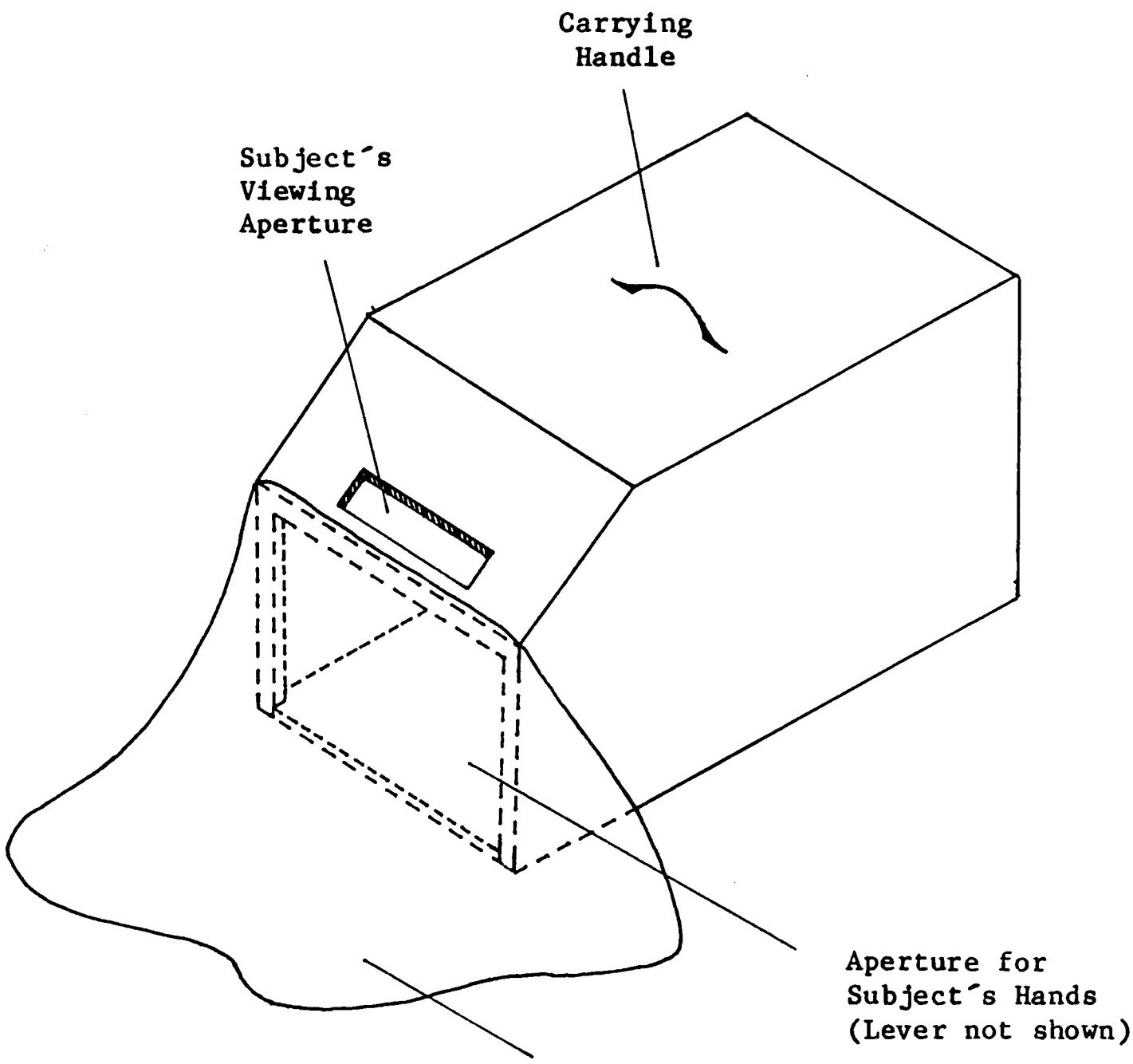
4) Lever movements, whether perceived visually or kinaesthetically, should be controlled by the experimenter.

In addition, the apparatus should be portable, to enable transportation to different schools.

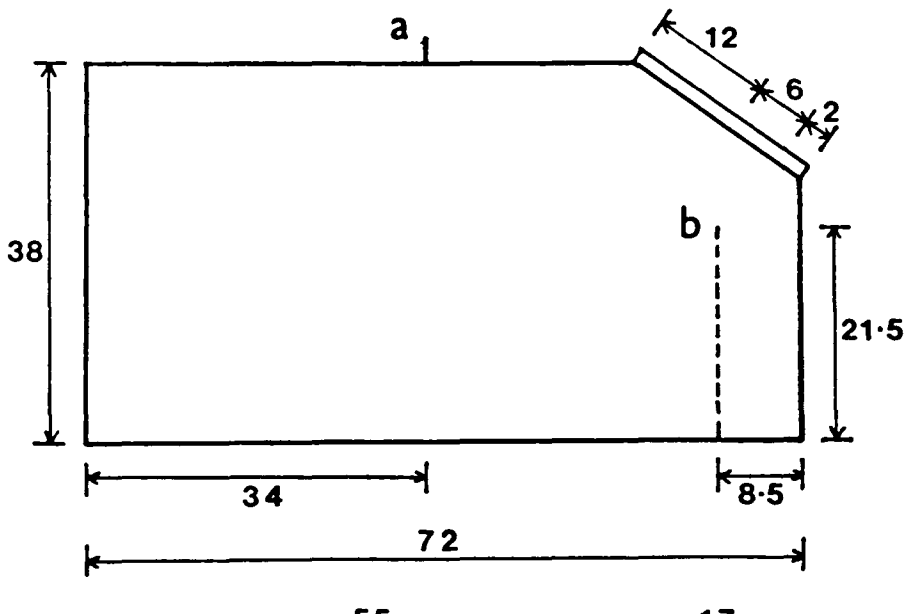
A description of the apparatus will now be given, followed by an account of how the apparatus met the above requirements.

Figure [2.3.1] shows the basic apparatus, which was the apparatus used in Experiment Ala. The dimensions are given in Figure [2.3.2]. Solid wood was used to provide a solid base and frame. The sides and top were constructed of hardboard, and painted with yellow gloss paint on the outside. The interior was painted matt black. A black cloth curtain separated the subject's half of the apparatus from the experimenter's half. A pale grey denim curtain hung over the subject's end of the apparatus. This served to prevent the subject having sight of his hands during kinaesthetic presentation, and to exclude light from the apparatus during visual presentation.

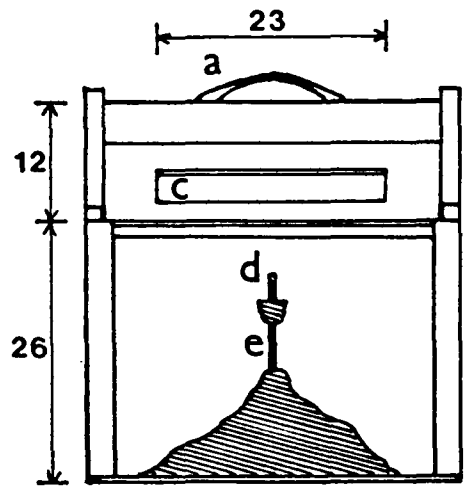
Figure [2.3.1] Diagram of the Basic Apparatus



SIDE VIEW

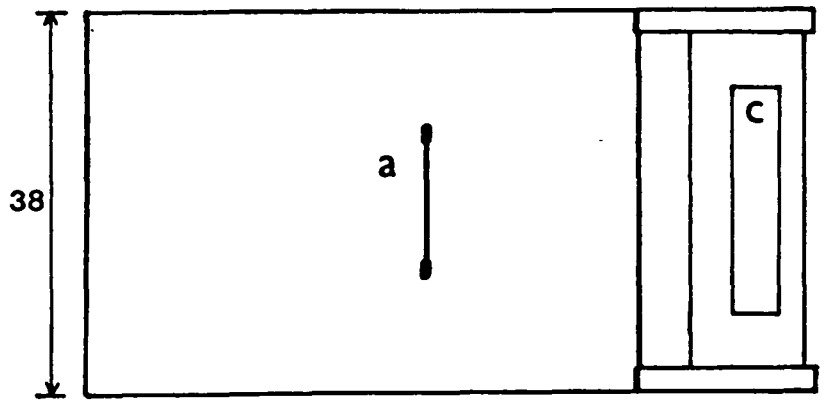


SUBJECT'S END VIEW



SCALE 1:10

All measurements in centimetres



PLAN VIEW

KEY

a carrying handle
b position of lever
c viewing aperture
d light (diameter 1.0)
e lever shaft (diameter 0.65) covered by black cloth at base

Figure [2.3.2] Dimensions of the Apparatus

Figure [2.3.3] The Experimenter's View of the Apparatus

Key

a Perspex protractor

b Lever

c Counterbalance for shutter, with cord passing through the screen which separates the subject's from the experimenter's end of the apparatus

d Socket for power supply to the fluorescent tube

e Socket for power supply to the light on the lever

f Redundant socket

g Fluorescent tube

h Sheet metal supporting the protractor

i Wooden block supporting the protractor

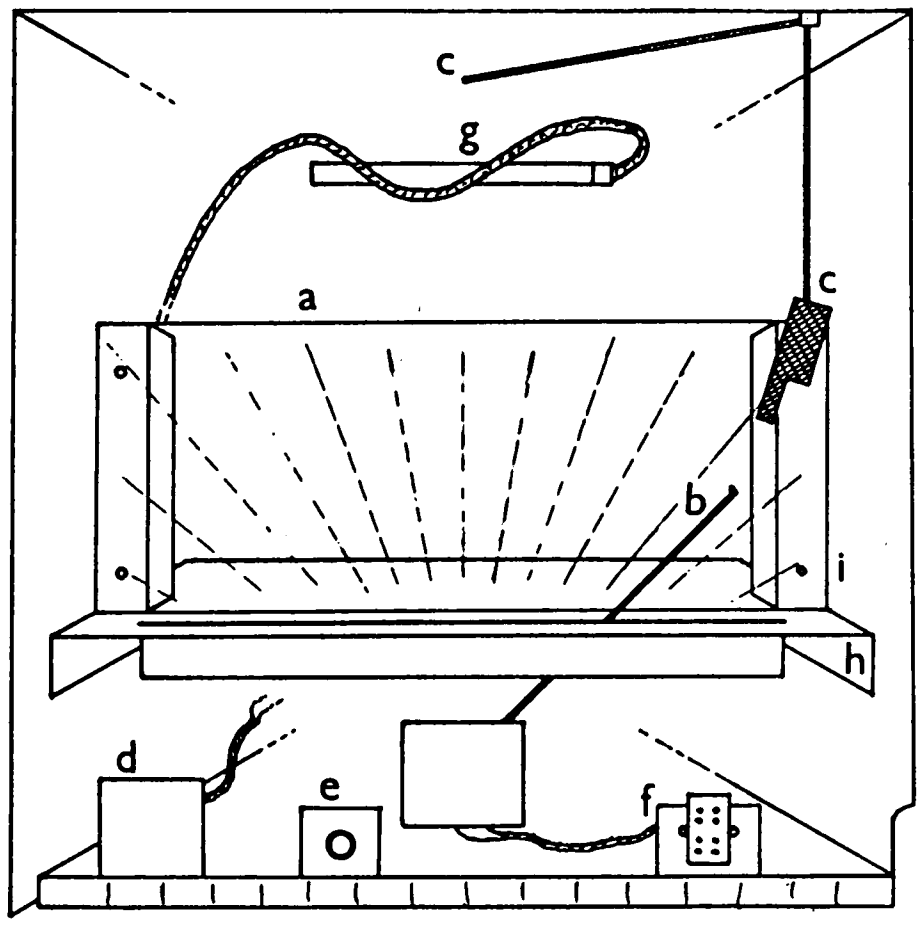
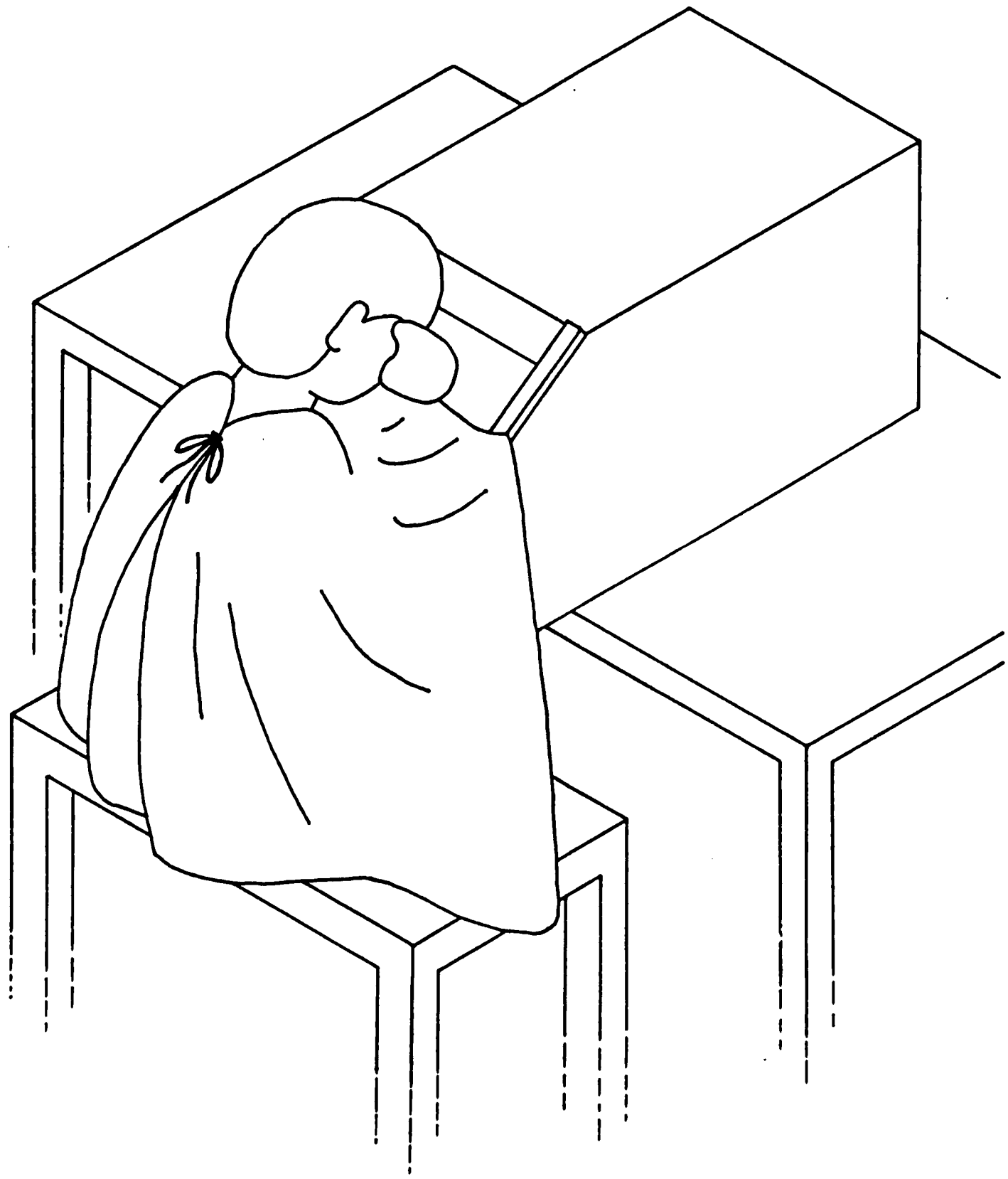


Figure [2.3.4] A Subject Seated at the Apparatus



The subject's lever was brass, with a plastic cover at its top. At its base, the lever was covered by a surround of black cloth. It was connected by means of two brass rods to the lever at the experimenter's end of the apparatus, so the two levers moved in synchrony. The levers could move through a total arc of 140 degrees, in the para-coronal plane, the centre position being in the subject's mid-sagittal plane ("midline").

At the top of the subject's lever, in a translucent plastic shield, was a small 12 volt bulb. The bulb was connected to two wires which ran through the hollow centre of the lever, along the two rods which connected the levers, to a socket at the experimenter's end of the apparatus. A mains transformer was connected to this socket, providing a 6 volt power supply. When lit, the bulb glowed dimly with an orange hue, so that it could be seen, but did not illuminate the interior of the apparatus.

Just below the bulb was a ledge on which the subject's fingers rested when he grasped the lever. This made it easier for the subject to grasp the lever at the same point every time he was required to do so, and to maintain the grasp at the same position.

The viewing panel could be opened and closed by the experimenter. A length of nylon wire passed from a sliding perspex plate in the plane of the viewing aperture, to a pulley at the experimenter's end of the apparatus, and was then attached to a counterweight. When the latter was pulled down, it held the sliding plate up. When the counterweight was released, the plate slid down under its own weight to cover the viewing panel.

Figure [2.3.3] shows the experimenter's end of the apparatus. The lever moved in a plane parallel to a vertical perspex protractor which was marked with 2.5 degree divisions. At its top, the lever was curved towards the protractor, so that positions could be read off

without parallax error.

The fluorescent tube was used to illuminate the interior of the apparatus for maintenance, and to provide a visible background in Experiments A2b and A5.

The apparatus was placed on a table, and the subject sat on a chair so that the tabletop was approximately at knee level. Cushions were used to adjust the subject's elevation if necessary. Figure [2.3.4] shows a subject seated at the apparatus.

Two aspects of the apparatus were changed in subsequent experiments. After the first experiment (Ala), a rubber tachistoscope mask was added. The purpose of this was to make the apparatus light-tight when in use. In order to fulfil this purpose it was necessary that the subject should keep his face resting on the mask. The mask proved to be uncomfortable for some children, the top of the mask was therefore padded. This was done by attaching quilting wadding, covered with dark blue cotton material. Tape was used to attach the padding so that it could be regularly removed for cleaning. The frame to which the mask was attached was made of uncoloured wood.

The second modification of the apparatus was improvement of the curtain at the subject's end of the apparatus. The curtain shown in Figure [3.2.1] was adequate for within-modal matching experiments, but did not enable rapid change of presentation modality without either letting in light or dragging on the subject's arm. A new curtain was therefore designed.

The new curtain formed a tube, into which the subject placed both arms. The subject's left hand was kept on the floor of the apparatus while the right hand was used in the experimental task. The curtain was loosely tied around the back of the subject's neck, using elastic cord; this kept the curtain in place but allowed the subject to sit up between trials.

In a number of cross-modal matching experiments, visual background cues have been eliminated by simply extinguishing the lights in a light-tight room (Diewert & Stelmach 1977, Jones 1973, Marteniuk & Rodney 1979, Newell et al 1979). In these cases, the subjects were adults, and the experiments were carried out in laboratories. Even if a total blackout could be achieved in schoolrooms, it would be too much to expect that all children could cope with carrying out a strange task in the presence of a stranger, in a completely dark room. It is likely that there would at least be considerable distraction from the task.

It was therefore decided to create a small dark space for the children to look into. Once this option was taken, further problems appeared. First, how can visual and kinaesthetic access to a light-tight space be achieved? In Experiment Ala, this problem was not properly solved for either modality, but the change in the curtain and the addition of the mask (as described above), provided the solution.

The second problem raised by using a light-tight space was the magnitude of the movements which could be presented: it became necessary to use short movements. The use of arc movements alleviated this problem by increasing the length of movement which could be presented with the given width of the apparatus. It must still be asked, however, what effect the use of small movements might have on the conclusions which can be drawn from the experiments.

First, with the exception of Diewert and Stelmach's (1977) work on adults, the differences reported have been found for all distances used, including distances overlapping with those used here (movements of between 8.75cm and 20.45cm, positions between 2.92cm and 11.67cm from the subject's midline, 10 degrees = 2.92cm.)

Second, the procedures used in my experiments (see Experiments

A1-A3 for details) created certain difficulties which might offset any greater ease of reproduction of shorter movements. In particular, the following procedures can be pointed out: (a) the isolation of position from length reproduction, and (b) the movement from the midline to the starting point.

Third, if one considers the size of the movement, relative to the size of the subject, then children should be given shorter movements than adults (particularly when kinaesthetic perception is involved). Fourth, any effect of length of movement may be difficult to analyse, since it could result from the length of the movement as such, or from the need for the subject's head to move in order to perceive the entire movement. Head movements could introduce information which is not present during kinaesthetic presentation.

Finally, consideration must be given to the nature of the comparisons to be made with past work. The important conclusions relate to comparisons within the series of experiments carried out using the same apparatus and range of distances.)

In this sense, the results are not intended to be generalised to longer movements. These conclusions are then tested as explanations of past work which differs in a number of aspects of methodology. The way is then open to test the conclusions using different types of movement, including longer movements.

[2.4] PROCEDURE

Subjects were tested individually. Adults were tested in a small testing room. Children were tested in their schools, in a spare room, to which they were lead by the experimenter or by a classmate who had previously taken part in the experiment.

Upon entering the testing room, the subject sat at the apparatus, as described in the previous section. It was ensured that

the subject was not seated in such a way as to be uncomfortable when looking into the apparatus.

When the subject was settled into place, he looked into the apparatus and saw the light. If conditions involving visual perception were to be given, it was ensured that the subject could see the light wherever it went.

If conditions involving kinaesthesis were to be given, the subject was taught to grasp (blind) the lever correctly, and to keep his right hand on the floor of the apparatus when not holding the lever. Having grasped the lever once, very few subjects had any problem finding the lever again in the same place (in any one condition, all the subjects learned to find the lever in the same place, usually the midline). It was then ensured that the subject was able to keep hold of the lever and follow it smoothly as it moved. Again, this presented few problems to the subjects.

Preparation was completed (except in Experiment A1) by instructing the subject to keep looking into the mask until told to look up. Subjects in the KK condition were told that they would not be able to see anything, but that looking in would help them "to think about what they were doing".

The kinaesthetic presentation can be described as passive, on the grounds that the experimenter, not the subject, was responsible for moving the lever. The subject actively moved his arm, and in this sense the presentation was active.

Ideally, presentation would have been completely passive, to match visual presentation. To achieve this would have required (a) more bulky equipment with which to move the subject's arm, (b) a training session to teach the subject to remain completely passive and (c) electromyograph recording to confirm passivity. These provisions were considered to be impractical, unjustifiably time-

consuming, and probably unworkable with child subjects. The evidence was that presentation was effectively passive, since in no case was there a significant cross-modal asymmetry for variable errors (see Experiments A3, A4, A5, B1, D2 and D3), as would have been predicted for active movements (see discussion of Jones (1973) in Section [1.3.2]).

In matching experiments, at a convenient point during the above procedure, it was mentioned to the subject that what he would be asked to do was to watch or feel the light moving around, and "remember where it goes".

The preparation procedures served to relax the subject, by asking him to carry out simple procedures. There followed a period of instruction and practice on the actual task to be given, before test trials began. The practice and test procedures for matching experiments "evolved" from Experiment A1 to A3, so that in Experiment A3, standard procedures were established for the four modality conditions. These procedures are not described here because they are best understood as a part of the sequence of experiments.

[2.5] THE STIMULI

In each matching experiment, (excepting Experiment A1) the same two standard positions were used (20 and 40 degrees left of the subject's midline). This procedure was kept to for three reasons:

1) It was considered to be desirable to have all the standard positions on the same side of the midline, since direction of movement would be expected to affect performance.

2) The distance of the standard position from the midline was found to have a significant effect on constant errors, and sometimes on variability. It was important therefore to include standard position as a factor in the design of experiments. This was simplified by

having only two standards, and interpretation of results was facilitated by using the same standard positions throughout.

3) It was important to ensure that the subjects' responses all fell within the range of possible lever movements, 40 degrees was the furthest position for which this could be guaranteed.

The repeated use of these standard positions with each subject throughout an experiment might be objected to on the grounds that the subject could perform the task by always making the same length of movement as a response. Against this objection, the following points can be made:

(a) For practice trials, different standards were used. The strategy of always making the same length of movement could therefore not be learned until at least the third test trial; it is in any case unlikely that it would be learned in the few trials given to each subject.

(b) Where there is an important effect of practice, the hypothesis under test is supported in the early trials, and not in later trials (see Experiments A5, B4).

(c) The strategy could not be learned unless the subject was first responding accurately without it.

(d) Performance was consistently affected by standard position, which would not be expected if the constant-length strategy were adopted.

Points (a) and (b) can also be offered in reply to the objection that subjects may have become familiar with particular standards. In addition, reference can be made to Gentile and Nemetz (1978). In this experiment, three criterion movements were each presented to each subject 18 times in a session, in six blocks of nine trials. The subjects were adults, and the task was KK matching.

Constant errors (in the direction of undershooting) decreased only after two blocks of trials. Variable errors decreased after one block, then increased again at a point in the series which varied with the motivational level of the subject. There were no changes within blocks. It seems, therefore, that even adults benefit little from repetition of the same standard.

[2.6] ERROR MEASURES

[2.6.1] INTRODUCTION

In the published literature on visual and kinaesthetic matching, a number of dependent variables have been used as measures of performance. An account of these dependent variables is given below. There then follows a description of additional dependent variables which I have used. First, however, I will summarise the experimental method and the type of data obtained.

The general procedure for each trial was as follows. The subject was first shown a movement made by a lever (the "standard movement"). This movement was perceived either visually or kinaesthetically. The lever was then shown moving again (the "response movement"), again with either visual or kinaesthetic presentation. The starting position of the response movement was different from that of the standard movement. In all but one matching experiment, the subject's task was "position matching", as defined in Section [1.1]. In one condition of Experiment A1, a length matching task was used, but this need not concern us in the present discussion, since the same principles apply as for position matching.

After each trial, the experimenter recorded by how much the subject's response deviated from the correct stopping position. It was also recorded, whether the response was an overshoot or an undershoot.

Split-plot factorial designs were used in order to examine the effect on performance of various factors. In order to assess the effects, the distributions of errors in different conditions must be statistically described and compared. How should this be done? I turn now to a discussion of the statistical methods that have been used in related published experiments.

[2.6.2] CONSTANT ERRORS

The raw recorded errors have both magnitude and sign (overshoots positive, undershoots negative). In this form, the errors are commonly called constant errors or algebraic errors. I will refer to them as constant errors, using the following abbreviations:

CE = the constant error on a single trial

$\overline{CE}$ = the mean constant error for a specified set of trials.

Constant errors are a measure of bias to undershoot or overshoot. Strictly speaking, constant errors were the only dependent variable. For the sake of simplicity, however, the various derived measures described below are also referred to as dependent variables.

The distribution of CE is usually approximately normal. The most obvious way in which to statistically describe the data is therefore in terms of mean CE and the standard deviation (or the variance) of CE. In practice, mean CE is often reported, but the standard deviation of CE is used only to test the significance of differences between means, not as a measure of performance in its own right.

In preference to the standard deviation of CE for each condition, either or both of two other measures have been used. These measures, variable error and absolute error, will now be described.

[2.6.3] ABSOLUTE ERROR AND VARIABLE ERROR

Variable error (VE) is the standard deviation of CE for each subject in each condition, and is a measure of performance efficiency. Variable error is discussed further below.

Absolute error (AE) is the magnitude of an error, without regard to sign. What do absolute errors measure? In the simplest terms: accuracy. For some purposes, this answer may be sufficient. For example, when comparing machinery controls, it may be necessary only to say which of several sets of controls allows the operator to work with the greatest accuracy. For my own purposes, the interpretation of AE must be examined more carefully.

Two extreme cases may be envisaged:

(a) If $\overline{CE} = 0$, in a given experimental condition, then mean AE ($\overline{AE}$) is a measure of performance variability.

(b) If every CE in a given condition has the same sign, then

$$\overline{AE} = \pm \overline{CE}$$

and $\overline{AE}$ is a measure of bias to undershoot or to overshoot.

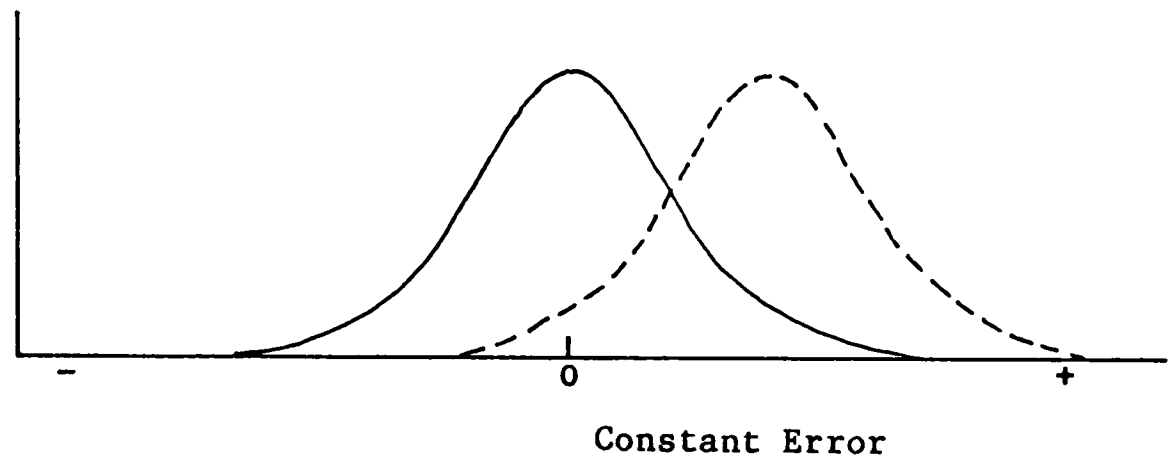
Clearly, interpretation of mean AE depends upon the distribution of CE. In real experiments, the situation is unlikely to be either of the the extreme cases noted above. The interpretation of mean AE will then not be so straightforward. If two conditions result in different values of mean AE, there are three basic ways in which the distribution of CE might differ between the two conditions. The three are illustrated in Figure [2.6.1].

The situations (a) and (b) shown in Figure [2.6.1] are fairly self-explanatory. Part (c) requires some explanation in light of the statement I have made that CE is usually normally distributed. The situation depicted could arise if a single condition in an

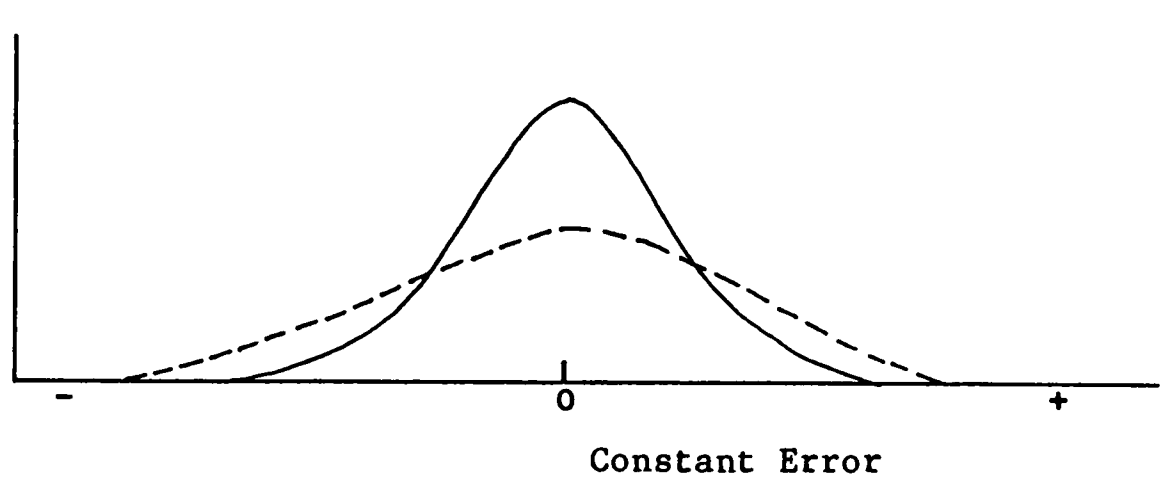
Figure [2.6.1] Three Possible Sources of a Difference in Mean Absolute Errors Between Two Conditions
(See text for further explanation)

Each diagram shows the frequency distribution of constant errors for two hypothetical distributions, the two conditions being indicated by continuous vs broken lines.

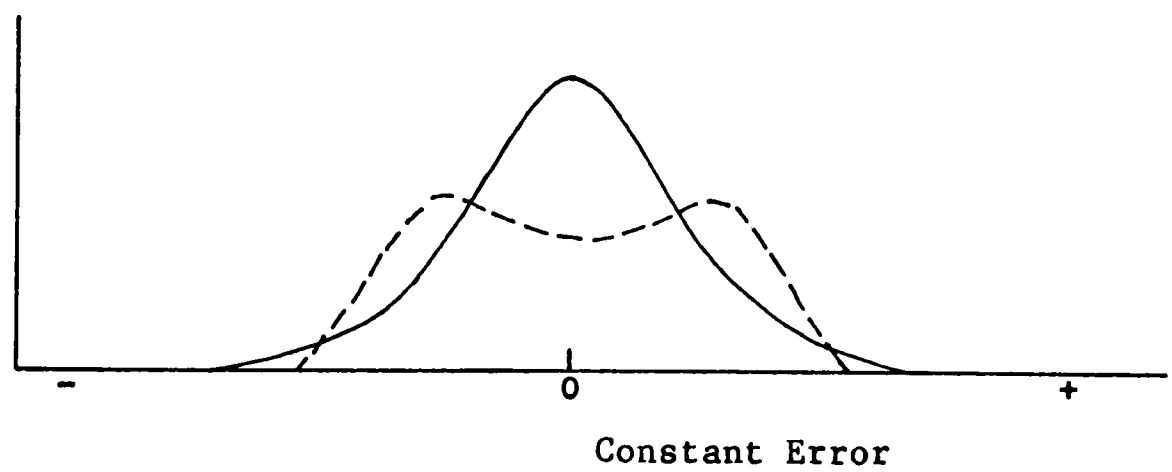
(a) A difference in $\overline{CE}$



(b) A difference in variance of CE



(c) Two or more sub-conditions within one condition, resulting in a multimodal distribution.



experiment actually contains two conditions because of an uncontrolled variable. An important variant of this is the possibility of strategy varying within a condition, as discussed below.

The problems of interpreting mean AE can be highlighted by means of some observations on Figure [2.6.1]. First, if only constant errors and absolute errors are analysed, differences in variability could go undetected because they are obscured by differences in mean CE. Second, mean AE and mean CE can be redundant measures. This is potentially serious because the same property of a distribution might be used twice, in order to draw conclusions about both 'accuracy' and 'bias'. Finally, in practice the situation is likely to be some combination of the possibilities shown in Figure [2.6.1]. If more than two conditions are compared, then identical differences in mean AE between conditions may have to be given different interpretations in different cases.

The main point of the preceding discussion is that AE is difficult to interpret because it has two components, bias and variability. This point has been made by several writers (eg. Howarth & Beggs 1981, Laabs 1973, Safrit et al 1980, Schmidt 1970, Schutz & Roy 1973).

Schutz and Roy (1973), in particular, have dealt with the issue mathematically in some detail. They point out that only two statistics should be necessary for description of a normal distribution. Any third statistic would be determined by the values of the other two. As a specific case, they show that AE is redundant (when mean CE and mean VE are both used), and has been misleading when reported without constant errors and variable errors. In conclusion, they recommend the use of constant errors and variable errors, with absolute errors as an "optional extra".

This conclusion was quickly challenged. Henry (1974) raised two main problems with the Schutz and Roy analysis. First, although CE and VE are independent within individuals, they may be associated within groups. They are therefore not the independent measures assumed by Schutz and Roy. Second, Schutz and Roy's equations require the assumption of normality of within-subject distribution of constant errors. Henry recommended that a third measure be used, but that mean AE be replaced by mean E^2,

$$\text{where mean } E^2 = CE^2 .$$

This recommendation followed Woodworth (1938, p.396), who made the comment "It is preferable to follow modern statistical practice" and use AE^2 in place of AE.

Schutz (1974) replied that E^2, although more amenable to statistical analysis than AE, is still subject to most of the problems associated with AE, which are still greater than the problems of using CE and VE. Despite a final word from Henry (1975), his recommendations have not noticeably been heeded. This is also the case with the suggestion of Schutz (1977) to analyse the absolute value of each subject's mean CE.

Roy (1976) suggested the use of multivariate analysis of variance to analyse CE, VE and AE (or E^2) simultaneously. Thomas (1977) warned that AE is too highly correlated with CE and VE for this to be feasible, a point accepted by Roy (1977).

Newell (1976) argued for "judicious" use of AE on two grounds. First, only AE will distinguish two groups if (a) 50% of errors in each group are negative, 50% positive, and (b) subjects are consistent in each group. This point is returned to at (d) below. Second, subjects are instructed, implicitly or explicitly, to minimise AE. Although this point has intuitive merit, it is important to remember that subjects minimise AE to the extent that they

minimise variability and constant bias.

Specifically in relation to my own experiments, there are problems with the Schutz and Roy analysis, as follows:

(a) Obviously, in order to use variable errors, it is necessary to replicate each within-subjects combination of conditions with each subject. In some experiments, few replications, or none at all, may be desirable. For example, when the subjects are children, maintaining the subjects' attention over a number of trials can be a problem. It is necessary that the subject should concentrate on each trial, and the trials are (to the subject) all quite similar, which would tend to create boredom. This problem is not entirely overcome, even if trials are divided among more than one session, and this option can increase replication effects, which are discussed next.

(b) Practice may affect constant errors, especially if a change in strategy occurs with practice. If practice effects occur in some conditions, but not others, variable errors will be relatively large in the affected conditions. If practice has a greater effect in "easier" conditions, then variable errors could be larger than in "difficult" conditions. In other words, the use of variable errors involves an assumption that differences between replications are the result of ramdom error, whereas there may in fact be a systematic effect. In theory, the systematic component could be identified and separated out, but this is not the common practice.

(c) One obvious means by which to alleviate the first problem noted above would be to employ experimental designs with only between-subjects factors. This, however, might increase practice effects, since each subject would receive a series of identical trials. Also, significant effects would be more difficult to detect, and the number of subjects required would increase. The disadvantage of between-subjects comparisons is highlighted in Section [6.D2.4].

(d) In any given condition, mean AE is affected both by within- and by between- subject variability. Mean AE therefore contains information not contained in mean CE and VE. Schutz and Roy miss this it seems, because their calculations are concerned with the inter-relation of dependent variables for each subject, not for each condition. This is implicit in the comments of Henry (1974) and Newell (1976), (see also Schutz (1974) and Henry (1975)). The theoretical importance of between-subject variability is discussed below. For the present it is sufficient to note that it is a component of mean AE but not mean CE or mean VE.

It may often be possible to discount these problems, depending on what a particular experiment is designed to test, and the actual effects of replication and between-subject variability. In my own experiments, I have taken two steps to make clearer the interpretation of results: modification of the calculation of variable errors, and additional analysis of between-subject variability.

[2.6.4] MODIFICATION OF VE: WITHIN-SUBJECT DEVIATION

A minimum number of two replications is needed in order to calculate variable errors. Considerations of experimental design and practice effects, as discussed above, led to a decision to use this minimum number (except in Experiment D2). Some increase in the error of estimate of each value of VE must result from this, but more values of VE could be obtained from the same number of trials, which would tend to increase the reliability of each value of mean VE.

For each subject, VE is normally calculated by:

$$VE^2 = \sum_{i=1}^{n} (CE_i - \overline{CE})^2/n$$

Where CE_i = constant error on the ith trial

$\overline{CE}$ = mean constant error over i trials

Calculation of VE was simplified according to the following rationale:

When n=2, $(CE_1 - \overline{CE})^2 = (CE_2 - \overline{CE})^2$

$$= [(CE_1 - CE_2)/2]^2$$

$$\Rightarrow VE^2 = 2[(CE_1 - CE_2)/2]^2/2$$

$$\Rightarrow VE = (CE_1 - CE_2)/2$$

Multiplying a variable by a constant has the effect of multiplying both the mean and standard deviation by a constant, and hypothesis-testing is not affected (otherwise the choice of units of measurement would affect the conclusions of an experiment). The variable which was analysed was therefore, for greatest simplicity, within-subject deviation (WSD), where

$$WSD = 2VE = |CE_1 - CE_2|.$$

[2.6.5] BETWEEN-SUBJECTS VARIABILITY

[2.6.5a] Introduction

Two matters must be dealt with, regarding between-subjects variability. First, how are differences between conditions best tested? Second, if a difference should be found, how is it to be interpreted? In this section, I will attempt to answer the first of these two questions. The second question is addressed in Section [2.6.6].

Consider an experiment having the following design:

(i) Two between-subjects variables, P and Q
having p and q levels respectively.

(ii) One within-subject variable, R, having r levels.

(iii) n subjects per cell, each subject being given w trials at each level of R.

The number of combinations of conditions is pqr,

The number of subjects is pqrn, and

The total number of trials is pqrnw.

For each subject, the difference $CE_{pqrn1} - CE_{pqrn2}$ is analysed as within-subject variability. Clearly, this difference should not be allowed to also contribute to measures of between-subject variability. For this reason, analysis of between-subject variability must be performed on the subject mean,

$$\overline{CE}_{pqrn.}$$

Three possible procedures for performing the analysis are described below: variance ratios, within-cell deviation, and between-pair difference.

[2.6.5b] **Variance Ratios**

The ratio of the variance in one condition to the variance in another condition is an F-ratio, and provides an obvious way to test the difference between conditions. There is, however, a problem. Consider, for example, how one would test the difference between the q levels of factor Q. The sum of squares at each level is given by:

$$SS_q = \sum_{p=1}^{p} \sum_{r=1}^{r} \sum_{n=1}^{n} (\overline{CE}_{pqrn.} - \overline{CE}_{.q...})^2 \qquad -(1)$$

A problem arises if at least one of the other factors, P or R has a significant effect on constant errors (either as a main effect or as an interaction). Such a result would affect the sum of squares, SS_q so that the same properties of the data would be interpreted

both as an effect on constant bias and as an effect on between-subject variability.

In order to avoid this, constant errors should be analysed before between-subject variability. The term

$\overline{CE}_{.q...}$ in equation (1)

can then be modified to allow for difference in mean CE between conditions. If P has a significant effect on constant errors, then

$\overline{CE}_{pq...}$

must be used; if R has a significant effect on constant errors, then

$\overline{CE}_{.qr..}$

must be used; if both P and R have significant effects, then

$\overline{CE}_{pqr..}$

must be used. Sums of squares for factors other than Q can be derived by analogy.

[2.6.5c] Within-cell Deviation

The quantity $(\overline{CE}_{pqrn.} - \overline{CE}_{.q...})$ can be used, as in equation (1), to calculate the between subjects variance in a given condition. An alternative procedure is to treat the absolute value of

$$(\overline{CE}_{pqrn.} - \overline{CE}_{.q...})$$

as a dependent variable, and analyse it by analysis of variance.

Depending on the results of analysis of constant errors, $\overline{CE}_{.q...}$ should be replaced with $\overline{CE}_{pq...}$ or $\overline{CE}_{.qr..}$ or $\overline{CE}_{pqr..}$ as explained in the previous section.

Within-cell deviation is not normally distributed, and it is usually appropriate to use either a logarithm or square root transform when performing the analysis of variance.

[2.6.5d] Between-pair Difference

This method involves the pairing of subjects within groups. The basis of pairing, in my experiments, was that the subjects in a pair should be of the same sex, and should have received the within-subject conditions in the same order of presentation.

The statistic which is analysed is the absolute difference between the values of

$$\bar{C}\bar{E}_{pqrn.}$$ for the two subjects in each pair.

[2.6.5e] Evaluation of measures of between-subject variability

What are the relative merits of the three methods of analysis which have just been described? The first method, analysis of variance ratios, is quite satisfactory when it is intended to analyse only one factor in an experiment, and this factor is a between-subjects factor. With other experimental designs, there are problems with the use of variance ratios, as follows:

(a) It cannot be assumed that the variances to be compared are independent.

(b) No error of estimate (analogous to the standard deviation for testing the difference between means) is generated by the experiment.

(c) The testing of interactions is complicated by the need to make non-orthogonal comparisons between variances.

These problems provided the motivation for inventing alternative methods, the first to be considered being within-cell deviation (WCD). Analysis of WCD is also also subject to problems, however. First, the departure of the distribution of WCD from normality is not always completely overcome by transformation of the

data. Second, the values of WCD within each cell are not independent of eachother, since each depends on

$$\overline{CE}_{.q...}$$

which itself depends on each value of

$$\overline{CE}_{pqrn}.$$

The within-cell variance estimate would be, on this account, not strictly valid. More precisely, there would be an increased probability of declaring chance differences to be significant.

Because of the above-mentioned statistical complications, wherever possible, the method of between-pair difference is used. However, the importance of analysing between-subject variability was not realised until quite late in the experimental programme. As a result, not all the experiments were designed in such a way as to allow analysis of between-pair differences. When this problem arises, one of the other two method is used, and a justification is given in each case, in the report of the experiment concerned.

The foregoing evaluation of statistical procedures is based on valuable discussions with Dr John Bithell and Dr I.G. Vlachonikolis of the Department of Biomathematics, Oxford University.

[2.6.6] INTERPRETATION OF VARIABILITY MEASURES

Consider two conditions in an experiment, X and Y. I wish to draw a distinction between two types of hypothesis which can be proposed about X and Y.

Type 1 hypotheses are of the form "X is a more difficult condition than Y". For example, the task in condition X may require more information to be recalled, or the sense modality used in X may allow less efficient processing of information, or the subjects in condition X may be less able than those in condition Y. An hypothesis

of this type can be tested by analysing variable errors or within-subject deviation.

Type 2 hypotheses are of the form "In X, strategy is more variable than it is in Y". For example, there might be two equally efficient or obvious strategies in X, but only one in Y. It is possible, in fact probable, that different strategies will result in different mean constant errors. If this is the case, then it would be predicted that variability should be higher in X than in Y. This, of course, is the same prediction that follows from the first type of hypothesis.

Is it the case, therefore, that there are always two possible interpretations of differences in variability? To rephrase the question, is it possible to resolve the ambiguity which is inherent in variability results? I believe that it is possible, if between-subject variability is analysed in addition to within-subject variability, and if one assumption is made. The necessary assumption is that strategy is more likely to vary between subjects than over trials, within subjects.

So then, if conditions X and Y differ in within-subject variability, but not in between-subject variability, or to a lesser extent in between-subject variability, an hypothesis of the first type is supported. If the reverse result is found (the difference in between-subject variability is greater than the difference in within-subject variability), then an hypothesis of the second type is supported. If the difference between X and Y is similar for the two types of variability, then either or both type of hypothesis is supported, with a bias towards the first type.

To some extent, a priori statements about possible strategies can be used to resolve ambiguities in the data. If only one strategy is possible in each condition, then there is no hypothesis of the

second type to be tested. All differences could then be interpreted in terms of the first type of hypothesis. In other cases, the predicted effect on variability might be in opposite directions for the two types of hypothesis, so that there is no ambiguity in the interpretation of significant differences.

It should be emphasised that hypotheses of the second type relate to the situation where the possible presence of two or more strategies within a condition might increase variability. If different strategies are used in different conditions, one per condition, then the conditions may differ as a result of one strategy being more efficient than the other: this situation is covered by hypotheses of the first type.

In conclusion, within- and between-subject variability should both be analysed, and the results of each analysis interpreted with reference to the results of the other analysis. The interpretation should also make reference to the results of the constant error analysis, for the reasons given in Section [2.6.3b], and to the possible strategies in each condition.

[2.7] STATISTICAL PROCEDURES

Analysis of variance was performed using the BMD statistical package (Dixon 1977), program BMDP2V on the Oxford University ICL 2980 and 2988 computers.

Subsequent multiple comparisons were performed according the the recommendations of Kirk (1968), as follows:

(a) T-tests for a priori orthogonal comparisons. This was rarely actually possible because too many comparisons were usually required for the number of conditions being compared.

(b) Newman-Keuls post hoc tests, employing the studentised range statistic q, for a priori non-orthogonal and a posteriori comparisons

among sets of single conditions.

(c) Sheffé tests for a priori non-orthogonal and a posteriori comparisons among sets of conditions which included combined conditions.

In addition, non-parametric tests were used in Experiment A1, for the reasons given in that experiment.

Where methods of analysis other than analysis of variance were used, the methods are referenced in the experiments concerned.

[2.8] PRESENTATION OF DATA

In experiments in which multifactorial analysis of variance is carried out, several sizeable tables of data need to be presented. In order to minimise interruption, as few tables as seems necessary are placed in the main text. Normally, the only results tables included in the text are tables of mean errors which are needed in order to explain significant effects. Complete tables of means are given at the end of the results section of each experiment, but these are not referred to in the text. ANOVA tables for experiments in Chapters Three to Six are presented in Appendices A to D respectively.

Analysis of absolute errors is not reported in the results section of each experiment, for the reasons given in Section [2.6.3]. For the benefit of those who may wish to examine absolute errors, ANOVA and mean tables are given in Appendix E.

In general, computer calculations were carried out to five decimal places, hand calculations were performed to three decimal places, and results are reported to two decimal places.

CHAPTER THREE: THE EXPERIMENTS, PART A*

[3.A] INTRODUCTION

In Chapter One, it was pointed out that confusion can easily arise, between three possible theoretical analyses of differences between cross- and within-modal matching performance. Briefly, the three possibilities are:

(a) There is modality-specific storage and processing of information, so that cross-modal matching requires translation of information between stores.

(b) Different information is normally available through different modalities, so that transformation of information is necessary, in order to perform cross-modal matching.

(c) Even when the same information is available, encoding strategy varies with modality, so that different information is encoded through different modalities. As with (b), this would make necessary a transformation, when cross-modal matching is performed.

In this chapter, an attempt is made to assess the relative merits of (a) and (b). The general method employed is to compare modality conditions under different levels of available information. This was considered to be the most appropriate method with which to start, because it can be said with certainty that modality conditions typically differ with respect to information available (see Section [1.3]), whereas it cannot be said with certainty that modality

* The experiments are organised into four sections, of which this is the first. The sections are for convenience of description only, and do not necessarily indicate the order in which the experiments were performed. For this reason, it will from time to time appear that later experiments provide the basis for earlier experiments, when this is in fact not the case.

conditions differ with respect to modality-specific channels or strategy. Differences in available information must therefore be eliminated before other differences (if any) can be identified.

EXPERIMENT A1a

[3.A1a.1] INTRODUCTION

This experiment can be regarded as a pilot for the whole series of experiments to be described. Its purpose was to refine the practical details of the experimental task, apparatus, and procedures to be used. Cross-modal conditions were not included in this experiment, since it was felt that most aspects of method could more economically be explored using within-modal matching only. Furthermore, the study of cross-modal matching is hindered when differences between within-modal tasks are not properly understood.

A secondary purpose was to gain experience working with children and teachers in schools. The task was also given to adults, mainly in order to gather comments on the difficulties experienced, strategies used by the subject, and the clarity of the instructions.

The following practical points were of interest:

1) Does the task in any way involve presentation of more information in one modality than in the other? If so, the task must be changed. The reason is that conclusions cannot be made regarding differences between sense modalities as such, unless the information available is controlled (see Section [1.2] for further discussion.)

2) Is the subject using the same strategy in each modality? Strategy, in addition to information available, must be controlled if differences between modality conditions are to be accounted for (Section [1.2]). One possible difference would be in the use of midline-relative coding. In each modality condition, the subject might or might not code positions relative to the midline. If the midline were used in one modality, but not in the other, a cross-modal deficit might result from the mismatch of information.

The effect of a planned mismatch is examined in experiments A2b and A5, and further experiments on the role of the midline are described in Chapter Five. In the present experiment, the main purpose regarding the role of the midline was to check for differences between visual and kinaesthetic strategies, before examining cross-modal matching.

Two standard movements are used in the experiment (see Section [3.A1a.2c] for details). For each standard, the distance moved from start to end is the same, both in acquisition and (if response is correct) in response. The only difference is in location relative to the subject's midline. The tendency to undershoot has often been found to increase with distance to be recalled (e.g. Laabs 1973, Marteniuk 1973, Millar 1972, Newell et al 1979). The distance to be recalled will only differ between standards if midline-relative coding is used: such coding should therefore, if used, be detected in the analysis of constant errors in this experiment.

3) In subsequent experiments, with cross-modal conditions, it was intended that the order of procedures in the acquisition-response interval would vary with modality condition. It was decided, therefore, to test the effect of different orderings of interval procedures. These procedures are: grasping the lever, releasing the lever, revealing the light, and occluding the light (see Section [3.A1a.2g] for details).

4) It is clearly important that the instructions should be understood and followed by the age-group to be used (6-year-olds). This experiment served to check this point.

5) There should be no incidences of children overshooting to such a degree that they try to respond at some point beyond the end of the scale in the apparatus. Again, the experiment provided a check for this.

[3.A1a.2] METHOD

[3.A1a.2a] Introduction

The rationale for the task, and the method, have been dealt with in some detail in Chapters One and Two. It is probably worth summarising at this point, what the subject had to do.

1) In the first phase of each trial, the subject either saw or felt a lever make an arc movement, crossing the midline. In the visual condition, the inside of the apparatus was dark, and the subject saw only the illuminated tip of the lever. In the kinaesthetic condition the subject felt the lever move with his hand, but did not see it.

2) There was then an interval, during which the lever did not move.

3) In the final phase, the subject had to feel or watch the lever as it moved again, and respond by saying "stop" at the appropriate position. In the Length condition, the lever started its movement from a different place, and the length of the first movement had to be matched, ignoring the end-point. In the Position condition, the end-point of the first movement had to be matched, again with a different starting point, ignoring the length of the movement.

[3.A1a.2b] Design

A factorial design was used, Age(2) x Sex(2) x Modality(2) x Matching Cue(2) x Interval Procedure(2) x Standard Position(2), with repeated measures on the last three factors. The design was incomplete (see "Counterbalancing", below) so that only two of the repeated measures factors could be analysed in any one ANOVA. This turned out not to be an important problem (see RESULTS).

[3.A1a.2c] Conditions

The two age groups were 6-year-olds and adults (see Subjects

for mean ages). The two modality conditions were visual and kinaesthetic within-modal matching (VV and KK). The matching cue was either length or position, as described in Section [3.A1a.2a]. The interval procedures are described under Procedure.

The movements involved in each standard condition are shown in Table [3.A1.1]. Points to the left of the experimenter's midline are given a negative sign, points to the right a positive sign.* The scale in the apparatus ranges from -70 to +70 degrees. With each standard, and in both matching cue conditions, acquisition involves a 50 degree movement. For both standards, the distance from the start of the response movement to the correct response position is the same (70 degrees). The only difference between the standards is therefore their position relative to the subject's body midline.

Table [3.A1.1] Summary of the Movements used in each Matching Cue Condition with each Standard

(All measurements in degrees)

STANDARD	ACQUISITION MOVEMENT	RESPONSE MOVEMENT (STARTING POINT TO CORRECT RESPONSE POSITION) "LENGTH" CONDITION	"POSITION" CONDITION
10	-40 to +10	-20 to +30	-60 to +10
40	-10 to +40	-30 to +20	-30 to +40

[3.A1a.2d] Counterbalancing

Combination of the group factors, Age, Sex and Modality, results in eight groups of subjects. Counterbalancing was the same within each of these eight groups. Children had Length and Position

* The experimenter's midline was aligned with the subject's midline. Positions were given signs with respect to the experimenter's midline so as to reduce the likelihood of errors of sign in recording the subject's response.

trials in separate sessions; adults had both conditions in the same session, but for convenience of description their Length and Position trials will be referred to as though they had been in separate sessions.

Each subject had 4 trials, two in each session. In each subject group, half the subjects had Length trials and half had Position trials in the first session. In each session, one trial with each standard was given, with a different interval procedure for each standard. In two sessions, each subject was given each of the four possible combinations of standards and interval procedures. Four subjects per group were used to counterbalance the order in which the four combinations were given, by a 4 x 4 Latin Square.

[3.A1a.2e] Apparatus

The apparatus was as described in Section [2.3].

[3.A1a.2f] Subjects

The subjects were 16 children of mean age 6yrs,3mths (range 5yrs,8mths to 6yrs,10mths) and 16 adults of mean age 22yrs,6mths (range 19yrs,10mths to 27yrs,7mths). Further details of child subjects are given in Section [2.2]. Adult subjects were all right-handed and had no known visual impairments. Of the 16, 12 were students, 4 were members of a regular panel of volunteers for psychology experiments. All were paid for taking part.

[3.A1a.2g] Procedures

Children were tested in a spare room in their school. Adults were tested in a small testing room.

Subjects in the visual matching group looked into the

apparatus, and watched the light as the experimenter moved it from one end of the scale to the other. This was done to ensure that they could see the light wherever it went.

Subjects in both modality conditions learned to grasp the lever without being able to see it. Subjects in the kinaesthetic matching condition also learned to follow the lever with their right hand, smoothly, as the lever moved.

After this preliminary training, practice was given on the procedure for the matching cue condition which was to be given first. In the second session, training began with training for the procedure in the matching cue condition which was to be given in that session.

Up to five practice trials were used to demonstrate which of Length and Position was to be remembered. Once this appeared to be understood by the subject, the instructions on each trial were similar for each matching cue condition. The principle difference was in the words used to direct the subject's response. In the Position condition he had to respond (by saying stop) "at the same place", in the Length condition he had to respond after "the same distance" or "as far as before".

The procedures are set out in Table [3.Ala.2]. Verbal instructions were used only at the following points in each trial: (i) at the start of each trial, to indicate that a trial was starting, (ii) when the lever was about to move to demonstrate a movement, (iii) whenever the lever had to be grasped or released, and (iv) when the lever was about to move for the subject to respond.

As mentioned above, the subject's response, in each modality condition was to say "stop". The delay before the experimenter stopped the lever moving should therefore have been the same in each modality condition (as argued by Diewert and Stelmach 1977).

Table [3.Ala.2] Procedures Carried Out During the Acquisition-Response Interval in Experiment Ala

This table shows the procedures carried out in each of four conditions, during the interval of one trial (15 seconds).

Order of Procedures	CONDITION*			
	VV1	VV2	KK1	KK2
1)	Experimenter closes the shutter		Subject releases the lever	
2)	Experimenter returns the lever to the midline			
3)	Subject grasps the lever		Experimenter opens the shutter	
4)	Subject releases the lever	Subject rests		Experimenter closes the shutter
5)	Experimenter opens the shutter			Subject grasps the lever
6)	Subject rests	Subject releases the lever	Experimenter closes the shutter	Subject rests
7)		Experimenter opens the shutter	Subject grasps the lever	
8)	Experimenter moves the lever to the starting position for the response movement			

*VV = visual within-modal matching
KK = kinaesthetic within-modal matching
1,2 are labels for the two interval procedure conditions

In each interval procedure and modality condition, the movements made by the lever were the same. The same events occurred in the acquisition-response interval in each condition, only the order of the events was varied. The major difference between

procedure conditions was that in condition 1, the subject rested for 5-10 seconds with the light visible at the midline. In condition 2, the lever was held by the subject, at the midline, during the same 'rest' period.

The total delay between acquisition and response was 15 seconds. The second trial in each session followed about ten seconds after the first.

[3.A1a.3] RESULTS

[3.A1a.3a] Practical Points

With varying amounts of practice and explanation (not systematically recorded), all the 6-year-olds were able to understand the distinction between Length and Position, and perform the task. The results indicated, however, that in spite of apparently understanding the distinction, there was a tendency to respond on the wrong basis. That is, it seems that sometimes Length was matched when Position was required, and vice versa. This is dealt with in more detail under the constant error results, and in the discussion.

Varying amounts of practice were needed to understand the task before test trials could be given, as a consequence of which, less able subjects had more practice. This may have affected accuracy, resulting in improved performance in the conditions that were more difficult to understand. For example, only two trials were usually needed in the position condition, but more often four or five in the length condition.

It was decided that, ideally, practice should in future be standardised. This is difficult, however. If minimum practice were given, variance would increase in the tasks and groups where understanding is most difficult. Linguistic understanding would then affect practical performance. If maximum practice were given, the

result would quite likely be boredom in the more able subjects, and with tasks that are easier to understand. The solution to this problem is taken up in Experiments A1(b) and A2(a).

There was no problem with subjects not responding before the end of the scale was reached. Most errors were in fact undershoots.

Adults reported coding relative to the borders of the viewing aperture. A tachistoscope mask was therefore attached to the aperture in subsequent experiments, preventing this strategy (see Section [2.3]). One adult reported using tactual cues given by the angle his forearm made with the table edge in the kinaesthetic condition. This was prevented in subsequent experiments by moving the apparatus to the edge of the table (i.e. nearer to the subject).

[3.A1a.3b] Constant Errors

As noted under 'Design', all three repeated measures factors could not be included in one ANOVA. Preliminary examinations of the data suggested that there was no effect of interval procedures. This was confirmed by two 2 x 2 x 2 x 2 x 2 ANOVA's, as follows -

1) Age x Modality x Sex x Matching Cue x Procedure

2) Age x Modality x Sex x Standard x Procedure.

The interval procedures factor was therefore dropped from the analysis, and constant errors analysed by 2 x 2 x 2 x 2 x 2 ANOVA (Age x Modality x Sex x Matching Cue x Standard) with repeated measures on the last two factors.

One main effect was significant, that of Age [$F(1,24) = 12.84$, $p < 0.002$]. Two interactions were significant, Modality x Cue [$F(1,24) = 5.39$, $p < 0.05$], and Cue x Standard [$F(1,24) = 10.51$, $p < 0.005$]. The relevant mean values for these effects are shown in Table [3.A1a.3]. Complete mean tables are given under RESULTS in Experiment A1b.

Table [3.A1a.3]

(a) Mean Constant Error for the Significant Effects

EFFECT	CONDITION	Mean Constant Error (in degrees)
Age	6 year olds	-10.82
	Adults	- 4.85
Modality x Cue	VV (L)	- 6.64
	VV (P)	- 5.31
	KK (L)	- 6.80
	KK (P)	-11.80
Cue x Standard	L 40 degrees	- 3.75
	L 10 degrees	- 9.69
	P 40 degrees	-11.72
	P 10 degrees	- 5.39

(b) Differences which were Significant in Mann-Whitney U Tests

Comparison	U	p<
6 vs Adult	1224.5	0.0001
KK(P) vs VV(L)	384.5	0.05
KK(P) vs VV(P)	312.0	0.005
KK(P) vs KK(L)	361.5	0.05
L 40 deg vs L 10 deg	346.5	0.05
L 40 deg vs P 40 deg	290.0	0.002
L 10 deg vs P 10 deg	384.0	0.05
P 40 deg vs P 10 deg	316.5	0.005

L= Length
P= Position

The interpretation of the significant effects is confused by variations in the shape of the distribution of errors (see below for further discussion of this). The significant effects were therefore re-examined using non-parametric tests.

It may with fairness be asked, therefore, why analysis of variance was used for the overall analysis of the experiment. It would in fact, be difficult to justify this mixed form of analysis in

the general case. In this present experiment, however, the following justification can be offered: 1) The use of analysis of variance allows the simultaneous analysis of more factors than would be possible with non-parametric tests. 2) Analysis of variance is a fairly robust test even when the required assumptions cannot strictly be justified. 3) The main deviation from the assumptions of ANOVA was that the distribution of errors tended to be bimodal. The analysis of variance would therefore tend to exaggerate differences between conditions i.e. significant differences would not be missed, and the non-parametric tests would remove any differences which had wrongly been declared significant.

The results of multiple comparisons, using the Mann-Whitney U test, are shown in Table [3.A1a.3], along with the means to which they relate. In summary,

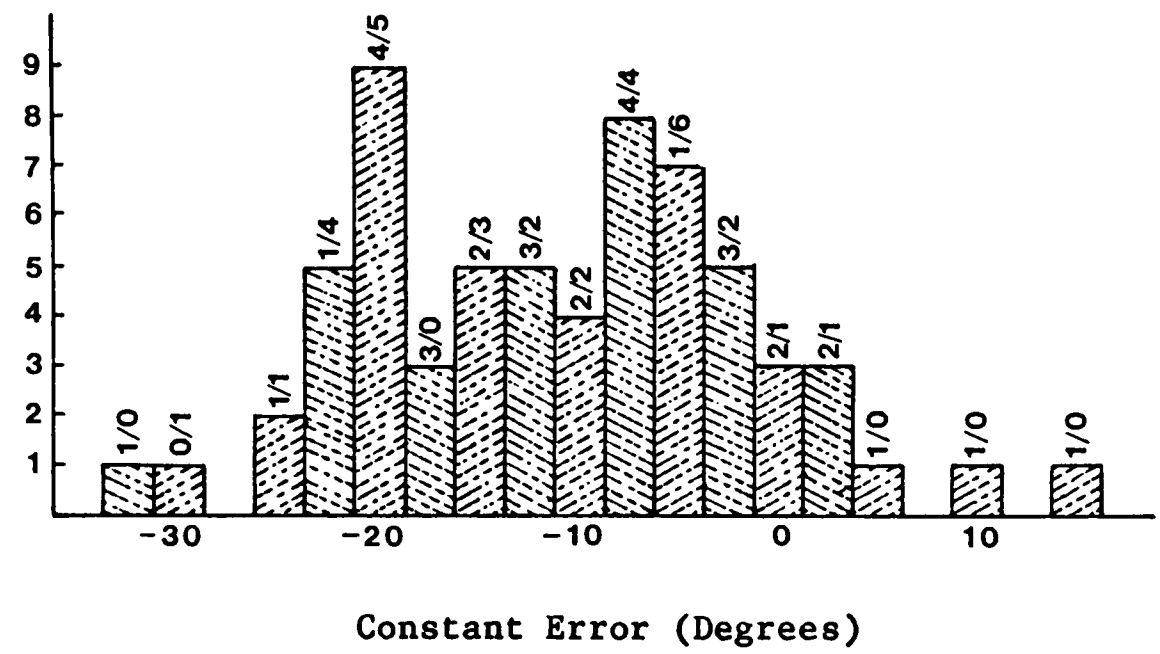
(a) the 6-year-olds had a greater tendency to undershoot than adults.
(b) kinaesthetic position matching resulted in greater undershooting than other combinations of modality and matching cue
(c) undershooting was greater in the position matching task, with the 40 degree standard, and in the length matching task, with the 10 degree standard, than in the other two conditions.

Figure [3.A1a.1] shows graphs of the distribution of constant errors. The separate graphs show data for combinations of matching cue and standard which resulted in low overshooting, and combinations which resulted in high overshooting (parts (a) and (b) of Figure [3.A1a.1] respectively). When there is high undershooting, the distribution takes a markedly bimodal form. In both graphs there is a main peak at about -5.0 degrees. In the upper graph there is an additional peak at about -20.0 degrees.

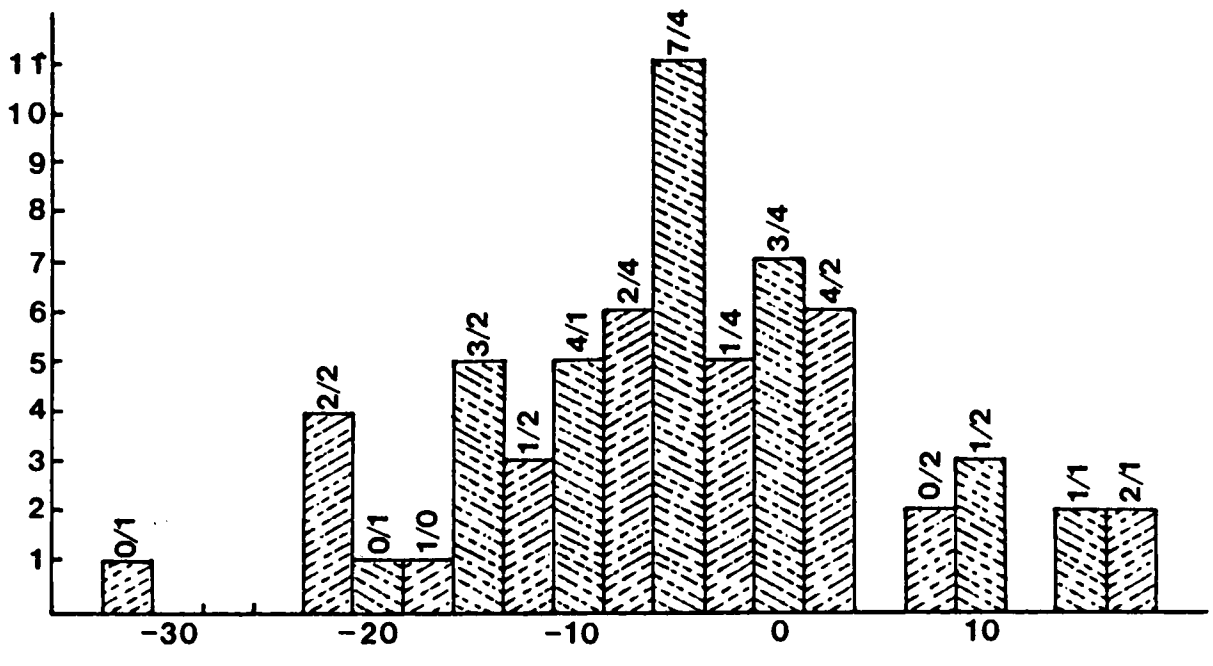
Figure [3.Ala.1] Distribution of Constant Errors in Experiment Ala

Two conditions are combined in each histogram, the frequency in each condition is noted above each column.

(a) Conditions showing bimodal distribution
(Length, 10 deg. standard / Position, 40 deg. standard)



(b) Conditions showing unimodal distribution
(Length, 40 deg. standard / Position, 10 deg. standard)



How can the bimodal distribution be explained? One obvious explanation is as follows. In the Position condition, with the 40 degree standard, the subjects may sometimes have responded according to the length of the standard, rather than according to its end-point. In the Length condition, with the 10 degree standard, the subjects may sometimes have responded at the end-point of the standard, rather than match on the basis of length. Either of these errors would result in an undershoot of 20 degrees.

If this explanation is correct, why do these 'criterion errors' not occur in the other two combinations of conditions? The simplest explanation is that in the other two conditions, the correct response position is reached first, and a criterion error would result in an overshoot of 20 degrees. It can therefore be hypothesised that criterion errors occur only when the incorrect response position is reached first.

The implications of these criterion errors are considered in the next experiment, under DISCUSSION.

[3.A1a.3c] Variability

Given the significant effects in the analysis of constant errors, analysis of absolute errors would be likely to be misleading, since constant errors are a component of absolute errors (see Section [2.6.3]). The design of the experiment, however, did not allow the alternative analysis of within-subject deviation or of between-pair difference. Within-cell deviation was therefore analysed.

These error measures are described in Section [2.6.5]. In that section, objections to the use of within-cell deviation were raised, but these objections do not apply in the present context. The first reason is that, because of the presence of criterion errors, any analysis of variability could only be approximate. Second, the

important comparison is with the results of Experiment Alb, in which the same method of analysis is used.

As was the case with the analysis of constant errors, two preliminary 2 x 2 x 2 x 2 x 2 ANOVAs were performed,

Age x Modality x Sex x Matching Cue x Procedure, and

Age x Modality x Sex x Standard x Procedure.

In neither case was the effect of Procedure significant, either as a main effect or in interaction. The Procedure condition was therefore dropped from the analysis. It was not considered necessary to repeat the experiment in order to retest the Procedure effect with an improved experimental design, for two reasons:

(a) The use of within-cell deviation raises the probability of declaring a difference significant, relative to the use of between-pair difference, absence of significant effects is therefore not subject to doubt because of the use of within-cell deviation.

(b) A change in the planned series of experiments meant that it was no longer important to control for the effect of Procedure.

The main analysis which was performed on within-cell deviation was a 2 x 2 x 2 x 2 x 2 ANOVA, Age x Modality x Sex x Matching Cue x Standard (4 measurements per cell). A square root transform was used to normalise the distributions.

The only significant main effect was that of Age [$F(1,24) = 11.67$, $p < 0.005$]. In addition, the Age x Matching Cue x Standard interaction was significant [$F(1,24) = 9.74$, $p < 0.005$]. The means for these effects are shown in Table [3.Ala.4]. For the reasons given in Section [3.Ala.3b], Mann-Whitney U tests were used to retest the differences. The results of these tests are given in part (b) of Table [3.Ala.4].

Table [3.A1a.4]

(a) Mean Within-cell Deviation (in degrees), for the Conditions Involved in Significant Effects

		AGE-GROUP			
MATCHING CUE	STANDARD	6	ADULT	6	ADULT
Length	10 deg	2.64	3.21	2.49	2.78
Position	40 deg	2.35	2.35		
Length	40 deg	3.12	2.38	3.65	2.24
Position	10 deg	4.18	2.10		

(b) Significant Differences

COMPARISON	MANN-WHITNEY U TEST RESULT U	p<
6 year olds, L40,P10 combined vs L10,P40 combined.....	700.0	0.02
L40,P10 combined, 6 year olds vs Adults.....	813.5	0.00005

Key: L = Length Matching P = Position Matching
10,40 = 10 degree and 40 degree standards, respectively.

In summary, variability was higher for 6-year-olds than for adults, but this difference was due to two conditions only, the Length condition with the 40 degree standard and the Position condition with the 10 degree standard. Within the 6-year-old group, the same two conditions resulted in higher variability than the other two conditions.

Unless the conditions are grouped in this way, the interaction cannot be interpreted. The same groupings are indicated by the occurrence of criterion errors, as discussed in the previous section. An explanation for the interaction is offered in the discussion

section of the next experiment. Briefly, it can be accounted for by the presence of external visual cues.

[3.A1a.4] DISCUSSION

Discussion of Experiments A1a and A1b is drawn together in the report of the latter experiment. Only a few selected points, relevant specifically to the present experiment, will be made here.

Regarding interval procedures, no evidence was found, in this experiment, of any effect of the order in which procedures were performed in the interval between acquisition and response. Therefore, in Experiment A1b and in all subsequent matching experiments, only one, simpler procedure was used in each modality condition.

Effects of age were in the expected direction. Adults were superior in terms of variability, and showed less bias. For variability, however, the age effect was limited to the 40 degree standard in the Length condition, and to the 10 degree standard in the Position condition. The same conditions resulted in greater variability within the 6-year-old group. If this latter result can be explained, some light may be shed on the reasons for effects of age.

Some necessary changes in apparatus and procedure have already been noted at various points in this report. They are summarised in Section [3.A1b.2].

The role of "criterion errors" makes difficult the interpretation of other results at this stage. It is worth noting, however, that the variability results indicate no clear "superiority" of one modality over the other. Also, on a matter of analytical procedure, this experiment serves to emphasise that it is not sufficient to simply compare means: the shape of the distributions of errors can also be important.

EXPERIMENT A1b

[3.A1b.1] INTRODUCTION

The conclusions which could be drawn from Experiment Ala were relevant mainly to methodology. The present experiment applies some of the suggested changes in methodology. These changes are summarised under METHOD, below. Apart from these changes, the present experiment is a replication of the previous experiment, with six-year-old subjects, but without adult subjects.

Interpretation of the results of Experiment Ala was made difficult by the occurrence of "criterion errors". That is, there was a tendency for subjects sometimes to recall the length of the lever movement when its end-point was asked for, and vice versa. It was predicted that the changes made in the present experiment would eliminate or reduce criterion errors, and that this would eliminate or reduce differences between conditions.

Apart from changes in procedure designed to reduce criterion errors, steps were taken to reduce differences between visual and kinaesthetic presentation. It was predicted that this would eliminate or reduce differences between visual and kinaesthetic matching performance.

[3.A1b.2] METHOD

[3.A1b.2a] Design

A factorial design was used, Modality(2) x Sex(2) x Matching Cue(2) x Standard(2), with repeated measures on the last two factors.

[3.A1b.2b] Subjects

The subjects were 16 children of mean age 6yrs,7mths (range 6yrs,4mths to 7yrs,1mth).

[3.A1b.2c] Conditions

The conditions were as described in Section [3.A1a.2c]. Briefly, modality conditions were visual and kinaesthetic within-modal matching, matching cue was length or position and the standard movement was 50 degrees, terminating 10 or 40 degrees left of the subject's midline.

[3.A1b.2d] Counterbalancing

Combination of the two between-subjects factors, Modality and Sex, results in four groups of subjects. In each group, the counterbalancing was the same. Length and Position trials were given in separate sessions, one trial with each standard per session. Half the subjects were given Length trials, half were given Position in their first session. Each subject was given the two standards, with the order of presentation reversed between sessions. Equal numbers of subjects received each possible order.

[3.A1b.2e] Apparatus

The apparatus was the same as in Experiment A1a, with two changes (the basic apparatus, and the changes, are described in detail in Section [2.3]).

First, a rubber tachistoscope mask was fitted to the viewing aperture, and the subject's face rested on this mask during the visual matching condition. This served to remove visual background cues altogether (while the subject was looking into the apparatus), and to maintain a fixed head-arm-apparatus relation throughout each trial.

Second, the apparatus was placed in such a way that the subject's arm could not rest on the table on which the apparatus was

supported, or on the base of the apparatus itself, while the subject was grasping the lever. This was done in order to prevent the strategy of using the point of contact between the arm and the support as a cue to angle moved.

[3.A1b.2f] Procedure

The procedure was the same as in Experiment A1a, with three changes. First, the procedures carried out during the acquisition-response interval were simplified, and only one set of procedures was used in each modality condition. The modified procedures are shown in Table [3.A1b.1]. The timing of the procedures was, as in the previous experiment, 15 seconds between acquisition and response, of which approximately 10 seconds was spent at rest.

Table [3.A1b.1] Order of Procedures Carried Out During the Acquisition-Response Interval in Experiment A1b

	MODALITY CONDITION	
	VISION	KINAESTHESIS
1)	Experimenter closes the shutter.	Subject lets go of the lever.
2)	Experimenter places lever at midline, subject rests.	
3)	Experimenter opens the shutter.	Subject grasps the lever.
4)	Experimenter moves lever to the starting position for the response movement.	

Second, changes were made in the practice procedure. More time was spent in explaining the task verbally, before demonstrations were given. This reduced the variability in the number of practice trials

needed by different subjects in different conditions.

Third, subjects were not tested within about five minutes of "playtime" or lunchtime. Some subjects in the previous experiment had shown concern about missing these breaks, and had consequently been distracted from the task.

[3.A1b.2g] Summary of Changes in Method

The following changes in method were introduced in this experiment:

(a) A mask was added to the viewing aperture in the apparatus.

(b) The apparatus was moved closer to the subject.

(c) Interval procedures were simplified, and only one set of procedures was used in each modality condition.

(d) Practice procedures were made less variable.

(e) Subjects were not tested immediately before "playtime" or lunch.

[3.A1b.3] RESULTS

[3.A1b.3a] Constant Errors

Constant errors from the present experiment were analysed with constant errors from the 6-year-old group in the previous experiment. The analysis was performed by 2 x 2 x 2 x 2 x 2 ANOVA, Experiment x Modality x Sex x Matching Cue x Standard.

There were no significant main effects, but three significant interactions:

Matching Cue x Standard $[F(1,24) = 20.31,\ p < 0.0001]$

Experiment x Modality x Sex $[F(1,24) = 7.33,\ p < 0.02]$

Sex x Matching Cue x Standard $[F(1,24) = 4.31,\ p < 0.05]$.

These three interactions will be discussed in turn. In each case, non-parametric tests were used to re-examine the significant differences. This was done because distributions were not normal. A

more detailed justification of the procedure is given in Section [3.Ala.3b].

(1) The Experiment x Modality x Sex Interaction

The means for this interaction are given in Table [3.Alb.2], along with the differences between conditions which were found to be significant.

Table [3.Alb.2]

(a) Constant Error Means (in degrees) for the Experiment x Modality x Sex Interaction

MODALITY:		VV		KK	
	SEX:	Male	Fem.	Male	Fem.
EXPERIMENT:	Ala	-6.56	-11.56	-12.66	-12.50
	Alb	-9.84	-3.75	-4.69	-13.59

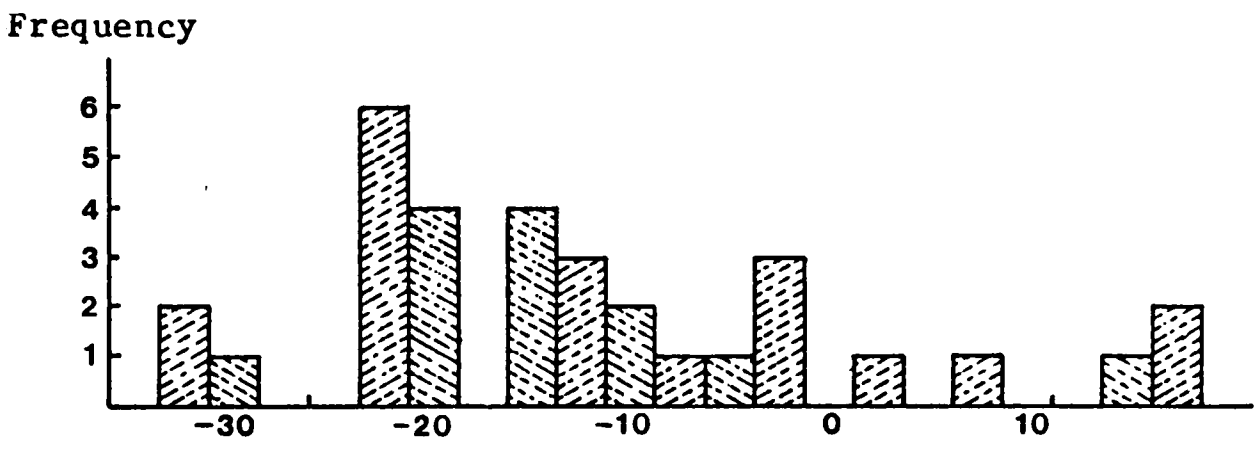
(b) Significant Differences

COMPARISON	MANN-WHITNEY U	p<
Experiment Ala,		
VV (male) vs [VV (female) & KK Combined]	261.0	0.05
Experiment Alb,		
KK (female) vs VV (female)	56.6	0.01
KK (female) vs KK (male)	64.0	0.05
Experiment Ala vs Alb,		
VV (female)	80.5	0.05
KK (male)	66.0	0.01

The difference between experiments was significant for VV (female) and KK (male). This difference was accompanied by a shift in the mode of the distribution of constant errors in these conditions, from -22.5 to 0.0 degrees (see Figure [3.Alb.1]). This suggests that a reduction in criterion errors has occurred.

Figure [3.Alb.1] Distribution of Constant Errors in Conditions VV (Females) and KK (Males)

(a) Experiment Ala
(6-year-olds only)



(b) Experiment Alb

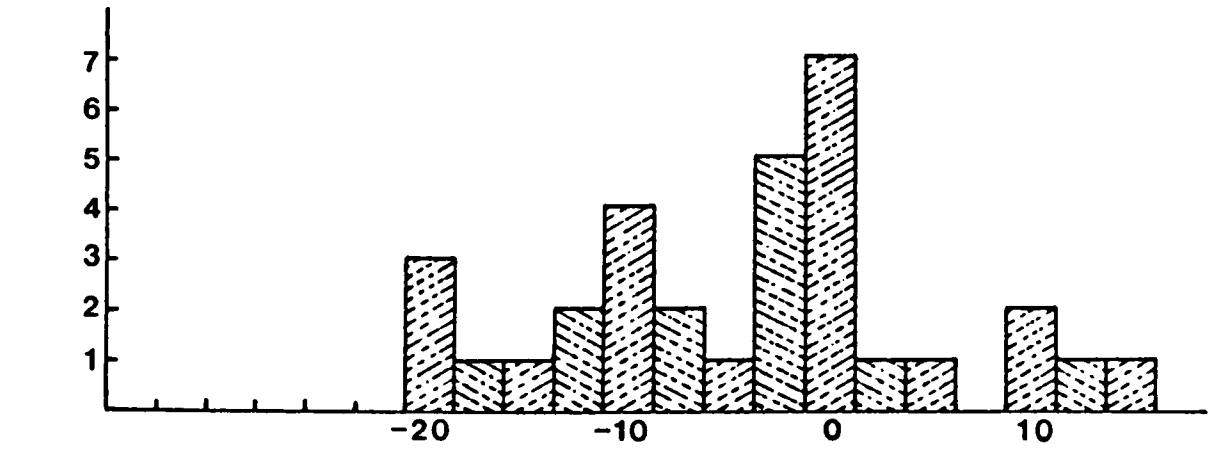


Table [3.A1b.3] shows the number of undershoots of 15 degrees or more in each Experiment/Modality/Sex condition (15 degrees is the position of the trough to the right of the -22.5 degree peak in Figure [3.A1b.1]). For VV (females) and KK (males) combined, the reduction in such errors between experiments is significant, $X^2(df=1) = 6.6$, $p < 0.05$). In KK (females), the number of such errors remains high, and in VV (males) the number is low in both experiments. This confirms that no difference, other than a difference in criterion errors, need be hypothesised in order to explain the differences in mean constant error.

Table [3.A1b.3] The number of undershoots of 15 degrees or more in each combination of the experiment, modality and sex conditions.

MODALITY:	VV		KK	
SEX:	Male	Fem.	Male	Fem.
EXPERIMENT: A1a	5	8	9	13
A1b	5	2	3	12

(2) The Matching Cue x Standard and Sex x Matching Cue x Standard Interactions

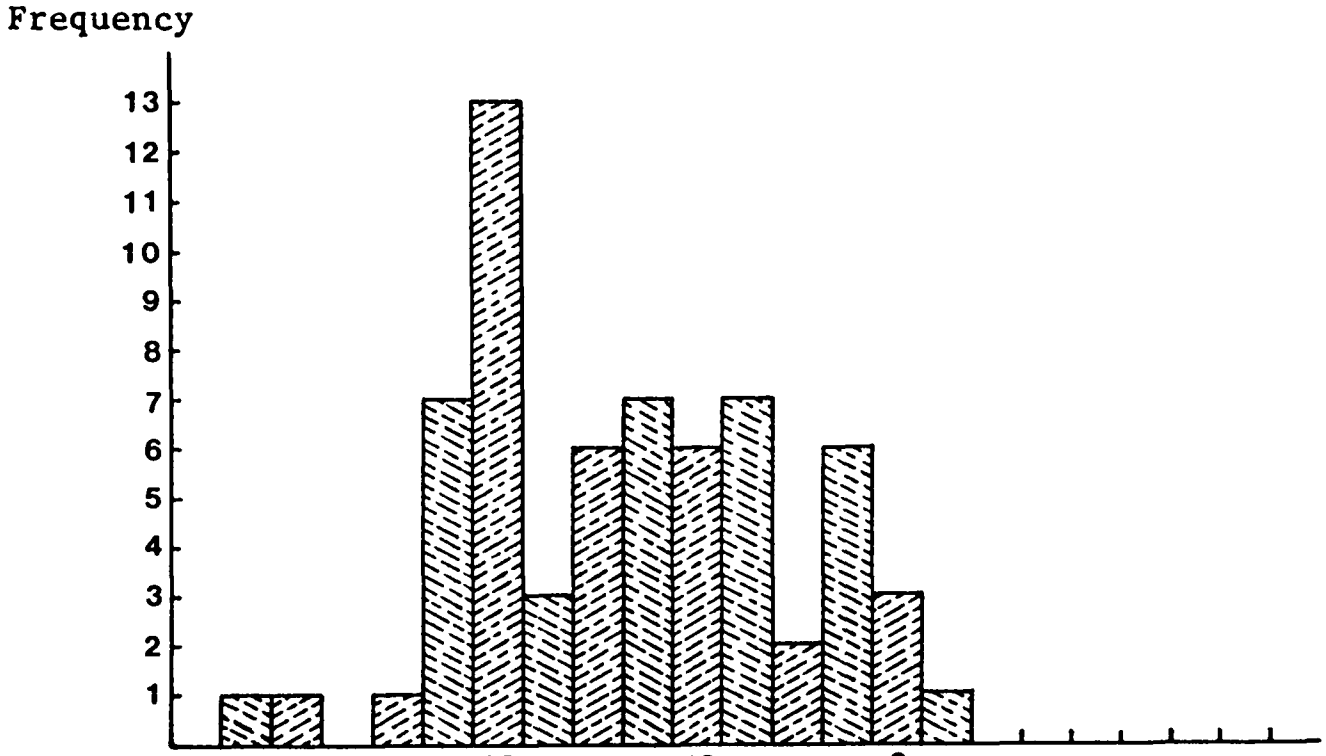
The means for these interactions are given in Table [3.A1b.4], along with the differences which were found to be significant.

In summary, undershooting was greater in the Length task (10 degree standard) and Position (40 deg.) than in Length (40 deg.) and Position (10 deg.), due mainly to females. The distributions of errors are shown in Figure [3.A1b.2]. As in Experiment A1a alone, there is a marked peak at about -20 degrees, in the distribution for Length (10 deg.) and Position (40 deg.), which can be well-explained in terms of criterion errors. There was, however, a reduction in criterion errors in Experiment A1b (from 20 to 12).

Figure [3.Alb.2] Distributions of Constant Errors for the Cue x Standard Interaction

Two conditions are combined in each histogram, the frequency in each condition is noted above each column.

(a) Conditions showing bimodal distribution
(Length, 10 deg. standard / Position, 40 deg. standard)



(b) Conditions showing unimodal distribution
(Length, 40 deg. standard / Position, 10 deg. standard)

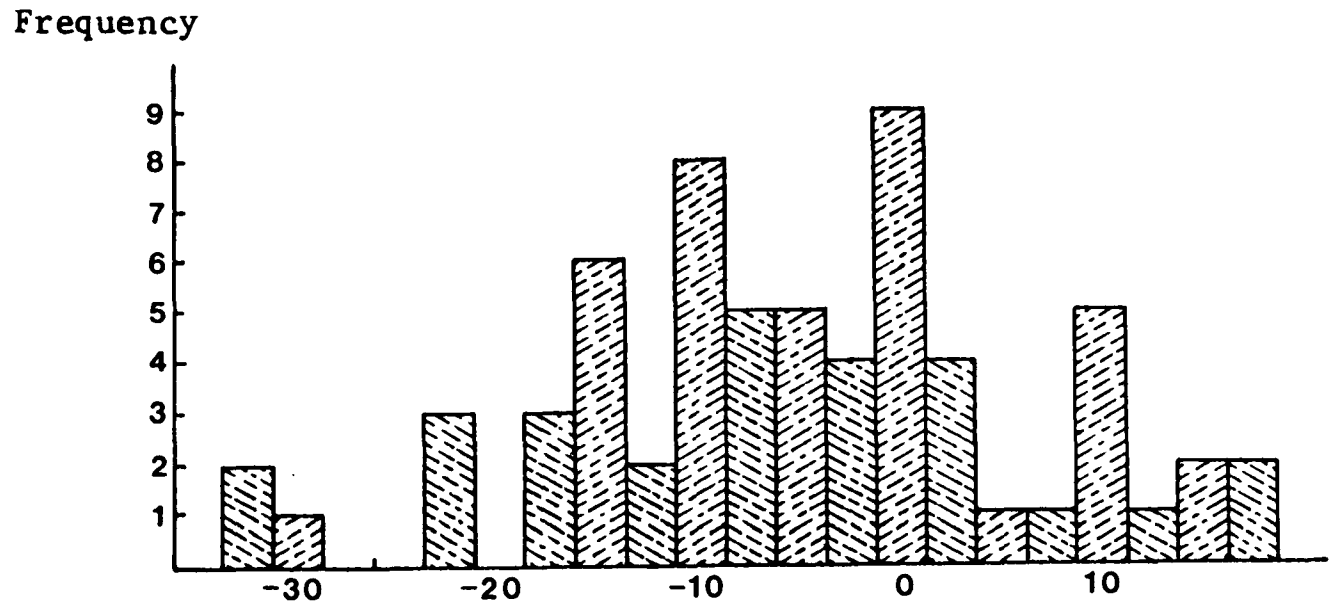


Table [3.A1b.4]

(a) Constant Error Means (in degrees) for the

Matching Cue x Standard

and Sex x Matching Cue x Standard interactions.

MATCHING CUE:	Length		Position	
STANDARD:	10 deg.	40 deg.	10 deg.	40 deg.
SEX: Male	-10.31	-3.75	-8.57	-11.09
Fem.	-18.44	-3.59	-4.84	-14.53

(b) Significant Differences

COMPARISON	MANN-WHITNEY U	p<
Length (10 deg.), Male vs Fem.	47.5	0.002
Male, Length (40 deg.) vs [Length (10 deg.) and Position (40 deg.) combined]	336.5	0.05
Female,		
Length (10 deg.) vs Length (40 deg.)	27.0	0.002
Length (10 deg.) vs Position (10 deg.)	34.5	0.002
Length (40 deg.) vs Position (40 deg.)	56.5	0.02
Position (10 deg.) vs Position (40 deg.)	58.5	0.02

To summarise the constant error results, there was a reduction in undershooting in Experiment A1b, relative to Experiment A1a, in some conditions in which undershooting was high in Experiment A1a. All these changes, also the effects within experiments, can be explained in terms of variation in the number of criterion errors. Possible reasons for this variation are considered under DISCUSSION.

[3.A1b.3b] **Variability**

As in Experiment A1a, within-cell deviation was subjected to square root transform, and analysed as the measure of variability. A

2 x 2 x 2 x 2 x 2 ANOVA was performed, Experiment x Modality x Sex x Matching Cue x Standard. There were no significant main effects, but two significant interactions:

Experiment x Modality x Standard [$F(1,24) = 5.03$, $p < 0.05$]

Matching Cue x Standard [$F(1,24) = 19.54$, $p < 0.0005$]

The means for the first interaction are shown in Table [3.Alb.5], along with the differences between conditions which were found to be significant.

Table [3.Alb.5]

(a) Mean Within-cell Deviation
(in degrees, with square root transform)
For the Experiment x Modality x Standard Interaction

MODALITY:		VV		KK	
STANDARD:		10 deg.	40 deg.	10 deg.	40 deg.
EXPERIMENT:	Ala	2.79	2.05	2.03	1.90
	Alb	1.51	2.18	2.44	2.10

(b) Significant Differences

COMPARISON	MANN-WHITNEY U	p<
VV (10 deg.), Experiment Ala vs Alb	71.0	0.05
Experiment Ala, KK (10 deg.) vs [KK (40 deg.) and VV combined]	253.5	0.05

The means for the Matching Cue x Standard interaction are shown in Table [3.Alb.6], along with the differences which were found to be significant. The Experiment x Matching Cue x Standard interaction [$F(1,24) = 4.18$, $p < 0.053$] is also of interest, and is shown in the same table.

Table [3.A1b.6]

(a) Mean Within-cell Deviation
(in degrees, with square root transform)
For two interactions, Experiment x Matching Cue x Standard
and Matching Cue x Standard

MATCHING CUE:	Length		Position	
STANDARD:	10 deg.	40 deg.	10 deg.	40 deg.
EXPERIMENT: A1a	2.01	2.23	2.81	1.73
A1b	2.03	2.17	1.92	2.10
MEAN	2.02	2.20	2.36	1.91

(b) Significant Differences

COMPARISON	MANN-WHITNEY U	p<
[L(40 deg) & P(10 deg)] vs [L(10 deg) & P(40 deg)]	1528.5	0.05
Experiment A1a,		
L (10 deg) vs L (40 deg)	57.5	0.05
L (10 deg) vs P (10 deg)	67.5	0.05
L (40 deg) vs P (40 deg)	69.0	0.05
L (40 deg) & P (10 deg) combined, A1a vs A1b	161.0	0.05

Key: L = Length P = Position

The significant effects may be summarised as follows: in those conditions in which variability was high in Experiment A1a, variability is reduced in Experiment A1b. These conditions are VV (10 deg.), Length (40 deg.) and Position (10 deg.). The reduction in the latter two conditions was due entirely to visual matching. It therefore seems that the improvement in performance in this experiment can largely be attributed to the addition of the mask, removing misleading cues. This would suggest that the age difference in Experiment A1a was largely due to adults being better able to discriminate useful from misleading cues, to make better use of the available cues.

Table [3.A1b.7] Constant Error Means (in degrees) For Experiment A1a

CUE:			LENGTH				POSITION			
STANDARD:			10 deg.		40 deg.		10 deg.		40 deg.	
PROCEDURE:			1	2	1	2	1	2	1	2
AGE	MOD	SEX								
6	VV	M	-13.75	-10.00	1.25	-10.00	-2.50	-3.75	-7.50	-6.25
		F	-16.25	-27.50	-16.25	9.88	-12.50	5.00	-20.00	-15.00
	KK	M	-8.75	-7.50	-12.50	1.25	-12.50	-17.50	-17.50	-26.25
		F	-21.25	-20.00	-15.00	0.00	-3.75	-5.00	-21.25	-13.75
Adult	VV	M	2.50	-12.50	-3.75	5.00	-2.50	5.00	-5.00	-5.00
		F	-10.00	1.25	1.25	-7.50	0.00	-1.25	-7.50	-6.25
	KK	M	-6.25	-6.25	-7.50	-1.25	-2.50	-7.50	-11.25	-12.50
		F	2.50	-1.25	0.00	-5.00	-11.25	-13.75	-5.00	-7.50

Table [3.A1b.8] Mean Constant Errors (in degrees)
For 6-year-olds in Experiment A1, Parts (a) and (b).

CUE:			LENGTH		POSITION	
STANDARD:			10 deg.	40 deg.	10 deg.	40 deg.
EXPERIMENT	MODALITY	SEX				
A1a	VV	Male	-11.88	-4.38	-3.13	-6.88
		Fem.	-21.88	-3.13	-3.75	-17.50
	KK	Male	-8.13	-5.63	-15.00	-21.88
		Fem.	-20.63	-7.50	-4.38	-17.50
A1b	VV	Male	-11.25	-5.63	-13.75	-8.75
		Fem.	-13.13	3.75	0.63	-6.25
	KK	Male	-10.00	0.63	-2.50	-6.88
		Fem.	-18.13	-7.50	-11.88	-16.88

Table [3.A1b.9] Mean Within-cell Deviation
(square root transformed scores) in Experiment Ala

CUE:			LENGTH				POSITION			
STANDARD:			10 deg.		40 deg.		10 deg.		40 deg.	
PROCEDURE:			1	2	1	2	1	2	1	2
AGE	MODALITY	SEX								
6	VV	Male	3.54	2.24	4.18	3.16	5.00	5.24	0.00	4.18
		Fem.	2.74	3.16	3.54	3.87	6.32	4.47	0.00	3.87
	KK	Male	3.54	3.16	2.24	5.70	4.47	3.16	2.24	2.74
		Fem.	2.74	0.00	2.24	0.00	1.58	3.16	1.58	4.18
Adult	VV	Male	3.87	3.16	3.54	4.47	2.24	2.24	3.16	0.00
		Fem.	2.24	1.58	1.58	2.24	2.24	1.58	3.16	2.74
	KK	Male	2.74	4.18	2.24	2.74	2.24	0.00	2.74	3.87
		Fem.	2.24	5.70	2.24	0.00	2.74	3.54	0.00	3.16

Table [3.A1b.10] Mean Within-cell Deviation
(square root transformed scores)
Experiment A1, Parts (a) and (b).

CUE:				LENGTH		POSITION	
STANDARD:				10 deg.	40 deg.	10 deg.	40 deg.
EXPERIMENT	AGE	MODALITY	SEX				
Ala	6	VV	Male	2.20	2.66	3.30	1.46
			Fem.	2.27	2.69	3.38	1.39
		KK	Male	2.50	2.65	2.72	1.97
			Fem.	1.07	0.90	1.83	2.09
	Adult	VV	Male	2.59	2.82	1.79	1.20
			Fem.	1.52	1.52	1.52	2.27
		KK	Male	2.53	1.97	0.90	2.46
			Fem.	2.65	0.90	2.37	1.20
Alb	6	VV	Male	1.93	1.52	1.52	2.37
			Fem.	1.97	2.72	0.63	2.09
		KK	Male	2.40	2.65	2.60	1.97
			Fem.	1.83	1.79	2.93	1.97

[3.A1b.4] DISCUSSION

As had been predicted, the influence of criterion errors was reduced in Experiment A1b, relative to A1a. It is not certain which methodological changes were responsible for this reduction, but it is not necessary to know. The important point is that criterion errors can largely be eliminated. In Experiment A2a, further steps are taken, in order to completely eliminate the effect of criterion errors.

The changes in methodology between Experiments A1a and A1b also resulted in a reduction in variability. This reduction was selective, being only in the conditions having the highest variability in A1a. As a result, there were no significant effects of modality on variability in A1b, as had been predicted. Indeed, there were no significant effects on variability at all, in A1b.

The occurrence of criterion errors has important implications for the interpretation of experiments. In any task in which both position and length are possible cues (whether or not each is an appropriate cue), consideration must be given to whether the subject responds on the basis of one or the other or both (see also Sections [1.3.2] and [3.A4.4]).

It is known that length can bias position, and vice versa, when the two cues are placed in conflict (Kerr 1978, Laabs 1974, Walsh et al 1980, 1981). It seems to have been implicitly assumed that these effects are continuous bias effects. The conclusion has therefore been drawn that the two cues interact. In the present experiment, and the previous experiment, however, the pattern is that one or the other cue is used, either appropriately or inappropriately. It may be, therefore, that position and length are independent encoded, and confused only in task comprehension.

The effects of cue selection will depend on the task. When only one of the two cues is defined by the experimenter as being the "correct" one, both bias and variability can be affected. The effect on bias is clear from the experiments already reported: if the subject uses the wrong cue, there will be a constant bias which depends on the particular stimuli used, not on performance as such.

If the cue employed varies between subjects and/or between trials, variability will be increased. This effect will be particularly marked if different directions of bias result from different stimuli used in an experiment. Absolute errors would be expected to be susceptible to both the bias and the variability effects of criterion errors (see discussion of Newell et al (1979) in Section [1.3.2]).

If a task is such that appropriate use of either cue will result in the same response, then no bias should result from cue selection alone. However, bias and/or variability may depend on the cue used (see Section [1.3.2] for further discussion).

I have phrased this discussion in terms of either length, or position. It is of course quite possible that a subject will make use of both cues on any one trial, when both are appropriate, and that there is variation in the weighting given to each cue.

In broader terms, the occurrence of criterion errors is a reminder that task comprehension and task performance should both be considered when attempting to explain results of matching experiments.

Returning to consideration of the present experiment, when criterion errors have been taken into account, there remain no reliable effects on constant errors to be explained. However, it could be asked why criterion errors are more frequent for females, and in kinaesthetic matching. With regard to the higher frequency for

females, the following hypotheses were considered:

(a) females find spatial tasks more difficult;

(b) females use different strategies, which are more susceptible to criterion errors;

(c) females are more easily distracted.

There is some evidence that (a) is true in general, although only from adolescence onwards (see Maccoby and Jacklin, 1974, for a review). No specific possibilities can be suggested for (b). In any case, neither of these hypotheses would explain why there is no main effect of sex on variability.

The distractibility explanation was proposed to account for the reduction of constant error magnitude in the visual matching condition, when the mask was added to the apparatus. Looking into the mask would focus attention on the task, whereas subjects in the kinaesthetic matching task were able to see potentially distracting objects in the room (e.g. the experimenter). The same explanation would account for the reduction in variability in Experiment Alb in the visual matching condition.

Further support for the distractibility hypothesis is given by closer analysis of the Matching Cue x Standard x Experiment interaction for variability (Table [3.Alb.6]). The improvement between experiments in the Position condition is due entirely to the visual matching condition. The only change in method between experiments, which is specific to the visual condition, is the use of the mask. In Experiment Ala, in the Position condition, the superiority of the 40 degree standard over the 10 degree standard is also due only to the visual matching condition (4.84 degrees vs 14.06 degrees for vision, 5.31 degrees for both the 10 degree and the 40 degree standard for kinaesthesis). The small difference in Alb is due

entirely to kinaesthetic matching.

A three-part hypothesis, based on the addition of the mask, can be proposed to account for this result:

(a) Without a fixed head position, subjects may not attend so well to the midline. As a result, the 10 degree standard may be coded as simply "near to the midline".

(b) Without the mask, the 40 degree standard may be readily coded relative to the corner of the viewing panel.

(c) In the visual condition, with mask, and in the kinaesthetic condition, the midline is attended to, allowing reliable coding relative to midline.

Part (a) of the hypothesis would also explain the occurrence of responses to the wrong side of the midline in the Position condition, with the 10 degree standard. The number of such responses was reduced from 6 in Experiment Ala to 3 in Experiment Alb, and to zero in subsequent experiments in which it was ensured that the subject looked into the mask throughout each trial.

The hypotheses put forward in the last few paragraphs are post-hoc, and detached from the main thread of the series of experiments in this thesis. The important conclusions from Experiment Ala relate to methodology: apparatus, procedures, tasks, and experimental design. The changes in method suggested from Experiment Ala have been shown to be useful. Further changes are made in Experiment A2a, and it is with that experiment that the important theoretical conclusions begin.

EXPERIMENT A2a

[3.A2a.1] INTRODUCTION

This experiment, and the following experiment, have the primary purpose of testing the following hypothesis: performance, in the task used in these experiments, depends not on modality per se, but on the information available to the subject. In the present experiment, the task previously used is further modified, with the aim of eliminating information differences between modality conditions. In the following experiment, differences in information are deliberately introduced. It was predicted that modality conditions would differ in variability only when they differ in available information.

Before describing in detail the manipulation of information as a variable, a simplification of the design of the experiment must be noted. In the previous experiment, position matching (returning to the same end point) and length matching (repeating the movement distance) were both examined, in separate trials. To continue to include both these conditions in all experiments would result in over-complicated experimental designs, and therefore, potentially, confusing results (even with only one of these conditions, the present study requires a 5-way ANOVA, with the risk of having to interpret fourth order interactions).

The inclusion of both conditions is not of critical importance. More important is the examination of one, independent of the other, so that it is known which feature is being used by the subject as the basis of matching. It was decided to use position matching in preference to length matching, for the following reasons:

(i) In Experiment A1, the difference between modalities was more marked for position matching, therefore this condition presented the

more stringent test of the hypothesis.

(ii) Position matching is the easier task for children to understand. Consequently, in future studies, this task could, in principle, be used with younger children. In the present study, training was easier to standardise, because none of the subjects required more than two training trials.

To recap, the subject was required to remember the end-point of an arc movement. The length of the movement was not a relevant cue, since the starting point of the movement was changed between acquisition and response phases of each trial.

The conditions of the experiment were such that the amount, and type, of information made available through each modality was as near as possible to being the same. In addition, the same strategies for encoding and response were possible in each modality condition. It was predicted that the modality conditions would not differ, either in constant error, or variability.

It has already been argued that previous studies have not successfully controlled information in VV and KK matching (see Chapter One). If valid conclusions are to be drawn regarding information processing in different modalities, it is essential that the same information be available in both modality conditions. Before the description of the execution of the experiment, there follows an account of how the equating was achieved. Some of the features described here are common to Experiments A1a, A1b and the present experiment, but for clarity, they are repeated.

The subject's possible strategies can be classified in a number of ways (Section [1.2.4]). If the starting point of the response movement is the same as the starting point of the acquisition movement, then the end-point may be coded relative to the start point, or relative to some other point. In this experiment, the

former strategy is made unreliable, which should eliminate this possible strategic difference between VV and KK matching.

If there is a visible background in the VV condition, direct external-referent coding (as defined in Section [1.2.4]) is possible, which is not possible in the KK condition. In other words, when perceived visually, the lever can be lined up with a background feature, rather than coded as a distance from an external feature or coded by self-reference.

For this reason, a point of light moving in darkness was used. This technique was used by Diewert & Stelmach (1977), Marteniuk & Rodney (1979), Millar (1972, 1975), and Newell et al (1979). This also overcomes the problem of comparing a "simultaneous" stimulus with a "successive" stimulus, which is found in some previous work (Connolly and Jones 1970, Freides 1974, Jones and Connolly 1970) These studies compared a line length with a movement length, so that the information available in the visual and kinaesthetic modality was quite different.

In both VV and in the KK conditions, the movements were passive in both acquisition and response phase, and the response was for the subject to say "stop". This control was used (with adults) by Diewert and Stelmach (1977), and its importance is discussed by Jones (1973).

In both modality conditions, the relation between head, body, and apparatus position was constant throughout each trial. The stimulus started at the subject's midline in each trial, and was seen/felt at the midline immediately before response. The midline position was further emphasised because it was at the zenith of the arc movement. Together, these procedures would in principle make self-referent coding (relative to the midline) a favoured strategy in both modality conditions.

In both modality conditions, the movements made by the lever were the same, as were the periods during which the lever could be perceived and when it was hidden (see Section [3.A2a.2], below, for details).

Finally, distraction may have been a problem in the KK condition of Experiment A1. For this reason, in the present experiment, the subject rested his face on the mask throughout each trial, in both modality conditions. The probability of distraction due to objects in the testing room was thereby reduced.

It should be emphasised that the purpose of these procedures was not, as such, to improve KK performance relative to VV performance. The purpose of the procedures was to equate the information available in the two modality conditions. It was the hypothesis that this would equate performance in the two modality conditions.

In Experiment A1, the problem of "criterion errors" was encountered: there was a tendency for subjects to respond on the basis of the end-point of the acquisition movement when its length was required, and vice versa. A number of methodological changes were made in the present experiment, in an attempt to overcome this problem:

a) The response movement was changed so that it started closer to the standard position than the acquisition movement. In other words, the starting point of the response movement was further to the subject's left than the starting point of the acquisition movement. The correct position was therefore reached before the position which would have been correct in a length matching task. In order to allow direct comparison with the results of Experiment A1, the previously used response movement was also used.

b) The 10 degree standard was replaced by a 20 degree standard, in order to facilitate discrimination from the midline position.

c) Training was improved and standardised (see Section [3.A2a.d]).

[3.A2a.2] METHOD

[3.A2a.2a] Design, Conditions and Counterbalancing

A factorial design was used, Modality(2) x Sex(2) x Standard(2) x Response Movement(2) x Experimental Session(2), with repeated measures on the last three factors.

The modality conditions were visual and kinaesthetic within-modal matching. The two standard positions were 20 and 40 degrees left of the subject's midline. As in Experiment A1, these positions were approached by a movement of 50 degrees in the acquisition movement. Response movements started 30 or 70 degrees to the right of the standard position, giving "short" and "long" response movements to the criterion position, respectively.

Each of the four combinations of standard position and response movement was given once in each session, and therefore twice to each subject. Standard positions were alternated within each session, and each of a subject's two sessions started with a different standard. Response movements were therefore given in ABBA or ABAB order within each session. The order in which the four combinations of conditions were given was counterbalanced by 4 x 4 Latin Square, so that each Modality x Sex combination required 4 subjects.

[3.A2a.2b] Subjects

The subjects were 16 children of mean age 6yrs,8mths (range 6yrs,0mths to 7yrs,1mth).

[3.A2a.2c] Apparatus

The apparatus was as described in Section [2.3].

[3.A2a.2d] Procedure

The procedure was as described for the Position condition in Experiment Alb, except for the training procedure, which will now be described. In the visual matching condition, the training procedure was as follows:

1) Preparation, as described in Section [2.4].

2) Demonstration of the task, using a standard position of 30 degrees and response movement of 70 degrees, with instructions following this format:

"The light will start from one place, move across, then stop"

[the experimenter moves the light across and stops it as he speaks].

"I want you to remember where the light stopped. Now, keep looking in..."

[the shutter closes, then re-opens with the light at the midline position].

"Now I'll move the light across again, and I want you to say 'stop' when you think the light is at the place where it stopped before"

[the light moves to the starting point, then to the left].

3) When the subject has said stop, he looks up, as he does between all trials, in order to avoid dark adaptation (which might allow detection of visual background cues), and discomfort.

4) The first training trial required more time and explanation than the experimental trials. A second training trial was therefore used to establish the time intervals and instructions used in experimental trials. This second training trial also served to ensure that the

task had been understood. The standard position for this trial was 60 degrees, and the response movement was 30 degrees. In the second session, only this practice trial was used.

In the kinaesthetic matching condition, the training procedure was as in the visual matching condition, with the following differences:

a) The preparation included learning to grasp the lever blind, as described in Section [2.4].

b) The subject was instructed to "feel the stick" rather than "watch the light".

c) Opening and closing the shutter were replaced by grasping and releasing the lever.

[3.A2a.3] RESULTS

[3.A2a.3a] Constant Errors

Constant errors were analysed by 2 x 2 x 2 x 2 x 2 ANOVA, Modality x Sex x Standard Position x Response Movement x Experimental Session. There were three significant effects,

Standard Position [$F(1,12) = 11.87$, $p < 0.005$]

Response Movement [$F(1,12) = 5.24$, $p < 0.05$]

Experimental Session [$F(1,12) = 6.68$, $p < 0.05$].

The effect of modality did not even approach significance. The means for the significant effects are shown in Table [3.A2a.1].

Regarding criterion errors, the distribution of constant errors was noticeably bimodal only when the 70 degree response movement was paired with the 40 degree standard. The first peak was at approximately -15 degrees. Undershoots of more than 10 degrees made up 16 of the total responses in this condition, compared with 1 with the 20 degree standard and 30 degree response movement, and 7 in

each of the other two conditions. This distribution of undershoots is significantly different from being homogeneous across Standard and Response Movement conditions [$X^2(df=3) = 14.81$, $p< 0.01$].

This supports the hypothesis that criterion errors are most likely to occur when the incorrect response point is reached before the correct response point and with the standard position further from the midline. This, in turn, indicates how criterion errors can be avoided.

Table [3.A2a.1] Mean Constant Errors (in degrees)

Standard Position	Response Movement	Session
20 deg : -3.63	30 deg : -3.75	1 : -6.48
40 deg : -7.30	70 deg : -7.19	2 : -4.45

[3.A2a.3b] Within-subject Variability

Within-subject deviation was subjected to logarithm transform, then analysed by 2 x 2 x 2 x 2 ANOVA, Modality x Sex x Standard Position x Response Movement. There was one significant effect, that of Standard Position [$F(1,12) = 7.43$, $p< 0.02$]. This meant that variability was higher with the 40 degree standard than with the 20 degree standard (mean log transformed scores were 1.82 and 1.35).

As with the constant error analysis, the effect of modality did not approach significance.

[3.A2a.3c] Between-subject Variability

The design of the experiment did not allow any acceptable pairing of subjects for analysis of Between-pair Difference. The variance of constant errors for each modality condition was therefore

calculated, by the method described in Section [2.6.5]. The variances were 4.77 and 4.66 for VV and KK respectively, a difference which does not even approach significance.

Table [3.A2a.2] Mean Constant Errors
(in degrees)

STANDARD:		20 deg.				40 deg.			
RESPONSE MOVEMENT:		30 deg.		70 deg.		30 deg.		70 deg.	
SESSION:		1	2	1	2	1	2	1	2
MODALITY	SEX								
VV	M	-4.38	-1.88	-6.88	-1.88	-9.38	-5.63	-9.38	-5.00
	F	-1.88	1.25	-8.75	-6.88	-8.75	-5.63	-11.25	-9.38
KK	M	-6.25	-3.13	-3.75	-5.63	-6.88	-1.88	-10.63	-4.38
	F	-0.63	-0.63	-1.88	-5.00	0.63	-5.00	-13.75	-10.63

Table [3.A2a.3] Mean Within-Subject Deviation
(Logarithm transformed scores)

STANDARD:		20 deg.		40 deg.	
RESPONSE MOVEMENT:		30 deg.	70 deg.	30 deg.	70 deg.
MODALITY	SEX				
VV	M	2.09	1.19	2.46	1.76
	F	1.21	1.66	1.45	2.01
KK	M	0.90	1.61	2.56	1.85
	F	1.25	0.91	1.25	1.18

[3.A2a.4] DISCUSSION

As had been predicted, no difference was found between visual and kinaesthetic performance. It may be argued that an hypothesis of no difference is a difficult one to test, particularly with a small number of subjects. However, the result was replicated in three other

experiments (A3, A4 and D3), and further experiments were performed in which a difference was predicted and found (A2b and A5).

The modality effect in Experiment A1 can be explained in terms of criterion errors, and hence task comprehension, rather than performance ability. The same can be said of the sex differences found in Experiment A1.

The factor with the largest effect, for both constant errors and within-subject deviation, was standard position. The position further from the midline resulted in greater undershooting and higher within-subject variability. The results therefore support the hypothesis that subjects code positions relative to their midlines.

Table [3.A2a.4] Mean Constant Error (in degrees)
For the Standard x Response Movement Interaction

		RESPONSE MOVEMENT	
		30 deg.	70 deg.
STANDARD:	20 deg.	-2.19	-5.08
	40 deg.	-5.31	-9.30

In Experiment A1, a similar effect of standard was found for constant errors, but the result could not readily be interpreted there because of "contamination" by criterion errors. Table [3.A2a.4] shows the constant error means for the Standard x Response Movement interaction in the present experiment (this interaction was not significant). It can be seen that the effect of Standard is present in both Response Movement conditions, and cannot therefore be attributed to criterion errors. The effect of standard position is considered in more detail in Chapter Five.

The main effect of length of response movement on constant errors suggests that the tendency to undershoot increases with the

total distance moved, in addition to the increase with distance moved from the midline. This is similar to the effect of length of movement found by other workers (e.g. Laabs 1973, Marteniuk 1973, Millar 1972, Newell et al 1979).

The most important result of this experiment is that there is no modality effect when there is no difference in the information available to the subject in the two modality conditions. This result is evidence against theories which propose that matching performance depends on modality as such (see Section [1.2] for discussion of specific theories).

EXPERIMENT A2b

[3.A2b.1] INTRODUCTION

In Experiment A2a, it was concluded that VV and KK matching performance do not differ when the same information is available to the subject in each modality. It could of course be argued that the experiment was in some way not sufficiently sensitive to show up any difference that does exist between the two modalities. For example, the task may have been too easy, or the lengths involved too short. It is important, therefore, to test the sensitivity of the method. The present experiment provides such a test. It also tests of the hypothesis that modality conditions will differ in variability if the information provided in each modality differs.

The method was made as similar as possible to that of Experiment A2a, except that three modality conditions were used:

VV-HI = Visual matching with high level of information available.

VV-LI = Visual matching with low level of information available.

KK = Kinaesthetic matching, with low level of information available.

In VV-HI, background cues were available (the inside of the apparatus was illuminated), and the light was seen at the midline prior to acquisition and prior to response in each trial. In VV-LI and KK, background cues were not available, and no attempt was made to emphasise the midline. A "KK-HI" condition was not used because of the problem of creating a kinaesthetic background, equivalent to the visual background.

The main prediction was that VV-HI performance would be superior to KK, because more information was available to the subject in VV-HI. The VV-LI condition was included with no definite

hypothesis. The information available in this condition was less than in VV-HI but more than in KK, because the midline could be established by gaze direction, given that head position was fixed. performance in this condition should therefore give an approximate indication of the relative importance of background cues and midline-relative coding.

The effect of Standard Position was also of interest. This effect can be taken to be an indication that midline-relative coding is used. It should therefore be weaker or absent when midline-relative coding is less reliable (as in VV-LI and KK), or when another strategy is reliable (as in VV-HI). A reduced effect of standard in this experiment would therefore be an indication that the attempted manipulation of information and strategy had been successful.

It was decided not to include sex as a factor in the present experiment, or in subsequent experiments, for the following reasons:

1) Within the schools used in each experiment, it was not possible to match samples of males and females properly on factors such as mental and chronological age, and social class. Apparent sex effects could therefore be due to confounded factors, and interpretation of them would be unlikely to have any general value.

2) Even without sex, the experimental designs were quite complex, so that there was a risk of finding uninterpretable higher-order interactions.

3) Sex was completely counterbalanced in each design.

4) The effect of sex was not significant in the previous experiment, indicating that any effect of sex in Experiment A1 had been an effect of attention or task comprehension, rather than performance as such.

[3.A2b.2] METHOD

[3.A2b.2a] Design

A factorial design was used, Modality(3) x Standard Position(2) x Response Movement(2) x Session(2), with repeated measures on the last three factors. The within-subject factors, and the counterbalancing of them, were the same as in Experiment A2a.

[3.A2b.2b] Subjects

The subjects were 24 children of mean age 6yrs,11mths (range 6yrs,5mths to 7yrs,3mths).

[3.A2b.2c] Apparatus

The apparatus was as described in Section [2.3]. In the VV-HI condition, the interior of the apparatus was illuminated by a small fluorescent light. This made it possible for the subject to code the position of the light relative to background features (the corners of the viewing aperture and the junctions of the sides and base of the apparatus).

[3.A2b.2d] Procedure

The procedure was the same as that used in the previous experiment, except for one change. In conditions VV-LI and KK, the lever did not start each acquisition and response movement from the midline, but from 60 degrees to the right of the (subject's) midline. This served to make the midline a less obvious and less reliable reference point.

[3.A2b.3] RESULTS

[3.A2b.3a] Constant Errors

Constant errors were analysed by 3 x 2 x 2 x 2 ANOVA, Modality x Standard Position x Response Movement x Session. There were no significant effects (all $p > 0.10$).

The trend was the same as in Experiment A2a, that is, undershooting was greater with the 40 degree standard and 70 degree response movement. The failure of the effect of Standard to reach significance supports the hypothesis that midline-relative coding was disrupted by the apparatus and procedures used in this experiment, as set out in the introduction.

Modality conditions in this experiment were designed to affect performance efficiency and hence variability. It was therefore not predicted that modality conditions should differ in constant error, since they had not differed in this way in the previous experiment. This was the obtained result.

[3.A2b.3b] Within-subject Variability

There was a problem with the analysis of within-subject variability. Each subject's two trials in the same condition were in separate sessions and in a different point in the sequence of conditions. This would probably have little effect on within-subject variability (as measured by within-subject deviation), but it was considered desirable to account for any possible effect.

A preliminary analysis was therefore performed, in which Within-subject Deviation was subjected to logarithm transform and analysed by 3 x 2 x 2 ANOVA, Modality x Standard Position x Response Movement. Only one effect was significant, the main effect of modality [$F(2,21) = 6.13$, $p < 0.01$].

By virtue of the fact that standard and response movement each had no effect either on constant errors or within-subject deviation, each subject's four within-subject deviation scores could be replaced by one variable error score. Variable errors were therefore calculated, subjected to logarithm transform, and analysed by one-way ANOVA. Modality was again found to be a significant effect [$F(2,21) = 7.28$, $p < 0.01$].

Newman-Keuls post hoc tests indicated that the significant main effect was due to the difference between VV-HI and KK ($q = 5.38$, $p < 0.01$)*. The other two differences were very nearly of the same magnitude as each other (VV-LI - VV-HI = 0.347, KK - VV-LI = 0.351). The mean errors for the three conditions are shown in Table [3.A2b.1]. The predicted difference between VV-HI and KK was therefore found, with VV-LI in its expected position, between the other two conditions.

Table [3.A2b.1] Variable Error Means (logarithm transformed scores) For the Modality Conditions

Modality Condition:	
VV-HI	1.59
VV-LI	1.94
KK	2.29

[3.A2b.3c] Between-subject Variability

As with Experiment A2a, the design of the present experiment did not permit any acceptable pairing of subjects for the analysis of between-pair difference. The variance for each modality condition was therefore calculated, as set out in Section [2.6.5].

* This difference was also significant when Newman-Keuls tests were based on the first analysis. An analogous analysis of experiment A2a revealed no difference between modality conditions.

The variance for the VV-HI condition was 4.24. This was significantly different from values of 44.52 in the VV-LI condition [$F(7,7) = 10.50$, $p < 0.05$] and 86.73 in the KK condition [$F(7,7) = 20.45$, $p < 0.05$]. The VV-LI and KK conditions did not differ significantly. The between-subject variance results therefore support the variable error results.

Table [3.A2b.2] Constant Error Means
(in degrees)

STANDARD:	20 deg.				40 deg.			
RESP.MOVE.:	30 deg.		70 deg.		30 deg.		70 deg.	
SESSION:	1	2	1	2	1	2	1	2
MODALITY								
VV-HI	-1.88	-1.56	-2.50	-1.25	0.63	-1.25	-2.19	-7.50
VV-LI	0.00	3.57	-0.31	-1.25	-8.75	-0.63	-4.69	-0.31
KK	-0.94	3.44	1.56	-0.31	-1.56	-0.94	-1.25	0.00

Table [3.A2b.3] Mean Within-subject Deviation
(logarithm transformed scores)

STANDARD:		20 deg.		40 deg.	
RESPONSE MOVEMENT:		30 deg.	70 deg.	30 deg.	70 deg.
MODALITY :	VV-HI	1.57	1.30	1.64	1.81
	VV-LI	1.37	2.29	1.70	1.79
	KK	2.37	2.75	2.41	2.00

[3.A2b.4] DISCUSSION

The main prediction in this experiment was that variability should be higher in KK than in VV-HI. This result was found for both within- and between-subject variability. The conclusions of Experiment A2a are therefore supported: there is no difference between visual and kinaesthetic matching unless there is a difference in the information available in the two modalities.

The intermediate variability in the VV-LI condition suggests that background cues and midline emphasis are of similar importance in tasks of this kind. The effect of Standard was not significant in any condition, but was greatest in VV-LI. This is consistent with the contention that some use was made of midline-relative coding in this condition, and that this explains the superiority of VV-LI over KK.

These conclusions are limited, of course, to the particular task used in this experiment. In the long term, it should be possible to test the same hypotheses using a variety of tasks, although the necessity of equating the information available will limit the number of tasks that can be used. In the short term, however, it was decided to carry out further experiments using the task that had been developed.

EXPERIMENT A3

[3.A3.1] INTRODUCTION

In Experiment A2, visual and kinaesthetic position matching were compared. In part (a) of the experiment, attempts were made to equate the amount and type of information which was available to the subject in each modality. Under these conditions, no difference in performance was found between the two modalities. In part (b) of the experiment, different information was provided in each modality, and differences in performance were found. It was concluded that the information available is a more important determinant of performance than is modality as such.

In the present experiment, the information available in each modality is again equated, and within-modal matching is compared with cross-modal matching. The hypothesis was that there would be no difference in within- or between-subject variability between modality conditions.

The reasoning behind such a hypothesis was dealt with in detail in Chapter One. Briefly, the theory is that a cross-modal deficit will occur only if there is a mismatch in the type of information encoded by each modality. In the present experiment, the same information was provided, and the same encoding strategies encouraged. There should therefore be no mismatch of information. This statement implies that there is no need to translate information between modality-specific stores in cross-modal matching.

Higher variability (i.e. poorer performance) in the cross-modal matching conditions, if found, would suggest that modality-specific stores are in fact involved, so that a translation process is necessary in the cross-modal conditions. The same result could also, however, be interpreted to mean that subjects' encoding

strategies are such that the information encoded by a given modality depends on whether the task is within- or cross-modal matching.

The other major change from previous experiments was that a group of nine-year-old children was included, in addition to 6-year-olds. Having developed a task with six-year-olds, older children were included in the present experiment in order to examine developmental trends.

Since the main hypothesis tested in this experiment was that there are no specifically cross-modal processes required in cross-modal matching, it was also predicted that there should be no specifically cross-modal improvement with age. More generally, it was predicted that, if there were any effect of age, this should be an overall improvement in performance (measured by variability) with age, rather than a selective improvement in any modality condition.

[3.A3.2] METHOD

[3.A3.2a] Design

A factorial design was used, Age(2) x Modality(4) x Standard Position(2) x Session(2), with repeated measures on the last two factors. The four modality conditions were within-modal visual (VV) and kinaesthetic (KK) matching, plus the two cross-modal conditions (VK) and (KV).

[3.A3.2b] Apparatus and Stimuli

The apparatus was as described in Section [2.3]. The two standards were 20 degrees and 40 degrees, with a 30 degree response movement, as described in Experiment A2.

Each standard was given once in each of two experimental sessions, in counterbalanced order. The results of Experiment A2a indicate that the two standard positions represent two levels of task

difficulty. The predictions made in the introduction to this experiment apply to both levels.

[3.A3.2c] Subjects

The subjects were 64 children in two age-groups. The mean ages of the two groups were 6yrs,7mths (range 6yrs,2mths to 7yrs,1mth) and 9yrs,4mths (range 8yrs,10mths to 9yrs,10mths).

[3.A3.2d] Procedure

The procedure for the within-modal conditions was as described for Experiment A2a. In the VK condition, initial practice was given at grasping the lever blind; the procedure was then the same as for VV for acquisition, and the same as for KK for response. In KV, the KK procedure was followed for acquisition, and the VV procedure for response. These procedures are the "standard procedures for matching experiments" which are referred to in subsequent experiments.

[3.A3.3] RESULTS

[3.A3.3a] Constant Errors

Constant errors were analysed by 2 x 4 x 2 x 2 ANOVA, Age x Modality x Standard Position x Session. Three main effects were significant,

Age [$F(1,56) = 4.33$, $p < 0.05$]

Modality [$F(3,56) = 14.40$, $p < 0.0001$]

Standard [$F(1,56) = 8.76$, $p < 0.005$] .

The mean errors for these effects are shown in Table [3.A3.1]. There were no significant interactions.

The significant effect of Modality was examined using Newman-Keuls post-hoc tests. VK was found to differ significantly from KV ($q = 9.209$, $p < 0.01$), KK ($q = 5.545$, $p < 0.01$) and VV ($q = 5.446$, $p <$

0.01). In addition, KV differs significantly from VV ($q = 3.762$, $p < 0.05$) and KK ($q = 3.663$, $p < 0.05$). For all comparisons, df = 56.

In other words, mean constant error is the same for VV and KK, but in VK the tendency to undershoot is greater than in VV and KK, and in KV the tendency to undershoot is less than in VV and KK.

Table [3.A3.1] Mean Constant Error (in degrees)
for the significant effects

(a) Age group:	6	-2.77			
	9	-4.41			
(b) Modality:	VV	VK	KV	KK	
	-3.28	-7.58	-0.31	-3.20	
(c) Standard:	20 deg.	-2.62			
	40 deg.	-4.57			

[3.A3.3b] Within-subject Variability

Within-subject deviation was subjected to logarithm transform, making the distribution closer to normal, then analysed by 2 x 4 x 2 ANOVA (Age x Modality x Standard). As had been predicted, the modality effect was not significant. Only the effect of Age was significant [$F(1,56) = 4.38$, $p < 0.05$]. The mean errors for 6-yr-olds and 9-yr-olds were 2.81 and 1.99 respectively.

[3.A3.3c] Between-subject Variability

Between-pair difference was subjected to logarithm transform, and analysed by 2 x 4 x 2 ANOVA, Age x Modality x Standard. As with within-subject variability, and as predicted, the effect of modality was not significant, in fact there were no significant effects.

Table [3.A3.2] Mean Constant Errors
(in degrees)

STANDARD:		20 deg.		40 deg.	
SESSION:		1	2	1	2
AGE	MODALITY				
6	VV	-3.44	-5.00	-2.19	-4.06
	VK	-5.31	-5.31	-5.94	-10.94
	KV	3.13	3.75	-1.56	-0.63
	KK	-1.88	0.31	-3.44	-1.88
9	VV	-0.63	-1.56	-3.75	-5.63
	VK	-7.50	-7.50	-8.13	-10.00
	KV	-3.75	0.94	-1.25	-3.13
	KK	-3.44	-4.69	-5.94	-4.94

Table [3.A3.3] Within-subject Deviation
(Logarithm Transformed Scores)

STANDARD:		20 deg.	40 deg.
AGE	MODALITY		
6	VV	2.34	4.06
	VK	3.13	2.81
	KV	2.81	2.03
	KK	2.34	2.97
9	VV	1.09	2.19
	VK	1.88	2.19
	KV	3.28	1.88
	KK	1.56	1.88

Table [3.A3.4] Mean Between-pair Differences
(Logarithm Transformed Scores)

STANDARD:		20 deg.	40 deg.
AGE	MODALITY		
6	VV	1.37	1.20
	VK	1.76	1.91
	KV	1.82	1.85
	KK	1.74	1.05
9	VV	1.61	1.37
	VK	1.32	1.94
	KV	1.24	1.45
	KK	1.38	1.60

[3.A3.4] DISCUSSION

The main result of Experiment A2a was replicated, since VV performance did not differ from KK performance in either constant errors or variability. It was also confirmed that undershooting is greater with the 40 degree standard, the position further from the subject's midline. The interpretation of these results remains the same as in Experiment A2a.

It had been predicted that, in addition to the results noted above, there would be no difference in variability between within- and cross-modal matching. This prediction was correct in that the effect of modality was not significant for either within- or between-subject variability. This supports the hypothesis that no translation between modality-specific memory stores was performed.

Within-subject variability decreased with age, but not selectively in the cross-modal conditions. This is in accordance with the findings for the same age range in past studies (Connolly & Jones 1970, Millar 1972, 1975), as discussed in Section [1.3.3]. There is therefore no evidence for improvement in a translation mechanism with age (which would be indirect evidence for a translation mechanism), contrary to the interpretation implicitly given by Connolly and Jones for their results.

In the absence of any effect of modality on variability, and with no constant error difference between VV and KK, there was a constant error difference between the cross-modal conditions. This effect was considered sufficiently important to devote a chapter to exploring it further. A theoretical discussion, and additional experiments are therefore presented in Chapter Four. At this point, it can simply be noted that constant error effects relate to differences in bias, not performance efficiency. The effect of

modality on constant errors does not, therefore, contradict the conclusions derived from the variability results. Error measures are discussed in more detail in Section [2.6].

An unexpected result was older children's tendency to undershoot more than younger children. This result was found by Millar (1972), but in the VK condition only. The general effect here may be accounted for if older children had a strategy which made their performance more consistent, but also increased their undershooting. For example, the older children might have been able to anticipate the trajectory of the lever and say "stop" immediately before they thought it should stop. The younger children, on the other hand, may have said "stop" only when the lever had actually reached the desired position.

This matter will be raised again in experiment D3. For the present, it can be said that there is no Age x Modality interaction, and that the effect is therefore coincidental to the main point of the experiment. The next experiment examines further the absence of modality effects on variability.

EXPERIMENT A4

[3.A4.1] INTRODUCTION

This experiment examines the effect of an unfilled retention interval on within- and cross-modal position matching. The purpose of this was to clarify both the interpretation of past studies, and the interpretation of the experiments in this thesis in relation to past studies. The theoretical implications of these clarifications are outlined below.

Two studies have examined the effects of an unfilled retention interval on visual-kinaesthetic matching performance: Jones and Connolly (1970) and Marteniuk and Rodney (1979). In both cases the subjects were adults. Jones and Connolly found an effect of delay on absolute errors only for KK and VK matching. Marteniuk and Rodney found the effect (on absolute and on variable errors) equally in all four modality conditions, in two experiments in which visual information was reduced relative to the Jones and Connolly experiment.

These experiments are discussed in more detail in Section [1.3.2]. It was argued there that the effect of delay found by Marteniuk and Rodney was due to subjects encoding length, rather than position information. It is therefore predicted that in the present experiment, there will be no effect of delay on variability, because position, not length, is the appropriate cue.

An effect of delay on variability, specifically in the cross-modal conditions would imply a specifically cross-modal difficulty, in contradiction of the overall hypothesis proposed in this thesis. A second prediction was therefore that, as in the previous experiment, there would be no difference in variability between modality conditions, whether reproduction is immediate or delayed. Such a

result would indicate that the cue employed in matching tasks (position vs length, as noted above) affects performance under conditions of delay, whereas modality conditions do not.

The experiment therefore provided a test of the conclusions of the previous experiment, in an immediate recall condition. This will allow comparison with previous work to be performed more easily, since most previous work has used an immediate recall paradigm.

[3.A4.2] METHOD

[3.A4.2a] Design, Conditions, & Counterbalancing

A factorial design was used, Age(2) x Modality(4) x Delay(2) x Standard Position(2) x Pair of trials(2), with repeated measures on the last three factors.

The Age, Modality and Standard conditions were the same as in the previous experiment. Two delay conditions were used, "immediate" recall and 20 second unfilled delay.

Each subject had two sessions, immediate recall being used in one session, delayed recall in the other (half the subjects were given delayed recall in the first session, half in the second session). In one of each subject's sessions, standards were presented in the order (ABAB), in the other session (BABA). The order of presentation of standards was counterbalanced within each combination of age, modality and delay.

Pair of trials (the 2 pairs of trials within each session) was included as a factor in order to check for effects of practice on constant errors, and thus to clarify interpretation of within-subject deviation as a dependent variable (see Section [2.6.3]).

[3.A4.2b] Apparatus

The apparatus was as described in Section [2.3].

[3.A4.2c] Subjects

The subjects were 32 children in each each age group, 64 children in total. The mean ages of the two age-groups were 6yrs,10mths (range 6yrs,6mths to 7yrs,4mths) and 9yrs,2mths (range 8yrs,5mths to 9yrs,9mths).

[3.A4.2d] Procedure

Except for the different delays between acquisition and response, the standard presentation procedures were used (see Section [3.A3.2]. In the "Immediate" response condition, the delay was approximately five seconds, the shortest delay within which all the interval procedures could be accommodated without rushing the subject. During the 20 second interval, subjects rested without grasping or having sight of the lever.

[3.A4.3] RESULTS

[3.A4.3a] Constant Errors

Constant errors were analysed by 2 x 4 x 2 x 2 x 2 ANOVA, Age x Modality x Delay x Standard Position x Pair of Trials. Two main effects and one interaction were significant,

Modality [$F(3,56) = 27.80$, $p < 0.0001$],

Standard [$F(1,56) = 25.42$, $p < 0.001$] and

Modality x Standard [$F(3,56) = 4.20$, $p < 0.01$].

Mean errors for the significant effects are given in Table [3.A4.1]. The effect of delay did not approach significance as a main effect or in any interaction.

Newman-Keuls post hoc tests were used to examine the main effect of Modality and the Modality x Standard interaction. The significant differences between modality conditions are shown in

Table [3.A4.2]. The difference between standards is significant in each modality condition ($p < 0.01$) except VV. The values of q are 4.61 (VK), 4.61 (KV) and 5.75 (KK).

Table [3.A4.1] Constant Error Means
(in degrees)
For the Significant Effects

MODALITY:		VV	VK	KV	KK	MEAN
STANDARD:	20 deg.	-2.07	-7.27	4.10	0.70	-3.70
	40 deg.	-1.56	-10.59	0.78	-3.44	-1.13
	MEAN	-1.82	-8.93	2.44	-1.37	-2.42

Table [3.A4.2] Significant Differences Between Modality Conditions
(Constant Errors)

	OVERALL		20 deg. Standard		40 deg. Standard	
COMPARISON	q	p<	q	p<	q	p<
VV vs VK	7.91	0.01	5.03	0.01	8.74	0.01
VV vs KV	4.74	0.01	5.98	0.01	-	-
VK vs KV	12.31	0.01	11.01	0.01	11.00	0.01
VK vs KK	8.41	0.01	7.71	0.01	6.92	0.01
KV vs KK	4.24	0.01	3.29	0.05	4.08	0.05

The modality effect was therefore the same as in the previous experiment, and the same in each delay condition. The effect of modality on constant errors in Experiment A3 cannot therefore be explained by the retention interval in that experiment. Further discussion of this effect is given in Chapter Four.

[3.A4.3b] Within-subject Variability

Within-subject deviation was subjected to logarithm transform and analysed by 2 x 4 x 2 x 2 ANOVA, Age x Modality x Delay x Standard Position. One effect was significant, that of Standard [$F(1,56) = 11.21$, $p < 0.002$]. Variability was higher with the 40

degree standard than with the 20 degree standard, the means being 1.52 and 1.46 respectively.

[3.A4.3c] Between-subject Variability

Between-pair Difference was subjected to logarithm transform and analysed by 2 x 4 x 2 x 2 ANOVA, Age x Modality x Delay x Standard Position. There were no significant effects.

Table [3.A4.3] Constant Error Means
(in degrees)

DELAY:		IMMEDIATE				DELAYED			
STANDARD:		20 deg.		40 deg.		20 deg.		40 deg.	
PAIR OF TRIALS:		1	2	1	2	1	2	1	2
AGE	MODALITY								
6	VV	-1.56	-1.56	-2.19	-1.88	-1.88	-0.31	-1.56	2.81
	VK	-6.25	-4.38	-10.63	-10.94	-7.19	-6.56	-10.00	-9.38
	KV	1.88	3.44	-2.19	-1.25	8.44	5.00	1.88	0.31
	KK	3.75	0.31	-2.81	-0.94	4.38	1.25	-3.13	-5.00
9	VV	-4.69	-2.50	-1.25	-0.94	-2.19	-1.88	-3.13	-4.38
	VK	-7.81	-6.88	-10.94	-10.31	-10.63	-8.44	-12.19	-10.31
	KV	3.44	3.44	0.00	3.75	2.50	4.69	1.56	2.19
	KK	0.63	-1.25	-6.56	-1.88	-0.94	-2.50	-5.00	-2.19

Table [3.A4.4] Mean Within-subject Deviation
(logarithm transformed scores)

DELAY:		IMMEDIATE		DELAYED	
STANDARD:		20 deg.	40 deg.	20 deg.	40 deg.
AGE	MODALITY				
6	VV	1.73	1.42	1.19	1.83
	VK	1.37	1.67	0.94	2.16
	KV	1.53	1.99	1.63	1.51
	KK	1.91	1.78	0.99	1.30
9	VV	0.99	1.86	0.92	1.63
	VK	1.03	1.32	1.36	1.51
	KV	1.41	1.19	1.77	1.57
	KK	1.19	1.95	1.57	1.42

Table [3.A4.5] Mean Between-pair Difference
(logarithm transformed scores)

DELAY:		IMMEDIATE		DELAYED	
STANDARD:		20 deg.	40 deg.	20 deg.	40 deg.
AGE	MODALITY				
6	VV	1.67	1.58	1.40	1.81
	VK	1.77	2.01	1.53	1.75
	KV	2.05	1.41	1.69	1.49
	KK	1.99	2.10	1.30	1.37
9	VV	1.59	1.79	1.46	1.90
	VK	1.19	1.88	1.66	1.04
	KV	2.45	1.78	1.41	2.06
	KK	1.48	1.16	1.02	1.89

[3.A4.4] DISCUSSION

The results of this experiment confirm and extend several conclusions of previous experiments:

1) The modality conditions do not differ in variability with either long or short delay. This gives further weight to the main conclusion of Experiment A3, that matching performance does not depend on modality as such, because there is no effect of modality when the same information is made available to the subject in each modality.

2) The absence of any effect of Delay or Modality x Delay interaction supports the contention that the same information is encoded in the same memory store in each modality condition.

3) The pattern of the modality effect on constant errors was the same as in the previous experiment. The reasons for this effect are discussed in the next chapter.

4) The effect of Standard Position was similar to that found in previous experiments, but weaker in the VV condition. This is dealt with in Chapter Five.

The absence of any effect of delay is in contrast to the results obtained with adult subjects by Jones & Connolly (1970) for KK and VK, and Marteniuk and Rodney (1979) for all four modality conditions. The discrepancy between the results of these two studies was discussed in Section [1.3.2], and explained in terms of the differing presentation conditions. The comparison which is of interest here is between Marteniuk and Rodney (1979, Experiment 1) and the present experiment. The stimulus presentations used by Marteniuk and Rodney were similar to those used in the present experiment, with two obvious differences.

First, their kinaesthetic presentation involved active movements, whereas passive movements were used in the present experiment. This could not explain the discrepancy between the two experiments in the VV condition, and it is unlikely in any case that an active movement would be less well remembered than a passive movement.

Second, both position and length information were available to Marteniuk and Rodney's subjects, whereas only position information was reliable in the present experiment. In Section [1.3.2], it was argued that both the effect of delay and the cross-modal deficit found by Marteniuk and Rodney could be accounted for if length were the cue encoded in KK and KV.

Briefly, the case made for this was that (a) kinaesthetic position reproduction is not normally subject to effects of delay, and (b) a transformation from (visual) position information to (kinaesthetic) length information could account for the cross-modal deficit. The implication of this account is that performance under conditions of delay is affected by the information available, and encoding strategy, but not by input modality.

This case is strengthened by the absence of both these effects

in the present experiment. There remains the problem, however, of the effect of delay found by Marteniuk and Rodney with visual presentation, an effect which was absent in the present experiment. They found the effect of delay in both variable errors and absolute errors. The modality effect was absent in the variable error results. This suggests that the modality effect was actually an effect of constant bias, rather than of variability. This in turn presents the possibility that the effect of delay was due to selective encoding of length in each modality condition, not just in KK and KV.

In summary, the present experiment provides further support for the proposition that modality effects can be accounted for in terms of the information available through each modality, and the encoding strategy adopted by the subject. The next experiment provides further evidence on this point, by means of a demonstration that modality conditions do differ when available information differs.

EXPERIMENT A5

[3.A5.1] INTRODUCTION

In previous experiments in this chapter it was shown that variability is not affected by modality condition (VV vs VK vs KV vs KK), when the information available is the same in each modality. In addition, in Experiment A2b, it was shown that VV and KK do differ when more information is made available in VV than in KK.

In the present experiment, the presentation conditions employed in Experiment A2b are adapted so as to include cross-modal conditions in addition to within-modal conditions. The expected pattern of results was that variability should be least in VV, greater in KK (because less information is available) and greatest in the cross-modal conditions (because there is a mismatch of information between acquisition and response). This expected pattern would be in contrast to the absence of modality effects when conditions differ only in modality. The experiment therefore tests the hypothesis that information is the important determinant of performance in different modality conditions, rather than modality itself.

With the visual presentation employed in this experiment, the position of the light could readily be coded relative to background cues. The midline was emphasised as a reference point, providing a secondary coding strategy. With kinaesthetic presentation, background cues were not available, and the midline was not emphasised. Kinaesthetic coding was thereby biased towards coding relative to starting-point, the midline being a less favoured reference position.

The experiment was designed to allow comparison of the pattern of the modality effect with the pattern found in Experiment A4. In the latter experiment, the information available and the favoured

strategies were the same in each modality condition.

As in Experiment A2b, the procedures in this experiment should reduce the use of midline-relative coding, compared with other experiments in this chapter. It was predicted, therefore, that the effect of Standard on constant errors should be reduced.

[3.A5.2] METHOD

[3.A5.2a] Design

The design was factorial, Age(2) x Modality(4) x Standard(2) x Session(2) x Pair of trials(2), with repeated measures on the last three factors.

[3.A5.2b] Subjects

The subjects were 64 children in two age groups, with mean ages 6yrs,11mths (range 6yrs,5mths to 7yrs,4mths) and 9yrs (range 8yrs,6mths to 9yrs,9mths).

[3.A5.2c] Apparatus

The apparatus was as described in Section [2.3].

[3.A5.2d] Procedure

The standard procedures for VV, VK, KV and KK matching with immediate recall were used (see Sections [3.A3.2] and [3.A4.2] for details), with the following modifications.

When presentation was visual, the interior of the apparatus was illuminated, and the light was moved faster than in previous experiments (not as fast as in the "Fast" condition in Chapter Six). The faster movement was intended to emphasise the end point of the movement, as opposed to the movement itself, thus promoting coding relative to background features. When presentation was kinaesthetic,

the starting-point of the lever was 60 degrees right of the subject's midline, rather than the midline.

[3.A5.2e] Counterbalancing

Each subject was given each standard position twice in each session. In each between-subjects condition, four presentation orders were used, each order being used for two subjects. The orders are shown in Table [3.A5.1]

Table [3.A5.1] Presentation Orders (see text for explanation)

Key: 20 = 20 degree standard
40 = 40 degree standard

ORDER	SESSION 1				SESSION 2			
1	40	20	40	20	20	40	20	40
2	40	20	20	40	20	40	40	20
3	20	40	20	40	40	20	40	20
4	20	40	40	20	40	20	20	40

[3.A5.3] RESULTS

[3.A5.3a] Constant Errors

Constant errors were analysed by 2 x 4 x 2 x 2 x 2 ANOVA, Age x Modality x Standard x Session x Pair of Trials. There were two significant main effects, Modality [$F(3,56) = 5.73$, $p < 0.002$] and Session [$F(1,56) = 14.32$, $p < 0.0005$]. There were two significant interactions,

Standard x Session [$F(1,56) = 6.27$, $p < 0.02$] and

Age x Standard x Session x Pair [$F(1,56) = 9.07$, $p < 0.005$].

The pattern of modality effects was the same, though weaker, as in comparable experiments (A3, A4, D3). Newman-Keuls tests indicated that KV differed significantly from the other three

conditions ($p < 0.01$), the values of q being 5.65 (KV vs VK), 4.18 (KV vs VV) and 4.05 (KV vs KK). The means are shown in Table [3.A5.2].

Table [3.A5.2] Mean Constant Error (in degrees) for the main effect of Modality

MODALITY:	VV	VK	KV	KK
	-1.13	-2.75	3.85	-0.98

The main effect of Session means that undershooting was greater in the first session. The Standard x Session interaction was examined using Newman-Keuls tests. Means for both these effects are shown in Table [3.A5.3]. The effect of Session is significant and in the same direction for both standards (20 degree Standard : $q = 2.94$, $p < 0.05$, 40 degree Standard $q = 6.4$, $p < 0.01$). In the first session only, the effect of Standard is significant, undershooting being greater with the 40 degree standard ($q = 2.95$, $p < 0.05$).

Table [3.A5.3] Mean Constant Errors (in Degrees) for the following significant effects:

Session
Standard x Session
Age x Standard x Session x Pair

	AGE:	6		9			
	PAIR OF TRIALS:	1	2	1	2		
						MEAN	MEAN
SESSION	STANDARD						
1	20 deg	-1.33	0.55	-1.48	-1.25	-0.88	-1.57
	40 deg	-2.03	-2.42	-3.05	-1.56	-2.27	
2	20 deg	1.25	1.64	-1.02	1.25	0.78	1.06
	40 deg	0.70	2.27	2.42	0.00	1.35	

It had been predicted that the effect of Standard on constant

errors would be reduced in this experiment, as it was in Experiment A2b, compared with other experiments in this chapter. To the extent that the effect of Standard was found only in the first session, this prediction was borne out. In Experiment A2b, however, the Standard effect was absent in both sessions, there being no Standard x Session interaction.

Therefore, a further analysis was performed, to check the replication of this effect. In this analysis, two factors were examined for their influence on the Standard effect. The first factor was session, the second was modality (within- vs cross-modal matching). This second factor was included because only within-modal matching was included in Experiment A2b.

The means from this analysis are shown in Table [3.A5.4]. The difference between standards is significant only for cross-modal matching in session one ($t = 2.74$, $p < 0.01$). It therefore seems that in session one, subjects used the midline as a reference point in cross-modal matching. In session two, this strategy seems to have been abandoned. The notion of a change in strategy between sessions is consistent with the variability results presented below.

Table [3.A5.4] Mean Constant Errors (in degrees)
For the Modality x Standard x Session interaction

	MODALITY:	VV + KK	VK + KV
SESSION	STANDARD		
1	20 deg.	-2.38	0.63
	40 deg.	-2.59	-1.95
2	20 deg.	-0.16	1.72
	40 deg.	0.90	1.80

Finally, the third-order interaction was examined using Newman-Keuls tests. The means for this interaction are shown in Table [3.A5.3]. No comparisons between the two age-groups were significant.

Undershooting was always greater in the first session, and significantly so for

9-yr-olds, 40 deg., Pair 1 ($q = 4.845$, $p<00.01$)

and 6-yr-olds, 40 deg., Pair 2 ($q = 4.153$, $p< 0.05$).

Undershooting was greater with the 40 degree standard, except

6-yr-olds, Session 2, second pair of trials

and 9-yr-olds, Session 2, first pair of trials.

The effect of Standard was significant for

6-yr-olds, Session 2, Pair 2 ($q = 3.162$, $p< 0.05$)

and 9-yr-olds, Session 2, Pair 1 ($q = 3.661$, $p< 0.05$).

The effect of Pair of trials was significant for

9-yr-olds in Session 2, for 20 deg. ($q = 2.946$, $p< 0.05$)

and 40 deg. ($q = 3.148$, $p< 0.01$).

Undershooting was greater in the first than in the second pair of trials in each session, except 6-yr-olds, 40 deg., Session 1,

and 9-yr-olds, 40 deg., Session 2.

[3.A5.3b] Within-subject Deviation: Analysis One

Two analyses were performed on within-subject deviation. The first compared part of the data from the present experiment with part of the data from Experiment A4. The main point of this analysis was to show that the different pattern of results in A5 is not due to minor procedural differences, or the use of only one delay condition. The second analysis was for data from the present experiment only. This analysis, being based on more data, is the more important one for the interpretation of the results of the present experiment.

Part of the design of the present experiment matches part of

the design of Experiment A4. The comparable parts are as follows:

A4 : Immediate recall condition; Session One.

A5 : Session One; Subjects with order of standards 20 - 40 - 20 - 40 or 40 - 20 - 40 - 20

For this quarter of the results, within-subject deviation was subjected to logarithm transform and analysed by 2 x 2 x 4 x 2 ANOVA, Experiment x Age x Modality x Standard. There were no effects of age, this factor was therefore dropped from the analysis, and a 2 x 4 x 2 ANOVA was performed. Two effects were significant, the main effect of modality, [$F(3,56) = 3.92$, $p < 0.02$] and the modality x experiment interaction [$F(3,56) = 2.94$, $p < 0.05$].

The significant interaction was examined using Newman-Keuls and Sheffé post-hoc tests. The means are shown in Table [3.A5.5]. In Experiment A4, there were no significant differences between modality conditions. The same result was obtained in the full analysis of Experiment A4. In Experiment A5, differences between modality conditions are found. The most important significant difference is the one between within- and cross-modal errors [$F(3,61) = 10.66$, $p < 0.05$]. This effect is clearly due more to KV than to VK, although the difference between these two conditions is not significant.

Table [3.A5.5] Mean Within-subject Deviation (Logarithm transformed scores).

	MODALITY CONDITION					
EXPERIMENT	VV	VK	KV	KK	VV & KK	VK & KV
A4	1.43	1.22	1.57	1.67	1.55	1.39
A5	0.82	1.39	2.04	1.27	1.04	1.72

[3.A5.3c] Within-subject Deviation: Analysis Two

Within-subject deviation was subjected to logarithm transform and analysed by 2 x 4 x 2 x 2 ANOVA, Age x Modality x Standard Position x Session. There were two significant effects, Modality [$F(3,56) = 3.14$, $p < 0.05$], and the Age x Standard x Session interaction [$F(1,56) = 4.02$, $p < 0.05$].

The main effect of Modality results from a pattern of mean errors which is in the predicted direction: both cross-modal conditions resulted in higher errors than KK, which in turn resulted in higher errors than VV. However, only three comparisons revealed a significant difference: VV vs KV ($t = 2.98$, $p < 0.005$),

VV vs VK ($t = 2.00$, $p < 0.05$),

KK vs KV ($t = 1.69$, $p < 0.05$).

This pattern of results provides some evidence for a cross-modal deficit in efficiency, since both cross-modal conditions differ significantly from VV, and KV differs significantly from KK. The evidence is not, however, as strong as had been hoped for, since KK did not differ significantly from VK. Also, the mean of the two cross-modal conditions combined did not differ significantly either from KK or from the within-modal conditions combined.

It seems, then, that the overall modality effect was not as strong as the modality effect in the portion of the data (from Session 1) which was analysed in Analysis One. For this reason, it was decided to analyse separately the data from each session. Table [3.A5.6] shows the mean errors in each modality condition, in each session and for the sessions combined.

In session two, there were no significant differences between modality conditions, not even the difference between KV and the other three conditions combined. In session one, the difference between VV

and KK was significant ($t = 1.88$, $p < 0.05$) as was the difference between KK and the cross-modal conditions combined ($t = 1.77$, $p < 0.05$). The predicted effect was therefore present in the first session, but not in the second session, when there was no effect of modality at all.

Table [3.A5.6] Mean Within-subject Deviation
(logarithm transformed scores)
For the Modality and Modality x Sessions Effects

	MODALITY CONDITION				
SESSION	VV	VK	KV	KK	VK & KV
1	0.95	1.63	1.78	1.37	1.71
2	1.24	1.28	1.47	1.29	1.38
MEAN	1.10	1.46	1.63	1.33	1.54

The analysis of constant errors had revealed that the difference between the 20 and 40 degree standards was present only in the cross-modal conditions, in the first session. In other words, there was evidence for a change of strategy between sessions, in the cross-modal conditions. It now seems that this change in strategy was associated with a reduction in variability. The constant error results indicate that the change in strategy was from coding relative to the midline to, perhaps, coding relative to the starting point of the movement.

Table [3.A5.7] Mean Within-subject Deviation
(logarithm transformed scores)
For the Age x Standard x Session Interaction

STANDARD:		20 deg.		40 deg.	
SESSION:		1	2	1	2
AGE:	6	1.54	1.33	1.40	1.37
	9	1.28	1.50	1.52	1.08

The second-order interaction, Age x Standard x Session, was examined using Newman-Keuls post-hoc tests within rows and columns. The means for this interaction are shown in Table [3.A5.7]. Only one difference was significant, the difference between sessions in the nine-year-old group, with the 40 degree standard ($q = 3.12$, $p < 0.05$).

[3.A5.3d] **Between-subject Variability**

Between pair difference was subjected to logarithm transform and analysed by 2 x 4 x 2 ANOVA, Age x Modality x Standard. There was one significant effect, the main effect of modality [$F(3,24) = 6.56$, $p < 0.005$].

Newman-Keuls post-hoc tests indicated that the significant effect was due to differences between four pairs of modalities:

KV vs VV ($q = 5.28$, $p < 0.01$),

KV vs KK ($q = 4.24$, $p < 0.05$),

VK vs VV ($q = 4.43$, $p < 0.05$),

VK vs KK ($q = 3.40$, $p < 0.05$).

In other words, variability was greater in each cross-modal condition than in either within-modal condition. The mean errors are shown in Table [3.A5.8].

Table [3.A5.8] Mean Between-pair Difference
(logarithm transformed scores)

MODALITY:	VV	VK	KV	KK
	1.20	1.96	2.11	1.37

The cross-modal deficit is evidently stronger in the between-subject variability results than in the within-subject variability results. In Section [2.6.6], it was argued that where an effect is stronger for between-subject variability, differences in variability

of strategy are indicated. If this argument is accepted, then it seems that strategy was more variable in the cross-modal conditions. This conclusion is consistent with indications of a change in strategy between sessions, which were noted in the analysis of constant errors and within-subject variability.

Table [3.A5.9] Mean Constant Errors
(in degrees)

STANDARD:		20 deg.				40 deg.			
SESSION:		1		2		1		2	
PAIR OF TRIALS:		1	2	1	2	1	2	1	2
AGE	MODALITY								
6	VV	-1.56	-1.56	1.88	-1.25	-1.56	-1.88	0.63	0.63
	VK	-3.13	0.94	-2.81	-2.19	0.00	-4.69	-2.19	-1.25
	KV	1.88	3.75	5.94	8.75	-2.50	-0.31	5.63	6.88
	KK	-2.50	-0.94	0.00	1.25	-4.06	-2.81	-1.25	2.81
9	VV	-1.88	-2.50	-3.13	-0.94	-3.75	-1.56	0.94	-0.63
	VK	0.00	-3.75	-4.06	-1.56	-7.50	-5.31	-1.25	-5.31
	KV	0.63	4.69	1.88	7.81	1.56	3.13	7.19	4.69
	KK	-4.69	-3.44	1.25	-0.31	-2.50	-2.50	2.81	1.25

Table [3.A5.10] Mean Within-subject Deviation
(Logarithm Transformed Scores)

STANDARD:		20 deg.		40 deg.	
SESSION:		1	2	1	2
AGE	MODALITY				
6	VV	1.16	1.43	1.23	1.07
	VK	1.43	1.07	1.87	1.40
	KV	1.92	1.34	1.57	1.38
	KK	1.63	1.47	0.95	1.64
9	VV	0.54	1.34	0.89	1.12
	VK	1.50	1.56	1.73	1.09
	KV	1.82	1.74	1.82	1.43
	KK	1.27	1.38	1.62	0.68

Table [3.A5.11] Mean Between-pair Difference
(Logarithm Transformed Scores)

AGE	MODALITY	STANDARD 20 deg.	STANDARD 40 deg.
6	VV	1.76	1.46
	VK	1.89	2.02
	KV	2.35	2.42
	KK	1.07	1.19
9	VV	0.78	0.77
	VK	2.08	1.85
	KV	1.68	1.98
	KK	1.84	1.39

[3.A5.4] DISCUSSION

The main prediction in this experiment was that variability should be lowest in VV, higher in KK and highest in VK and KV. This pattern was found in the within-subject variability results, though only in the data for the first session. In the second session, there was no effect of modality on within-subject variability. This difference between sessions was associated with a change from coding relative to the midline in the cross-modal conditions, indexed by the effect of standard position on constant errors.

It therefore seems that the information available in each modality affects performance in a manner which is dependent on strategy. This conclusion is further supported by the between-subject variability results, which indicate that strategy was more variable in the cross-modal conditions.

The effect of modality on constant errors was the same as in previous experiments, though weaker. This effect is discussed further in the next chapter, but it can be noted here that the effect is largely independent of effects of modality on variability, and hence independent of the attempted manipulations of available information.

[3.A5.5] OVERVIEW OF CHAPTER THREE

The experiments described in this chapter are concerned with the reasons why performance has been found to vary with modality condition. Experiment A1 was a necessary step in the design of a task in which the same information is available through each modality. When the task was given to children, in Experiments A2a, A3 and A4, there was no effect of modality on performance variability.

In Experiment A2b and in the present experiment, more information was available to vision than to kinaesthesis, and this information was of a different type (external-referent vs self-referent). Under these conditions, visual within-modal performance was superior to kinaesthetic. In addition, in the present experiment, within-modal performance was superior to cross-modal performance. This effect was dependent on the strategy adopted by the subject in the cross-modal conditions. Note that it would have been difficult to detect this change if variable errors had been used, rather than within-subject deviation, because the comparison of sessions would not have been possible.

From the above results, it can reasonably be concluded that:

(a) Within-modal performance depends on the information available to the subject in each modality.

(b) Cross-modal performance suffers when different information is provided through each of the two modalities, or when strategy is such that different information is encoded with different modalities of presentation.

(c) The experiments provide no evidence for a difference between modality conditions in performance efficiency, when there is not a difference in the information available and/or encoding strategy.

In Section [1.3.2], it was concluded that adults show no

cross-modal deficit or visual superiority under conditions in which the same information is available through each modality, and the same strategy is used with each modality of presentation. In Section [1.3.3], it was predicted that the same could be shown to be true of children. This prediction has been borne out. The implication of this is that there is no specifically cross-modal improvement with age between the age of six and adulthood.

Within the 6 to 9 years age-range studied, there was little overall improvement with age. Only in Experiment Three was the effect of age significant. This is consistent with previous studies. Connolly and Jones (1970) found a difference between 5- and 8-year-olds, but not between 8- and 11-year-olds. Millar (1972) found a difference between 4- and 6-year-olds, but not between 6- and 8-year-olds. There was no specifically cross-modal improvement with age over the age-range covered by these experiments.

The data on age effects are therefore consistent with with the conclusion that there is no dependence of modality effects on modality-specific stores or translation. Such a dependence would be expected to give rise to a specifically cross-modal improvement with age unless cross-modal development were complete in the youngest subjects. Even where there is apparently a change in strategy with practice (Experiment A5), performance remains similar in both age-groups. Age effects are also discussed in Section [1.3.3] and in the final chapter.

Experiments in this chapter have mainly been concerned with performance efficiency, as measured by within- and between subject variability. There were also important effects on bias, as measured by constant errors. The effect of modality on bias is considered in more detail in the next chapter, and the effect of standard position in Chapter Five.

CHAPTER FOUR: THE EXPERIMENTS, PART B

[4.B] INTRODUCTION

In this chapter, experiments are described which examine the effect of modality on constant errors. These experiments were carried out in order to explain a result which was found in Experiment A3. In that experiment, comparisons were made among four modality conditions: VV, VK, KV and KK.

From the point of view of the series of experiments to be described in the section, the important result of Experiment A3 is the main effect of modality on constant errors. The tendency towards undershooting was greatest in condition VK, least in condition KV, intermediate (and equal) in VV and KK. This result was subsequently replicated three times, in Experiments A4, A5 and D3, and was found whether or not there was a significant effect of modality on variability.

Before proceeding with discussion of this result, a summary is given of the procedure used in Experiment A3, in order to facilitate comparison with past studies which have used different methods.

Subjects in each modality condition were presented, visually or kinaesthetically, with a lever movement (the "acquisition movement"). This movement was initiated, controlled and terminated by the experimenter. A second lever movement (the "response movement") was then presented, again either visually or kinaesthetically. This movement was initiated by the experimenter, and had a different starting point to the acquisition movement. The subject terminated the movement, by saying "stop" when he thought the lever was at the end-point of the acquisition movement. In brief, the experiment involved cross- and within-modal matching of the end point of lever

movements.

Although constant error results have been reported in a number of past studies (see below), rarely has the relevance for theories of cross-modal matching been discussed. The consistent difference between VK and KV in my experiments provides an opportunity to fill this gap in the current literature. The same result has in fact been found in a number of published studies.

The earliest instance of which I am aware is a paper by Raffel, published in 1936, and this is the only case of the result being discussed. Adult subjects were given the task of matching the lengths of strips of cardboard, within- and cross-modally, under various presentation conditions. Visually perceived lengths were consistently matched to shorter kinaesthetically perceived lengths. Raffel took her results to demonstrate that kinaesthetic perception gives an impression of greater magnitude than does visual perception. Some alternative interpretations are discussed below, but first some more recent literature will be surveyed.

Not all reports of visual-kinaesthetic matching experiments have included constant errors, the results from those that have are summarised in Table [4.1] (Raffel did not report means for each condition, only the ratios of kinaesthetic to visual judgements). The procedure used in these experiments has already been described in Section [1.3], and relevant aspects are mentioned below. In each case the subjects were adults.

It can be seen that, with one exception (Newell et al, 1979 Experiment 1), undershooting is greater in VK than in KV. In addition, it can be noted that Millar (1972) reported that the percentage of childrens' errors which were undershoots was greatest in VK at age 6 and at 8 years (there was no constant error modality effect at 4 years).

Table [4.1] Summary of Constant Error Results in Published Studies of Visual-Kinaesthetic Matching

EXPERIMENT		Mean Constant Error/cm VV	VK	KV	KK
Diewert & Stelmach (1977)*		1.2	-0.7	2.1	1.4
Legge (1965)[a]		-0.1	-1.3	1.9	-0.2
Marteniuk & Rodney (1979)[b]	Expt 1	0.2	-1.4	1.8	0.4
	2	0.4	-0.1	0.2	
Newell et al (1979)**	Expt 1	-11.4	-4.3	-7.3	3.2
	2	-7.3	-60.0	-52.5	-3.0
	3	-3.5	-42.2	-25.7	-12.7
	4	1.5	-33.3	16.2	-0.3

[a] The result shown is for the undrugged condition. The Drug Dose x Modality interaction was significant ($p < 0.01$).

[b] Level of significance of modality effect was not reported.

Significant main effects of modality are indicated by *$p < 0.05$ **$p < 0.01$

The results reported by Newell et al are difficult to interpret because of the high incidence of "criterion errors" of the kind discussed in Section [3.Alb.4]. In other words, the subjects appear to have confused length and position matching. Therefore, while it has to be admitted that the results of this series of experiments may represent an exception to the usual pattern, it is unlikely that any useful conclusions can be drawn regarding why an exceptional result should have occurred in this case.

The remaining studies in Table [4.1] are used below to evaluate possible explanations for the general pattern of the modality effect on constant errors. First, a broad classification of the possible explanations is offered.

[4.B.1] POSSIBLE EXPLANATIONS OF THE EFFECT OF MODALITY ON CONSTANT ERRORS

In this section, three broad types of explanation for the modality effect on constant errors are described: translation and/or transformation bias, additive bias and encoding bias.

[4.B.1a] Translation and/or Transformation Bias

It may be that, in the cross-modal conditions, input information must be translated from a visual to a kinaesthetic store, or vice versa. As an alternative to, or as a result of, such translation, there may be a transformation of input information from one form of coding (eg. length) to another (eg. velocity and time). In either case, if re-scaling is not perfect, then directional bias in performance will result.

One example of this type of explanation would be if visual inputs were coded as lengths, but kinaesthetic inputs as velocity and duration. Cross-modal matching would then require transformation of information from one code to the other. On the basis of the experiments reported by Newell et al (1979), it is reasonable to assume that the transformation would be from the response-modality code to the acquisition-modality code. In any case, one need only assume that the direction of transformation is independent of modality condition (VK vs KV).

The assumptions must then be made, that lengths derived from velocity and duration are overestimates, and that durations derived from length and velocity are underestimates. Finally, one must assume an overall bias to undershoot, constant for all four modality conditions, in order to explain the undershooting in the within-modal conditions (this last assumption is also necessary with the other types of explanation, discussed at (b) and (c)).

The roles of length, duration and velocity coding are dealt with in more detail in Chapter Six.

[4.B.1b] **Additive Bias**

It is possible that biases arise in acquisition processes and in response processes, which summate in matching tasks. More specifically, it would be necessary to hypothesise that visual acquisition (in VV and in VK) and kinaesthetic response (in VK and in KK) each, independently, cause undershooting relative to visual response and kinaesthetic acquisition. This could take any of the four forms depicted in Table [4.2]. In this table, a plus sign indicates a hypothesised tendency to overshoot resulting from the process at the head of the column. A minus sign indicates a hypothesised tendency to undershoot, and a zero indicates no hypothesised bias. Overall bias is the bias expected in the mean of the four modality conditions.

The first of the four patterns depicted in Table [4.2] is the most parsimonious, not requiring the additional assumption of an overall bias to undershoot in all matching conditions.

Table [4.2] Possible Patterns of Additive Bias
(see text for explanation)

Four Possible Patterns	Process VA	VR	KA	KR	Overall Bias
1	-	0	0	-	0
2	-	+	0	0	-
3	0	0	+	-	-
4	0	+	+	0	-

Key : VA=Visual Acquisition VR=Visual Response
KA=Kinaesthetic Acquisition KR=Kinaesthetic Response

[4.B.1c] Encoding Bias

This is the account given by Raffel (1936), as noted above. The modality effect would be explained if visually perceived distances appear shorter than kinaesthetically perceived distances.

[4.B.2] EVALUATION OF POSSIBLE EXPLANATIONS OF THE EFFECT OF MODALITY ON CONSTANT ERRORS

The three types of explanation are not mutually exclusive, but discussion is simplified if they are considered independently. Each type of explanation faces certain problems, which will now be discussed.

It was argued, in Chapter Three, that neither translation nor transformation is required in the cross-modal conditions. Further evidence for this is presented in Chapter Six. If this is the case, then processes of translation and/or transformation cannot be used to explain constant error effects on modality. If anything, the effect of modality on constant errors is weaker when a transformation is required (Experiment A5). In addition, it is not clear, from the point of view of the task or of the adaptation of the organism, why biases should arise in this way. Finally, if effects of translation and/or transformation are occurring, it would be expected that these effects should decrease with age. In my own experiments, there is no effect of age. Millar (1972) reports an increasing effect from age 4 to 8 years.

Does the additive bias hypothesis fare any better? The answer is probably "no". In the first instance, the notion of different directions of bias arising in the acquisition and response phases of the same modality is rather contrived, and difficult to propose specific reasons for.

This type of account is especially difficult to apply when a

similar modality effect is found under very different acquisition and response conditions. My own experiments employed a lever movement in each modality (movements from right to left, across the subject's midline), with a reproduction paradigm. Raffel (1936) used visual lengths, kinaesthetic movements, and same/different judgements. Legge (1965) gave subjects a simultaneous matching task, in which they had to judge when two stimuli were the same distance in front of them. Marteniuk and Rodney (1979) confounded length and end-point information, whereas my own experiments deal with end-points only.

Are there any possible sources of additive bias which would be present in all these varied conditions? This question becomes marginally less daunting if each of the four processes in Table [4.2] is considered separately.

First, then, why should it be expected that visual acquisition would result in a tendency to undershoot, regardless of response modality? One possibility is that (a) when acquisition is visual, the end-point is coded as a distance from the midline, so that the end-point can be anticipated during response; and (b) when acquisition is kinaesthetic the duration of the movement, from the midline to the end-point, is encoded, so that, during response, the subject does not anticipate the end-point, but rather, allows the movement to continue until the correct time has elapsed. This will be referred to as the anticipation/hesitation hypothesis.

Apart from being rather contrived, this explanation has little power when faced with same/different judgements and simultaneous visual presentation (Raffel 1936) or simultaneous matching (Legge 1965). Also, a process of transformation must be hypothesised, which has already been argued against.

Another possibility is that visual inputs are coded as underestimates, which grow to become overestimates during

acquisition-response delay. This would not account for the modality effect when matching is simultaneous (Legge 1965), or the finding that delay does not alter the modality effect (Experiment A4).

Essentially the same problems arise when attempting to explain why kinaesthetic acquisition should result in overshooting, but not kinaesthetic response.

Of course, the explanation for the modality effect might be different in different experiments, but coincidence required considerably reduces the likelihood of the additive bias hypothesis being correct.

The Legge (1965) study, in particular, is difficult to account for on any hypothesis other than the encoding bias hypothesis. The encoding bias hypothesis has to its credit that it is simple - only one source of bias need be proposed - and that there are at least four reasons for expecting a scaling difference between vision and kinaesthesis.

First, in my own experiments, encoding bias might arise because an arc movement is used. It is possible that the entire length of the movement is perceived kinaesthetically, whereas visual judgements are based on the linear distance from the midline, or starting-point, of the movement, to its end-point. This possibility is examined in Appendix F, and shown to be a poor predictor of the actual results. It would also be inapplicable when linear distances are used, unless an illusion of curvature were created by arm-movements.

Second, there may be range effects. Relative to the greatest distance that could be perceived using each sense, kinaesthetically perceived movements are longer. Scaling differences might therefore be expected (Teghtsoonian and Teghtsoonian 1978), resulting in encoding bias.

Third, it is shown in Chapter Six, that the velocity and/or duration of movements can bias kinaesthetic, but not visual, position judgements. The differences in velocity which are involved in Chapter Six are large and obvious. It is possible that small changes in velocity, unnoticed by the subject, could have a similar effect, resulting in a scaling difference between modalities.

Fourth, during kinaesthetic perception of movement, subjects may experience resistance to movement which are not present during visual perception. This possibility is discussed in more detail in Experiment B2.

At this stage, a problem must be noted, regarding the encoding bias hypothesis. The magnitude of the modality effect (the difference between VK and KV) would be expected to increase with distance from the midline, or with total distance moved. This is simply not the case, either in my own experiments, or in published studies which have presented the relevant results (Millar 1972, Diewert & Stelmach 1977, Newell et al 1979).

It seems, therefore, that if there is encoding bias, it is not a constant proportion of the length encoded, but rather a constant amount, regardless of length. This would seem to exclude the curve vs line coding hypothesis, and the range effect hypothesis. The distance vs duration hypothesis also is to be doubted, since the biasing effect of velocity/duration shown in Chapter Six depends on distance from the midline. Intuitively, it would be expected that the resistance hypothesis would predict a bias dependent on distance moved, though this expectation is challenged by the results of Experiment B2.

The experiments which now follow are attempts to resolve the problems, discussed above, which which make it difficult to arrive at an obvious explanation of the effect of modality on constant errors.

EXPERIMENT B1

[4.B1.1] INTRODUCTION

This experiment tests the additive bias hypothesis (set out in Section [4.B.1b]) as an explanation for the effect of modality on constant errors. In the introduction to this chapter, it was noted that the effect of modality conditions on constant errors is the same in four of my own experiments (A3, A4, A5 and D3) and in a number of published experiments (Diewert & Stelmach 1977, Legge 1965, Marteniuk & Rodney 1979, Millar 1972, Raffel 1936). It has consistently been found that constant errors tend to be more positive (i.e. there is a greater tendency towards overshooting) in KV than in VK.

This consistency was taken as evidence against one possible explanation of the modality effect, the additive bias hypothesis. This hypothesis states that independent sources of bias arise in visual and kinaesthetic acquisition and response, and summate to produce the observed modality effect. It was argued that procedures were so different in the different experiments that it would be unlikely that the same sources of bias would arise in all of them.

However it may be that procedures are so very different that different sources of bias in the various experiments all result in the same modality effect. Unlikely as this would seem, it was decided to test the hypothesis by making a small change in method.

Previously, the procedure for response had entailed the subject terminating the response movement. In the present experiment, this procedure was changed, so that the experimenter terminated both the acquisition and the response movement. The subject simply had to judge whether the response movement had continued too far, or not far enough, to match the end point of the acquisition movement. In other words, a same/different judgement paradigm was substituted for the

usual reproduction paradigm.

This change in the procedure provided a test of a particular variant of the additive bias hypothesis, the "anticipation-hesitation hypothesis", which was described in the previous section. Briefly, this hypothesis is as follows: (a) when acquisition is visual, the end-point is coded as a distance from the midline, so that the end-point can be anticipated during response; and (b) when acquisition is kinaesthetic the duration of the movement is encoded, in addition to the distance, so that during response, the subject does not anticipate the end-point, but rather allows the movement to continue until the correct time has elapsed.

If the effect of modality on constant errors is due to the subject anticipating the stopping point during response, then the effect should be eliminated if the subject does not control the stopping point. Both the general and the specific version of the additive bias hypothesis were thought to be incorrect, it was therefore predicted that the effect of modality on constant errors would be found in the present experiment. In other words, it was predicted that undershooting would be greatest in VK, least in KV, intermediate and equal in VV and KK.

[4.B1.2] METHOD

[4.B1.2a] Design

Modality was a between-subject variable, and the only variable to be examined in this experiment. The usual four modality conditions were used, i.e. VV, VK, KV and KK. In order to make the results comparable with previous experiments, the same two standard positions were employed, i.e. 20 and 40 degrees. For reasons given In Section [4.B1.2e] standard was not analysed as a factor in the experiment.

[4.B1.2b] Subjects

The subjects were 24 children of mean age 8yrs,11mths (range 8yrs,4mths to 9yrs,7mths). A pilot study had shown younger children to have considerable difficulty understanding the task, tending either to always judge standard and comparison positions as equal, or to confuse undershoots with overshoots. Only the older age-group was therefore included. Extrapolation of the results of this experiment to the younger age-group used in previous experiments is reasonable, since the modality effect does not differ between age-groups.

[4.B1.2c] Apparatus

The apparatus was as described in Section [2.3].

[4.B1.2d] Procedure

The procedure was adapted from that used in previous matching experiments, as described in Sections [3.A2a.2] and [3.A3.2]. The basic change in the procedure was that the response movement was terminated by the experimenter, before the subject gave any indication as to where it should terminate.

Testing was carried out in two sessions for each subject, on successive days. On the first day, training was given, and only the data from the second day of testing were analysed. The training day had been found to be necessary in the pilot experiment.

In the first session, the subject was introduced to the apparatus and taught, if necessary, to grasp the lever blind. There followed six practice trials, with the standard alternating from trial to trial. In each trial, the subject was presented with a standard movement, in the same manner as in previous experiments. After a delay of 15 seconds (the delay used in Experiments A3 and

D3), the response movement began.

The response movement started 20 degrees to the (subject's) left of the starting position of the acquisition movement. It terminated in one of three places: the place at which the acquisition movement had terminated, or 10 degrees to the right or left of this position. The order in which these three were given was counterbalanced for each standard, across subjects and within each modality condition. The subject was asked to say whether or not the lever had stopped in the same place as it did in the acquisition movement. If the subject said that it had not, he was then asked whether it had gone "too far" or "not far enough".

Apart from giving the subject a general familiarity with the task the training day had two specific purposes. First, it had been found that there was a tendency for subjects to always say that the lever had stopped in the same place. On the second day, the "same place" option was therefore not given. The training day served to emphasise that the end-point, not the length, of the movement was to be matched. Second, some subjects needed practice to overcome a linguistic confusion as to whether the lever had passed or failed to reach the correct position.

In the second session, the subject was told that the lever would not stop at the same place, and that he would therefore have to say whether the lever had gone "too far" or "not far enough" each time. Up to five trials were given with each standard. The stopping position on each subject's first trial with each standard was determined by his responses in the first session. On subsequent trials, the stopping position was determined by contingency rules, based on the subject's responses to previous trials. These contingency rules were designed to progressively estimate the point at which the subject thought the lever should stop.

[4.B1.2e] Scoring

A constant error measure was calculated from each subject's responses with each standard. This measure was the mid-point of the range from the furthest point at which the subject said "not far enough" to the earliest point at which the the subject said "too far". This range varied between 0.0 and 7.5 degrees, but was normally 2.5 or 5.0 degrees.

The measures for both standard conditions were then pooled within each modality condition, in order to provide sufficient scores (12 per condition) for statistical analysis. Variability was too great for two factors to be analysed, and by pooling the standard conditions it was made unnecessary to add more subjects from a different school.

[4.B1.3] RESULTS

Only constant errors were analysed. The means and standard deviations for each modality condition are shown in Table [4.B1.1]. The within-modal conditions did not differ from each other, and were combined for the purpose of analysis.

Table [4.B1.1] Constant Error Means and Standard Deviations (in degrees)

	MODALITY CONDITION				
	VV	KK	VK	KV	VV & KK
MEAN	-1.98	-1.88	-6.15	11.46	-1.93
S.D.	5.60	7.61	5.99	5.97	6.68

One-tailed t-tests for independent samples were used to test differences between modality conditions. The within modal conditions

(combined) differ from KV ($t = 5.86$, $p < 0.001$), and from VK ($t = 1.85$, $p < 0.05$). VK differs from KV ($t = 7.21$, $p < 0.001$). There were no differences in variability between modality conditions.

[4.B1.4] DISCUSSION

The modality effect clearly has the same pattern as in previous experiments. If anything, the difference between VK and KV is greater than previously found, due to increased overshooting in KV. This increased overshooting requires some explanation. The distribution of errors in KV is bimodal, peaks occurring at approximately 6 and 18 degrees. This suggests that subjects were making "criterion errors" (see Section [3.A1a.36]). In other words, subjects were sometimes responding on the basis of the length, rather than the end-point of the movement.

When errors of this kind occurred in Experiment A1, the result was undershooting, because the response movement was longer than the acquisition movement. Criterion errors were successfully eliminated in Experiment A2 and in subsequent experiments, by making the response movement shorter than the acquisition movement. This procedure meant that the correct response position was reached before the position which would have been correct for length matching.

In the present experiment, the different response procedure would make the relative length of the response and acquisition movement irrelevant so far as criterion errors are concerned. The recurrence of criterion errors is therefore not entirely surprising, particularly in the KV condition, in which there is a tendency to overshoot apart from criterion errors.

Taking only the six KV responses from the first peak of the distribution (constant error less than 13.75 degrees), the difference between KV and the within-modal conditions is still significant

($t=2.51$, $p< 0.01$). The results of this experiment are therefore contrary to the predictions of the additive bias hypothesis in general, and specifically the anticipation/hesitation version of the hypothesis.

In general, a change in response procedure would be expected to change the pattern of the modality effect on constant errors. The present experiment adds to the weight of evidence, discussed in the introduction to this series of experiments, that the pattern of the modality effect does not change when response and/or acquisition procedures are changed.

The anticipation/hesitation hypothesis predicts that VK and KV should not differ when response involves a same-different judgement. This prediction was not found to be accurate in the present experiment, or by Raffel (1936) or Legge (1965).

EXPERIMENT B2

[4.B2.1] INTRODUCTION

In the previous experiment, evidence was found which was opposed to the additive bias hypothesis as an explanation of the effect of modality on constant errors. The present experiment tests the encoding bias hypothesis, and in particular examines one of the possible sources of encoding bias listed in Section [4.B].

The hypothesis is that kinaesthetically perceived distances appear longer than visually perceived distances, as a result of resistance to movement during kinaesthetic perception. It was further hypothesised, that the effect of such resistance on perceived distance is a constant, not varying with criterion position (within the range used in the earlier matching experiments).

One problem with testing this hypothesis is that it is difficult to remove resistance. Previous experiments were designed to reduce resistance to a minimum. Instead, it was decided to increase resistance. This was done in two ways. In one condition, the subject was instructed to resist the lever's movement during the acquisition movement of KK matching. In a second condition, the subject was instructed to attempt to push the lever further, after it had been stopped, during acquisition. These conditions were compared with a control condition, in which subjects were instructed, as in previous experiments, not to try to move the lever at all, but simply follow the movements it made.

It was predicted that in both experimental conditions, there would be overshooting relative to the control condition, because the acquisition movement would appear to be longer. Two previous studies are relevant to these predictions. Keele and Ells (1972) found that increasing the resistance which a lever offered to (active) criterion

movements results in a tendency to undershoot. It is proposed that the opposite direction of resistance used in the present experiment will have the opposite effect on bias. Hollingworth (1909) found evidence that impact (rather than deliberate pressure) against a stop tends to increase bias to overshoot.

[4.B2.2] METHOD

[4.B2.2a] Design

A factorial design was used, Resistance Condition(3) x Standard(2) x Session(2) with repeated measures on all factors.

[4.B2.2b] Subjects

The subjects were 18 children of mean age 8yrs,4mths (range 8yrs,0mths to 8yrs,8mths). Given that the effect of modality on constant errors was the same for 6-year-olds and 9-year-olds, it was considered a justifiable economy to use one intermediate age group in this experiment.

[4.B2.2c] Counterbalancing

Subjects were assigned to six groups of three subjects each (2 groups of boys, 2 groups of girls, one group of 2 boys/1 girl, and one group of 1 boy/2 girls). In each group, the order of resistance conditions was counterbalanced by 3 x 3 latin square. Each subject had one trial with each standard in each condition in each session, a total of twelve trials. The order of standards in each condition was randomly assigned, with the following constraints: that each standard should come first (a) at least once per session for each subject, (b) three times in total for each subject, and (c) three times per condition per group of three subjects, in each session.

[4.B2.2d] Apparatus

The apparatus was as described in Section [2.3].

[4.B2.2e] Procedure

In the control condition, the procedure was the same as in the KK condition in experiments in the previous chapter (see Section [3.A2a.2]). In the experimental conditions, the procedure was modified as follows.

In the "Resistance" condition, the subject was instructed to push lightly against the lever, as it made the acquisition movement, as though trying to slow it down. The subject was not permitted to actually slow the movement, since this in itself would result in a tendency to overshoot (Experiment D1). The response procedure was the same as in the control condition. Practice was given, without recall of the end-point, at pushing against the lever.

In the "Stop" condition, the subject was instructed to push the lever gently as though trying to make it go further, when given the instruction "push". The instruction "push" was given when the acquisition movement had stopped, and the lever was not allowed to actually move. As with the resistance condition, practice was given without recall of the end-position.

Each subject's first session began with two practice trials in the first condition for the session (using positions 60 degrees and 30 degrees left of the subject's midline). Practice trials were not given before the other two conditions. The second session began with one practice trial in the first condition for the session (using the 30 degree position).

[4.B2.3] RESULTS

[4.B2.3a] Constant Errors

Constant errors were analysed by 3 x 2 x 2 ANOVA (Resistance Condition x Standard x Session). Two effects were significant, Resistance Condition [$F(2,34) = 11.22$, $p < 0.0002$] and Standard [$F(1,17) = 9.88$, $p < 0.01$]. The means for these effects are shown in Table [4.B2.1].

Newman-Keuls post-hoc tests showed that the control condition differed significantly from the other two conditions (Control vs Resistance: $q = 4.65$, $p < 0.01$; Control vs Stop: $q = 6.50$, $p < 0.05$). This analysis shows that the two predictions are both supported: overshooting results from resistance to the acquisition movement, or from pushing against a stop at the end of the acquisition movement. The "Resistance" and "Stop" conditions did not differ.

Table [4.B2.1] Mean Constant Errors (in Degrees)

	STANDARD		
	20 deg	40 deg	MEAN
CONDITION			
Control	-0.69	-3.47	-2.08
Resistance	3.06	-0.07	1.49
Stop	4.10	1.74	2.92
MEAN	2.15	-0.60	0.78

The relative magnitudes of these effects, for each standard, is of interest. Table [4.B2.2] gives the relevant data. It can be seen from this table that the effect of resistance is not greater for the position more distant from the midline (the resistance x standard interaction was not significant). The trend is in fact in the opposite direction.

Table [4.B2.2] Differences Between Conditions (Constant Errors) in Degrees.

STANDARD:	20 deg.	40 deg.
RESISTANCE minus CONTROL	3.75	3.40
STOP minus CONTROL	4.79	5.21

Table [4.B2.3] Mean Errors

CE = Mean Constant Error (in degrees)

BSV = Between-subject Variance (in degrees^2)

WSD = Mean Within-subject Deviation (logarithm transformed scores)

* = Mean value is for both sessions

RESISTANCE CONDITION	STANDARD	SESSION	CE	BSV*	WSD*
Resistance	20 deg	1	2.36		
		2	3.75	13.76	1.69
	40 deg	1	0.42		
		2	-0.56	9.53	2.01
Stop	20 deg	1	4.17		
		2	4.03	7.22	1.52
	40 deg	1	3.47		
		2	0.00	15.06	1.61
Control	20 deg	1	-2.08		
		2	0.69	13.86	1.33
	40 deg	1	-4.31		
		2	-2.64	8.35	1.48

[4.B2.3b] Variability

Within subject deviation was analysed by 3 x 2 ANOVA, (Resistance Condition x Standard). There were no significant effects.

The experimental design did not allow subjects to be paired for the calculation of between-pair differences. Within-cell variances were therefore calculated. F-tests indicated that no difference between conditions is significant. Although the use of F-tests in this situation is slightly suspect (see Section [2.6.5]), the results here have validity, since the use of F-tests increases the likelihood of finding significant differences.

[4.B2.4] DISCUSSION

The results of this experiment indicate that a kinaesthetically perceived movement is perceived to be longer when either (a) the subject tries to resist the movement, or (b) the subject pushes on a stop at the end of the movement. Moreover, the amount by which these procedures increase perceived length does not change when the distance of the end-point of the movement from the midline is changed.

This last result, in the Resistance condition, requires a word of explanation. Although it had been predicted that the effect of resistance would not depend on standard position, the prediction was made only on the grounds that resistance could not otherwise explain the effect of modality on constant errors. Even in retrospect, it is not clear why this effect should occur. Three possibilities can be suggested : (1) The effect of resistance reaches a maximum in less than 20 degrees, (2) Resistance during the deceleration phase is critical, (3) Effects of resistance arise from the entire length of the movement, not from the section between the

midline and the criterion position only.

The main point is that the effects of modality and of resistance on constant errors are similar in not depending on standard position. This also indicates that resistance, not velocity, was the effective variable in this experiment, since the effect of velocity does depend on standard position (see Chapter Six).

From these results, then, an explanation can be offered for the constant error difference between VK and KV. It is likely that, even with the kinaesthetic presentation used in previous experiments, there is some inherent resistance in kinaesthetic perception of the movement and/or some pressure or impact at the termination of the standard movement. Here, then, are two likely sources of encoding bias.

Resistance during movement is probably the more important factor. If pushing against a stop had any large effect, then in Experiment B1, VV should have differed from KK. The reason for this is as follows. In previous matching experiments, when the response movement was terminated by the subject, VV and KK did not differ. In Experiment B1, the kinaesthetic response movement was stopped by the experimenter, and should therefore have been perceived to be larger, resulting in undershooting in KK. Further, it seems unlikely that the effect of resistance and the effect of pressing against the stop could both occur on any one trial, unless the subject changes the direction of pressure after the lever stops.

This conclusion regarding the Stop condition is relevant to the distinction between active (self-terminated) movements and passive or active movements to a stop (eg. Jones 1973). Taking the present experiment together with the previous experiment, the indication is that movement to a stop has an effect on constant errors, but only when the subject deliberately presses against the

stop.

On a wider scale, the present experiment indicates that constant errors are affected by resistance to movement, and movement to a stop. These factors must be controlled when constant errors are of interest in any experiment. They must also be controlled when variability is of interest, because if either factor is variable within a condition of an experiment, performance variability will be increased in that condition.

EXPERIMENT B3

[4.B3.1] INTRODUCTION

Experiments B1 and B2 provided indirect support for the encoding bias hypothesis. Direct evidence is more difficult to produce, the problem being essentially one of arriving at a common measure, against which to compare both visual and kinaesthetic judgements of length. If judgements in each modality are compared to some third means of describing lengths, one is essentially studying two matching operations, rather than one. In consequence, further experiments would have to be performed to examine the characteristics of each of these two matching processes before the first experiment could be reliably interpreted. These further experiments would necessitate yet more experiments, and so ad infinitum.

The present experiment, and the next experiment to be described, are attempts to by-pass this tangled web. The present experiment is an adaptation of the method of magnitude estimation (Stevens 1956). The subject is presented with two lengths, in succession, in the same modality. The experimenter tells the subject that the first length has a certain magnitude, represented by a number. The subject's task is to assign a number to describe the magnitude of the second length.

The subject is instructed to use a ratio scale. In other words, if the second length is perceived to be n times the length of the first, then the number given by the subject should be n times the number given by the experimenter.

How can this method be used to test the encoding bias hypothesis? Recall that the hypothesis states that kinaesthetically perceived distances appear greater than visually perceived distances, by a constant amount.

Let this constant amount be k

Let the length of the standard movement be s

Let the length of the comparison movement be m

Let the number assigned to the standard movement be S

Then the number assigned by the subject to the comparison lengths (Assuming estimation of length and assigning of numbers to be accurate apart from the constant bias), for visual and kinaesthetic estimates, respectively will be given by:

$$V = mS/s$$

$$\text{and } K = (m+k)S/(s+k)$$

$$\Rightarrow V - K = mS/s - (m+k)S/(s+k)$$

$$= S[\, m/s - (m+k)/(s+k)] \qquad (1)$$

It follows that, when m = s,

$$m/s = 1$$

$$\text{and } (m+k)/(s+k) = 1$$

$$\text{hence } V - K = S[1-1] = 0$$

$$\text{also, when } m > s, \quad V - K > 0$$

$$\text{when } m < s, \quad V - K < 0$$

More generally, $V - K = [1/(1+k/s)][1/s]^2 kS(m - s)$

If s is constant, then $V - K = c(m - s)$

$$= cm - b$$

where b and c are constants.

Upon these equations must be superimposed the power function for each modality. Unfortunately, these are not known, and cannot be known independently of the hypothesised effect of k. Therefore, the prediction must be weakened to say that the difference V - K should become greater as m increases, but possibly never actually exceed

zero. In other words, visual estimates of length should increase more rapidly than kinaesthetic estimates, as actual length increases.

A final problem: can it be relied upon that children will use a ratio scale when requested to do so? It can by no means be guaranteed that they will, since even adults sometimes have problems with this (Stevens 1956). If they use an interval scale, then

EITHER

$$V - K = [S + dm/s] - [S + d(m+k)/(s+k)]$$

$$= d[(m/s) - (m+k)/(s+k)] \qquad (2)$$

where d = a constant

OR

$$V - K = [S + (m-s)e] - [S + [(m+k)-(s+k)]e] \qquad (3)$$

$$= e[(m-s) - (m+k) + (s+k)]$$

$$= 0$$

where e = a constant

Therefore, if an interval scale is used, the predicted value of V - K is the same as for ratio scale responding, multiplied by a constant which is greater than or equal to zero.

If an ordinal scale is used, then equations (2) and (3) must be modified by adding a variable to the constants. The result of this would be that equation (1) would be modified so that over a large number of trials, the expected value of V - K would be given by:

$$x[(m/s) - (m+k)/(s+k)] \qquad \text{where } x \geq 0$$

To summarise, two statements can be made:

(a) The encoding bias hypothesis will be supported if $(V - K) = 0$, and the support will be stronger if $(V - K) > 0$ and increasing with the value of x.

(b) If $(V - K) < 0$, this will be strong, though not conclusive, evidence against the encoding bias hypothesis.

In other words, although it is hypothesised that perceived length differs between modalities by a constant amount, the difference between magnitude estimates is predicted to increase with the length estimated.

[4.B3.2] METHOD

[4.B3.2a] Design

There were two independent variables, modality (Vision vs Kinaesthesis), and length of movement (5 degrees to 50 degrees, in 5 degree steps, 10 lengths in total). Each subject received each condition, 20 in total, in two experimental sessions (one session in each modality).

[4.B3.2b] Subjects

The subjects were 30 children of mean age 8yrs,9.5mths (range 8yrs,6mths - 9yrs,3mths). Two further subjects were replaced in the experiment after the first testing sessions, because their estimates did not show a rank correlation with actual length.

[4.B3.2c] Apparatus

The apparatus is described in Section [2.3].

[4.B3.2d] Counterbalancing

The data were originally collected with a different hypothesis under test, as a pilot experiment. When it became clear that the method was not suitable for testing that hypothesis, testing was terminated. When the hypothesis which is presently under test was developed, the already available data were found to provide an appropriate test. The counterbalancing of the experiment was, however, incomplete (four partly filled 10 x 10 Latin Squares

counterbalanced subjects by the order in which different lengths were presented).

The following procedure was therefore carried out. First, the data were divided into four sets, according to modality condition, and order of presentation of modality conditions. Each set of data was arranged into a 10 x 10 table (10 lengths x 10 trials per session). Because the counterbalancing was incomplete, some positions in the table had two entries, some only one. Where there were two entries, they were replaced by their mean, to give only one entry.

The overall effect of this procedure was to give a design counterbalanced for order of presentation of modality and length conditions, with some data points based on one subject, some based on two. The number of data points based on two subjects was the same in each combination of modality and length conditions. This arrangement of the data provided a sufficient test of the hypothesis, it was therefore decided that it was unnecessary to test any additional subjects.

[4.B3.2e] **Procedure**

When seated at the apparatus, the subject was allowed to see, or feel, as appropriate, the lever moving inside the apparatus. It was explained to the subject that he should imagine the light/lever "walking" as it moves around, and that he would have to guess how many steps it walked. It was immediately explained that the experimenter would give "a clue": that "this far is ten steps" (a movement of 10 degrees from midline to the left was demonstrated). The subject was told that each time he had to make a guess he would be shown ten steps, then another distance, for which he would have to give a number (of steps).

It is obviously important, that when a perceptual judgement

has been made, the subject should apply numbers to the judgement, on a ratio scale. Questions were therefore asked, having the general form "if the light/lever moves twice as far, how many steps would that be?". The numbers used in questioning were selected so as to be as different as possible to the correct answers for the first three trials. There was no evidence that subjects repeated numbers from the questioning.

Without practice trials, the test trials began. On each trial, the lever was initially seen or grasped at the midline, it made an excursion of 10 degrees to the subject's left, then returned to the midline. The experimenter asked "if this far is ten steps" (as the lever made the 10 degree movement), "then how many steps is this?" (as the lever made one of the comparison movements). All movements were controlled in velocity and extent by the experimenter.

Feedback on accuracy was not given; the experimenter simply said "good" or "good guess" or another phrase of encouragement after each trial.

The subject looked up from the apparatus briefly between trials, in order to avoid dark adaptation and discomfort. In kinaesthetic testing sessions, the subject released the lever and rested his hand on the bottom of the apparatus between trials.

The object of the experiment was to study perception, not memory. The standard and comparison lengths were therefore presented as rapidly in succession as possible.

[4.B3.2f] Scoring

As might be expected with a task that requires first-school children to deal with ratios, the estimates in each condition were highly variable. In addition, the subjects were much more likely to give numbers which are multiples of ten than any other numbers. Very

few estimates were not multiples of five. The data were therefore treated as frequency distributions of interval five degrees, and with intervals centred on multiples of five. The assumption behind this is that the subject does not actually perceive lengths in multiples of five, but rather gives a number to what he perceives which is the nearest multiple of five to the perceived length.

From the frequency distributions, medians were calculated. Length conditions were grouped so that the medians for each group, with modality conditions combined, were evenly spaced, at approximately 10, 20, 30 and 40 degrees. In this way, division of the data about the mean did not produce the problem of large frequencies close to the median, but arbitrarily on one side of the median or the other. Median tests (Siegel 1954, p111) were then performed.

[4.B3.3] RESULTS

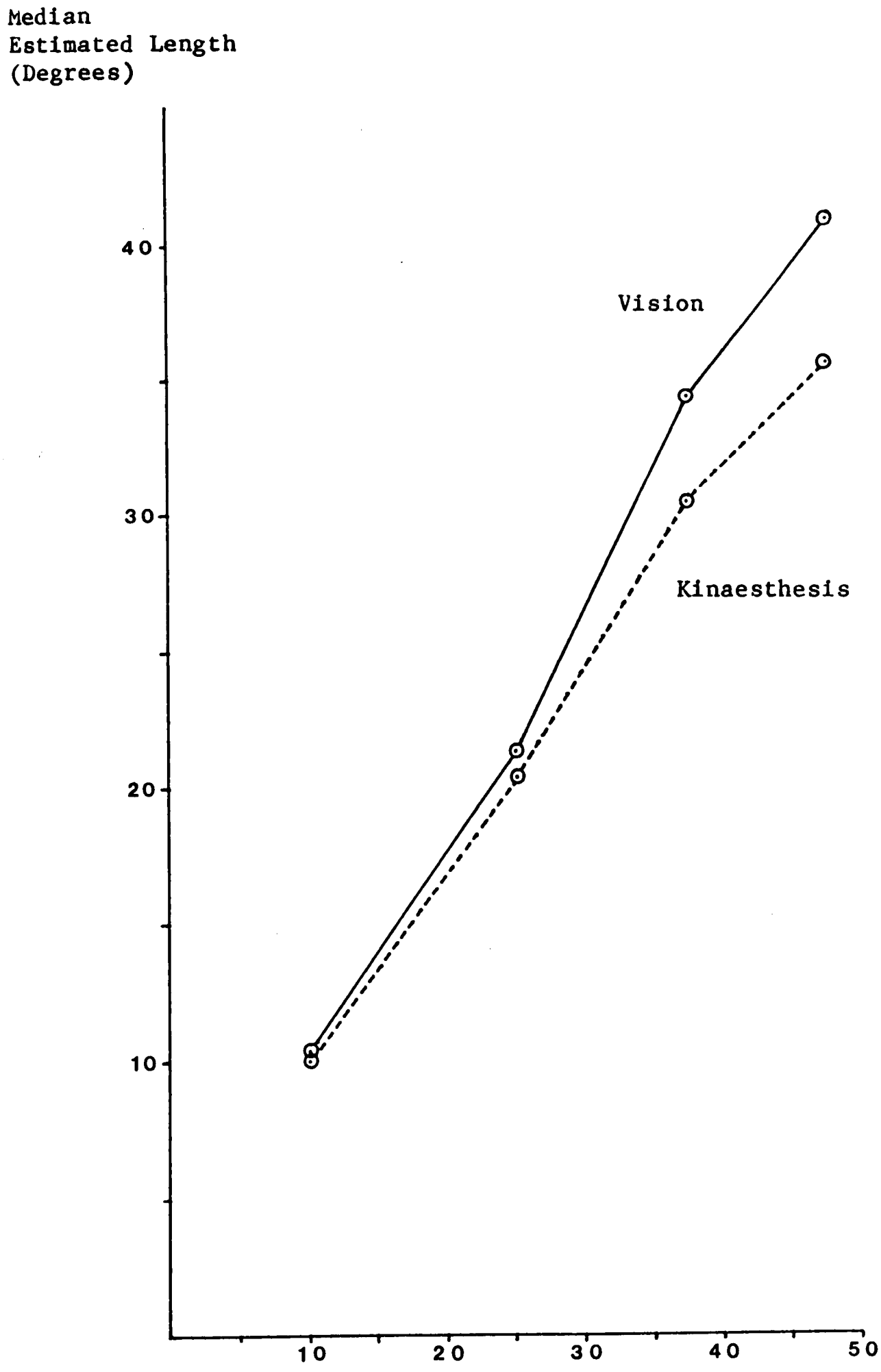
Figure [4.B3.1] shows the medians for grouped data. The data are tabulated in Table [4.B3.1], along with the results of median tests (for the difference between visual and kinaesthesis). Further analysis is given in Appendix [G].

Table [4.B3.1] Medians (in degrees) and results of median tests for the difference between vision and kinaesthesis

MOVEMENT LENGTH (in degrees):	5/10/15	20/25/30	35/40	45/50	Combined
Vision:	10.47	22.29	34.17	41.73	24.69
Kinaesthesis:	10.17	21.11	30.50	35.28	21.42
Combined:	10.32	21.79	32.26	39.34	
X^2 (df=1):	0.429	2.651	3.908*	7.941**	

* $p < 0.05$ ** $p < 0.01$

Figure [4.B3.1] Median Estimated Length Plotted Against Actual Length



[4.B3.4] DISCUSSION

It had been predicted that visual estimates should be larger than kinaesthetic estimates of the comparison lengths. It was further predicted that the difference between modalities should increase with comparison length. This was the finding.

It is unlikely that the results of this experiment are to be explained simply by the second of two kinaesthetic judgements appearing longer than the first, by a constant amount. There is no obvious reason why this should occur with kinaesthetic judgements, but not with visual, and if it were to occur in this way then it would not explain two effects on constant errors in matching tasks:

(a) the cross-modal asymmetry,

(b) the lack of a difference between VV and KK (there should be undershooting in KK if the second movement appears shorter).

The results are therefore compatible with the hypothesis that lengths are perceived to be greater when perceived kinaesthetically than when perceived visually. The next experiment is a further test of this hypothesis.

EXPERIMENT B4

[4.B4.1] INTRODUCTION

In this experiment, an attempt is made to further test the encoding bias hypothesis, proposed in Section [4.B] to account for the effect of modality on constant errors. Briefly, the hypothesis is that, relative to visual perception, kinaesthetic perception results in overestimation of length. The overestimation is hypothesised to be a constant amount, regardless of the length estimated (within the range of lengths used in this series of experiments).

The problems of designing an experiment to test this hypothesis were discussed in Section [4.B3.1]. The method used in the present experiment is to ask children to make estimates of lengths, using centimetres. The simple prediction is that estimates made kinaesthetically should be larger than estimates made visually, the difference being the same for all lengths. Some problems with this prediction are discussed in the last section of this experiment.

[4.B4.2] METHOD

[4.B4.2a] Design and Counterbalancing

In each modality, each subject made two estimates of each of eight lengths, in a single session. Half the subjects started with visual estimates, half with kinaesthetic. In each modality, each length was given once, before any length was repeated. Each session therefore consisted of four blocks of eight trials each. Within each block, the order of lengths was counterbalanced across subjects by 8 x 8 Latin Squares.

[4.B4.2b] Subjects

The subjects were 16 children of mean age 9yrs,4mths (range

9yrs,1mth - 9yrs,10mths). This was the youngest age-group which could readily be got to estimate distances without the use of a ruler (using a ruler would effectively turn the visual and kinaesthetic conditions into VV and KV matching, respectively).

The difficulty experienced by younger children indicates that, as was intended (Section [1.2.4]), the stimuli used in matching experiments were not amenable to verbal coding.

[4.B4.2c] Apparatus

The apparatus was as described in Section [2.3].

[4.B4.2d] Procedure

The procedure for visual estimation trials was as follows. First, the subject was shown the light moving from side to side. It was then explained that the light would start from "the middle" (the subject's midline), move across, and stop. The subject was told that he would be asked how far, in centimetres, he thought the light had moved from the middle. This procedure was then carried out for the first and subsequent trials. The shutter on the apparatus closed, occluding the light, between trials.

The procedure for kinaesthetic trials was essentially the same, except that the subject grasped the light, instead of watching it. Initial practice was given in grasping the light blind. The subject released the light after making each estimate, then re-grasped it at the midline position.

The inter-trial interval was about five seconds within blocks of trials. The lengths used correspond to angles of 15 to 50 degrees, in 5 degree steps.

[4.B4.3] RESULTS

The mean of each subject's two estimates of each length was calculated for each modality. The kinaesthetic estimate means were then subtracted from the visual estimate means, yielding eight difference scores per subject. T-tests were then performed, to test the hypothesis that the mean difference scores differ from zero. Table [4.B4.1] shows the mean difference scores, and results of t-tests for each length. Mean estimates for each modality and length are shown in Figure [4.B4.1] and Table [4.B4.2].

Table [4.B4.1] Mean Difference Scores

(Difference Score = Visual Estimate minus Kinaesthetic Estimate).

LENGTH/cm:	4.38	5.84	7.30	8.75	10.21	11.67	13.13	14.59	MEAN
Mean :	1.48	1.83	1.72	0.95	1.56	1.67	1.98	2.66	1.61
S.D. :	1.23	1.55	2.01	2.06	2.05	2.27	4.22	2.12	2.34
t=	4.80	4.73	3.42	1.85	3.05	2.99	1.88	5.01	7.77
p<	0.01	0.01	0.01	0.10*	0.01	0.01	0.10*	0.01	0.001

* for these two lengths analysed together, t = 2.504, p< 0.02

Table [4.B4.2] Mean Estimates of Length (in centimetres).

Actual Length	4.38	5.84	7.30	8.75	10.21	11.67	13.13	14.59
Mean Visual Estimate	4.01	5.56	6.63	7.92	9.14	11.11	13.02	14.48
Mean Kinaesthetic Estimate	2.53	3.73	4.84	6.94	7.58	9.38	11.03	11.83

The results are clearly opposed to the prediction that kinaesthetic estimates should be larger. Visual estimates were larger than kinaesthetic estimates over the entire range of lengths,

although all mean estimates were in fact underestimates.

However, closer inspection of the data, based on the hypothesis under test, indicated a more complex pattern of results. Each subject had 32 trials. When this was introduced as a variable in the analysis, an important practice effect was found. For the first two trials of the first block, and the first trial of the second block, kinaesthetic estimates were the larger ($t = 1.85$, $p < 0.05$).

The mean differences between visual and kinaesthetic estimates are shown in Table [4.B4.3].

Table [4.B4.3] Mean Difference Between Visual and Kinaesthetic Estimates of Length

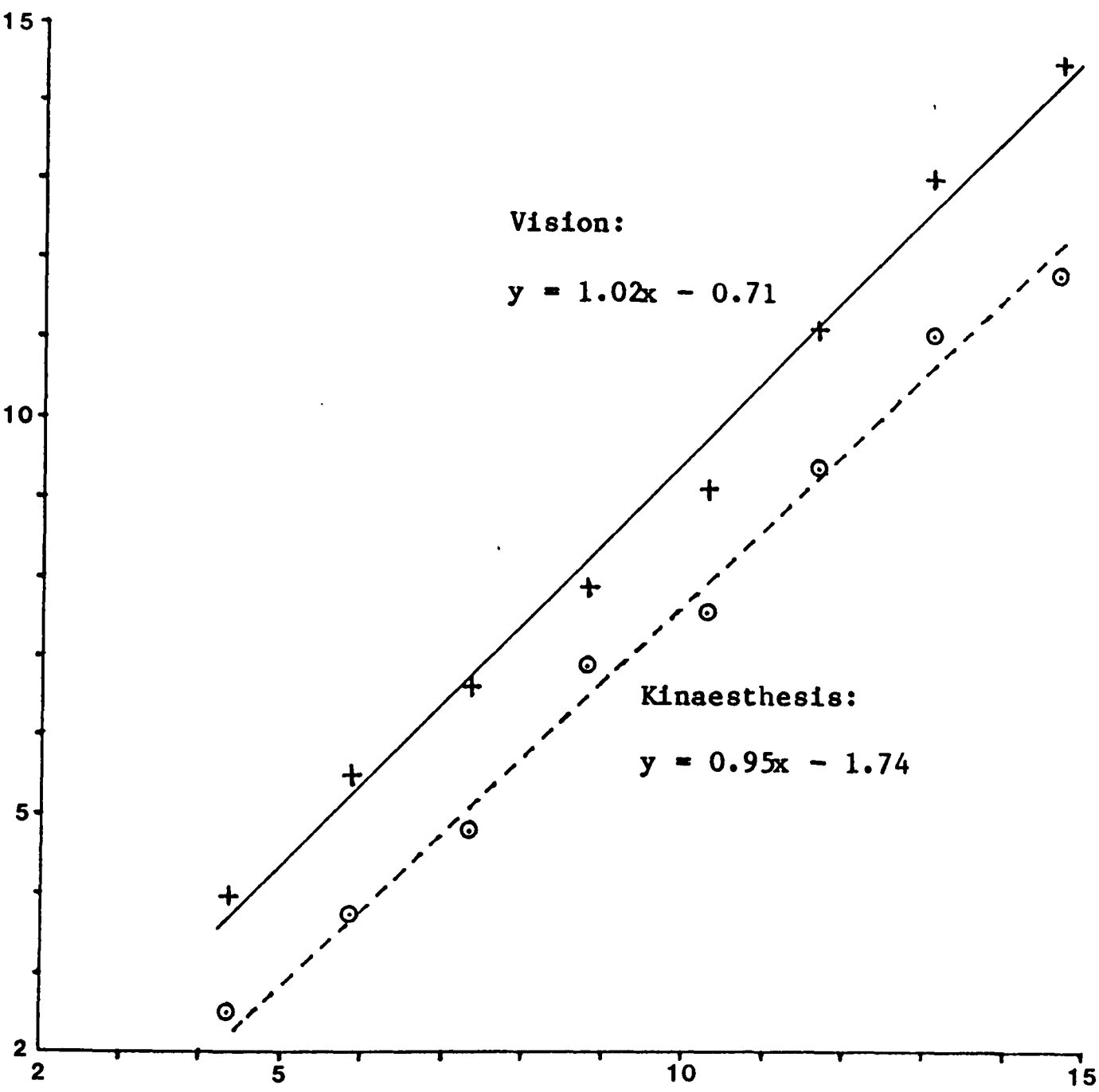
(Visual estimate minus kinaesthetic estimate)

TRIALS:	1,2 and 9	3 - 8 and 10 - 16	17 - 32
MEAN DIFFERENCE/cm:	-1.29	2.01	2.21

Further tests were performed in order to determine whether the reversal in the modality effect was due to changes in visual and/or kinaesthetic estimates over trials. For each modality, trials 1, 2 and 9 were compared with the other 13 trials in the first two blocks. The means for vision were 8.79cm and 9.26cm, not significantly different. For kinaesthesis, the means were 10.08cm and 7.25cm, a significant difference ($t = 2.23$, $p < 0.025$).

To summarise, estimates of length followed the predicted pattern at the start of the first two blocks of trials. That is, kinaesthetic estimates were larger on average than visual estimates. On subsequent trials, the magnitude of kinaesthetic estimates decreased, becoming less than visual estimates.

Figure [4.B4.1] Mean Estimates of Lengths (in centimetres) Plotted Against Actual Length



[4.B4.4] DISCUSSION

Overall, the results ran contrary to the hypothesis under test. There was support for the hypothesis, however, in the data for the first trials of the first two blocks of trials. Two issues must now be dealt with. First, are the trials in which the hypothesis was supported to be regarded as more reliable than the other trials? Second, why did the modality effect change over trials?

Regarding the first issue, it should be recalled that the encoding bias hypothesis was intended to explain the results of experiments having at most four trials per session. The present experiment had 32 trials per session. Herein is a case for giving greater weight to the early trials in this experiment. Furthermore, it should be noted that the brief pause (10-15 seconds), between blocks one and two, was sufficient to reinstate the predicted effect for one trial. In matching experiments, the inter-trial interval was of this order of magnitude, so that persistence of the modality effect for up to four trials, (rather than only two, as in the present experiment), is not surprising.

The encoding bias hypothesis remains tenable, therefore, although support for it in the present experiment rests on the assumptions discussed in the previous paragraph.

Why did the modality effect change over trials? One possible reason is that perceived magnitudes did actually change, in the kinaesthetic condition. Previous experiments indicate that this would happen if there were a change in the velocity of the movements, or a change in the resistance which the subject offers against the movement. With practice, the subject would be quite likely to be able to move more smoothly with the lever, therefore offering less resistance. This would explain why visual estimates do not change

over trials. It is also compatible with resistance to movement as an explanation of the modality effect in matching experiments.

Alternatively, the reversal of the modality effect could be in some way related to the subject using numbers. This is unlikely, since on this hypothesis a similar effect on each modality would be expected.

A third possibility is that visual estimates would be expected to be greater than kinaesthetic estimates if centimetres are regarded as a visual code for distance. If this were the case, then kinaesthetic estimation in the present experiment would be equivalent to KV matching, which consistently results in underestimation. It is for this reason that the present experiment unfortunately has limited power to reject the encoding bias hypothesis. However, this would not explain the change in the modality effect in this experiment, unless centimetres become a visual code, then cease to be a visual code, then revert to being a visual code in the course of the experiment.

There is a fourth option, that over trials, proactive interference increases, so that recall of the kinaesthetically perceived midline position moves to the subject's left. This would have the effect of decreasing estimates of the distance between the midline and the end point of the movement. A similar effect would not necessarily be expected in the visual condition, since the midline can be referred to by altering gaze direction, so that it need not be remembered.

In summary then, there is one hypothesis that accounts for the modality effect in the early trials, and the change in modality effect, and the modality effect in matching experiments. That hypothesis is that there is a constant encoding bias, due to resistance to movement in kinaesthetic perception of movement.

This hypothesis would also explain why Teghtsoonian and

Teghtsoonian (1965, 1970) found visual and kinaesthetic power functions to be equivalent when index finger separation provides the kinaesthetic stimulus, and converging when thumb-finger separation was used, in contrast with the diverging power functions found in Experiment B3. In the Teghtsoonian and Teghtsoonian studies, there was no movement which involved any external resistance.

Further, an hypothesis which appeals to a feature of kinaesthetic, rather than visual presentation more readily explains the consistency of the modality effect over a variety of experiments (Section [4.B]). Visual presentation varies from experiment to experiment rather more than kinaesthetic presentation.

There is, then, a simple explanation for the effect of modality on constant errors. So simple an explanation invites complication from further work. For the present, it is offered as the best hypothesis on available evidence. Perhaps more important than this specific hypothesis is the wider implication that the modality effect on constant errors does not constitute evidence for a cross-modal deficit.

CHAPTER FIVE: THE EXPERIMENTS, PART C

[5.C] INTRODUCTION

The three brief experiments which make up this chapter each examine the role played, in matching tasks, by the strategy of coding relative to the body midline. Experiments previously described (in particular, A2b, A5, B2) indicated that interpretation of midline effects is relevant to the interpretation of modality effects. Before discussing this further, a brief introduction is given to the use of coding relative to the midline.

In broadest terms, the experiments in this chapter examine whether or not children code positions relative to the body midline (the sagittal plane). If the answer is in the affirmative, then further questions can be posed: do children code in this way in all sense modalities, or under what conditions in each modality?

The midline is a spatial reference which, potentially at least, is common to the three main spatial senses (vision, hearing, and kinaesthesis). For each of these senses, the midline is mid-way between paired receptors (eyes, ears and hands). For this reason, the midline represents a potentially important "anchor" for cross-modal matching of positions in space.

Is the potential of the midline realised when children perform matching tasks? If so, then the following factors would be expected to affect performance:

(a) The reliability of the subject's knowledge of where his midline is. This would vary with, for example, external marking of the midline, and consistency of body orientation.

(b) The distance between the position to be matched and the midline, in the absence of another reliable reference point.

What evidence is there, concerning the effects of these factors on children's matching performance? Considering the apparent value of midline-relative coding, surprisingly little evidence is available. Probably the best, if not the only, relevant evidence is an unpublished study cited by Millar (1981, p.300). In this study, blind and (blindfolded) sighted were given kinaesthetic matching tasks. Two reference conditions were given. In one condition, the end-points of movements were to be matched, and the end-points coincided with the midline. In the other condition, length was to be matched, and neither the start nor the end-point coincided with the midline. In each condition, recall was either immediate, or after unfilled delay, or after a delay during which mental tasks were performed.

In the midline-position task, blind and sighted performed with equal accuracy, and judgements were sensitive to processing demands during delay. In the length task, both blind and sighted performed less accurately, the sighted were superior to the blind, and only the sighted were affected by processing demands.

The above study demonstrates differential coding strategies in blind and sighted children: both blind and sighted were able to take advantage of the alignment of the movement with the midline, but in the length task strategies differed between the two groups. It has, however, limited relevance to the issue with which I am concerned, namely midline-relative coding, since it is not clear from this experiment, whether or not performance is affected by the distance of the start- or end-point from the midline. In other words, it cannot be said with certainty that midline-relative coding is used in a task of this kind when the midline is not itself the position coded.

Further evidence is provided by my own experiments, in Chapters Three, Four and Six. In these experiments, two standard

positions were used. The position more distant from the midline was consistently found to be matched with greater bias to undershoot (Experiments A1, A2, A3, A4, A5, B2, D1, D2, D3). This indicates that the midline is used as a reference point, because the total distance moved was the same for each standard position: only position relative to the midline was varied. Two issues are, however, raised by these experiments.

First, in three experiments (A4, D2, D3), there is a significant Modality x Standard interaction. In each case, the effect of standard is significant for VK, KV, and KK, but not for VV. This would suggest that the strategy adopted in VV is sometimes different from that adopted in other modality conditions. If this is the case, it is an important result, for two reasons:

a) Under other experimental conditions, VV performance efficiency might be affected, in addition to bias. The strategy should therefore be controlled if further research is to be carried out into effects of modality on variability.

b) If strategy in VV differs from strategy in other conditions, it must be inferred that the strategy adopted depends on response modality, in addition to acquisition modality. Otherwise the effect of standard should be absent in VK when it is absent in VV. An effect of response modality on strategy would be consistent with the results of Experiment A5, in which the standard effect was found selectively in the cross-modal conditions.

Second, it is not completely clear what the role of the midline is. Except in the 70 degree response movement condition, in Experiments A1 and A2, the standards differ in two ways. There is the previously noted difference that one standard position is further from the midline than the other (40 vs 20 degrees). Also, however, the response movement crosses the midline only for the 20 degree

standard. If this feature of the procedure is responsible for effects of standard position, then these effects can be regarded as interference effects, rather than encoding strategy effects.

Since the interpretations of several experiments (A2b, A5, B2) assume a strategy account, this matter must be clarified. Experiments A1 and A2 provide some evidence on this point. In these experiments, in the 70 degree response movement condition, the response movement crossed the midline for each standard position. The effect of standard was nevertheless significant. However, "criterion errors" in these experiments make this result difficult to interpret. Further evidence is therefore needed.

The experiments which follow address these two issues.

EXPERIMENT C1

[5.C1.1] INTRODUCTION

This experiment addresses the question, raised in the previous section, of whether the effect of standard position is simply one of distance from midline, or whether it is important that the response movement crosses the midline. The hypothesis adopted here is that crossing the midline is not the important feature. This hypothesis is based on two considerations:

(a) In Experiment A1 and A2, 70 degree response movement condition, the midline-crossing explanation is not valid, as noted in the previous section.

(b) All trials begin with the light/lever at the midline, so crossing the midline provides little additional information.

The hypothesis was tested for VV matching, in the following way. With the exception of Experiments A1 and A2, the matching experiments concerned have used an acquisition movement with a total extent of 50 degrees, and a response movement starting 20 degrees nearer to the standard position. If the acquisition and response movement for each standard are increased in extent, by 20 degrees, then all the movements cross the midline. The prediction from the hypothesis is clearly that the effect of standard will remain under these conditions. In other words, it is predicted that there should be (a) a main effect of standard, and (b) no interaction of standard with movement length with decreased standard effect with longer movements.

The secondary purpose of this experiment was to confirm that the standard effect is found with VV matching, in spite of failures to replicate this in Experiments A4, D2 and D3.

[5.C1.2] METHOD

[5.C1.2a] Design

A factorial design was used: Standard Position(2) x Movement Length(2) x Session(2), with repeated measures on all factors.

[5.C1.2b] Subjects

The subjects were 16 children of mean age 6yrs,8mths (range 6yrs,3mths - 7yrs,7mths).

[5.C1.2c] Apparatus

The apparatus was described in Section [2.3]

[5.C1.2d] Procedure

The standard procedure for VV matching was used, as described in Section [3.A3.2]. The standards and movement lengths are shown in Table [5.C1.1]. The standards and movement lengths for practice trials are shown in Table [5.C1.2].

Table [5.C1.1] Standards and Movement Lengths for Test Trials in Experiment C1

		ACQUISITION MOVEMENT		RESPONSE MOVEMENT
Standard	Movement Length	Starts	Ends	Starts
20 deg.	SHORT	-30 deg.	20 deg.	-10 deg.
	LONG	-50 deg.	20 deg.	-30 deg.
40 deg.	SHORT	-10 deg.	40 deg.	10 deg.
	LONG	-30 deg.	40 deg.	-10 deg.

Table [5.C1.2] Standards and Movement Lengths for Practice Trials in Experiment C1

SESSION	PRACTICE TRIAL	STANDARD	MOVEMENT LENGTH*
1	1	60 deg.	A
1	2	30 deg.	B
2	1	30 deg.	B

* A = Movement length used for first trial.
B = Movement length used for second trial.

[5.C1.2e] **Counterbalancing**

Each subject was given each of the four combinations of standard and movement length once in each session. The subjects were grouped into fours. In the first session, two subjects from each group were given the standards in the order 20-40-20-40, the other two in the order 40-20-40-20. One from each pair of subjects was given the movement lengths in the order LONG-SHORT-LONG-SHORT, the other in the order SHORT-LONG-SHORT-LONG. In the second session, each subject was given the standards and movement lengths in the alternative order to his first session.

[5.C1.3] RESULTS

[5.C1.3a] Constant Errors

Constant errors were analysed by 2 x 2 x 2 ANOVA (Standard x Movement Length x Session). Two effects were significant: standard [$F(1,15) = 13.28$, $p < 0.005$] and movement length [$F(1,15) = 7.53$, $p < 0.02$]. The effect of standard means that undershooting was greater for the 40 degree than for the 20 degree position. The effect of movement length means that undershooting was greater with longer movements. The means are shown in Table [5.C1.3].

The important comparison is the comparison of standards in the long movement condition. Pre-planned t-tests indicated that this difference is highly significant ($t = 4.068$, $p < 0.001$), whereas the difference is not significant for the short movement length ($t = 1.47$, $p > 0.05$).

Table [5.C1.3] Mean Constant Errors (in degrees)

MOVEMENT LENGTH	STANDARD 20 deg.	40 deg.	MEAN
SHORT	1.95	0.00	0.98
LONG	0.78	-4.61	-1.91
MEAN	1.37	-2.31	

Table [5.C1.4] Mean Constant Error (in degrees)

STANDARD	MOVEMENT LENGTH	SESSION	
20 deg.	Short	1	2.19
		2	1.72
	Long	1	1.72
		2	-0.16
40 deg.	Short	1	0.47
		2	-0.47
	Long	1	-3.59
		2	-5.63
		MEAN	-0.47

[5.C1.3b] **Variability**

Within-subject deviation was subjected to logarithm transform, and analysed by 2 x 2 ANOVA, Standard x Movement Length. There were no significant effects. Between-pair difference was also analysed by 2 x 2 ANOVA, Standard x Movement Length. Again there were no

significant effects. With both measures of variability, there was a non-significant trend for performance to be more variable with the longer movement.

Table [5.C1.5] Mean Within-subject Deviation (WSD)
(Logarithm transformed scores)

and Mean Between-pair Difference (BPD)
(Logarithm transformed scores)

STANDARD	MOVEMENT LENGTH	WSD	BPD
20 deg.	Short	1.58	1.37
	Long	1.63	1.58
40 deg.	Short	1.47	1.59
	Long	1.89	1.97
	MEAN	1.64	1.63

[5.C1.4] DISCUSSION

The results support the hypothesis that the effect of Standard is due to the distance of the standard position from the midline. There is no support for the hypothesis that the effect of standard is related to crossing the midline. If anything, the effect of standard position is greater when the midline is crossed during response for both standards (i.e. in the long movement condition).

The secondary purpose of the experiment was to replicate the standard effect. This was achieved only with the long movements. The implications of this are considered in the discussion of Experiment C3.

EXPERIMENT C2

[5.C2.1] INTRODUCTION

This experiment was performed with the same primary purpose as Experiment C1, but using KK, rather than VV matching.

[5.C2.2] METHOD

Design, apparatus, and counterbalancing were the same as for Experiment C1. The standard procedure for KK replaced that for VV, with the same standard positions and movement lengths. The mean age of the subjects was 6yrs,8mths (range 6yrs,5mths - 7yrs,1mth).

[5.C2.3] RESULTS

[5.C2.3a] Constant Errors

Constant Errors were analysed by 2 x 2 x 2 ANOVA (Standard x Movement Length x Session). One effect was significant, that of movement length [$F(1,15) = 4.83$, $p < 0.05$]. Undershooting was greater with the longer movements. The means for this effect, and for the effect of standard are of interest, and are shown in Table [5.C2.1].

Table [5.C2.1] Mean Constant Errors
(in degrees)

MOVEMENT LENGTH	STANDARD 20 deg.	40 deg.	MEAN
Short	3.59	1.33	2.46
Long	0.55	-2.42	-0.94
MEAN	2.07	-0.55	0.76

Pre-planned t-tests indicated that the effect of standard is not significant with either movement length (with the long movement, $t = 1.29$, $p > 0.10$). However, it is of some importance that the

Standard x Movement Length interaction does not approach significance [$F(1,15) = 0.05$, $p > 0.80$]. This supports the hypothesis that the effect of standard position is due to the distance of the standard position from the midline, rather than being due to crossing or not crossing the midline during response.

Closer inspection of the data revealed that the effect of standard is significant in Session 1 ($t = 2.845$, $p < 0.01$), but not in Session 2. The mean difference between standards is 5.469 degrees in Session 1, and 0.235 degrees in Session 2.

In conclusion, then, when the effect of standard is present, it is unaffected by length of movement, and hence unaffected by crossing the midline during response.

[5.C2.3b] Within-subject Deviation

Within-subjects Deviation was subjected to logarithm transform, and analysed by 2 x 2 ANOVA (Standard x Length). One effect was significant, the Standard x Length interaction [$F(1,15) = 4.99$, $p < 0.05$]. The means for this effect are shown in Table [5.C2.2].

Table [5.C2.2] Within-subject Deviation
(Logarithm transformed scores)

		STANDARD 20 deg.	40 deg.	MEAN
MOVEMENT LENGTH	Short	1.68	2.24	1.96
	Long	2.24	2.08	2.16
	MEAN	1.96	2.16	2.06

Newman-Keuls post-hoc tests indicated that only one difference is significant, the difference between standards, with the short movement ($q = 3.44$, $p < 0.05$).

[5.C2.3c] Between-subject Variability

Between-pair Difference was subjected to logarithm transform and analysed by 2 x 2 ANOVA, Standard x Movement Length. There was one significant effect, that of Standard [$F(1,7) = 7.20$, $p < 0.05$]. This meant that variability was higher with the 20 degree than with the 40 degree standard, the means being 2.16 and 1.65 respectively. This suggests that strategy was more variable with the 20 degree standard.

Table [5.C2.3] Mean Constant Errors
(in degrees)

STANDARD	MOVEMENT LENGTH	SESSION	
20 deg.	Short	1	5.31
		2	1.88
	Long	1	2.19
		2	-1.09
40 deg.	Short	1	-1.09
		2	3.75
	Long	1	-2.34
		2	-2.50
		MEAN	0.76

Table [5.C2.4] Mean Within-subject Deviation (WSD)
(Logarithm transformed scores)

and Between-pair Difference (BPD)
(Logarithm transformed scores)

STANDARD	MOVEMENT LENGTH	WSD	BPD
20 deg.	Short	1.68	2.35
	Long	2.24	1.97
40 deg.	Short	2.24	1.86
	Long	2.08	1.44

[5.C2.4] DISCUSSION

The results of this experiment are less clear-cut than the results of Experiment C1. The effect of standard is present in both movement-length conditions, but the difference is not statistically significant overall. The effect is highly significant in the first session, but marginally reversed in the second session.

The important result is that the effect of Standard is not altered by whether or not the response movement crosses the midline. This supports the hypothesis that the Standard effect depends on distance from the midline, not the act of crossing the midline during response.

EXPERIMENT C3

[5.C3.1] INTRODUCTION

This experiment further examines the role of midline-relative coding in position matching. It was previously noted (Section [5.C]), that the effect of standard is sometimes absent in VV matching. This is difficult to explain, because there is no obvious difference between those experiments where there is a Modality x Standard interaction, and those experiments where there is not. There is no single factor which is present (or absent) when the interaction is absent (or present).

The hypothesis is therefore proposed, that for reasons elaborated below, the subjects' strategy in VV matching varies from experiment to experiment.

What might this variation in strategy be? A number of two-class classifications of possible strategies are described in Section [1.2]. Of these, one was thought to have the potential to explain the variability in the standard effect in VV. A distinction can be drawn between two strategies:

(a) As the light moves, the subject continuously updates its position relative to the midline.

(b) When the light has ceased moving, the subject encodes its position, relative to the midline.

These two strategies are not mutually exclusive. The subject may update his representation of the light's position, then apply a correction when the light has stopped. It is hypothesised that small changes in the instructions to the subject, or in the subject's understanding of the instructions, will alter the weight given to the position indicated by each strategy.

In the present experiment, the standard VV matching procedure is compared with VV matching with slightly altered instructions (VV_I). The altered instructions involved the addition of two simple commands (see "Procedure"), which would not be expected to have any retroactive interference effect. To test for such an effect, variability was analysed, in addition to constant errors.

The predicted result was that instructions will alter the effect of Standard. More specifically, it was predicted that "updating" will be reduced by the instructions given in condition VV_I, and that this would reduce the effect of Standard on constant errors.

The reasoning behind this prediction is as follows. The first additional instruction in VV_I is "keep looking in", given at the end of the acquisition movement. It was expected that this would tend to reduce the likelihood of the subject looking away from the end-point of the movement. This should make it more likely that the subject will estimate at this stage of the trial, how far the light is from the midline.

The second additional instruction is "watch the light again", as the shutter re-opens for the response movement. This should tend to make the subject look to the midline, where the light is at this stage of the trial. Without this instruction, a subject who is still looking towards the correct end-point might attempt to continue looking in that direction. The attempt would be unlikely to be successful, as the subject's attention would be drawn toward the light, as it moved. As a result, the subject would have a clear idea of neither the midline nor the end-point, and would need to perform retrospective updating.

It remains to explain why it was predicted that reducing updating should reduce the standard effect. First, updating was

thought to be more likely when kinaesthesis is involved (i.e. in conditions VK, KV and KK), the conditions in which the Standard effect is consistently found. The effect of velocity on kinaesthetic estimation, demonstrated in Chapter Six, could not occur unless there were updating. Second, while a subject is updating, attention may by drawn from the midline, promoting bias.

Clearly, more drastic changes in the instructions could have been introduced, with greater likelihood of gaining significant results. However, it was desired to mimic the effects of children's spontaneous shifts in strategy and/or small variations in the experimenter's phrasing of the instructions. Large changes in the instructions would therefore have been inappropriate.

[5.C3.2] METHOD

[5.C3.2a] Design

A factorial design was used, Condition(2) x Standard(2) x Pair of trials(4), with repeated measures on the last two factors.

[5.C3.2b] Subjects

The subjects were 24 children of mean age 6yrs,10mths (range 6yrs,3mths - 7yrs,6mths).

[5.C3.2c] Apparatus

The apparatus was described in Section [2.3].

[5.C3.2d] Procedure

In condition VV, the standard procedure for VV matching was used (see Section [3.A2a.2]). In condition VV_I, two modifications were introduced:

(a) At the end of the acquisition movement, as the shutter was

closing, the experimenter said "keep looking in". This instruction reminded the subject not to look up from the apparatus during the acquisition-response interval.

(b) When the shutter re-opened for the response movement, the experimenter said "watch the light again".

[5.C3.2e] Counterbalancing

In each condition, half the subjects were given the standards in the order 20-40-20-40 in the first session, and half in the order 40-20-40-20. In the second session, the alternative order was given to each subject.

[5.C3.3] RESULTS

[5.C3.3a] Constant Errors

Constant errors were analysed by 2 x 2 x 4 ANOVA (Condition x Standard x Pair of Trials). Only one effect was significant, the Condition x Standard interaction [$F(1,22) = 4.39$, $p < 0.05$]. The means for this interaction are shown in Table [5.C3.1]

Table [5.C3.1] Mean Constant Error
(in degrees)

	STANDARD		
CONDITION	20 deg.	40 deg.	MEAN
VV	-0.21	-2.92	-1.56
VV_I	-1.98	-0.42	-1.20
MEAN	-1.09	-1.67	-1.38

Newman-Keuls post-hoc tests were used to compare the means involved in the significant interaction. No differences were significant. Only one comparison could be made using a pre-planned t-test, the comparison between standards for condition VV. For this

comparison, $t = 1.88$, $p < 0.05$. It might be argued that, if t-tests were possible for the other comparisons, they might also show significant differences. This is not the case. Neither is there a significant difference between the two larger means combined and the two smaller means combined (Sheffé test, $F(3,189) = 3.26$, $p > 0.05$).

[5.C3.3b] Variability

Within-subjects deviation was subjected to logarithm transform, and analysed by 2 x 2 ANOVA (Condition x Standard). There were no significant effects.

Between-pair difference was also subjected to logarithm transform, and analysed by 2 x 2 ANOVA (Condition x Standard). There were no significant effects.

Table [5.C3.2] Mean Constant Errors
(in degrees)

STANDARD:	20 deg.				40 deg.			
PAIR:	1	2	3	4	1	2	3	4
CONDITION VV	-0.42	-1.46	1.04	0.00	-1.88	-5.21	-2.08	-2.50
VV_I	-1.88	-3.33	-1.46	-1.25	-2.29	1.25	0.21	-0.83

Table [5.C3.3] Mean Variable Error (VE)
(Logarithm transformed scores)

and Mean Between-pair Difference (BPD)
(Logarithm transformed scores)

CONDITION	STANDARD	VE	BPD
VV	20 deg.	1.60	1.38
	40 deg.	1.71	1.12
VV_I	20 deg.	1.54	1.62
	40 deg.	1.75	2.10

[5.C3.4] DISCUSSION

As predicted, there was a significant Condition x Standard interaction. This supports the reasoning and the hypothesis which were set out in the introduction to this experiment, namely that the standard effect in VV matching depends on minor features of the instructions.

A sufficient explanation of the interaction is that the standard effect occurs only when the subject continuously "updates" an estimate of the position of the lever, a strategy favoured in the condition in which the standard effect was found. It can also be noted that two of the three experiments in which the standard effect was absent in VV involved a fast acquisition movement (D2, D3). This procedure should tend to bias the subject against updating.

Thus, it seems that the midline is used in the VV condition, but the manner in which it is used depends on task conditions, so that strategy sometimes differs from other modality conditions.

In broader terms, the difference between modality conditions in previous experiments, represented by the Modality x Standard interaction, can be explained by small variations in strategy. The same variations in strategy can in principle be used to explain the absence of the standard effect in the previous experiment.

The altered instructions did not affect variability. There is no support, therefore, for the notion that the altered instructions have their effect through retroactive interference.

A return can now be made to Experiment C1, where it was found that the effect of Standard on VV matching was significant only when longer movements were used. Using longer movements should decrease the likelihood of updating. Hence, there is further support here for the hypothesis proposed in the present experiment.

CHAPTER SIX: THE EXPERIMENTS, PART D

[6.D] INTRODUCTION

In previous chapters, some aspects of encoding strategy have been examined: coding relative to midline (self-referent coding), coding relative to external features, selective encoding of position and length, and "updating". In this chapter, another aspect of encoding strategy is examined in more detail. Specifically, this chapter is concerned with the influence of temporal parameters of a movement on judgements of the spatial parameter referred to as "position".

In Section [1.1], it was noted that kinaesthetic presentation of a position in space necessarily involves presentation of a duration, whereas visual presentation does not necessarily involve presentation of a duration. In other words, spatial and temporal parameters of a movement are separable visually, but not kinaesthetically.

It is also the case, that "reference" positions are normally available when an individual needs to remember a position visually. Only the distance from the reference position need be remembered. Kinaesthetically, one hand cannot normally perceive simultaneously a reference position and the position to be remembered. A strategy of continuously updating estimates of position would therefore seem to be more likely when perception is kinaesthetic. The hypothesis was therefore proposed, that the interdependence of spatial and temporal judgements is greater for kinaesthesis than for vision.

This hypothesis was tested by varying the velocity of the movement which was used to present the standard position, in position matching tasks. The method is described in more detail in the

experimental reports which follow. First, a digression is necessary.

Although it was not a consideration in their design, the experiments to be described will be seen to be relevant to the explanation of the 'tau effect' proposed by Jones and Huang (1982), as an extension to the hypothesis of Price-Williams (1954).

The tau effect may be summarised as follows. Two or more successive stimuli can be used to present one or more spatial intervals, each spatial interval of course being associated with a corresponding temporal interval. For example, two light flashes, ten centimetres apart, one flash occurring two seconds after the first, define a spatial interval of ten centimetres and a temporal interval of two seconds. It has been found that subjects judge the spatial interval to be greater, the longer the temporal interval (Benussi 1913, Bill and Teft 1969, 1972, Helson 1930, Helson and King 1931, Jones and Huang 1982, Lechelt and Borchert 1977, Scholtz 1924).

It is also known that the temporal interval is judged longer, the greater the spatial interval (Abe 1935, Cohen et al 1953, Newman and Lee 1972, Parks 1968, Price-Williams 1954, Jones and Huang 1982). This is known as the kappa effect.

The tau effect has been found using displays in which there is no actual movement: a stationary stimulus in one place is followed by another stationary stimulus in another place. It has been suggested that subjects in some way treat the display as though a movement had occurred, and thereby introduce a phenomenal inter-dependence of the spatial and temporal intervals (Cohen et al 1955, Collyer 1977, Jones and Huang 1982, Price-Williams 1954). As Jones and Huang put it, the subject is said to

"make a reasonable imputation of motion to a discontinuous display, and hence integrate temporal and spatial information by means of a functional relation with velocity".

Jones and Huang note that the tau effect is not found when movement is (as an illusion) perceived (Mashour 1964). It is some interest, therefore, to determine whether or not the effect occurs when there is actual movement, as there is in my experiments. If the tau effect is found, in either modality, then some modification will be required to apply the Jones and Huang hypothesis to a phenomenon which occurs when there is no movement and with real movement, but not when there is illusory movement.

Because explanation of the tau effect was not the reason for performing the experiments which follow, the tau effect will not be considered again until the discussion at the end of this chapter.

EXPERIMENT D1

[6.D1.1] INTRODUCTION

This experiment was performed in order to test the hypothesis that spatial judgements (position matching) are biased by movement duration, when movements are perceived kinaesthetically, but not when they are perceived visually.

The method chosen was to vary the velocity of the movement used to present the standard position. The experiment was designed to give the maximum probability of detecting a tau effect, while using procedures similar to those used in the previous matching experiment. For this reason, two velocities of movement were used (one described as Fast, the other described as Slow), the two being as different as possible, and all the relevant comparisons were within-subjects. It was intended that further experiments would be used to examine the tau effect further, if it could be demonstrated at all.

In this experiment, then, the subject is faced with a change in movement velocity between acquisition and response. This makes the duration of the movement (or the time taken to move from midline to end-point) an unreliable cue by which to reproduce the end-point of the movement. There are two basic strategies available to the subject in these circumstances. One strategy would be to attempt to ignore the duration of the movement altogether, and match only on the basis of the distance moved. A second possible strategy would be to attempt to compensate for the change in velocity, by allowing a slower movement more time to cover a given distance.

The hypothesis was that (a) with visual matching, the first strategy will be used successfully, and that (b) with kinaesthetic matching, either strategy might be used, less successfully, so that judgements of distance will be biased by duration of the movement.

The primary prediction was therefore that, in the kinaesthetic condition, undershooting would be greater with a fast acquisition movement than with a slow acquisition movement.

Secondary predictions can be made, concerning performance variability. There are two reasons for expecting variability to be higher for kinaesthetic than for visual matching. First, success in ignoring or compensating for duration would be expected to vary from trial to trial and from subject to subject, introducing an additional source of variability. Second the two strategies may have differing effects on constant errors: there is no reason why two quite different strategies for coping with a problem should result in exactly the same degree of bias.

The prediction, that variability should be higher for kinaesthetic than for visual matching, is stronger for the Slow than for the Fast condition. This is because in the Fast condition, the compensation strategy would be difficult and unlikely to be used, while the presence of the lever in two places in rapid succession would facilitate the use of the alternative strategy. Hence, in the Fast condition there is only one likely strategy, and there is likely to be greater consistency in the use of this strategy than in the Slow condition.

Finally, the effect of velocity on variability should be greater with younger subjects, if they find it more difficult to adopt a consistent strategy.

[6.D1.2] METHOD

[6.D1.2a] Design and Counterbalancing

A factorial design was used, Age(2) x Modality(2) x Velocity(2) x Standard Position(2), with repeated measures on the last three factors.

The velocity conditions are described under Procedure, below. The two standard positions were 20 and 40 degrees, as described in detail in Section [3.A2a.2].

Each subject was given eight trials in total, four in each of two sessions. VV matching was given in one session, KK in the other, the order of modality conditions was counterbalanced across subjects. In each session, two Fast and two Slow trials were given. In each pair of trials, each standard position was given once. The order of Velocity conditions and standards was counterbalanced across subjects.

[6.D1.2b] Apparatus

The apparatus was as described in Section [2.3].

[6.D1.2c] Subjects

The subjects were 64 children in two age-groups. The mean ages of the two groups were 6yrs,6mths (range 6yrs,3mths to 7yrs,0mths) and 9yrs,0mths (range 8yrs,8mths to 9yrs,4mths).

[6.D1.2d] Procedure

As far as possible, instructions and practice were the same as in Experiments A2 - A5, as described in Section [3.A2a.2]. Clearly, some changes were necessary in order that the subject should be prepared for movements of different velocities. This was particularly important in the KK condition, in which the subject had to keep his hand on the lever as it moved.

In the Fast condition, the acquisition movement was as fast as possible, within the limit that the subject had to be able to easily keep his hand on the lever as it moved. This meant that the movement was completed in approximately 0.5 seconds. In the Slow movement

condition, the duration of the movement was approximately 6 seconds. The velocities were therefore approximately 100 and 8.3 degrees per second, compared with approximately 25 degrees per second for the response movement.

The initial training trials were those described in Section [3.A2a.2], using the Velocity condition which was to be given first. For the Slow condition, the subject was told that the lever would move very slowly the first time, then more quickly when it was his turn to say "stop". For Fast trials it was explained to the subject that the lever would move very quickly the first time, then more slowly when it was his turn to say "stop". The experimenter said "ready, go" before moving the lever quickly.

After the first pair of test trials, practice for each pair consisted of one trial in the next velocity condition to be used.

[6.D1.3] RESULTS

[6.D1.3a] Constant Errors

Constant errors were analysed by 2 x 2 x 2 x 2 ANOVA, Age x Modality x Velocity x Standard Position. One main effect was significant, that of Standard Position [$F(1,62) = 42.72$, $p < 0.0001$]. As in previous experiments, this was due to undershooting being greater with the 40 degree standard. In addition, there were two significant interactions,

Modality x Velocity [$F(1,62) = 18.42$, $p < 0.0001$] and

Velocity x Standard [$F(1,62) = 8.41$, $p < 0.01$].

The mean errors for the significant effects are shown in Table [6.D1.1]. Newman-Keuls post hoc tests were used to examine the significant interactions.

The Modality x Velocity interaction had been predicted (see the introduction to this experiment). More specifically, it had been

predicted that the velocity of the acquisition movement would affect only KK, resulting in greater undershooting in the Fast condition than in the Slow condition in KK. This predicted difference between Fast and Slow was significant ($q = 5.70$, $p < 0.01$). In VV there was a non-significant trend in the opposite direction.

In addition, the difference between modality conditions was significant only in the Slow condition ($q = 5.11$, $p < 0.01$). This suggests that the effect of velocity was mainly due to the Slow condition, and that an effect of the fast movement cannot strictly be inferred from the results.

Table [6.D1.1] Mean Constant Errors (in degrees)
For the Significant Main Effect and Interactions

(a) Standard, and Velocity x Standard

	STANDARD:	20 deg.	40 deg.
VELOCITY:	Fast	1.33	-3.84
	Slow	0.84	-1.09
	Mean	1.08	-2.47

(b) Modality x Velocity

	MODALITY:	VV	KK
VELOCITY:	Fast	-0.51	-2.01
	Slow	-1.84	1.58

The Velocity x Standard interaction means that the difference between standards is significant in both Velocity conditions. In the Fast condition, $q = 9.37$ ($p < 0.01$); in the Slow condition, $q = 3.51$ ($p < 0.05$). An examination of the means for this interaction suggests that the velocity effect is stronger with the 40 degree standard than with the 20 degree standard. This is not surprising, for two reasons. First, the position closer to the midline should be easier to encode

as a distance from the midline because a shorter distance is encoded. Second, a biasing effect of velocity would be expected to be approximately constant per unit distance.

Further comparisons were therefore performed, using t-tests to test the predicted difference between Fast and Slow for KK. This difference was found to be significant with the 40 degree standard ($t = 5.48$, $p < 0.001$) but not with the 20 degree standard ($t = 1.43$, $p > 0.05$).

A more thorough examination of the velocity effect with the 20 degree standard was then carried out. Results for the first pair of trials in each session showed a strong velocity effect, the means being -0.94 degrees and 3.28 degrees for the Fast and Slow conditions respectively. The difference between these means is significant ($t = 2.21$, $p < 0.05$). In the second pair of trials in each session, there was actually a non-significant reversal of the velocity effect, the means being 2.81 degrees and 1.56 degrees for the Fast and Slow conditions respectively.

This suggests a change in susceptibility to the biasing effect of velocity between sessions. In other words, practice enabled the subjects to ignore irrelevant information, at least for the 20 degree standard.

It is also worth noting, that in the Slow condition, the difference between visual and kinaesthetic conditions is approximately the same for the 20 degree and 40 degree standards (3.16 degrees and 3.67 degrees respectively). In the fast condition, the difference between modality conditions is less overall (as indicated by Newman-Keuls tests on the Modality x Velocity interaction), and is smaller for the 20 degree standard than for the 40 degree standard (0.78 degrees and 2.26 degrees respectively). This supports the analysis given in the introduction to the experiment, in

which it was argued that it should be easier to ignore duration in the Fast kinaesthetic condition than in the Slow kinaesthetic condition.

[6.D1.3b] Between-subject Variability

Each subject was given only one trial in each condition. Within-subject deviation could therefore not be calculated. This design was used in order to maximise the chances of detecting a difference in constant errors between conditions. Effects on variability were predicted because strategy was hypothesised to be more variable in the KK condition. Analysis of between-subject variability is suitable for testing a hypothesis of variable strategy (see Section [2.6.6]).

Subjects could be paired on all factors except the order in which the two different standards were presented. In order to minimise any possible effects of this, between-pair difference was calculated from the mean of the two standards for each subject and condition.

The analysis of between-pair difference was then carried out by 2 x 2 x 2 ANOVA (Age x Modality x Velocity) with logarithm transform. Two effects were significant: the main effect of Age [$F(1,30) = 12.44$, $p < 0.002$] and the main effect of Modality [$F(1,30) = 15.04$, $p < 0.0005$]. These effects indicated that variability was higher in the 6-yr-old group (means: 2.19 vs 1.69), and higher in the kinaesthetic condition (means: 2.11 vs 1.76).

It was predicted that the effect of modality would be greater in the Slow condition, and greater for 6-yr-olds. This prediction was tested by using t-tests to compare the two modality conditions in each of the four combinations of age and velocity conditions. Table [6.D1.2] shows the results of this analysis. Only for 6-yr-olds in

the Slow conditions is the effect of modality significant. The prediction is therefore supported.

Table [6.D1.2] Values of Student's t for comparisons between modality conditions

	VELOCITY		
AGE	Fast	Slow	
6	0.63	2.72*	
9	1.30	1.69	*p< 0.025

One caveat must be noted, with regard to the variability results. Because the velocity of the movements was determined by the experimenter's judgement, there may well have been some variation in velocity from trial to trial, particularly in the case of "slow" movements. If this is the case, then part of the variability in the Slow condition may be explained on this basis. Partly for this reason, subsequent experiments used the Fast condition in preference to the Slow condition.

Table [6.D1.3] Mean Constant Errors (in degrees)

MODALITY:		VV				KK			
VELOCITY:		Fast		Slow		Fast		Slow	
STANDARD:		20deg.	40deg.	20deg.	40deg.	20deg.	40deg.	20deg.	40deg.
AGE:	6	3.20	-2.11	-1.02	-3.36	1.09	-5.23	1.95	-0.23
	9	0.23	-3.36	-0.47	-2.50	0.78	-4.69	2.89	1.72

Table [6.D1.4] Mean Between-pair Difference (logarithm transformed scores)

MODALITY:		VV		KK	
VELOCITY:		Fast	Slow	Fast	Slow
AGE:	6	2.05	1.95	2.19	2.55
	9	1.46	1.58	1.75	1.96

[6.D1.4] DISCUSSION

The primary prediction in this experiment was that the velocity of the acquisition movement would affect constant errors in kinaesthetic, but not in visual matching. The results fit this prediction: undershooting was greater in the Fast condition than in the Slow condition, in the kinaesthetic condition only.

This effect was absent, however, in the second of the two pairs of trials, when the 20 degree standard was given. It therefore seems that subjects did have a limited capacity for ignoring irrelevant velocity/time information.

The second prediction was that variability would be higher in kinaesthetic than in visual matching, especially in the 6-year-old group, and in the Slow condition. This prediction was supported in that variability was consistently higher in kinaesthetic matching, but significant only for 6-year-olds in the Slow condition.

The effect seems not to be related to the time available for encoding the movement (i.e. a difference in rate of processing between modalities). If a difference in rate of processing were responsible for the velocity effect, the effect should have been greater in the Fast condition, whereas in fact it was greater in the Slow condition.

Overall, then, there is good support for the hypothesis that velocity/time coding plays a more important role in kinaesthetic than in visual coding of movements. In more general terms, it can be said that there is a strategic difference (as defined in Section [1.2]) between vision and kinaesthesis. The next two experiments were performed in order to examine this difference in more detail.

EXPERIMENT D2

[6.D2.1] INTRODUCTION

In the previous experiment, a difference was demonstrated between visual and kinaesthetic position matching. The velocity of the movement used to present the standard position (the "acquisition movement") was made either faster or slower than the movement used for the response. The task therefore required the subject to either

(a) ignore the velocity and duration of the movement, and respond only on the basis of distance moved, or

(b) take into account the change in velocity, if responding on the basis of the velocity and duration of the movement.

As had been predicted, the velocity conditions differed with kinaesthetic matching, but not with visual matching. The effect of velocity of acquisition movement was shown primarily in the constant error results. In the kinaesthetic matching condition, a fast acquisition movement resulted in a tendency to undershoot, a slow acquisition movement resulted in a tendency to overshoot. In addition, variability was greater in the kinaesthetic condition than in the visual condition when the slow acquisition movement was used.

In the present experiment, cross-modal conditions were included, in addition to within-modal, and only the Fast movement condition was used. The Fast movement was used in preference to the Slow movement, because the velocity of the Fast movement could be made more consistent from trial to trial than could the velocity of the Slow movement (see Section [6.D1.3b]). Also, the previous experiment did not provide unequivocal evidence for an effect of the fast movement, since VV and KK did not significantly differ in mean constant error in the Fast condition. The present experiment was designed to extend the results of the previous experiment, in the

following ways.

First, cross-modal conditions were included in order to evaluate two hypotheses regarding the effect of velocity on kinaesthetic matching. One hypothesis is that the cue used when matching positions kinaesthetically is distance, but the velocity and duration of the movement bias the perception of distance. The second hypothesis is that velocity and duration are actually used as the basis of matching, and the velocity effect results from incomplete correction for the change in velocity of movement between acquisition and response. Only if the second hypothesis is correct would there be a mismatch of information between acquisition and response in the cross-modal conditions. If the second hypothesis is correct, therefore, within-modal performance should be superior to (less variable than) cross-modal performance.

Second, the inclusion of cross-modal conditions also provided a test of the hypothesis, implicit in the previous experiment, that the change in velocity of movement has its effect on perception of the acquisition movement. If this hypothesis is correct, then bias should arise in the KV condition, but not in the VK condition. The experiment was designed to make possible a comparison of the results with the results of Experiment A3, in which the acquisition and response movements were the same velocity.

Third, the conclusions drawn from the previous experiment were based on one measure of variability, between-subject variability. Only in the Slow condition was there a reliable difference in variability between VV and KK. In the present experiment, both within- and between subject variability are analysed, in order to test the reliability of the finding that there is no difference in variability between VV and KK.

[6.D2.2] METHOD

[6.D2.2a] Design and Counterbalancing

A factorial design was used, Age(2) x Modality(2) x Standard Position(2) x Pair of trials(3), with repeated measures on the last two factors.

Each subject's trials were divided between two sessions, one pair of trials in the first session, two pairs in the second session. Each standard (20 and 40 degrees, as described in Section [3.A2a.2]), was given once in each successive pair of trials. The order in which the standards were given to each subject was ABBAAB or BAABBA, counterbalanced across subjects.

This design was the same as that of Experiment A3, except that a second pair of trials was added in the second session. This design was used to permit comparison of results for the first two pairs of trials in the present experiment with the results of Experiment A3. The extra pair of trials was included in order to increase the number of measurements available for within-experiment analysis.

[6.D2.2b] Subjects

The subjects were 64 children in two age-groups. The mean ages of the two groups were 6yrs,8mths (range 6yrs,2mths to 7yrs,2mths) and 9yrs,1mth (range 8yrs,9mths to 9yrs,5mths).

[6.D2.2c] Apparatus

The apparatus was as described in Section [2.3].

[6.D2.2d] Procedure

The procedure was the same as in Experiment A3, except that the acquisition movement was faster than the response movement, as in

the Fast condition of Experiment D2.

[6.D2.3] RESULTS

[6.D2.3a] Constant Errors

Constant errors were analysed by 2 x 4 x 2 x 3 ANOVA, Age x Modality x Standard Position x Pair of trials. Two main effects were significant, Modality [$F(3,56) = 10.40$, $p < 0.0001$], and Standard [$F(1,56) = 31.47$, $p < 0.0001$].

Two interactions were also significant,

Modality x Standard [$F(3,56) = 7.67$, $p < 0.0002$]

and Age x Modality x Standard x Pair of Trials

[$F(6,112) = 2.75$, $p < 0.02$].

The means for the main effects and for the first order interaction are given in Table [6.D2.1]. Newman-Keuls post hoc tests were used to examine the Modality and Modality x Standard effects. The results of these tests are also given in Table [6.D2.1].

Overall, the Modality effect was consistent with predictions. It had been predicted that undershooting would be greater in KK and KV in this experiment than in the same conditions in Experiment A3. In Experiment A3, undershooting was greatest in VK, least in KV, with VV and KK intermediate and equal. In the present experiment, undershooting was high in VK and KK, and low in VV and KV. This was also the pattern in the results for the 40 degree standard alone. With the 20 degree standard, however, the pattern of the Modality effect was more like the pattern found in Chapter Three, with the acquisition movement the same velocity as the response movement. This supports the conclusion reached in the previous experiment, that the velocity effect is weaker with the 20 degree standard.

Except in VV, the Standard effect was significant, and in the same direction as in previous experiments, indicating that the midline is still used as a reference point when the acquisition movement is fast. The absence of the Standard effect in VV was examined in the previous chapter.

Table [6.D2.1]

(a) Mean Constant Error (in degrees)
For the Effects of Modality, Standard, and Modality x Standard.

		STANDARD		
		20 deg.	40 deg.	Mean
MODALITY:	VV	1.20	2.14	1.67
	VK	-3.49	-6.93	-5.21
	KV	3.75	0.47	2.11
	KK	-0.73	-7.03	-3.88
	Mean	-0.18	-2.84	

(b) Results of Newman-Keuls Tests for Significant Differences in the Above Effects

COMPARISON		q	p<
20 deg. Standard,			
	VV vs VK	3.65	0.05
	VK vs KV	5.64	0.01
	KV vs KK	3.49	0.01
40 deg. Standard,			
	VV vs VK	7.06	0.01
	VV vs KK	7.14	0.01
	VK vs KV	5.76	0.01
	KV vs KK	5.84	0.01
Overall,			
	VV vs VK	5.90	0.01
	VV vs KK	4.76	0.01
	VK vs KV	6.28	0.01
	KV vs KK	5.14	0.01
20 deg. vs 40 deg. Standard,	VK	4.51	0.01
	KV	4.31	0.01
	KK	8.28	0.01

Table [6.D2.2]

(a) Mean Constant Errors
(in degrees)

STANDARD:		20 deg.			40 deg.		
PAIR OF TRIALS:		1	2	3	1	2	3
AGE	MODALITY						
6	VV	0.94	3.44	2.19	0.94	1.25	2.50
	VK	-4.06	-1.56	-4.06	-4.69	-10.00	-4.38
	KV	2.81	3.44	1.88	-1.56	2.81	2.50
	KK	2.19	-2.50	-2.81	-6.25	-5.94	-9.38
9	VV	-1.56	2.81	-0.63	0.31	2.19	5.63
	VK	-3.44	-4.38	-3.44	-7.81	-6.56	-8.13
	KV	3.44	5.94	5.00	1.88	0.94	-3.75
	KK	-2.19	1.25	-0.31	-7.19	-9.38	-4.06

(b) Results of Newman-Keuls Tests for Significant Differences
Between Cell Means (all $p < 0.05$)

COMPARISON			q
6-yr-olds,	40 deg.,	Pair 2: VV vs VK	3.60
"	"	" : VK vs KV	4.10
"	"	Pair 3: VV vs KK	3.80
"	"	" : KV vs KK	3.80
9-yr-olds,	"	" : VV vs VK	4.40
20 deg. vs 40 deg.:			
	6-yr-olds,	VK, Pair 2	4.59
	"	KK, Pair 1	4.59
	"	", Pair 3	3.41
	9-yr-olds,	VV, Pair 3	3.40
	"	KV, Pair 3	4.76
	"	KK, Pair 2	5.78
6-yr-olds,	VK, 40 deg:	Pair 1 vs 2	2.88
"	" " :	" 2 vs 3	3.05
9-yr-olds,	VV, " :	" 1 vs 3	2.88
"	KV, " :	" 1 vs 3	3.05
"	KK, " :	" 2 vs 3	2.88

Table [6.D2.2] gives the mean constant errors for the significant third-order interaction. Newman-Keuls post hoc tests were used to make comparisons between means of cells which differ on only one variable. The results of these tests are also given in Table

[6.D2.2]. The significant differences, or the larger non-significant differences, do not follow any obvious pattern, except the one given by the Modality x Standard interaction.

The only significant difference in Table [6.D2.2], which represents an important deviation from the pattern given by the Modality x Standard interaction, is the difference between Pairs 2 and 3 for 9-year-olds in KK with the 40 degree standard. This difference indicates a reduction in the velocity effect in the third pair of trials, so that KK did not differ from KV or from VV.

[6.D2.3b] Comparison of Constant Errors in Experiments A3 and D2

The design of the present experiment, ignoring the third pair of trials, allows a comparison to be made with the results of Experiment A3, in which the acquisition and response movements were the same velocity. It was predicted that undershooting would be greater in the present experiment, in KV and KK only (see Section [6.D2.1].

Constant errors were analysed by 2 x 2 x 4 x 2 x 2 ANOVA, Experiment x Age x Modality x Standard Position x Pair of Trials. There were four significant effects,

Experiment [$F(1,112) = 9.11$, $p < 0.005$],

Modality [$F(3,112) = 18.67$, $p < 0.0001$],

Standard [$F(1,112) = 35.07$, $p < 0.0001$],

and Age x Modality x Standard x Pair of Trials

[$F(3,112) = 3.25$, $p < 0.05$].

Only effects which include the Experiment factor are of interest, other effects being dealt with in the individual analyses of the two experiments. The significant effect of Experiment means that undershooting was greater, overall, in Experiment A3 than in the

present experiment. This difference is in the opposite direction to the difference which was predicted (since it was predicted that undershooting would be greater in the present experiment in two conditions, KV and KK, the overall trend should have been for undershooting to be greater in the present experiment).

The predicted Experiment x Modality interaction was not significant ($p > 0.1$). The mean errors for this interaction, and for the main effect of Experiment, are shown in Table [6.D2.3]. It can be seen that, although the interaction is not significant, the effect of Experiment is absent in KK and approximately twice as large in VV as in VK and KV.

Possibly the overall difference between experiments could be explained by a difference in subject population or in the detail of experimental conditions (e.g. room size, the height of the table on which the apparatus was placed). Given that the Modality effect within the present experiment fits with predictions, this is a reasonable explanation for the unexpected results in the comparison of Experiments A3 and D2.

In Experiment D3, two velocity conditions are compared within-subjects, in order to put the above explanation to the test. In the remainder of the results and discussion of the present experiment, only within-experiment effects will be discussed.

Table [6.D2.3] Mean Constant Errors (in degrees)
For the Experiment and Experiment x Modality Effects

MODALITY:	VV	VK	KV	KK	Mean
EXPERIMENT: A3	-3.38	-7.58	0.31	-3.20	-3.59
D2	1.29	-5.31	2.46	-3.75	-1.33

[6.D2.3c] Variability

Variable Errors and Between-pair Difference were each subjected to logarithm transform and analysed by 2 x 4 x 2 ANOVA, Age x Modality x Standard. There were no significant differences in either analysis. This result supports and extends the result found in the previous experiment, that VV and KK do not differ in variability in the Fast condition.

The same result is consistent with the hypothesis that velocity/duration biases kinaesthetic encoding of distance, but is not itself used as the basis for matching. If velocity/duration were the basis of matching for kinaesthetically perceived movements, and distance the basis of matching for visually perceived movements, then there would be a mismatch of information in the cross-modal conditions, and increased variability would be expected in these conditions.

Table [6.D2.4] Mean Variable Errors
(logarithm transformed scores)

		STANDARD	
		20 deg.	40 deg.
AGE	MODALITY		
6	VV	1.40	1.39
	VK	1.57	1.67
	KV	1.54	1.41
	KK	1.44	1.58
9	VV	1.56	1.61
	VK	1.23	1.41
	KV	1.69	1.43
	KK	1.47	1.32

Table [6.D2.5] Mean Between-pair Difference
(logarithm transformed scores)

AGE	MODALITY	STANDARD 20 deg.	STANDARD 40 deg.
6	VV	2.29	2.15
	VK	1.29	1.93
	KV	2.13	1.70
	KK	1.71	1.51
9	VV	1.32	1.99
	VK	1.99	2.28
	KV	2.01	1.67
	KK	1.12	1.32

[6.D2.4] DISCUSSION

Because of the unexpected difficulty in comparing the results of this experiment with the results of Experiment A3 (see Section [6.D2.3b]), the main discussion of the the results of this experiment is included with the discussion of the next experiment, D3. It is only noted here that the results of this experiment are largely in support of the hypotheses set out in the introduction, the discrepancy being in the comparison of constant error results between experiments.

EXPERIMENT D3

[6.D3.1] INTRODUCTION

The aims of this experiment were the same as those of the previous experiment, Experiment D2. In that experiment, children were required to match the end-point of a movement, within and between modalities. The velocity of the acquisition movement was faster than the velocity of the response movement. It was intended, in Experiment D2, to compare the results with the results of Experiment A3. The latter experiment had a similar design, but the acquisition and response movements were the same velocity. This comparison proved unsatisfactory, owing to difficulties with comparisons between subjects (between experiments).

The present experiment therefore attempts the same comparison of velocity conditions, within subjects. The main hypothesis tested in this experiment is the same as that tested in the previous experiment, namely that the velocity of a movement biases kinaesthetic, but not visual, judgements of distance. The predictions were:

(a) A fast acquisition movement will result in undershooting when acquisition is kinaesthetic, whether response is visually or kinaesthetically guided.

(b) A fast acquisition movement will cause no specifically cross-modal effects, either on bias or on variability.

[6.D3.2] METHOD

[6.D3.2a] Design and Counterbalancing

A factorial design was used, Age(2) x Modality(4) x Velocity(2) x Standard Position(2) x Pair of trials(2), with repeated measures on the last three factors.

The velocity conditions were Equal (the acquisition and response movements being of the same velocity) and Fast (acquisition movement faster than response movement).

The two standard positions were 20 and 40 degrees left of the subject's midline, as described for Experiment A2a. "Pair of trials" refers to the first vs the second pair of trials in each session.

Each subject was given two testing sessions, one for Equal and one for Fast trials, with the order of sessions counterbalanced. In each session, four trials were given, two with each standard, in counterbalanced order.

[6.D3.2b] Subjects

The subjects were 128 children, in two age-groups. The mean ages of the two groups were 6yrs,8mths (range 6yrs,1mth to 7yrs,5mths) and 9yrs,1mth (range 8yrs,5mths to 9yrs,8mths).

[6.D3.2c] Apparatus

The apparatus was as described in Section [2.3].

[6.D3.2d] Procedure

The procedure for the Equal velocity condition was the standard procedure described in Section [3.A3.2]. In the Fast condition the procedure was the same as in Experiments D1 and D2.

[6.D3.3] RESULTS

[6.D3.3a] Constant Errors

Constant errors were analysed by 2 x 4 x 2 x 2 x 2 ANOVA, Age x Modality x Velocity x Standard Position x Pair of trials. Significant effects are shown in Table [6.D3.1] below.

The significant effects were examined using Newman-Keuls post

hoc tests. The main effects of Modality and Velocity were of little interest, owing to the interaction between these two factors. The mean errors for this interaction are shown in Table [6.D3.2]. As had been predicted, the difference between velocity conditions was significant only in the KV and KK conditions ($q = 4.71, 4.99$ respectively, $p < 0.01$).

Table [6.D3.1] Significant Effects in the ANOVA for Constant Errors.

EFFECT	F	d.f.	p<
Modality	7.49	3,120	0.0001
Velocity	8.74	1,120	0.005
Modality x Velocity	5.48	3,120	0.002
Age x Modality x Velocity	3.39	3,120	0.05
Standard	47.28	1,120	0.0001
Age x Standard	4.65	1,120	0.05
Modality x Standard	7.70	3,120	0.0001
Age x Velocity x Pair	12.37	1,120	0.001

Within each velocity condition, the differences between modality conditions confirmed the results of Experiments A3, A4, A5 and D2. In the Equal velocity condition, undershooting was greatest in VK, least in KV, and intermediate in VV and KK. The results of Newman-Keuls tests were as follows: VV vs VK $q = 2.87$, $p < 0.05$

VV vs KV $q = 4.43$, $p < 0.01$

VK vs KV $q = 7.30$, $p < 0.01$

VK vs KK $q = 3.90$, $p < 0.05$

KV vs KK $q = 3.40$, $p < 0.05$.

In the Fast velocity condition, undershooting was greater in VK and KK than in VV and KV. The values of q for the significant differences were 4.38 (VV vs VK), 3.47 (VV vs KK), 4.11 (VK vs KV) and 3.21 (KV vs KK), $p < 0.05$ in each case.

The results reported so far fit exactly with predictions. The Age x Modality x Velocity interaction complicates the picture. The

mean errors for this interaction are shown in Table [6.D3.2].

Table [6.D3.2] Constant Error Means (in degrees) for the Interactions Modality x Velocity and Age x Modality x Velocity

AGE	VELOCITY	MODALITY VV	VK	KV	KK
6	Equal	-1.29	-2.11	1.02	0.31
	Fast	0.12	-4.06	-0.31	-3.56
9	Equal	-0.74	-4.61	4.18	-0.66
	Fast	-0.16	-3.13	-0.16	-2.15
Mean	Equal	-1.02	-3.36	2.60	-0.18
	Fast	-0.02	-3.59	-0.23	-2.85

The difference between age groups is not significant in any combination of the Modality and Velocity conditions. In each age group, the pattern of modality and velocity effects is the same as for the two age groups combined. Differences between modality conditions only reach significance, however, for 9-year-olds in the Equal velocity condition. The Newman-Keuls test results in this case were as follows: VV vs VK $q = 3.35$, $p < 0.05$

VV vs KV $q = 4.26$, $p < 0.05$

VK vs KV $q = 7.61$, $p < 0.01$

VK vs KK $q = 3.42$, $p < 0.05$

KV vs KK $q = 4.19$, $p < 0.01$.

The difference between velocity conditions was significant for 6-year-olds in KK and for 9-year-olds in KV ($q = 4.82$, 5.40 respectively, $p < 0.05$ in each case). The implications of this interaction are discussed under DISCUSSION, below.

The mean errors for the Standard, Age x Standard interaction and Modality x Standard interaction effects are shown in Table [6.D3.3]. As in previous experiments, undershooting was greater with the 40 degree standard than with the 20 degree standard. This

difference was significant in both age groups and in each modality condition except VV ($p < 0.01$ in each case). The values of q were 9.04 (6-year-olds), 4.73 (9-year-olds), 4.43 (VK), 10.05 (KV) and 4.47 (KK). The absence of a difference between standards in the VV condition was the subject of Chapter Five.

The difference between age groups was not significant with either standard position.

Differences between modality conditions could not readily be interpreted because both velocity conditions contributed to the results. With the 20 degree standard, KV differed significantly from the other three conditions ($p < 0.01$), the values of q being 5.42 (vs VV), 7.66 (vs VK) and 5.25 (vs KK). With the 40 degree standard, VK differed significantly from VV ($q = 4.99$, $p < 0.01$), and from KV ($q = 3.72$, $p < 0.05$). These results suggest that the effect of velocity is stronger with the 40 degree standard, as was found in the previous two experiments.

Table [6.D3.3] Mean Constant Errors (in degrees) for the Standard, Age x Standard, and Modality x Standard Effects

(a) Standard, Age x Standard.

AGE GROUP:	6	9	MEAN
STANDARD			
20 deg.	0.60	0.03	0.31
40 deg.	-3.07	-1.89	-2.48

(b) Modality x Standard

MODALITY:	VV	VK	KV	KK
STANDARD				
20 deg.	-0.37	-2.21	4.06	-0.23
40 deg.	-0.66	-4.75	-1.70	-2.80

The means for the last significant interaction (Age x Velocity x Pair of trials) are shown in Table [6.D3.4]. There are no significant differences between age groups. The difference between

Velocity conditions is significant for 6-year-olds in their first pair of trials ($q = 5.71$, $p < 0.01$) and for 9-year-olds in their second pair of trials ($q = 3.16$, $p < 0.05$). The difference between pairs of trials is significant for 6-year-olds in the Equal condition ($q = 3.87$, $p < 0.01$) and in the Fast condition ($q = 3.11$, $p < 0.05$).

These differences are small and have no obvious theoretical implications, given that effects of Velocity are not readily interpretable except in interaction with Modality.

In Experiment A3, 9-year-olds were found to undershoot to a significantly greater degree than 6-year-olds. This result was not replicated in Experiment A4, or in the Equal condition of the present experiment, which is comparable to Experiment A3. This lack of replication would suggest that the effect in Experiment A3 is of little general importance.

Table [6.D3.4] Mean Constant Errors (in degrees)
For the Age x Velocity x Pair of Trials Interaction

VELOCITY:		Equal		Fast	
PAIR OF TRIALS:		1	2	1	2
AGE:	6	0.23	-1.27	-2.56	-1.35
	9	-0.86	-0.06	-1.19	-1.60

[6.D3.3b] **Within-subject Variability**

Within-subject deviation was subjected to logarithm transform and analysed by 2 x 4 x 2 x 2 ANOVA, Age x Modality x Velocity x Standard Position. Only one effect was significant, the Age x Modality interaction [$F(3,120) = 2.79$, $p < 0.05$]. The mean errors for this interaction are shown in Table [6.D3.5].

Newman-Keuls post-hoc tests were used to compare conditions. No difference between age groups was significant, neither was any

difference between modality conditions, within each age-group. The interaction is therefore due to differences between combinations of conditions, and is not readily interpretable because there is no a priori basis for combining groups. No comparable effect was found in comparable experiments (A3, A4, D1 and D2). There was certainly no overall cross-modal deficit. The trend was for errors to be highest for 6-year-olds when the standard was presented visually, and for 9-year-olds when the standard was presented kinaesthetically.

Table [6.D3.5] Mean Within-subject Deviation
(logarithm transformed scores)
For the Age x Modality Interaction

MODALITY:	VV	VK	KV	KK
AGE: 6	1.52	1.65	1.45	1.29
9	1.30	1.38	1.59	1.54

[6.D3.3c] Between-subject Variability

As in Experiment D1, subjects could be paired on all factors except the order in which standard positions were presented. For each subject, therefore, the mean for the two standards was used in the calculation of between-pair difference. This measure was then subjected to logarithm transform and analysed by 2 x 4 x 2 ANOVA, Age x Modality x Velocity. There were no significant effects.

The absence of any effect of modality, or velocity x modality interaction again supports the hypothesis that the same information (spatial information) is used as the basis of matching in each modality condition, the effect of velocity/time being simply to bias estimates of distance.

Table [6.D3.6] Mean Constant Errors
(in degrees)

VELOCITY:		'EQUAL'				'FAST'			
STANDARD:		20 deg.		40 deg.		20 deg.		40 deg.	
PAIR OF TRIALS:		1	2	1	2	1	2	1	2
AGE	MODALITY								
6	VV	-0.47	-0.78	-0.94	-2.97	-0.78	1.25	0.16	-0.16
	VK	1.09	-1.41	-2.97	-5.16	-3.44	0.16	-7.50	-5.47
	KV	4.69	3.59	-1.88	-2.34	2.50	4.38	-3.44	-4.69
	KK	2.03	-0.31	0.33	-0.78	-2.03	-0.94	-5.94	-5.31
9	VV	-2.19	0.47	-0.94	-0.31	-0.63	1.56	-0.16	0.00
	VK	-4.38	-3.28	-5.63	-5.16	-2.19	-4.22	-1.88	-4.22
	KV	6.25	6.56	1.72	2.18	2.97	1.56	-3.13	-2.03
	KK	0.16	0.78	-1.88	-1.72	-0.63	-0.94	-3.91	-3.13

Table [6.D3.7] Mean Within-subject Deviation
(logarithm transformed scores)

VELOCITY:		'EQUAL'		'FAST'	
STANDARD:		20 deg.	40 deg.	20 deg.	40 deg.
AGE	MODALITY				
6	VV	1.48	1.75	1.50	1.34
	VK	1.58	1.60	1.55	1.89
	KV	1.54	1.47	1.40	1.40
	KK	1.40	1.08	1.35	1.34
9	VV	1.24	1.26	1.18	1.53
	VK	1.28	1.30	1.44	1.49
	KV	1.61	1.69	1.44	1.61
	KK	1.55	1.66	1.49	1.46

Table [6.D3.8] Mean Between-pair Difference
(logarithm transformed scores)

		VELOCITY	
AGE	MODALITY	'EQUAL'	'FAST'
6	VV	1.55	1.71
	VK	1.94	1.69
	KV	1.88	1.56
	KK	1.66	1.56
9	VV	1.13	1.44
	VK	1.60	1.54
	KV	1.93	1.91
	KK	1.49	1.36

[6.D3.4] DISCUSSION

The two main predictions were supported. First, the fast acquisition movement resulted in undershooting in KV and KK, but not in VV or VK. This supports the hypothesis that kinaesthetic position judgements depend on the velocity of the movement by which positions are approached, whereas visual judgements do not.

One implication of this finding is that the velocity of movements must be carefully controlled when presentation is kinaesthetic. If there is a difference in velocity between acquisition and response movements, a bias may result. If the difference in velocity varies from trial to trial or from subject to subject, then variability may also be increased. This is particularly relevant to experiments in which the subject controls the velocity of the movement, since there is evidence that the velocity of movement selected differs between the acquisition and the response movement (Hall and Wilberg 1978).

Second, the fast acquisition movement had no specific effect on the cross-modal conditions. This result supports the hypothesis that the effect of velocity on kinaesthetic judgements is to bias judgements of movement extent, and is in opposition to the hypothesis that kinaesthetic position judgements are actually made on the basis of the velocity and duration of the movement.

The above conclusions must be qualified as a consequence of the significant Age x Modality x Velocity interaction. This interaction meant that the difference between velocity conditions was significant only for 6-year-olds in the KK condition and for 9-year-olds in the KV condition.

No equivalent significant effect was found in Experiments D1 and D2. In D1, in the KK condition, the difference between velocity

conditions was if anything higher for 9-year-olds. However, the difference between modality conditions (VV vs KK) in the Fast condition was non-significantly greater for 6-year-olds. The trend was therefore for the biasing effect of a slow acquisition movement to be greater for 9-year-olds, and for the biasing effect of a fast acquisition movement to be greater for 6-year-olds. To this extent, the results of Experiment D1 are compatible with the results of the present experiment.

Regarding Experiment D2, the effect of velocity on KK can be assessed, as in Experiment D1, by calculating the difference between VV and KK. The results of previous experiments indicate that these conditions should not differ unless KK is biased by the velocity of the acquisition movement. KV can also be compared with the presumed stable reference of VV. Without an effect of velocity, the mean constant error in KV should be positive relative to VV. The more negative KV is in relation to VV, the greater the implied effect of velocity.

On these measures, the effect of velocity both on KK and on KV was greater for 6-year-olds with the 20 degree standard, but greater for 9-year-olds with the 40 degree standard. In both age groups, the effect of velocity was greater for the 40 degree standard. As with Experiment D1, these interactions were non-significant, and serve only to give some indication of the extent to which the results are compatible with the results of the present experiment. In the case of D2, the results show no indication of any interaction of age and modality with velocity.

There is, then, an unpredicted interaction effect, which is not replicated in Experiments D1 and D2. The interaction clearly cannot be explained by a straightforward effect of age on susceptibility to the biasing effects of velocity. More likely there

is a change in strategy with age, which decreases susceptibility to bias in KK, but increases susceptibility in KV. There is evidence in Experiment D1 that subjects can adopt a strategy which enables them to eliminate bias due to velocity, in the KK condition. It is not clear, however, what the relevant strategy is. Two lines can be followed in attempting to determine the strategy might be.

First, in Section [6.D1.1], two possible strategies for dealing with the change in velocity were noted. The subject could attempt to ignore the velocity/duration of the movement, or attempt to compensate for the change in velocity by allowing slower movements to continue for a longer period. It was pointed out that the compensation strategy was more likely in the Slow condition of that experiment. In the Slow condition that there was a stronger effect of velocity on KK than in the Fast condition, and a tendency for the effect to be greater for 9-year-olds. It may be, therefore, that the effect of velocity is dependent on the adoption of a compensation strategy.

More complex models could be built up along these lines. For example, it may be that older children are more likely to adopt a compensation strategy, which is effective in the KK condition but not in the KV condition; younger children's strategy of trying to ignore velocity/duration may be more successful for KV than for KK. A further series of experiments would be necessary in order to further examine these possibilities.

Second, in the introduction to this chapter, a brief discussion was given of the tau effect, the tendency for a spatial interval defined by successive stimuli to appear larger, the greater the temporal separation of the stimuli. It was noted that Jones and Huang (1982) had explained this effect by an hypothesis stating that

the subject imputes a velocity of movement of the stimuli defining the interval, assumes the same velocity for successive presentations (or a velocity more constant than would actually be required), and therefore confounds temporal with spatial intervals.

It was also noted that the tau effect does not occur when the subject actually perceives illusory movement. Presumably when a velocity is directly perceived, there is less scope for erroneous imputation of velocity. The evidence on illusory movements was supported for visual presentation by the experiments using real movements, reported here. Overall, with kinaesthetic presentation, there was an effect of velocity even with real movements.

This raises the possibility that two effects are involved: a tau effect proper (i.e. an effect of temporal on spatial intervals), and an effect of velocity. The tau effect would be expected if, when subjects attempt to ignore the change in velocity and concentrate on spatial parameters, attention is distracted from the actual velocity. An effect of velocity as such would be expected if a compensation strategy were adopted.

The prediction which was made in the introduction to this chapter was that the interdependence of space and time perception should be greater for kinaesthesis than for vision. This prediction has been borne out. The difference is not absolute, however. It has been found that the effect of velocity on kinaesthetic position matching depends on strategies which are as yet incompletely specified. As with the effect of standard position on constant errors in visual matching (Chapter Five), there may be important effects of subtle differences in the understanding of instructions.

In conclusion, there is evidence for different encoding strategies in visual and kinaesthetic perception of movements, and

the difference in strategies was predicted from an examination of the stimuli which are normally available to vision and to kinaesthesis (Section [6.D]). This is evidence that the importance of the information available to the subject is not limited to the information available during the experiment. Consideration must also be given to the information normally available through each modality, as a determinant of encoding strategy.

CHAPTER SEVEN: DISCUSSION

[7.1] INTRODUCTION

The central question which was posed in the introduction to this thesis was in what ways (if any) does cross-modal matching differ from within-modal matching? Of particular interest was visual-kinaesthetic matching, since with this pair of modalities performance had frequently been shown to be poorer in cross-modal than in within-modal conditions. This presented a puzzle because cross-modal matching had sometimes been shown to be no worse than within-modal matching with other pairs of modalities, notably visual-haptic shape matching. Even infants had been shown to be capable of cross-modal matching.

Therefore the present studies were carried out as an intensive investigation into children's visual-kinaesthetic matching performance. The major concern was to understand the conditions that lead to differences between within- and cross-modal matching. An account of these conditions was sought by examining the following three broad classes of possible difference.

(a) Memory and processing depend upon the modality through which information is presented, so that cross-modal matching requires a "translation" between stores.

(b) The information provided by a stimulus object or event depends on the modality by which it is perceived, so that cross-modal matching will often require a "transformation" of information in order for inputs from different modalities to be compatible.

(c) Subjects adopt different encoding strategies with different presentation modalities. Hence different information may be encoded, whether or not the same information is provided through each

modality. Consequently, transformation of information or a change of strategy is required in cross-modal matching.

These classes are to be understood as compatible possibilities, rather than mutually exclusive hypotheses. The hypothesis which was tested in the foregoing chapters was that matching performance depends on a particular combination of classes: the information available and the strategy adopted.

Throughout the experiments, methodology was a key issue, since it was considered likely that in the past, methodological problems have meant that results have been open to more than one interpretation. Consequently, theoretical statements had been made on the basis of ambiguous findings. Visual-kinaesthetic matching is the case in which the ambiguity was considered to be greatest, in addition to being the case for which evidence for specific cross-modal difficulties appeared to be strongest. Of first importance in the methodology was control of the information available through each modality and detection of the strategies employed.

It was also stressed that performance has to be understood in terms of two measures: not only variability (efficiency) but also bias (the tendency to overshoot or undershoot). Performance in different modalities may show different levels (magnitude and direction) of bias but the same level (magnitude only) of variability, or vice versa, and the interpretation of the two measures may be quite different. Variability is the more important measure for assessing cross-modal deficits and developmental changes, since it reflects efficiency of performance.

The experimental results reported in the previous chapters show that processing information across the visual and kinaesthetic modalities is not necessarily identical with processing information

within modalities. At the same time the experiments provide clear evidence that apparent differences are not a "deficit" that can be ascribed to a need for a translation mechanism that mediates between modalities. Rather, differences in performance among modality conditions are due to differences in the information available to the subject and to differences between modality conditions in the strategies adopted by the subject. In addition to supporting this general hypothesis, the present studies provide detailed evidence on the conditions under which differences in processing occur.

These findings are discussed in greater detail below, first with reference to efficiency (variability) and second in terms of bias (constant errors). This is done in the following two sections. A model is then offered which provides a framework for the description of the processes involved in cross-modal matching. Suggestions for further work are then made.

[7.2] EFFECTS ON VARIABILITY: MODALITY AND AGE

[7.2.1] MODALITY

The fact that visual information is generally far more effective in terms of efficiency than kinaesthetic information is well attested (Section [1.3]). The question is whether this is a necessary concomitant of the modality or can be shown to depend on the type of information that is available to subjects and/or to strategy adopted by the subjects.

The results, replicated in five experiments, showed that it was perfectly possible to equate efficiency in the two modalities. This was achieved by making the same information available to vision and kinaesthesis. This had not previously been established for visual-kinaesthetic matching, and provided a base line from which to assess cross-modal performance.

When information was equated in the two modalities, there was no difference in performance efficiency between within- and cross-modal matching. In other words, in the absence of differences in available information, there were no differences in performance efficiency between modality conditions. This also had not previously been established for visual-kinaesthetic matching, and provided a second base line, from which to assess the effects of varying information.

Differences between modalities could be experimentally altered by enhancing visual information in two ways: (a) the addition of visual background cues, and (b) emphasis of the body midline as a reference point by starting all movements from this position. With visual presentation enhanced in this way, variability was lower for VV than for KK. In addition, the enhancement of visual presentation made visual differ from kinaesthetic presentation, and a cross-modal deficit was found. This deficit was observed only in the first of the two experimental sessions given to each subject. In the second session, all modality effects were eliminated, apparently as a result of a shift from dependence on midline-relative coding in the cross-modal conditions (see Section [3.A5.3]). Practice and encoding strategy were therefore important, and differences in information did not have an absolute, unavoidable effect.

It can be concluded from the above results that efficiency in cross-modal conditions depends not as such on the fact that information from two modalities must be combined, but on the extent to which compatible information is encoded through the two modalities. More detailed discussion of the results is given below.

In the first pair of experiments, there was additional evidence for effects both of information and of strategy. Variability was selectively decreased in VV matching, compared with KK, when a

mask was fitted to the apparatus to exclude visual external reference cues. The interior of the apparatus was dark, but it seems that coding relative to the edges of the viewing aperture had been an obvious, though detrimental strategy in Experiment A1a. Eliminating the distracting information improved performance.

The effect of external visual cues was thus to increase variability in Experiment A1a, but to decrease variability in Experiments A2b and A5. This can be explained in terms of the reliability of the cues. In Experiment A1a, the subject's head position was not fixed, hence the alignment of the standard position with external cues was not necessarily the same during acquisition and during response.

Millar (1972) found that varying the visual presentation conditions (successive vs simultaneous presentation, with vs without visual guidelines) did not affect performance. It was suggested in Section [1.3.3] that differences were not found because extraneous visual cues were available. The effects of minimal external visual cues found in Chapter Three give good support to this interpretation, since the visual presentation was very similar to that used by Millar, and the same minimal visual cues in her experiments may have obscured any effect of specifically given visual cues.

The main conclusion is therefore that children's visual-kinaesthetic matching performance depends on the information made available to each modality, and on strategy, not on the modality or change of modality as such. Performance in within-modal conditions depends on the amount and type of information available through each modality; cross-modal performance depends additionally on the compatibility of information provided by each modality. If the information is not compatible, errors are increased in the cross-modal conditions.

This conclusion has important implications for theories about relations between sense modalities. The differences found between modality conditions do not require a "hardware" explanation in the form of modality-specific stores. Neither was there any specifically cross-modal improvement with age (this is discussed further below). This is further evidence against structural models in general (e.g Birch and Lefford 1963, 1967, Ettlinger 1967), and in particular the one proposed by Connolly and Jones (1970), since this model predicts a cross-modal deficit, cross-modal asymmetry, and visual superiority (as discussed in Sections [1.2] and [1.3]). Neither was there evidence that spatial inputs are visually processed, regardless of input modality, as proposed by Pick (1970, 1974).

A selective access model, in common with translation models, would predict that within-modal conditions should differ (Section [1.2.2]). Previous studies of children have often found VV to be superior to KK (Connolly and Jones 1970, Millar 1972, 1975 Experiments 1 and 3). In the present series of experiments, VV and KK did not differ, in either bias or variability, except where information and strategic differences were introduced. Selective access of visual inputs to spatial processing is therefore counter-indicated.

The model proposed by Marteniuk and Rodney (1979) is a modification of Pick's (1970, 1974) theory, and proposes that input information is translated to the modality most adept for the task (assumed to be vision for spatial tasks), but that this translation is "contingent on the amount of information present for the modality most adept for the task". In other words, the occurrence of translation is dependent on the amount of visual information provided.

This model was proposed in order to account for variation

between experiments in the occurrence and direction of differences in accuracy between VK and KV conditions. Because a cross-modal deficit was found, it was assumed that a translation occurs in the cross-modal conditions, whatever the level of information provided.

It has already been argued that information, strategy and bias effects provide a sufficient account of these results, including Marteniuk and Rodney's own results (Section [1.3]). Hence, the view held here is that the data upon which the model was based were misinterpreted, and further modification is indicated. It must be assumed that translation to a visual modality-specific code occurs only if sufficient visual information is provided, and that otherwise an amodal code is used.

In principle, this could be tested by equating within-modal performance by means of increasing kinaesthetic information, rather than reducing visual information. Marteniuk and Rodney should predict that there would be a cross-modal deficit under these conditions, whereas an information account would predict a cross-modal deficit only if vision and kinaesthesis provide different information.

It remains possible, of course, that a translation process does occur during cross-modal matching, but that this is error-free (within the sensitivity of the error measures used) from the age of six onwards, in the task employed. The crucial role of experimental technique in the generation of theories has been central in my arguments; it may be that some further modification of the task will provide evidence for translation. The onus is now however, I believe, on protagonists of such a view to provide evidence for it.

Verbal coding might be suggested to mediate between the two modalities, thus accounting for the absence of a difference between modality conditions. This would not account for the fact that in conditions of unequal information there are differences between

modality conditions. Furthermore, in the course of the experiments reported in Chapter Four, children were given the task of assigning numbers to lengths, either directly estimating lengths in centimetres (Experiment B4), or using the method of magnitude estimation (Experiment B3). Only the older children in the age-range studied (8 years 6 months onwards) were successful in using numbers in this way. Both age groups could perform the cross-modal matching tasks, and there was no differential developmental improvement in the cross-modal conditions. It is therefore highly unlikely that a precise verbal code was used to represent lengths in the matching experiments.

These conclusions have implications for the interpretation of past studies of children's cross-modal matching performance, supporting the analysis given in Chapter One. Modality effects found by Connolly and Jones (1970) and Millar (1972) in the same age range can be attributed to differences in information and/or strategy between conditions.

In the case of Connolly and Jones, these differences have been discussed in Section [1.3.2]. Briefly, visual presentation differed from kinaesthetic in being passive and simultaneous, rather than active and successive, and in making available external visual cues. Under these conditions, VV performance was superior to KK, which was superior to KV, with VK performance being the poorest.

In the case of Millar (1972), the differences between modality conditions are not so obvious, since precautions were taken to eliminate differences in available information. Two possible differences can, however, be noted: uncontrolled external visual cues (see discussion of external visual cues above) and strategic selection of position and length cues. Both position and length were appropriate cues in this experiment, it is therefore possible that

the cue employed by the subject, or the cue given the most weight, differed between modality conditions. Either of these factors could explain why VV performance was superior to performance in the other three conditions in this study.

An important aspect of the results reported here is that information need not be thought of as being tied inextricably to the modality through which it is perceived. How does this conclusion affect theories of amodal perception? Such theories have two components. First, there is the notion that there is amodal information: information which is in principle available through more than one modality. This is not theoretical, but an observation of reality.

Then there is the hypothesis that amodal information is amodally encoded, either by direct perception (J.J. Gibson 1966), or as logical abstractions from perception (Anderson and Bower 1973, Neisser 1976, Pylyshyn 1973, Shank and Abelson 1977). For example, E.J. Gibson (1969) wrote of the term 'amodal':

"I mean the term to suggest that there is information which is not tied to specific sensations, but is rather invariant over them...",

and that this information

"may be extracted from more than one kind of sensory experience".

The results reported here do not exclude the formulation proposed by Gibson, but this is not to say that modality is an irrelevant factor. In any practical context, modality is of great importance: under uncontrolled conditions, performance is strongly influenced by modality, through both the available information and strategy.

Thus, although theories of amodal perception do not necessarily predict equal performance in all modality conditions, or

parallel development in all modalities, such theories require considerable ad hoc elaboration in order to make predictions about performance or to specify mechanisms. The current series of experiments begins to provide the specification necessary for more precise models.

Amodal information is not necessarily encoded in the same way in more than one modality, or even encoded in the most efficient manner in each modality. In addition, some information is modality-specific. In fact, if modality-specific information is salient and irrelevant, it may constitute a distraction. For example, colour would be irrelevant to shape matching in most experiments, but important in combination with shape for identifying objects in the real world. Effects of distracting information are discussed further in the following section.

The 'best' way of encoding information depends on the demands of the task, in particular on whether cross- or within-modal matching is required. Further, the same feature can be differently scaled when perceived through different modalities. Indeed the evidence is that this is the case, as a result of different accompanying information; thus, judgements of length were biased by both resistance and velocity of movement (as discussed in Section [7.3]).

In Gibson's terms, invariant information may be directly perceived. However, it is not invariably perceived, and irrelevant information may also be encoded and used. The first-school child can vary strategy and use information flexibly. The conclusions drawn here relate to the issue of why modality differences should arise: what can be inferred about mechanisms of perception, and what can be predicted about performance.

[7.2.2] AGE

An information/strategy model does not necessarily predict specifically cross-modal improvement with age. This is because developmental changes in strategy may be geared to optimal information-gathering within a modality, rather than information-gathering which is optimal for cross-modal performance in the somewhat unusual activities demanded by experimenters.

Thus, for example, Millar (1975) found that if anything, cross-modal difficulties increase with age. This is perfectly understandable if older children gather more information, but information which is less compatible between the modalities.

It is the general expectation in child development that performance will improve with age, and this has been found in visual-kinaesthetic matching tasks (Connolly and Jones 1970, Millar 1972). In the experiments reported here, variability was, as would be expected, higher for younger than for older subjects, although this effect was not large. The most important point is that there was no evidence of a specific improvement with age in any modality condition, and crucially no specific improvement in the cross-modal conditions. Thus, the results add to the weight of evidence that there is no specifically cross-modal improvement in visual-kinaesthetic matching, between six and nine years. The evidence here is against structural accounts of development or of differences between modality conditions which require translation between modality-specific stores, since translation should improve with age.

Past studies support the hypothesis that in fact the age range over which there is no specifically cross-modal improvement in visual-kinaesthetic matching can be extended to the age of four through to adulthood, and that there is no change at all in the

pattern of modality effects from age five to adult (see Sections [1.3.3] and [3.A5.4]).

It seems therefore, that by the age of six years, whatever development might occur in specifically cross-modal processes in visual-kinaesthetic position matching abilities is complete. Beyond this age, developmental trends are accounted for by improvement in within-modal performance. This places further limits on what kind of translation model might be at all acceptable, since a model which proposes a role for learned translations should predict a specific improvement in cross-modal performance with increasing age.

Between the ages of 4 and 5, there is a specific difficulty with kinaesthetic performance, in particular with kinaesthetic response, in spite of attempts to equate visual and kinaesthetic information (Millar 1972, 1975). In the context of the approach to matching adopted here, how is this kinaesthetic difficulty to be explained?

The simplest explanation is that younger children's performance is disrupted by the relative lack of spatial cues provided by kinaesthetic presentation. The additional cross-modal asymmetry in absolute errors (KV superior to VK) found by Millar (1972) may be at least partly explained by the greater magnitude of the constant errors in VK.

The present studies of children aged 6 to 9 years have implications for both younger and for older groups. Regarding adults, the results give indirect support to the discussion of results of experiments on adults (Section [1.3.3]), in which it was concluded that there are no differences in variability between modality conditions when there is no difference in information or strategy. When differences in performance can be explained by actual differences in conditions (i.e. information and strategy), it is

superfluous to suggest alternative explanations in terms of hypothetical differences (i.e. modality-specific stores). The discussion in Section [1.3.3] involved a degree of conjecture, but the results reported here for children support the information/strategy account of results with adults insofar as it is possible to generalise from child to adult performance.

Four important modality effects were dealt with in that discussion: cross-modal deficit (superiority of within-modal performance over cross-modal), cross-modal asymmetry (superiority of one cross-modal condition over the other), within-modal asymmetry (superiority of one within-modal condition over the other), and effects of an unfilled retention interval on matching in each modality condition.

An explanation was given for each of these effects in terms of the information available to the subject, and the subject's strategies in using that information. Previous research on children's visual-kinaesthetic matching had not been conclusive in this regard. The modality effects which had been found were not consistent from study to study, and there were too few published studies to permit detailed analysis of the task differences which might account for the inconsistencies, or, more generally, the aspects of information which determine level of performance. In the experiments reported in this thesis, all four effects were absent with child subjects, when the same information was available through each modality.

The first three effects have already been discussed. The absence of an effect of an unfilled retention interval (Experiment A4) was particularly interesting in light of previous findings that adults' performance declines when a filled retention interval is introduced (Marteniuk and Rodney 1979). This was taken as evidence confirming that the information available in each condition, and

encoding strategy, were responsible for the effect of retention interval found by Marteniuk and Rodney (see Section [1.3.2] for details). Both position and length cues were available in their experiment, whereas in Experiment A4, only position cues were available. This suggests that Marteniuk and Rodney's subjects used the length cue, and supports the hypothesis (Section [1.3.2]) that the cue employed (position vs length) is more important than modality as a determinant of performance.

The conclusion remains, therefore, that cross-modal differences are due to differences in information and strategy between modalities. The principles governing performance are the same as those applicable to adults, such that the pattern of the modality effect is the same for 6-9 year olds as it is for adults. The implication of this is that any improvements with age must be due to improvement in information gathering within each modality, and to strategic improvements. The absence of a specific improvement in cross-modal conditions therefore indicates that there was no improvement, within the age range investigated here, in children's ability to deal with differences in information, since effects of age were essentially the same with or without differences in available information.

One major exception to this general rule was found, which constitutes evidence for change in strategy with age. This is the finding that the effect of unreliable external cues is an important aspect of the difference between adults and 6-year-olds (Experiment A1a). Adults differed from children only in those conditions in which children improved as a result of the mask being added, the effect of the mask being to remove external visual cues, but to facilitate self-reference. Thus, an important aspect of development may be the ability to distinguish appropriate from inappropriate cues. This

ability entails dealing with discrepancies between conclusions from different encoding strategies.

While this conclusion would be difficult to maintain on the basis of this one pair of experiments, there is other evidence that performance in separating appropriate from inappropriate cues undergoes development. For example, Smith et al (1975) found that children under six years are more likely than older children to attend to all cues present, not just those specified to be relevant.

This does not constitute a trend towards obligatory focussing with increasing age, but rather a trend towards greater flexibility, such that accompanying information is used if it is a reliable predictor of the specified cue (Chapman 1981). In Chapman's experiments, the relevant information was specified by the experimenter. In my experiments, child had the additional task of deciding what information was appropriate. This is an aspect of the effect of distracting cues which would be worthy of further investigation.

Sex effects in Experiments A1a and A1b could also be explained in terms of distraction. There was evidence that females were more easily distracted than males by extraneous visual stimuli. Differences between the sexes disappeared when distracting cues were eliminated. It should not immediately be concluded that a general sex effect has been uncovered here. The male and female samples were matched on chronological age and school, but (in accordance with common practice in research of this kind) were not sufficiently precisely matched to allow definite conclusions about sex differences. Also, the observed effects may be specific to the task and testing conditions employed in these experiments. Nevertheless, there may be a case for further investigation of this effect.

Turning to younger age groups, one implication of the present

studies for developmental studies is that the search for any age change in the perceptual processes underlying cross-modal performance should now be conducted using subjects aged up to six years. There must be a limit to how young the subjects could be while retaining the task used in the experiments reported here, even if the limit is finally set by linguistic competence. New tasks would therefore be necessary. At any age level, care must be taken to ensure that compatible information is encoded from each modality, before conclusions can be drawn about intersensory coordination.

This imperative has implications which depend on the age-range to be considered. The research reported in the preceding chapters was concerned with performance in cross-modal conditions, relative to performance in within-modal conditions. Research of this kind has been carried out with children as young as four years for visual-kinaesthetic length matching (Millar 1972) and three years for visual-haptic shape matching (Millar 1972a, Rose et al 1972). In each case, there is no convincing evidence for specifically cross-modal difficulties.

Where younger children and infants have been studied, different questions have been asked. The debate has not been about whether within-modal performance is better than cross-modal, but about whether there is cross-modal performance at all. Unless this difference in the aims of research is realised, there is a danger of concluding that equal cross- and within-modal matching should be found in childhood, on the grounds that cross-modal matching can be demonstrated in infancy. Such a conclusion would not be valid, hence work on children does answer a question not tackled by work on infants.

There is evidence that there are at least some cross-modal capacities at birth, and certainly that salient features of objects

and events can be matched cross-modally within the first year (e.g. Allen et al 1977, Bryant et al 1972, Gibson and Spelke 1983, Gottfried et al 1977, Meltzoff and Borton 1979). These findings are clearly compatible with the finding that there is no cross-modal deficit in children, but opposed to the hypothesis that the senses are originally separate (e.g. Piaget 1952).

Lewkowitz (1985) has cast doubt on studies which have found evidence of visual-auditory temporal matching in infants up to 4 months of age. In three studies, he reports that there was not proof of matching on the basis of temporal frequency. He argues that the findings of apparent matching on the basis of temporal patterning can be explained on the basis of intersensory scaling of intensity or "total amount of stimulation".

This conclusion may be criticised on two grounds, as follows. (1) There was some evidence of matching in two of the three frequency conditions of Study 1. (2) Lewkowitz did not find matching in conditions in which apparent matching due to intersensory scaling was possible (Study 2). If this is due to some insensitivity in the method, the same insensitivity may account for the weak evidence for true matching. Thus, although his experiments provide evidence for intersensory intensity effects, they do not show that matching does not occur. It seems likely that the method employed was in some way not optimal for the demonstration of matching. For example, the flashing checkerboards may have been sufficiently aversive combined with a modulating tone to suppress exploratory behaviour.

Demonstrations that cross-modal capacities are present in infancy address the question of whether cross-modal capacities are innate. However, it is also necessary to examine the processes which underlie cross-modal performance. Comparison of within- and cross-modal performance introduces instructive control conditions in the

context of an information/strategy account of cross-modal abilities. That is, the conclusion that information and strategy are the important factors in cross-modal performance can be applied to the findings from infant research, and the question to be asked becomes under what conditions do infants exhibit cross-modal abilities.

In cases where intersensory coordination is not demonstrated, it is essential to demonstrate that the equivalent within-modal task can in fact be performed. This is related to the previously discussed point, made by Bryant (1968), about comparing children's relative performance in cross- and within-modal matching tasks. Another important reason for analysing within-modal performance is to identify the encoding strategies employed with each modality: there may be a mismatch of information between modalities.

Infant visual-auditory spatial coordination, although not strictly identifiable as matching can be used to illustrate these points. Length and position matching have not been used with infants, and in fact these are not realistic tasks, given the very limited voluntary motor control exhibited by infants. The most similar task which has been used is visually elicited reaching. Visual-auditory coordination of spatial location is a case for which a sufficiently clear pattern is emerging to merit more detailed discussion.

One paradigm which has been used to examine spatial visual-auditory cross-modal abilities is to test whether infants will turn their eyes towards an unseen source of sound. It appears that infants will do this when the sound has moderate intensity, but that eye movements contralateral to the position of the source of the sound may result from louder sounds (Butterworth and Castillo 1976, Hammer and Turkewitz 1975, Mendelson and Haith 1976, Turkewitz et al 1966, Wertheimer 1961). Also, occular orientation may depend on the presence of some visual target, such that if one is present,

orientation is towards it regardless of the position of the sound source (Castillo and Butterworth 1981).

Failures to demonstrate these phenomena (e.g. McGurk et al 1977) must be assessed relative to within-modal performance, i.e. the incidence (in the same laboratory conditions) of turning the eyes towards a visual target, and turning the ears (i.e. the entire head) towards a sound.

For example, neonates orient their eyes to visual targets having greater eccentricity if there is not a central fixation target (Harris and MacFarlane 1974). Hence, failure of neonates to orient to auditory targets outside the visual field cannot be taken to demonstrate failure of integration. This is especially relevant if there is a central fixation target, and this is the usual experimental procedure.

Even with infants aged 1 month, Aslin and Salapatek (1975) observed saccadic eye movements only for visual targets of up to 30 degrees' eccentricity. Occular response to auditory stimuli has been observed with stimuli of greater eccentricity - up to 90 degrees from the midline. This suggests that response to visual stimuli may be limited principally by the salience of the stimulus.

The type of sound may also be important. An uninteresting sound (e.g. a pure tone) may not attract attention sufficiently well, or may specify an uninteresting sight. Thus, complex sounds may be more effective in eliciting occular orientation (Alegria and Noirot 1978, Crassini and Broerse 1980, Field et al 1979, 1980, Mendelson and Haith 1976, Muir and Field 1978).

In addition to the information provided by the stimulus, encoding strategy must be considered. For example, the problem may not be relating a visual to an auditory stimulus, but relating a stimulus located by (auditory) self-reference to a stimulus located

by (visual and more reliable) reference to external reference points. Evidence for such a mismatch is provided by the finding of conflict (noted above), whereby a (visual) stable external target influences occular orientation following an auditory stimulus (Castillo and Butterworth 1981).

Conflict between external and self-referent location coding may also be relevant to difficulties in demonstrating visually elicited reaching by neonates. Research on visual-kinaesthetic matching has not been possible with infants, but research on reaching for visually perceived objects involves some problems which are similar. It is not considered here that reaching is a cross-modal matching problem, though it is relevant to cross-modal performance and to spatial abilities which are discussed further in Section [7.3.2].

Extensive evidence of reaching for seen objects was found by Bower and co-workers (see Bower 1979 for summary). Unfortunately, others have not in general been able to replicate this research (see Bushnell 1981, von Hofsten 1982). More recently, von Hofsten and co-workers have found evidence of quite sophisticated reaching behaviour by infants. It seems that newborn infants' arm movements are oriented towards seen objects, but the 'reaching' is imprecise (von Hofsten 1982). In addition, older infants anticipate the trajectory of a moving object, and reach for it, almost as soon as they can reach for a stationary object (von Hofsten 1979, 1980, von Hofsten and Lindhagen 1979).

Others have observed components of visually elicited and/or guided reaching by the age of three months (Bruner and Koslowski 1972, McDonnell 1979, Provine and Westerman 1979). Since there is evidence that neonates do have some capacity for reaching for seen objects, the right question to ask may be why they sometimes do not

reach.

Visual-manual spatial coordination may be present, but this may not be obvious to the observer if it is normally adaptive for the infant to use different cues with each modality. For example, if the infant lacks the motor coordination accurately and purposefully to guide movements, then there may be no value in relating arm movements to external cues: a series of ballistic arm movements in several directions may be the most economic strategy for contacting an object. In other words, the problem may be one of motor control, not integration.

This would account for Provine and Westerman's (1979) finding that infants are more likely to contact objects ipsilateral to the reaching arm than objects at the midline or contralateral. There is no reason why cross-modal integration should be affected by laterality in this way, whereas more distant objects would simply be more difficult to reach. Similarly, Bruner and Koslowski's (1972) observation that the form of reaching varies with the size of the seen object supports a motor control account. The ballistic nature of children's reaching is evidenced by the finding that shifting the visual field by means of a prism disrupts reaching (Bower 1977, p76): non-ballistic reaching should be modifiable by the sight of the hand missing the object. Failures to observe reaching at all may be explained by distraction: the least distraction may suppress reaching if reaching requires a high level of attention.

Similarly, if there are normally stable external reference points in the immediate environment (e.g. the edge of the cot or pram) there may be no advantage to visual self-reference in familiar settings. If arm movements are coded by self-referent and/or movement information, and visual inputs are coded by reference to stable external cues, there is a mismatch of information, not necessarily of

modalities. These points will be discussed further when effects on bias are considered.

There may of course be reasons other than mismatch of information why infants do not exhibit reaching of which they are capable. For example, it may be adaptive for infants to reach only when they are in an environment which they perceive to be safe. As the ability to discern such conditions improves with age, so should the incidence of reaching for objects. If this is the case, then conditions in the experimental room should be the crucial factor in determining whether or not infants reach.

If the failure of neonates to show intersensory coordination raises problems of interpretation, so does their success. Taking again the example of visual-auditory location, what does it mean when a neonate exhibits eye movements towards the source of a sound? Such behaviour might result from the expectation of visual consequences, but cannot initially be taken as evidence for anything more than a visual-auditory reflex which facilitates learning of cross-modal equivalence. Mendelson and Haith (1976) present evidence that the delay and time-course of the occular response is longer than would normally be associated with a reflex, but further evidence on the nature of the response is needed.

Direct comparison with within-modal performance would clarify the above result in two ways. First, if there is reflex facilitation of learning, then presumably the accuracy of occular orientation to sound should improve with age, relative to orientation to a visual target. Second, if cross-modal performance is as accurate or consistent as within-modal performance as soon as it can be demonstrated, this would imply an innate capacity for cross-modal performance, not observable at younger age levels for reasons of strategy or response limitations.

In general, once cross-modal performance can reliably be demonstrated at a given age level, it should then be compared with within-modal performance in both the contributing modalities. This would enable identification of any specifically cross-modal sources of error, which might be indicative of a noisy translation process, or of scaling differences between modalities. Where matching of position, length or duration is involved, translation should be indexed by variable errors, and scaling differences by constant errors.

There is a problem, however, that the paradigms commonly used in much of the neonate research (e.g. visually elicited reaching, auditorily elicited eye movements, imitation, habituation) do not readily lend themselves to within-modal analogies in which the same information is specified by each modality. In each case, if a cross-modal deficit were found, information and strategy differences (including interfering irrelevant information) could be held responsible.

In conclusion then, it is not clear what development might occur in cross-modal abilities between the neonatal period and the age of four years, but there is no evidence that any process need be assumed other than the process of learning to encode information which is compatible with task requirements (i.e. information which is compatible between the sense modalities involved), and to ignore irrelevant information. Intermodal facilitation may be important here, in the sense that searching for and encoding information in one modality may be influenced by knowledge and strategies acquired via other modalities.

Thus, development can be seen in terms of strategy, which can be related to what information is important to the infant. This entails a model of development based neither on integration of

initially separate sense modalities, nor on a literally unitary sensory system (see also Millar 1981). Rather, there is intersensory development, but this is only one aspect of learning to gather and use information efficiently.

There is evidence in the literature about certain aspects of information and strategy which are important determinants of matching performance, as reviewed in Section [1.3]. The experiments reported here add to this body of knowledge in several ways, as detailed above. It is of some importance that the effects were detected relative to an established baseline of no difference in variability between modality conditions. This permits greater certainty in conclusions regarding the effects of stimulus conditions (information) and strategy.

[7.2.3] OTHER PAIRS OF MODALITIES: HOW GENERAL ARE THE CONCLUSIONS?

To what extent are the findings for visual-kinaesthetic matching reproduced with other pairs of modalities? Shape matching was discussed in Section [1.4], where it was noted that there is no clear evidence for a cross-modal deficit with visual-haptic matching at any age level. Since it is now clear that this is also the case for visual-kinaesthetic matching, there is no case for advancing theories of why there is a cross-modal deficit only with visual-kinaesthetic matching (Abravanel 1981, Bryant 1974, Freides 1974, 1975).

The problem is instead one of determining why, when visual matching of shape is superior to haptic, there is no cross-modal deficit. The answer given to this in Section [1.4] was that, although more information is acquired visually, the information is of the same type as that acquired haptically, hence there is no mismatch of information. With this as a starting point, further observations can

be made.

First, why is the same type of information encoded about shape, but not about length/position? This question can be answered in terms of information and possible strategy; i.e. these aspects of tasks can explain not only results for a particular pair of modalities, but also differences in results with different pairs of modalities. Gibson (1969) has proposed that with multidimensional stimuli there are more cues on which to base matching, and a mismatch of cues between modalities may therefore be seen to be less likely. In fact, the number of cues cannot in itself be used to predict the likelihood of mismatch. Other factors must be taken into account.

Shape information is more complex in the sense that it contains more information, but can be argued to be simpler to match, since it requires only:

(a) relative judgements rather than absolute in the sense of requiring reference to an internal measuring system (Bryant 1974, Gibson 1969), and

(b) nominal/ordinal rather than interval/ratio measurement (ibid p70).

Hence, specific encoding strategies may be more obvious for shape, and better established in children.

Second, apart from accounting for the difference in the general pattern of results between length and shape matching, the above explanation can be extended to cover some of the deviations from the general pattern found in shape matching. This is important, since in some cases the results could be interpreted as indicating a cross-modal deficit. The following account is developed from an analysis by Jones (1981).

Jones found that much of the variation in the pattern of

results of shape matching experiments could be explained by reference to whether presentation of standard and comparison shapes was simultaneous or successive. He argued that visual presentation would allow more information to be retained, to serve as guidance for exploration of comparison items. Hence, when comparison and standard shapes cannot be simultaneously examined, VH performance should be superior to HV. With simultaneous presentation, this advantage will not accrue to VH. On the other hand, since the HV condition involves fewer haptic examinations of an object than VH, HV might have the advantage.

Certainly Jones' analysis is able to account for some of the variation in results between studies. However, even bringing in other factors which he considers to be relevant (age, 'nonsense' vs 'geometric' forms, method of occluding shapes for haptic presentation), one common result is not accounted for: VV is often superior to the other conditions, with no other significant differences. According to Jones' analysis, a cross-modal asymmetry should always occur when VV is superior to HH, and should not simply disappear "As tactual perception becomes more efficient over time" (p118, my emphasis).

Jones also predicts that HH should result in poorest performance, and so long as HH is not inferior to both cross-modal conditions, there is the possibility (though not strong evidence) that there is a cross-modal deficit.

However, two explanations can be offered for the above results. First, it may be that haptic presentation does provide some information which is selectively beneficial to haptic comparisons. Second, if all features encoded haptically are also encoded visually, but additional features are encoded visually, then HH, VH and HV matching may all be based on the same set of features.

This second hypothesis can be extended to account for the results of studies which have involved greater memory demands (e.g. Abravanel 1972, Millar 1972a, Milner and Bryant 1968, Rose et al 1972). In these studies, as reviewed by Jones (1981), one cross-modal condition commonly results in poorer performance than HH. Millar (1972a) found both cross-modal conditions to be worse than HH in one condition (9 second interval, digit distractor task, age 5,4 - 6,8).

Suppose that fewer features of a shape are encoded haptically than visually, and that the features selected are more consistent across presentations (within modality conditions) than between modalities. With delay, fewer features may be recalled (this notion is derived from G.V.Jones' (1975) fragmentation hypothesis). Then, with delay, the overlap between visually and haptically encoded features would become less. Hence, a cross-modal deficit could arise from selective encoding followed by fragmentation. An analogous effect would not be expected for length matching, simply because there is only one feature to be recalled in this case.

Experiments are required, in which the number, type and prominence of features are examined, in order to resolve difficulties in interpreting results of visual-haptic shape matching experiments.

Cross-modal (visual-tactual) judgements of texture have been found to be more variable than within-modal (Björkman 1967). This is not clear evidence, however, for any specifically cross-modal difficulty: the result can be explained in terms of incompatibility of information. Tactual texture information is normally dynamic, depending on the rate of movement of the skin over the surface, and hence on vibrations caused in the skin by the surface. If tactual perception is made static, the primary information must be the distance between the centres of the grains in the surface. Visual texture perception is essentially static, depending on particle size

and separation. It would be predicted, therefore, that there should be a reduced cross-modal deficit in variability with static tactual perception, although considerably scaling differences might arise.

The other pair of modalities for which cross- vs within-modal comparisons have been made is vision and audition. Some experiments have been subject to the problem that the task described as visual-auditory matching can equally well be thought of as matching from a spatial to a temporal code (see Goodnow 1971a). This is a more explicit difference than in the case of velocity affecting kinaesthetic coding (ibid, Chapter Six), since auditory-visual differences have been part of the task conditions, rather than effects of strategy.

Where there is not this problem, there is no cross-modal deficit or specifically cross-modal improvement with age (Jones 1974, Jones and Alexander 1974, Rudel and Teuber 1971). Even in infancy, visual and auditory temporal patterns can be related (Allen et al 1977, Gibson and Spelke 1983).

The task used by Jones and Alexander (1974) involved matching a temporal series of four stimuli which varied in spatial position (right vs left of the subject). Alexander (1977) used the same task, but with faster presentation rate. Between 6,6 and 9,0 years of age, there was a selective improvement in the cross-modal conditions. Given the failure of other experiments to show any such effect (including another in the same paper, replicating Jones and Alexander 1974), an explanation should be sought, based not on translation, but on task conditions.

Jones (1981) suggested one such explanation: that the younger children in Alexander's study based their responses solely on temporal interval structure, using newly-learned spatial-temporal correspondence rules, the intervals being psychophysical rather than

actual. Presumably this means that stimuli would differ in apparent duration according to presentation position. The scaling of visual and auditory spatial intervals would not necessarily be identical, thus explaining the cross-modal deficit.

It may be questioned, whether apparent and presumably small differences between intervals would be more salient than spatial position. An alternative account is that the faster presentation rate caused younger children to change visual encoding strategy. Two strategies can be distinguished. First, the complete series could be encoded (e.g. LRRL), in other words a code in which the primary parameter is time. Second, a primarily spatial code could be used, of the form "R occurred second and third in the sequence". The first code is less economical, but possibly more obvious, and consequently used until presentation is sufficiently rapid to upset a primarily temporal code in the visual condition. In this way, a mismatch could arise between visual and auditory codes.

The important point is that Alexander's results do not demand a translation explanation, and that other results militate against such an explanation.

In conclusion, a model based on information and strategy would seem to be appropriate for matching between all pairs of modalities. The specification of the details for each pair is not perfect, but general trends have been seen. The analysis of constant errors provided further evidence for this point of view, and further elaboration of the specific aspects of information and strategy which are important. These results are discussed next.

[7.3] EFFECTS ON BIAS: RESISTANCE, THE MIDLINE, EXTERNAL CUES AND VELOCITY

[7.3.1] MODALITY AND RESISTANCE

The constant error results do not index performance efficiency. Instead they provide important information about cues and mechanisms which underlie performance: they index differences in strategy between modalities and possible changes in strategy with age and experimental condition.

The major finding on bias was that there was an effect of modality on constant errors. This effect was found in experiments in which modality conditions were equated for variability (efficiency) and also when modality conditions were not equated for variability. Although VV and KK did not differ in variability or bias, there was greater undershooting in VK and greater overshooting in KV. The tendency for VK errors to be undershoots relative to KV errors was also found in published studies, as discussed in Section [B.4].

This difference between VK and KV matching does not relate to deficits in performance. It does however indicate that a cross-modal deficit or asymmetry might erroneously be presumed to have been detected if absolute errors are analysed, since constant errors are a component of absolute errors. Hence, again there is evidence that absolute error results cannot be taken as definitive evidence for effects on performance efficiency. This gives further support to the greater weight given to variable errors in the discussion of adult performance in Section [1.3.2].

Neither should the cross-modal asymmetry be taken to indicate a translation process. The findings were explained by a difference in information which was not immediately obvious, without the need to propose any extra processes in cross-modal conditions. This difference in information between vision and kinaesthesis was

resistance to motion when presentation is kinaesthetic. It was suggested that this resistance causes kinaesthetically judged lengths to appear longer than visually judged lengths. Other possible explanations were considered, but the resistance explanation was considered to be a sufficient, simple explanation of all the findings.

The effect of resistance has an analogy in temporal judgements: filled intervals are judged to be longer than unfilled intervals (Thomas and Brown 1974, Long and Beaton 1980). Also interesting are the findings of a series of experiments reviewed by Goldstone and Lhamon (1972). The duration of brief auditory stimuli is consistently judged to be greater than the duration of visual stimuli. This is reflected in a cross-modal asymmetry analogous to that found here for vision and kinaesthesis.

Resistance has, at least colloquially, a clear effect on the perception of both space and time. Adults speak of a 'long day', and perceive a walk uphill to be longer than a walk downhill. In children, this has been observed to extend to cases in which an objective judgement of distance is required. Piaget (1946a) observed that children aged up to 8 years judge the distance up a model mountain to be greater than the distance down. This may be argued to be mainly a linguistic confusion, but the main point remains, that resistance (or 'work') and judgements of space and time are interrelated.

Hence, in the experiments reported here it was intuitively obvious that resistance should increase perceived distance, not decrease it. The effect of resistance was observed with minimal scope for linguistic confusion, and was constant across the age range 6 to 9 years. This indicates that it is a phenomenon which is distinct from that observed by Piaget (1946a).

Past studies have focussed on variable errors and absolute errors (efficiency and accuracy) as the important indices of cross-modal performance. In the studies reported here, constant errors have been shown to be equally worthy of analysis, since they index important differences between conditions which are undetected by variable errors and confounded with variability results if absolute errors are analysed. New areas of research are therefore opened up by the use of this measure.

Constant errors were found to index important effects of external cues, distraction, coding relative to the body midline and velocity of movement. In general, constant error effects did not depend on age, certainly not in any simple manner. Hence there was no evidence for any change in the quantitative coordination of visual and kinaesthetic perception of space, beyond the age of six years.

[7.3.2] SPATIAL CODING: THE BODY MIDLINE AND EXTERNAL CUES

The cross-modal matching experiments reported here have implications for our understanding of children's spatial abilities. Spatial coding requires a system of spatial references and/or movement cues. A system of spatial references might in principle make use of self as a point of reference ('egocentric' coding - see for example Howard and Templeton 1966, Piaget and Inhelder 1948); external cues or a constructed Euclidean frame of reference which might allow more efficient use of self of external cues as reference points.

It is clear from past research that none of these factors alone can account for children's spatial performance: more than one type of code is used. This becomes clear when two codes are put in conflict. For example, there is evidence that specific difficulties in rotation tasks (e.g. Acredolo 1981, McKenzie et al 1984) or in

search tasks (e.g. Bremner 1978, Butterworth 1977, Butterworth and Jarrett 1982, Harris 1983, Rieser 1979) can be explained by reference to conflicting ('egocentric' vs 'allocentric') codes.

In other words, children do use egocentric cues, but they also use external references depending on their availability and salience. A notable example of infants' use of external cues is Presson and Ihrig's (1982) demonstration of the importance of the mother as a spatial reference point for infants solving rotation problems. Conflict between external and self-referent location coding may also account for findings on visually elicited reaching by neonates, as discussed in Section [7.2.2].

The matching experiments reported here provide further evidence on children's strategies of spatial coding, in particular the use of the body midline and external stimuli as cues for spatial reference.

Even in infancy, simple spatial behaviour such as arm movements can be seen to depend on the midline position (e.g. Bruner 1968, Provine and Westerman 1979). Butterworth (1975) found a bias to search for an object at the midline, when it was seen to be hidden elsewhere, although this was at least partly an artifact of the proximity of the hiding place to the infant. Bornstein and Krinsky (1985) demonstrated that symmetry about the midline has a positive effect on infants' pattern preferences. The importance of the midline is also demonstrated in the rotation and search tasks referred to above.

In the present series of experiments, in conditions in which the midline was a reliable reference, it was normally used as such in all modality conditions, as discussed in Section [C.5]. This was indexed primarily by constant errors, not by variability: with the total distance moved held constant, the tendency to undershoot

increased with distance of the end of the movement from the midline. This result is important, since it shows that children use the midline not only as a delineator between left and right, but also as a reference axis from which distances are measured.

More generally, it is clear that children at six years can make quantitative judgements of distance relative to a chosen reference point. This is one prerequisite for Euclidean conception of space. The other major requirement is that information from two or more reference points should be combined. This capacity is present from the age of four years (Somerville and Bryant, 1985). This evidence for early application of Euclidean concepts of space contradicts the theory of spatial development proposed by Piaget & Inhelder (1948).

In the context of the experiments reported here, the midline should not necessarily be thought of as an egocentric reference. It was made salient not only by its potential inherent in being equidistant between paired receptors, but also by being at the zenith of the movement arc and in most experiments by being the starting point of movements. When the midline was not used as a starting point for movements, the tendency to use it as a reference was reduced.

In the previous section, it was noted that one effect of making more information available visually than kinaesthetically was to cause a cross-modal deficit, but that this effect was dependent on practice. In the first session in Experiment A5, a cross-modal deficit was found (in variability), and in the cross-modal conditions only, constant errors were affected by distance from the body midline. In the second session, both these effects were absent.

The most obvious interpretation of these results is that the midline is important as a reference axis, and that in the cross-modal conditions of the experiment concerned other references were less

reliable or less obvious than the midline. Thus, even in conditions in which the midline was not made entirely reliable or salient, it was initially used. Hence, it appears that the midline was used initially (perhaps because it is a reference which is normally reliable), then, with practice, the midline was no longer used, or was used in such a way that the distance moved from the midline ceased to have an effect on performance. Thus, the midline is not an obligatory reference, but rather it is used with flexibility.

There was an important exception to the general rule that constant errors are affected by distance from the midline, when the midline is a reliable reference. In three experiments (A4, D2 and D3), this result was not found in the visual within-modal matching condition. It was shown, in Experiment C3, that this could be explained by a variation in strategy. When instructions biased the subject towards continuously updating an estimate of the end-point, during the lever movement, the midline effect occurred. When instructions biased the subject towards encoding the end-point only after the termination of the movement, the midline effect was not found.

Thus, again modality can be seen to affect performance insofar as performance depends on the information available to the subject and on strategy, which is in turn dependent on modality: updating is a less likely strategy in visual matching, since the midline can be located by direction of gaze.

No developmental changes in use of the midline were found. This has the specific implication that the midline is used as an anchor for spatial reference from early childhood, and the more general implication that encoding strategies vary little within the age range 6-9 years. This is inconsistent with Piaget and Inhelders's (1948) and White's (1965) accounts of development, which predict

fundamental changes at around 7 years.

External cues (background cues provided by the apparatus) were also used as references, as discussed in the previous section. The use of external cues did not always improve performance, however. In the first two experiments, subjects did not always correctly follow instructions to select either length or end-point of the standard movement as the cue to be reproduced. These "criterion errors" caused large undershoots when the correct response position for the inappropriate cue was reached first.

Criterion errors were more frequent in KK than in VV. It was found that this could be accounted for by the higher probability of distraction in KK, resulting from the subject being free to look up at whatever might be in the room, in contrast to being constrained to looking into the apparatus in VV: the difference between KK and VV was eliminated by requiring the subject to look into the apparatus throughout each trial in both modality conditions.

It was shown that criterion errors could be eliminated by ensuring that the correct response position is always reached first, by extending and standardising the instructions given to the subject, and by reducing distraction as described above. These procedures, combined with other procedures designed to reduce information differences between modalities already employed in Experiments Ala and Alb, had the effect that differences between VV and KK were eliminated. The conclusion was reached that the previously observed differences between VV and KK were the result of using the wrong cue, not of using the correct cue inaccurately, and that this was a result of task conditions, not modality as such.

The implications of criterion errors for the interpretation of past studies of visual-kinaesthetic matching have already been discussed (Section [3.Alb.4]). Briefly, three points can be made:

1) Findings of an interaction between position and length cues in performance (Kerr 1978, Laabs 1974, Newell et al 1979, Walsh et al 1980, 1981) must be re-examined. The interaction may result not from each subject's response being affected by both position and length, but from different subjects using a different one of the two cues.

2) The interpretation of absolute errors as a measure of accuracy becomes inappropriate when criterion errors occur (e.g. Newell et al 1979).

3) The more general point that task comprehension and task performance should be separated as factors affecting error measures.

The confusion of length with position information is not evidence that the subjects are at a primitive stage of understanding spatial concepts, since adults make the same type of error.

Thus, children's spatial performance is governed by a variety of spatial cues, but not only by spatial cues. Temporal cues are also relevant, and this is discussed in the next section.

[7.3.3] VELOCITY

Experiments in Chapter Six examined the effect of velocity on perception of distance travelled. These experiments are distinct from studies of concepts of velocity and time, as discussed below. The focus of the experiments was the influence of temporal parameters on spatial perceptual judgements.

The results showed that kinaesthetic position judgements are biased by the velocity of the presentation movement, whereas visual judgements are not so affected. This difference was predicted from examination of the cues which are normally available to vision and to kinaesthesis; the evidence supports the hypothesis that encoding strategy is geared to the normal context in which each sense modality

is used.

The velocity of the presentation movement affected only kinaesthetic estimates, but did not always affect kinaesthetic estimates: there were two important exceptions to this finding.

First, in each of the three experiments concerned the effect of velocity was weaker for the position closer to the midline, and significant for this position in only one experiment, in the first session. This was explained by the fact that the position closer to the midline should be easier to encode as a distance, ignoring the velocity of the movement, and that a biasing effect of velocity would be expected to be approximately constant per unit distance moved.

Second, in Experiment D3, there was a significant interaction between age, modality and velocity: velocity biased response in the KK condition only in the 6-year-old group, and in the KV condition only in the 9-year-old group. This interaction was not found in the previous two experiments, the most likely explanation for it was therefore thought to be some variation in strategy. A complex effect of compensation for change in velocity, and attempting to ignore velocity altogether, was proposed as an explanation.

Clearly, there are unanswered questions regarding the effect of velocity. Perhaps the most important is the question of why there was no effect of velocity on visual acquisition, whereas the tau effect has been demonstrated with visual stimuli (see Jones & Huang 1982 for review). There are two main differences between the present experiment and tau effect studies. The latter have been performed using adult subjects, and have not involved actual movement. There may be a developmental change in the influence of temporal on spatial visual judgements.

Alternatively, there may be two different effects detected by the two types of experiment, an effect of velocity and an effect of

time. Recent evidence provides some support for this notion. Bonnet (1984) has found evidence that under some conditions (longer duration and higher velocity of movement), judgements of velocity are independent of the motion: time and distance form the basis of judgements. If qualitative differences in presentation conditions can affect the manner in which velocity is judged, the same may be true of the manner in which velocity affects spatial judgements.

For the present, the most important conclusions are that the effect occurs only with kinaesthetic presentation as predicted, but that the effect also depends on factors other than modality: the position to be reproduced, practice, and age. In more general terms, it can be reiterated that where time information is given along with space information, this should be taken into account when drawing conclusions from results.

These findings should not be taken as evidence that spatial inputs are visually processed, regardless of input modality, (e.g. Pick 1970, 1974). They are best explained by encoding strategy, since the bias effect was not necessarily associated with variability differences.

It should also be noted that the effects of velocity/duration reported here do not appear to result from errors in understanding the concepts of velocity and time. The evidence for this is that:

(a) only kinaesthetic judgements were affected;

(b) position judgements were only biased, not replaced, by velocity/duration;

(c) the biasing effect did not decrease with age;

(d) the effect is found in adults, even with visual presentation, when the movement is apparent rather than real.

It can also be pointed out that Berndt and Wood (1974) found

that at 5-7 years of age, children's (visual) judgements of the duration of a movement are biased by the distance travelled, even when the children are able to make correct judgements of duration based on auditory cues. This again implies that perceptual rather than conceptual factors are involved.

Thus, the findings on velocity are not directly relevant to work on children's concepts of time and velocity (e.g. Piaget 1946, 1946a). Judgements of time and velocity were not assessed, and simultaneous comparative judgements of these parameters were not made. Thus the effect of velocity occurred without perception of overtaking or simultaneous perception of two terminal positions, two aspects of movements reported by Piaget to be important in children's judgements of velocity. In fact there was no evidence that conceptual problems were responsible for the results at all. Temporal and spatial parameters did interact to determine response, but the two were not confused: one biased the other at the level of perception.

It is possible of course, that time or velocity judgements under similar conditions might have been confounded with spatial judgements, just as length was confounded with position in earlier experiments [A1a, A1b]. Equally, the use of matching rather than verbal description (Piaget 1946a) might obviate any such problems. The relevant data are not available from the studies reported here (since time/velocity judgements were not required) or (with the exception of Berndt and Wood 1974, cited above) from past work (e.g. Fraisse 1984)

It can simply be noted, however, that the direction of the effect of velocity is the same as that found with adults. That is, faster movements result in perception of shorter distance. The alternative reasoning reported by Piaget (1946a), that by travelling faster the lever must have gone further, was not evidenced. To this

extent, therefore, there was no evidence that primitive concepts of velocity were responsible for the observed effects.

It can also be noted in passing that time, space and velocity judgements all appear to suffer from the use of inappropriate cues under at least some conditions. The selection of appropriate cues is therefore a factor in performance.

In summary then, children's spatial coding needs to be conceived in terms of the conditions under which particular cues are used. The research reported here has indicated several cues which children use interactively when making spatial judgements:

(a) Dynamic cues: velocity and/or duration of movement, resistance to movement.

(b) Static cues: end-point of movement, distance moved, body midline, external visual cues.

Millar (e.g. 1979, 1981) sets out an argument that several types of stimulus are coded flexibly by self reference, external reference and movement cues, and that failure to direct attention to external reference points or frames is a fundamental problem for the blind (although the blind can in fact make use of external references when these are available).

The theoretical and experimental distinction drawn by Millar between egocentric spatial coding and the use of movement cues is an important one, and one which is relevant to the interpretation of the experiments reported here: it is clear from these experiments that the use of movement cues is not restricted to the direction and extent of the movement, but also involves the velocity of and resistance to the movement.

The role of each of the cues listed above in position matching, and in spatial performance in general, is not indicated

precisely by the experiments reported here. It has been made clear, however, that the cues children use in making position judgements are subject to variable coding strategies. For example, the importance of the midline does not imply that children rely on egocentric cues solely or even principally: it is instead an indicator that children use cues which are obvious and reliable. Interaction of the cues listed above with each other and with other variables is to some extent explained by the present studies, as discussed on the preceding pages, although much remains for future research.

The key point I wish to make here is that children use several cues, flexibly and varying with experimental conditions. Modality of presentation is only one of these conditions. The use of cues is flexible, but not in all cases optimal. Thus, as with the conclusions drawn from effects on variability, it can be said that an important aspect of development is learning efficient strategies for the use of available information.

The finding that multiple cues are used is thus consistent with notions of conflict between cues which have been used to explain spatial perceptuo-motor difficulties in other areas of research. Taken together with the results of past studies, the results reported here do not support the view that spatial coding evolves from the use of egocentric (body-referent and movement) cues to the use of external cues. Rather, there is a developmental improvement in the ability to identify and mentally manipulate relevant and reliable cues. This is consonant with the conclusion stated in Section [7.2.2], that the ability to select relevant and reliable from irrelevant and unreliable cues is an important source of developmental changes in matching performance.

More generally, the results presented here (both variability and bias) indicate that cross-modal performance, from reaching

behaviour in infancy to position matching in childhood through to adulthood depends on the information available and the strategies adopted. This also seems to be true of other aspects of spatial performance. The next section discusses how information and strategy interact in the case of visual-kinaesthetic matching.

[7.4] INFORMATION AND STRATEGY: A MODEL

The main conclusion from the experiments is that information and strategy are the important factors in cross-modal performance; there is therefore a need to give an account of how these factors affect performance. Some relevant aspects of information and strategy have been detailed above. It is at least equally important to discuss the implications of the specific instances of information and strategy effects for a general model of cross-modal performance.

The fundamental task is to account for how particular items of information come to be encoded and used in matching tasks. The starting point is necessarily the total information available in the environment during task performance. In the hypothetical perfectly controlled experiment, this is the information provided by the experimenter. How is information selected from this total, in the process of perception by a given modality?

Sensory specialisation accounts for a large proportion of the information reduction: any one sense does not pick up information from every source. Orientation and search strategies further reduce the information available for encoding. The importance of this stage is discussed by Abravanel (1981). In my own experiments, the main indication of the importance of search strategies is in Experiments Ala and Alb. Irrelevant parameters, and irrelevant cues to relevant parameters were encoded in some conditions, apparently as a result of distraction from the task. In practice, effects of search strategies

are not always readily distinguishable from effects of encoding strategies: if a subject encodes particular aspects of information, does this result from failure to search out and perceive other aspects, or from failure to encode, having perceived them?

In the experiments reported here, the aspects of information to be used were specified in the instructions, and the internal evidence is that they were perceived. Indeed, the critical information was "given", in the sense that the subject did not need to search for it, and would fail to perceive it only by actively avoiding doing so. Discussion of the experiments is therefore limited mainly to encoding strategy, rather than search strategy.

The results show that strategy depends on the information that is available, in three ways: strategy depends on the information which is actually available during stimulus presentation; the information which is normally available when the subject performs similar tasks; and the information which the subject anticipates will be available when the time comes to use the encoded information.

First then, the encoding strategy adopted must itself depend on the information which is presently available. An example of this is found in Experiments A2b and A5. In these experiments, midline information was either redundant (with visual presentation) or unreliable (with kinaesthetic presentation) compared with other experiments in the same chapter. Under these conditions, in VV and KK, there was no evidence that the midline was used as a reference position, as it had been in other experiments (Section [5.C]).

In the cross-modal conditions in Experiment A5, the midline was used, in the first session, to the detriment of performance, as measured by variability. Practice eliminated this strategy, improving performance. This introduces the second way in which information was found to affect strategy: an initial strategy was adopted, apparently

on the basis of what information is normally available and reliable, and this strategy was only with practice modified in light of the actually available information. This is similar to Goodnow's (1971) expression of the importance of "experience in working with a modality".

The role of normally available information was also indicated in Chapter Six, where the velocity of movements affected only kinaesthetic perception of the end-point of the movements. This had been predicted on the basis of (a) the inseparability of temporal and spatial cues when perception is kinaesthetic, and (b) the fact that external reference points are normally available visually, facilitating the encoding of positions in terms of distance from these reference points.

The third effect of information on strategy is also demonstrated in Experiment A5. A different strategy was adopted in the within- and cross-modal conditions, as noted above. This implies an effect of anticipated response modality, either in initially adopting a strategy, or in modifying that strategy, or both.

The research reported in this thesis has gone some way towards demonstrating the value of comparing broad classes of theory before becoming involved with details. Cross-modal matching was seen as a condition in which performance is prone to disruption by multiple types of information and it was found to be necessary to ask "what information is available to the subject?", and "what strategies are employed?", rather than "which modality is used?".

The model proposed here is therefore that strategy is not independent of the available information: the two interact such that strategy selection by child subjects is influenced by information available, past, present and future. The available information, and strategy, together determine the information encoded, and this

determines within-modal performance. Cross-modal performance logically must also depend on the compatibility of the information encoded through each modality, and on basing matching decisions on the compatible information once it has been encoded.

The above model is clearly not an all-inclusive predictive model of matching performance. It is a structure into which data can be fitted as knowledge of cross-modal performance increases. It is in important ways similar to the picture presented by Millar (1981), the details of which were derived primarily from studies of within-modal matching by blind and sighted subjects. These studies include shape matching and perception of large and small scale space, in addition to length and position matching.

The experiments reported here serve to strengthen considerably the case for the general model presented by Millar with regard to position matching, and to extend the model into the domain of perception of velocity and force (Section [7.3]). So general a model requires specification in particular areas of application, and this is achieved by the present series of experiments.

In similar fashion to the experiments reported here, the studies discussed by Millar include many which examine how children use information in each modality, without directly examining cross-modal matching. This is seen here as an important aspect of research into cross-modal matching: we are unlikely to fully understand strategies involved in cross-modal matching unless we understand the strategies called upon when only within-modal matching is required.

Although there is much in common between Millar's model and my own, there are differences. Millar concludes that:

"coding strategies related consistently to three main conditions: the type of references currently available, how reliable they were, and the ease and speed with which other codes could be accessed in memory".

There is clearly some divergence between this and my own classification of effects of information on strategy, although this is largely a matter of emphasis. Millar's categories refer broadly to present and past information, whereas I would also include anticipation of what information will be available. This presumably depends on accessing information from memory, but not accessing only codes from memory as in Millar's classification. One factor noted by Millar is the ease and speed with which codes can be accessed in memory. This implies a value for verbal coding, since this enables efficient organisation of information. Verbal coding would be important for, say, meaningful shapes, but does not appear to be important for position matching in the conditions of my experiments (e.g. Experiments B3 and B4).

There is therefore a reasonably consistent picture emerging from investigations of different modalities, pairs of modalities and subject populations: children use a variety of cues and spatial references, flexibly and with a level appropriateness which depends among other things on age. Indeed it seems that they will use any aspect of information which is made available to them, under appropriate conditions. Modality is just one factor which determines the cues which are available and how they are used.

Finally, given that this is the picture which is emerging, why should it be that the senses are organised in this way? The adaptiveness of the system can be presented in terms of three categories of perceived information. First, there is information which is modality-specific in the sense that it is perceived through only one modality (e.g. colour, temperature). Then there are parameters which are perceived through two or more modalities, but which tend to be encoded in different ways in different modalities. An example would be spatial position, perceived visually using

external references when they are available, and kinaesthetically using self-referent and movement cues. Third, there is information which is encoded in different modalities in the same way, for example visual and tactual coding of shape by reference to the number of edges.

To use Millar's terms, the senses are complementary and convergent. They are complementary in that they provide different information about the same object or event. They are convergent in that they provide the same, confirmatory and facilitatory, information about an object or event. Maladaptiveness is only apparent in the "middle ground", where the same information is coded in different ways, resulting in inaccuracy in cross-modal matching tasks.

This apparent maladaptiveness may in fact be largely a product of artificial matching tasks. Explicit matching, in the form of cross-modal recognition, is not the principal function of the senses. The more normal task is implicit matching in the process of integrating information: information is simultaneously available from more than one sense, and matching is sufficiently accurate for one sense to confirm the other, without any question of precise matching. Where matching is required under natural conditions (e.g. reaching for an object which is out of sight), the context of the act, and the possibility of self-correction would tend to ameliorate any difficulties.

The model presented here is therefore consistent both with experimental evidence and with the view that the human brain is organised in a way that is adaptive. Within the model, the present findings provide some elaboration of detail, and there is room to accommodate new data as it emerges.

[7.5] SUGGESTIONS FOR FURTHER WORK

In the results and conclusions reported above, there are implicit suggestions for further work. These are here made explicit, and extended. Beyond the immediate implications for research on visual-kinaesthetic matching, comments are made concerning cross-modal studies in general.

The research reported in the past four chapters indicates a number of factors which must be taken into account if the information available to each modality is to be specified, and the effects of different aspects of information investigated. The following factors can be noted:

(a) The presence of visual background cues (A1a, A1b, A2b, A5)

(b) Emphasis of the midline as a reference point (A2a, A5, Chapter Five)

(c) Interference between length and position cues ([1.3], A1a, A1b)

(d) Velocity of kinaesthetic presentation movements (Chapter Six)

(e) Resistance to kinaesthetic presentation movements, and pressure against a stop (Chapter Four)

(f) Detail of instructions (C3, D3)

Regarding the effect of velocity, the discussion in Section [6.D3.4] makes it clear that this effect is not a simple one. The interaction between spatial and temporal parameters would bear extensive further study, which might include analysis of concurrent effects of resistance.

From the literature reviewed in Section [1.3], other aspects of available information can be noted, which have been found to affect performance:

(a) Simultaneous vs successive presentation

(b) Passive vs active presentation

(c) Constrained vs preselected presentation

The prospect of using preselected, active kinaesthetic presentation movements is particularly interesting. Under these conditions, KK performance should be superior to that observed with the passive constrained presentation used in the present series of experiments (see Runnings and Diewert 1982, Summers et al 1981). KK should therefore also be superior to VV, and it would be predicted that a cross-modal deficit would result from the mismatch of information received through the two modalities.

Some remaining aspects of method, which have varied from study to study (see Section [1.3.2]), have not systematically been examined to show whether or not they have any important effect:

(a) Curved (arc) vs linear movements

(b) Vertical vs horizontal (sagittal) vs horizontal (coronal) movements

(c) Lever vs stylus movements

(d) Judgements with vs without allowing for immediate correction by the subject

(e) One- vs two-dimensional movements.

Two techniques should be particularly useful in the study of encoding strategy. First, the cueing paradigm employed by Newell et al (1979) can be used to identify the point in time at which strategic effects occur.

Second, structural interference designs, more familiar from attempts to identify the modality of storage, could be adapted to identify the information stored. For example, Diewert (1975) compared the effects of visual and kinaesthetic distractor tasks on kinaesthetic length and position reproduction. Length and position tasks were differentiated by the distractor tasks. Position

reproduction was affected equally by kinaesthetic and visual tasks, whereas length reproduction deteriorated more with the kinaesthetic distractor.

The labelling of the distractor tasks as visual and kinaesthetic in this experiment is not entirely satisfactory since the kinaesthetic task was comparison of movement lengths, whereas the visual task was comparison of line lengths. This difference could explain the different effects of the two tasks equally as well as the modality difference.

Thus, information and strategy effects may depend on many features of the task, such as those noted above. It has already been noted that these effects are not independent, but interactive. It is to be hoped that future studies can do more than catalogue stimulus features which affect performance; in the longer term, as interaction effects are clarified, there is the potential to investigate intersensory processes in important human activities such as writing, sport, and motor tasks in general.

There is already some indication of the relevance of research into simple movement tasks to behaviour. Hulme et al (1983) investigated the relation between motor skills in children and performance on within- and cross-modal matching tasks. They predicted a correlation between cross-modal performance and motor skills. They found no significant relation between these two variables, but motor skills were correlated with within-modal performance.

As a means of assessing cross-modal performance, the task used in this experiment is subject to most of the criticisms offered against Connolly and Jones' task (ibid p47). Individual differences in performance of this task probably index primarily specific strategic abilities, rather than cross-modal judgement of distances or coordination of movements. Hence, it is not altogether surprising

that within-modal performance provided the better predictor of motor skills.

In addition to task conditions, the error measure used may have important effects on the conclusions reached. This was pointed out in relation to the literature on adults' matching performance in Section [1.3.2], and is clearly the case for the experiments reported in Chapters Three to Six. The large effects of certain variables (e.g. modality, velocity, resistance, standard position) on constant errors would almost certainly lead to erroneous conclusions if absolute errors were analysed and regarded as a measure of accuracy.

It can also be noted that bias and variability effects are only mathematically independent. In practice they can be inter-related and one type of effect can be used to facilitate explanation of the other type. Examples of this are the effects of velocity found in Experiment D1 and the effect of practice on cross-modal performance in Experiment A5.

In general, between-pair difference (BPD) was analysed as a checking procedure: to support the analysis of within-subject variability and to detect cases of variable strategy (as discussed in more detail in Section [2.6.6]). Only in two experiments were BPD results of particular interest.

In Experiment A5, BPD indicated that strategy was more variable in the cross-modal conditions. This supported the conclusion from constant error and within-subject deviation analysis, that coding relative to the midline changed with practice. In Experiment D1, analysis of BPD allowed a test of the hypothesis that strategy was more variable in kinaesthetic than in visual matching.

Thus, the analysis of BPD would appear to be worthwhile as a supplement to constant errors and within-subject deviation, or where conditions militate against an experimental design which would permit

analysis of within-subject variability.

Conclusions reached in this thesis relate to a restricted range of tasks and stimuli; the generality of the conclusions should be tested. Three variables in particular should be the subject of further experimentation: the length of the presentation movement (and hence the distance of the standard position from the midline), the use of length rather than position as the appropriate cue, and age.

Length matching provides a task which is different from that used in most of the experiments reported here, but which would require much the same control of information. It is predicted that information and strategy would determine modality differences in length matching, as in position matching.

Further research into effects of velocity or resistance should be considered. It would be important in such research to make an investment in equipment designed to permit precise control of these variables. In the experiments reported here, only coarse divisions have been possible. This aspect of equipment design may be particularly important if longer movements are to be studied.

A further refinement of apparatus would be to simplify as far as possible the transition between visual and kinaesthetic perception. The physical acts involved in the transition may have effects on performance under some conditions, although no evidence of this was found here.

The challenge of adapting a task for use with younger children is an ever-present one in developmental psychology. It would particularly be useful to adapt the task used here, in order to determine whether younger children show effects of modality conditions which cannot be explained in terms of information and strategy. Such research could provide evidence that co-ordination of the senses increases with age, although the evidence is that, at

least as young as four years, such a result would be unexpected (Millar 1972, 1975).

The most important lesson to be learned for cross-modal research in general is that the information and strategies available in each modality condition must be controlled. With any pair of modalities with which this is possible, the generality of the conclusions reached here can be tested.

It is unfortunately also the case that achieving the appropriate controls is not necessarily a simple and obvious procedure. Only attention to more than two decades of research has allowed a reasonable level of control to be reached for visual-kinaesthetic length matching. In some cases, an alternative paradigm may be necessary.

For example, rather than equate modality conditions in terms of information and strategy, it may sometimes be more appropriate to include additional control groups. Where modality conditions cannot be equated, within-modal matching with different levels of information provided in acquisition and response would provide a suitable control group. This procedure has not been used in the experiments reported here, because at least eight groups of subjects are required, compared with four with the design chosen.

In summary, new tasks and stimuli can be used to advance understanding of cross-modal performance, within the framework indicated by the present results and conclusions.

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APPENDIX A: ANOVA TABLES FOR EXPERIMENTS IN CHAPTER THREE

Interaction terms are designated by the initial letter or letters of the factors involved.

EXPERIMENT Ala

Table [Ala.1] Constant Errors - Analysis One

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	7464.89	1	7464.89	73.87	0.0001
AGE	1297.31	1	1297.31	12.84	0.0015
MODALITY	352.78	1	352.78	3.49	0.1
SEX	46.92	1	46.92	0.46	0.6
AM	1.22	1	1.22	0.01	1.0
ASe	46.92	1	46.92	0.46	0.6
MSe	147.71	1	147.71	1.46	0.3
AMSe	5.91	1	5.91	0.06	0.9
ERROR	2425.39	24	101.06		
STANDARD	1.22	1	1.22	0.02	0.9
StA	17.63	1	17.63	0.30	0.6
StM	10.99	1	10.99	0.19	0.7
StSe	10.99	1	10.99	0.19	0.7
StAM	35.60	1	35.60	0.61	0.5
StASe	0.44	1	0.44	0.01	1.0
StMSe	30.52	1	30.52	0.52	0.5
StAMSe	10.99	1	10.99	0.19	0.7
ERROR	1401.95	24	58.41		
PROCEDURE	59.81	1	59.81	0.32	0.6
PA	127.00	1	127.00	0.67	0.5
PM	17.63	1	17.63	0.09	0.8
PSe	90.28	1	90.28	0.48	0.5
PAM	3.96	1	3.96	0.02	0.9
PASe	193.80	1	193.80	1.03	0.4
PMSe	46.92	1	46.92	0.25	0.7
PAMSe	2.39	1	2.39	0.01	1.0
ERROR	4528.52	24	188.69		
StP	82.08	1	82.08	1.05	0.4
StPA	41.06	1	41.06	0.53	0.5
StPM	25.83	1	25.83	0.33	0.6
StPSe	1.22	1	1.22	0.02	1.0
StPAM	2.39	1	2.39	0.03	0.9
StPASe	289.50	1	289.50	3.72	0.1
StPMSe	0.05	1	0.05	0.00	1.0
StPAMSe	66.85	1	66.85	0.86	0.4
ERROR	1867.58	24	77.82		

Table [Ala.2] Constant Errors - Analysis Two

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	7464.89	1	7464.89	73.87	0.0001
AGE	1297.31	1	1297.31	12.84	0.0015
MODALITY	352.78	1	352.78	3.49	0.1
SEX	46.92	1	46.92	0.46	0.6
AM	1.22	1	1.22	0.01	1.0
ASe	46.92	1	46.92	0.46	0.6
MSe	147.71	1	147.71	1.46	0.3
AMSe	5.91	1	5.91	0.06	0.9
ERROR	2425.39	24	101.06		
CUE	107.86	1	107.86	1.82	0.2
CA	30.52	1	30.52	0.51	0.5
CM	320.36	1	320.36	5.39	0.0290
CSe	30.52	1	30.52	0.51	0.5
CAM	1.22	1	1.22	0.02	0.9
CASe	181.69	1	181.69	3.06	0.1
CMSe	59.81	1	59.81	1.01	0.4
CAMSe	219.19	1	219.19	3.69	0.1
ERROR	1425.39	24	59.39		
PROCEDURE	59.81	1	59.81	0.32	0.6
PA	127.00	1	127.00	0.67	0.5
PM	17.63	1	17.63	0.09	0.8
PSe	90.28	1	90.28	0.48	0.5
PAM	3.96	1	3.96	0.02	0.9
PASe	193.80	1	193.80	1.03	0.4
PMSe	46.92	1	46.92	0.25	0.7
PAMSe	2.39	1	2.39	0.01	1.0
ERROR	4528.52	24	188.69		
CP	14.11	1	14.11	0.29	0.6
CPA	21.53	1	21.53	0.44	0.6
CPM	169.97	1	169.97	3.49	0.1
CPSe	10.99	1	10.99	0.23	0.7
CPAM	35.60	1	35.60	0.73	0.5
CPASe	10.99	1	10.99	0.23	0.7
CPMSe	82.08	1	82.08	1.68	0.3
CPAMSe	5.91	1	5.91	0.12	0.8
ERROR	1169.14	24	48.71		

Table [Ala.3] Constant Errors - Analysis Three

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	7464.89	1	7464.89	73.87	0.0001
AGE	1297.31	1	1297.31	12.84	0.0015
MODALITY	352.78	1	352.78	3.49	0.1
SEX	46.92	1	46.92	0.46	0.6
AM	1.22	1	1.22	0.01	1.0
ASe	46.92	1	46.92	0.46	0.6
MSe	147.71	1	147.71	1.46	0.3
AMSe	5.91	1	5.91	0.06	0.9
ERROR	2425.39	24	101.06		
CUE	107.86	1	107.86	1.82	0.2
CA	30.52	1	30.52	0.51	0.5
CM	320.36	1	320.36	5.39	0.0290
CSe	30.52	1	30.52	0.51	0.5
CAM	1.22	1	1.22	0.02	0.9
CASe	181.69	1	181.69	3.06	0.1
CMSe	59.81	1	59.81	1.01	0.4
CAMSe	219.19	1	219.19	3.69	0.1
ERROR	1425.39	24	59.39		
STANDARD	1.22	1	1.22	0.02	0.9
StA	17.63	1	17.63	0.30	0.6
StM	10.99	1	10.99	0.19	0.7
StSe	10.99	1	10.99	0.19	0.7
StAM	35.60	1	35.60	0.61	0.5
StASe	0.44	1	0.44	0.01	1.0
StMSe	30.52	1	30.52	0.52	0.5
StAMSe	10.99	1	10.99	0.19	0.7
ERROR	1401.95	24	58.41		
CSt	1203.56	1	1203.56	10.51	0.0035
CStA	459.42	1	459.42	4.01	0.1
CStM	98.88	1	98.88	0.86	0.4
CStSe	30.52	1	30.52	0.27	0.7
CStAM	17.63	1	17.63	0.15	0.7
CStASe	459.42	1	459.42	4.01	0.1
CStMSe	41.06	1	41.06	0.36	0.6
CStAMSe	10.99	1	10.99	0.10	0.8
ERROR	2748.83	24	114.53		

Table [Ala.4] Within-cell Deviation - Analysis One

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	721.59	1	721.59	717.44	0.0001
AGE	11.82	1	11.82	11.75	0.0022
MODALITY	1.39	1	1.39	1.38	0.3
SEX	0.00	1	0.00	0.00	1.0
AM	2.78	1	2.78	2.77	0.2
ASe	0.02	1	0.02	0.02	0.9
MSe	0.10	1	0.10	0.09	0.8
AMSe	1.54	1	1.54	1.53	0.3
ERROR	24.14	24	1.01		
STANDARD	0.49	1	0.49	0.46	0.6
StA	0.00	1	0.00	0.00	1.0
StM	0.13	1	0.13	0.12	0.8
StSe	0.00	1	0.00	0.00	1.0
StAM	1.12	1	1.12	1.05	0.4
StASe	1.01	1	1.01	0.94	0.4
StMSe	1.09	1	1.09	1.02	0.4
StAMSe	0.08	1	0.08	0.08	0.8
ERROR	25.70	24	1.07		
PROCEDURE	1.29	1	1.29	0.94	0.4
PA	0.00	1	0.00	0.00	1.0
PM	0.75	1	0.75	0.55	0.5
PSe	0.04	1	0.04	0.03	0.9
PAM	0.03	1	0.03	0.02	0.9
PASe	0.43	1	0.43	0.32	0.6
PMSe	0.00	1	0.00	0.00	1.0
PAMSe	0.18	1	0.18	0.13	0.8
ERROR	32.84	24	1.37		
StP	0.18	1	0.18	0.21	0.7
StPA	1.75	1	1.75	2.10	0.2
StPM	0.51	1	0.51	0.61	0.5
StPSe	0.01	1	0.01	0.01	1.0
StPAM	0.99	1	0.99	1.19	0.3
StPASe	0.01	1	0.01	0.01	1.0
StPMSe	1.75	1	1.75	2.10	0.2
StPAMSe	0.64	1	0.64	0.77	0.4
ERROR	19.98	24	0.83		

Table [Ala.5] Within-cell Deviation - Analysis Two

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	721.15	1	721.15	721.85	0.0001
AGE	11.76	1	11.76	11.77	0.0022
MODALITY	1.37	1	1.37	1.37	0.3
SEX	0.00	1	0.00	0.00	1.0
AM	2.76	1	2.76	2.76	0.2
ASe	0.02	1	0.02	0.02	0.9
MSe	0.09	1	0.09	0.09	0.8
AMSe	1.52	1	1.52	1.52	0.3
ERROR	23.98	24	1.00		
CUE	1.67	1	1.67	2.12	0.2
CA	1.00	1	1.00	1.27	0.3
CM	0.11	1	0.11	0.13	0.8
CSe	0.08	1	0.08	0.10	0.8
CAM	3.28	1	3.28	4.16	0.1
CASe	0.40	1	0.40	0.50	0.5
CMSe	0.10	1	0.10	0.13	0.8
CAMSe	0.32	1	0.32	0.40	0.6
ERROR	18.90	24	0.79		
PROCEDURE	1.31	1	1.31	0.96	0.4
PA	0.00	1	0.00	0.00	1.0
PM	0.73	1	0.73	0.54	0.5
PSe	0.04	1	0.04	0.03	0.9
PAM	0.03	1	0.03	0.02	0.9
PASe	0.42	1	0.42	0.31	0.6
PMSe	0.00	1	0.00	0.00	1.0
PAMSe	0.18	1	0.18	0.14	0.8
ERROR	32.68	24	1.36		
CP	1.72	1	1.72	1.75	0.2
CPA	0.58	1	0.58	0.59	0.5
CPM	0.29	1	0.29	0.30	0.6
CPSe	0.02	1	0.02	0.02	0.9
CPAM	0.04	1	0.04	0.04	0.9
CPASe	0.15	1	0.15	0.16	0.7
CPMSe	2.10	1	2.10	2.14	0.2
CPAMSe	1.00	1	1.00	1.02	0.4
ERROR	23.59	24	0.98		

Table [Ala.6] Within-cell Deviation - Analysis Three

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	722.13	1	722.13	717.54	0.0001
AGE	11.75	1	11.75	11.67	0.0023
MODALITY	1.41	1	1.41	1.40	0.3
SEX	0.00	1	0.00	0.00	1.0
AM	2.75	1	2.75	2.73	0.2
ASe	0.02	1	0.02	0.02	0.9
MSe	0.10	1	0.10	0.10	0.8
AMSe	1.51	1	1.51	1.50	0.3
ERROR	24.15	24	1.01		
CUE	1.68	1	1.68	2.13	0.2
CA	1.04	1	1.04	1.32	0.3
CM	0.10	1	0.10	0.13	0.8
CSe	0.08	1	0.08	0.10	0.8
CAM	3.34	1	3.34	4.25	0.1
CASe	0.37	1	0.37	0.48	0.5
CMSe	0.10	1	0.10	0.13	0.8
CAMSe	0.30	1	0.30	0.38	0.6
ERROR	18.80	24	0.79		
STANDARD	0.50	1	0.50	0.47	0.6
StA	0.00	1	0.00	0.00	1.0
StM	0.12	1	0.12	0.11	0.8
StSe	0.00	1	0.00	0.00	1.0
StAM	1.10	1	1.10	1.03	0.4
St	1.03	1	1.03	0.96	0.4
St	1.07	1	1.07	1.00	0.4
St	0.08	1	0.08	0.07	0.8
ERROR	25.75	24	1.07		
CSt	0.52	1	0.52	0.56	0.5
CStA	9.04	1	9.04	9.74	0.0046
CStM	0.84	1	0.84	0.91	0.4
CStSe	1.34	1	1.34	1.44	0.3
CStAM	0.22	1	0.22	0.24	0.7
CStASe	0.33	1	0.33	0.35	0.6
CStMSe	0.21	1	0.21	0.22	0.7
CStAMSe	0.86	1	0.86	0.92	0.4
EKROR	22.27	24	0.93		

EXPERIMENTS A1a and A1b

Table [A1.1] Constant Errors

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	11296.92	1	11296.92	101.88	0.0001
EXPERIMENT	260.21	1	260.21	2.35	0.2
MODALITY	274.66	1	274.66	2.48	0.2
SEX	117.24	1	117.24	1.06	0.4
EM	10.99	1	10.99	0.10	0.8
ESe	8.25	1	8.25	0.07	0.8
MSe	193.80	1	193.80	1.75	0.2
EMSe	812.55	1	812.55	7.33	0.0123
ERROR	2661.33	24	110.89		
CUE	17.63	1	17.63	0.22	0.7
CE	0.44	1	0.44	0.01	1.0
CM	98.88	1	98.88	1.25	0.3
CSe	137.16	1	137.16	1.74	0.3
CEM	82.08	1	82.08	1.04	0.4
CESe	53.17	1	53.17	0.67	0.5
CMSe	46.92	1	46.92	0.59	0.5
CEMSe	246.14	1	246.14	3.11	0.1
ERROR	1897.27	24	79.05		
STANDARD	169.97	1	169.97	2.36	0.2
StE	98.88	1	98.88	1.38	0.3
StM	59.81	1	59.81	0.83	0.4
StSe	2.39	1	2.39	0.03	0.9
StEM	2.39	1	2.39	0.03	0.9
StESe	5.91	1	5.91	0.08	0.8
StMSe	1.22	1	1.22	0.02	0.9
StEMSe	1.22	1	1.22	0.02	0.9
ERROR	1725.39	24	71.89		
CSt	2257.08	1	2257.08	20.31	0.0001
CStE	74.27	1	74.27	0.67	0.5
CStM	0.44	1	0.44	0.00	1.0
CStSe	478.56	1	478.56	4.31	0.0489
CStEM	25.83	1	25.83	0.23	0.7
CStESe	25.83	1	25.83	0.23	0.7
CStMSe	90.28	1	90.28	0.81	0.4
CStEMSe	41.06	1	41.06	0.37	0.6
ERROR	2667.58	24	111.15		

Table [A1.2] Within-cell Deviation

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	662.02	1	662.02	406.10	0.0001
EXPERIMENT	4.68	1	4.68	2.87	0.2
MODALITY	2.32	1	2.32	1.42	0.3
SEX	0.43	1	0.43	0.26	0.7
EM	0.23	1	0.23	0.14	0.8
ESe	0.05	1	0.05	0.03	0.9
MSe	0.37	1	0.37	0.23	0.7
EMSe	3.43	1	3.43	2.11	0.2
ERROR	39.12	24	1.63		
CUE	0.01	1	0.01	0.02	0.9
CE	0.04	1	0.04	0.05	0.9
CM	0.20	1	0.20	0.23	0.7
CSe	0.04	1	0.04	0.04	0.9
CEM	1.40	1	1.40	1.65	0.3
CESe	0.04	1	0.04	0.05	0.9
CMSe	3.53	1	3.53	4.15	0.1
CEMSe	0.16	1	0.16	0.18	0.7
ERROR	20.39	24	0.85		
STANDARD	1.02	1	1.02	0.96	0.4
StE	0.13	1	0.13	0.12	0.8
StM	0.05	1	0.05	0.05	0.9
StSe	1.06	1	1.06	0.99	0.4
StEM	5.37	1	5.37	5.03	0.0345
StESe	1.55	1	1.55	1.45	0.3
StMSe	2.60	1	2.60	2.43	0.2
StEMSe	4.40	1	4.40	4.11	0.1
ERROR	25.65	24	1.07		
CSt	12.15	1	12.15	19.54	0.0002
CStE	2.60	1	2.60	4.18	0.1
CStM	0.15	1	0.15	0.24	0.7
CStSe	0.83	1	0.83	1.33	0.3
CStEM	2.35	1	2.35	3.77	0.1
CStESe	0.36	1	0.36	0.57	0.5
CStMSe	0.00	1	0.00	0.00	1.0
CStEMSe	1.37	1	1.37	2.20	0.2
ERROR	14.92	24	0.62		

EXPERIMENT A2a

Table [A2a.1] Constant Errors

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	3828.12	1	3828.12	96.63	0.0001
MODALITY	33.01	1	33.01	0.83	0.4
SEX	0.20	1	0.20	0.00	1.0
MSe	19.53	1	19.53	0.49	0.5
ERROR	475.39	12	39.62		
STANDARD	431.44	1	431.44	11.87	0.005
StM	7.03	1	7.03	0.19	0.7
StSe	50.00	1	50.00	1.38	0.3
StMSe	15.82	1	15.82	0.44	0.6
ERROR	436.33	12	36.36		
RESP.MOVE.	378.12	1	378.12	5.24	0.041
RM	9.57	1	9.57	0.13	0.8
RSe	187.69	1	187.69	2.60	0.2
RMSe	0.00	1	0.00	0.00	1.0
ERROR	865.23	12	72.10		
StR	9.57	1	9.57	0.30	0.6
StRM	132.03	1	132.03	4.19	0.1
StRSe	0.78	1	0.78	0.02	0.9
StRMSe	23.63	1	23.63	0.75	0.5
ERROR	377.73	12	31.48		
SESSION	132.03	1	132.03	6.68	0.024
SsM	43.94	1	43.94	2.22	0.2
SsSe	70.51	1	70.51	3.57	0.1
SsMSe	19.53	1	19.53	0.99	0.4
ERROR	237.11	12	19.76		
StSs	15.82	1	15.82	0.35	0.6
StSsM	12.50	1	12.50	0.28	0.7
StSsSe	12.50	1	12.50	0.28	0.7
StSsMSe	9.57	1	9.57	0.21	0.7
ERROR	537.11	12	44.76		
RSs	0.78	1	0.78	0.01	1.0
RSsM	0.20	1	0.20	0.00	1.0
RSsSe	1.76	1	1.76	0.03	0.9
RSsMSe	28.12	1	28.12	0.52	0.5
ERROR	650.39	12	54.20		
StRSs	33.01	1	33.01	1.04	0.4
StRSsM	50.00	1	50.00	1.58	0.3
StRSsSe	7.03	1	7.03	0.22	0.7
StRSsMSe	1.76	1	1.76	0.06	0.9
ERROR	380.08	12	31.67		

KEY: RESP.MOVE = RESPONSE MOVEMENT

Table [A2a.2] Within-subject Deviation

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	159.41	1	159.41	70.68	0.0001
MODALITY	1.42	1	1.42	0.63	0.5
SEX	4.26	1	4.26	1.89	0.2
MSe	0.80	1	0.80	0.36	0.6
ERROR	27.06	12	2.26		
STANDARD	3.57	1	3.57	7.43	0.0184
StM	0.13	1	0.13	0.26	0.7
StSe	0.46	1	0.46	0.95	0.4
StMSe	0.11	1	0.11	0.22	0.7
ERROR	5.76	12	0.48		
RESP.MOVE	0.21	1	0.21	0.31	0.6
RM	0.02	1	0.02	0.02	0.9
RSe	2.01	1	2.01	2.96	0.2
RMSe	1.43	1	1.43	2.10	0.2
ERROR	8.13	12	0.68		
StR	0.21	1	0.21	0.29	0.6
StRM	0.61	1	0.61	0.86	0.4
StRSe	0.23	1	0.23	0.33	0.6
StRMSe	0.33	1	0.33	0.47	0.6
ERROR	8.42	12	0.70		

EXPERIMENT A2b

Table [A2b.1] Constant Errors

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	293.78	1	293.78	0.81	0.4
MODALITY	161.00	2	80.50	0.22	0.9
ERROR	7588.18	21	361.34		
STANDARD	246.39	1	246.39	2.28	0.2
StM	93.95	2	46.97	0.43	0.7
ERROR	2271.39	21	108.16		
RESP.MOVE	35.45	1	35.45	0.52	0.5
RM	53.32	2	26.66	0.39	0.7
ERROR	1444.82	21	68.80		
StR	3.94	1	3.94	0.11	0.8
StRM	172.72	2	86.36	2.38	0.2
ERROR	763.18	21	36.34		
SESSION	65.92	1	65.92	0.43	0.6
SsM	219.34	2	109.67	0.71	0.6
ERROR	3223.34	21	153.49		
StSs	0.03	1	0.03	0.00	1.0
StSsM	170.77	2	85.38	1.60	0.3
ERROR	1119.04	21	53.29		
RSs	91.44	1	91.44	2.27	0.2
RSsM	17.64	2	8.82	0.22	0.9
ERROR	846.39	21	40.30		
StRSs	3.94	1	3.94	0.11	0.8
StRSsM	63.35	2	31.68	0.88	0.5
ERROR	756.93	21	36.04		

Table [A2b.2] Within-subject Deviation

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	352.93	1	352.93	387.63	0.0001
MODALITY	11.16	2	5.58	6.13	0.008
ERROR	19.12	21	0.91		
STANDARD	0.05	1	0.05	0.09	0.8
StM	1.69	2	0.84	1.48	0.3
ERROR	11.94	21	0.57		
RESP.MOVE	0.51	1	0.51	1.01	0.4
RM	1.55	2	0.78	1.52	0.3
ERROR	10.74	21	0.51		
StR	0.94	1	0.94	1.31	0.3
StRM	2.08	2	1.04	1.45	0.3
ERROR	15.05	21	0.72		

EXPERIMENT A3

Table [A3.1] Constant Errors

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	3306.25	1	3306.25	83.01	0.0001
AGE	172.27	1	172.27	4.33	0.0421
MODALITY	1721.09	3	573.70	1.27	0.3
AM	151.17	3	50.39	1.27	0.0001
ERROR	2230.47	56	39.83		
STANDARD	244.14	1	244.14	8.76	0.0045
StA	1.56	1	1.56	0.06	0.9
StM	20.70	3	6.90	0.25	0.9
StAM	153.91	3	51.30	1.84	0.2
ERROR	1560.94	56	27.87		
SESSION	6.25	1	6.25	0.28	0.6
SsA	0.39	1	0.39	0.02	0.9
SsM	113.28	3	37.76	1.72	0.2
SsAM	25.39	3	8.46	0.39	0.8
ERROR	1229.69	56	21.96		
StSs	39.06	1	39.06	2.29	0.2
StSsA	0.39	1	0.39	0.02	0.9
StSsM	52.34	3	17.45	1.02	0.4
StSsAm	66.80	3	22.27	1.31	0.3
ERROR	953.91	56	17.03		

Table [A3.2] Within-subject Deviation

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	138.73	1	138.73	319.85	0.0001
AGE	1.90	1	1.90	4.38	0.0409
MODALITY	0.44	3	0.15	0.34	0.8
AM	0.86	3	0.29	0.66	0.6
ERROR	24.29	56	0.43		
STANDARD	0.14	1	0.14	0.36	0.6
StA	0.10	1	0.10	0.25	0.7
StM	1.87	3	0.62	1.63	0.2
StAM	0.06	3	0.02	0.05	1.0
ERROR	21.32	56	0.38		

Table [A3.3] Between-pair Difference

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	151.40	1	151.40	202.10	0.0001
AGE	0.15	1	0.15	0.20	0.7
MODALITY	1.14	3	0.38	0.51	0.7
AM	1.18	3	0.39	0.52	0.7
ERROR	17.98	24	0.75		
STANDARD	0.00	1	0.00	0.01	1.0
StA	0.55	1	0.55	1.04	0.4
StM	1.05	3	0.35	0.66	0.6
StAM	0.52	3	0.17	0.33	0.9
ERROR	12.69	24	0.53		

EXPERIMENT A4

Table [A4.1] Constant Errors

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	2991.03	1	2991.03	28.90	0.0001
AGE	184.68	1	184.68	1.78	0.2
MODALITY	8631.19	3	2877.06	27.80	0.0001
AM	133.04	3	44.35	0.43	0.8
ERROR	5795.21	56	103.49		
DELAY	6.46	1	6.46	0.12	0.8
DA	80.10	1	80.10	1.45	0.3
DM	131.77	3	43.92	0.80	0.6
DAM	57.56	3	19.19	0.35	0.8
ERROR	3093.65	56	55.24		
STANDARD	844.35	1	844.35	25.42	0.0001
StA	105.58	1	105.58	3.18	0.1
StM	418.10	3	139.37	4.20	0.0095
StAM	56.68	3	18.89	0.57	0.7
ERROR	1860.45	56	33.22		
DSt	0.01	1	0.01	0.00	1.0
DStA	0.99	1	0.99	0.05	0.9
DStM	49.55	3	16.52	0.76	0.6
DStAM	135.88	3	45.29	2.09	0.2
ERROR	1214.36	56	21.68		
PAIR	39.66	1	39.66	1.98	0.2
PA	39.66	1	39.66	1.98	0.2
PM	37.83	3	12.61	0.63	0.6
PAM	72.79	3	24.26	1.21	0.4
ERROR	1120.21	56	20.00		
DP	10.23	1	10.23	0.44	0.6
DPA	0.60	1	0.60	0.03	0.9
DPM	42.61	3	14.20	0.60	0.7
DPAM	67.71	3	22.57	0.96	0.5
ERROR	1317.09	56	23.52		
StP	39.66	1	39.66	2.27	0.2
StPA	0.01	1	0.01	0.00	1.0
StPM	123.38	3	41.13	2.36	0.1
StPAM	32.75	3	10.92	0.63	0.7
ERROR	976.86	56	17.44		
DStP	2.06	1	2.06	0.13	0.8
DStPA	13.29	1	13.29	0.85	0.4
DStPM	27.77	3	9.26	0.59	0.7
DStPAM	23.77	3	7.92	0.51	0.7
ERROR	877.64	56	15.67		

Table [A4.2] Within-subject Deviation

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	567.84	1	567.84	871.30	0.0001
AGE	1.28	1	1.28	1.96	0.2
MODALITY	0.92	3	0.31	0.47	0.8
AM	0.70	3	0.23	0.36	0.8
ERROR	36.50	56	0.65		
DELAY	0.27	1	0.27	0.37	0.6
DA	1.78	1	1.78	2.51	0.2
DM	2.78	3	0.93	1.30	0.3
DAM	1.32	3	0.44	0.62	0.7
ERROR	39.89	56	0.71		
STANDARD	5.28	1	5.28	11.21	0.0015
StA	0.01	1	0.01	0.02	0.9
StM	2.86	3	0.95	2.02.	0.2
StAM	3.52	3	1.17	2.49	0.1
ERROR	26.37	56	0.47		
DSt	0.07	1	0.07	0.12	0.8
DStA	2.16	1	2.16	3.73	0.1
DStM	1.73	3	0.58	0.99	0.5
DStAM	2.40	3	0.80	1.38	0.3
ERROR	32.45	56	0.58		

Table [A4.3] Between-pair Difference

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	346.68	1	346.68	390.33	0.0001
AGE	0.17	1	0.17	0.19	0.7
MODALITY	1.11	3	0.37	0.42	0.8
AM	2.00	3	0.68	0.75	0.6
ERROR	21.32	24	0.89		
DELAY	1.22	1	1.22	3.04	0.1
DA	0.23	1	0.23	0.57	0.5
DM	0.36	3	0.12	0.30	0.9
DAM	1.32	3	0.44	1.09	0.4
ERROR	9.63	24	0.40		
STANDARD	0.23	1	0.23	0.54	0.5
StA	0.15	1	0.15	0.35	0.6
StM	0.99	3	0.33	0.75	0.6
StAM	0.37	3	0.12	0.28	0.9
ERROR	10.46	24	0.44		
DSt	0.68	1	0.68	1.85	0.2
DStA	0.04	1	0.04	0.11	0.8
DStM	2.69	3	0.90	2.44	0.1
DStAM	1.96	3	0.65	1.78	0.2
ERROR	8.81	24	0.37		

EXPERIMENT A5

Table [A5.1] Comparison of Experiments A4 and A5 (Within-subject Deviation)

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	260.16	1	260.16	379.00	0.0001
EXPERIMENT	0.27	1	0.27	0.39	0.6
MODALITY	8.07	3	2.69	3.92	0.0131
EM	6.06	3	2.02	2.94	0.0409
ERROR	38.44	56	0.69		
STANDARD	0.90	1	0.90	1.26	0.3
StE	0.15	1	0.15	0.21	0.7
StM	0.12	3	0.04	0.06	1.0
StEM	0.85	3	0.28	0.40	0.8
ERROR	40.04	56	0.71		

Table [A5.2] Constant Errors

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	33.01	1	33.01	0.18	0.7
AGE	56.45	1	56.45	0.31	0.6
MODALITY	3119.04	3	1039.68	5.73	0.0017
AM	73.54	3	24.51	0.14	1.0
ERROR	10158.59	56	181.40		
STANDARD	21.53	1	21.53	0.58	0.5
StA	30.52	1	30.52	0.82	0.4
StM	91.26	3	30.42	0.82	0.5
StAM	133.64	3	44.55	1.20	0.4
ERROR	2076.17	56	37.07		
SESSION	889.89	1	889.89	14.32	0.0004
SsA	2.39	1	2.39	0.04	0.9
SsM	351.42	3	117.14	1.88	0.2
SsAM	112.94	3	37.65	0.61	0.7
ERROR	3480.86	56	62.16		
StSs	122.07	1	122.07	6.27	0.0152
StSsA	0.20	1	0.20	0.01	1.0
StSsM	19.82	3	6.61	0.34	0.8
StSsAM	30.57	3	10.19	0.52	0.7
ERROR	1089.84	56	19.46		
PAIR	50.00	1	50.00	2.70	0.2
PA	7.03	1	7.03	0.38	0.6
PM	120.41	3	40.14	2.17	0.2
PAM	67.48	3	22.49	1.22	0.4
ERROR	1036.33	56	18.51		
StP	41.06	1	41.06	1.65	0.3
StPA	10.99	1	10.99	0.44	0.6
StPM	76.81	3	25.60	1.03	0.4
StPAM	80.52	3	26.84	1.08	0.4
ERROR	1396.88	56	24.94		
SsP	3.96	1	3.96	0.16	0.7
SsPA	10.99	1	10.99	0.44	0.6
SsPM	9.62	3	3.21	0.13	1.0
SsPAM	19.19	3	6.40	0.25	0.9
ERROR	1409.38	56	25.17		
StSsP	12.50	1	12.50	0.65	0.5
StSsPA	175.78	1	175.78	9.07	0.0039
StSsPM	38.38	3	12.79	0.66	0.6
StSsPAM	116.70	3	38.90	2.01	0.2
ERROR	1084.77	56	19.37		

Table [A5.3] Within-subject Deviation

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	486.08	1	486.08	479.48	0.0001
AGE	0.27	1	0.27	0.27	0.7
MODALITY	9.53	3	3.18	3.14	0.0325
AM	1.66	3	0.55	0.55	0.7
ERROR	56.77	56	1.01		
STANDARD	0.30	1	0.30	0.37	0.6
StA	0.04	1	0.04	0.06	0.9
StM	1.12	3	0.37	0.46	0.8
StAM	1.18	3	0.39	0.48	0.7
ERROR	45.53	56	0.81		
PAIR	0.80	1	0.80	1.45	0.3
PA	0.00	1	0.00	0.00	1.0
PM	4.06	3	1.35	2.44	0.1
PAM	2.86	3	0.95	1.72	0.2
ERROR	31.08	56	0.55		
StP	0.93	1	0.93	1.35	0.3
StPA	2.77	1	2.77	4.02	0.0499
StPM	0.80	3	0.27	0.39	0.8
StPAM	1.68	3	0.56	0.81	0.5
ERROR	38.65	56	0.69		

Table [A5.4] Between-pair Difference

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	176.27	1	176.27	367.45	0.0001
AGE	0.81	1	0.81	1.69	0.3
MODALITY	9.44	3	3.15	6.56	0.0021
AM	4.14	3	1.38	2.88	0.1
ERROR	11.51	24	0.48		
STANDARD	0.04	1	0.04	0.08	0.8
StA	0.05	1	0.05	0.10	0.8
StM	0.31	3	0.10	0.22	0.9
StAM	0.55	3	0.18	0.40	0.8
ERROR	11.11	24	0.46		

APPENDIX B: ANOVA TABLES FOR EXPERIMENTS IN CHAPTER FOUR

Interaction terms are designated by the initial letter or letters of the factors involved.

EXPERIMENT B2

Table [B2.1] Constant Errors

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	129.89	1	129.89	1.07	0.4
ERROR	2062.30	17	121.31		
RESISTANCE	955.61	2	477.81	11.22	0.0002
ERROR	1447.51	34	42.57		
STANDARD	409.75	1	409.75	9.88	0.1
ERROR	705.35	17	41.49		
RSt	5.27	2	2.63	0.16	0.9
ERROR	574.94	34	16.91		
SESSION	2.34	1	2.34	0.05	0.9
ERROR	731.51	17	43.03		
RSs	146.01	2	73.00	2.51	0.1
ERROR	990.45	34	29.13		
StSs	69.47	1	69.47	1.93	0.2
ERROR	612.30	17	36.02		
RStSs	11.17	2	5.58	0.20	0.9
ERROR	952.37	34	28.01		

Table [B2.2] Within-subject Deviation

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	279.46	1	279.46	288.59	0.0001
ERROR	16.46	17	0.97		
RESISTANCE	3.63	2	1.82	1.89	0.2
ERROR	32.73	34	0.96		
STANDARD	0.98	1	0.98	2.49	0.2
ERROR	6.70	17	0.39		
RSt	0.26	2	0.13	0.21	0.9
ERROR	20.36	34	0.60		

APPENDIX C: ANOVA TABLES FOR EXPERIMENTS IN CHAPTER FIVE

Interaction terms are designated by the initial letter or letters of the factors involved.

EXPERIMENT C1

Table [C1.1] Constant Errors

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	28.13	1	28.13	0.26	0.7
ERROR	1653.13	15	110.21		
PAIR	56.45	1	56.45	1.60	0.3
ERROR	527.93	15	35.20		
STANDARD	431.45	1	431.45	13.28	0.0024
ERROR	487.30	15	32.49		
PSt	0.78	1	0.78	0.03	0.9
ERROR	389.84	15	25.99		
LENGTH	267.38	1	267.38	7.53	0.0151
ERROR	532.62	15	35.51		
PL	12.50	1	12.50	0.66	0.5
ERROR	284.38	15	18.96		
StL	94.53	1	94.53	3.99	0.1
ERROR	355.47	15	23.70		
PStL	0.20	1	0.20	0.00	1.0
ERROR	602.93	15	40.20		

Table [C1.2] Within-subject Deviation

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	172.94	1	172.94	371.13	0.0001
ERROR	6.99	15	0.47		
STANDARD	0.09	1	0.09	0.18	0.7
ERROR	7.88	15	0.53		
LENGTH	0.87	1	0.87	1.01	0.4
ERROR	12.96	15	0.86		
SL	0.56	1	0.56	0.58	0.5
ERROR	14.48	15	0.97		

Table [C1.3] Between-pair Difference

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	84.69	1	84.69	80.39	0.0001
ERROR	7.37	7	1.05		
STANDARD	0.73	1	0.73	1.13	0.4
ERROR	4.52	7	0.65		
LENGTH	0.69	1	0.69	1.49	0.3
ERROR	3.22	7	0.46		
SL	0.06	1	0.06	0.05	0.9
ERROR	7.81	7	1.12		

EXPERIMENT C2

Table [C2.1] Constant Errors

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	74.27	1	74.27	0.37	0.6
ERROR	3034.33	15	202.29		
STANDARD	219.19	1	219.19	2.30	0.2
ERROR	1426.90	15	95.13		
LENGTH	369.58	1	369.58	4.83	0.0441
ERROR	1148.39	15	76.56		
SL	3.96	1	3.96	0.05	0.9
ERROR	1120.26	15	74.68		
DAY	8.25	1	8.25	0.12	0.8
ERROR	1028.47	15	68.56		
SD	260.21	1	260.21	2.97	0.2
ERROR	1314.01	15	87.60		
LD	46.92	1	46.92	0.85	0.4
ERROR	830.42	15	55.36		
SL	53.17	1	53.17	0.92	0.4
ERROR	867.92	15	57.86		

Table [C2.2] Within-subject Deviation

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	271.31	1	271.31	252.31	0.0001
ERROR	16.12	15	1.07		
STANDARD	0.65	1	0.65	1.48	0.3
ERROR	6.56	15	0.44		
LENGTH	0.64	1	0.64	0.92	0.4
ERROR	10.46	15	0.70		
SL	2.05	1	2.05	4.99	0.0411
ERROR	6.16	15	0.41		

Table [C2.3] Between-pair Difference

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	116.03	1	116.03	140.41	0.0001
ERROR	5.78	7	0.83		
STANDARD	2.06	1	2.06	7.20	0.0314
ERROR	2.00	7	0.29		
LENGTH	1.30	1	1.30	1.75	0.3
ERROR	5.21	7	0.74		
SL	0.00	1	0.00	0.01	1.0
ERROR	2.86	7	0.41		

EXPERIMENT C3

Table [C3.1] Constant Errors

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	365.76	1	365.76	3.06	0.1
MODALITY	6.38	1	6.38	0.05	0.9
ERROR	2627.86	22	119.45		
STANDARD	15.76	1	15.76	0.32	0.6
SM	218.88	1	218.88	4.39	0.0479
ERROR	1096.61	22	49.85		
PAIR	67.84	3	22.61	0.83	0.5
PM	67.84	3	22.61	0.83	0.5
ERROR	1801.82	66	27.30		
SP	16.28	3	5.43	0.21	0.9
SPM	87.11	3	29.04	1.13	0.4
ERROR	1702.86	66	25.80		

Table [C3.2] Variable Errors

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	130.72	1	130.72	506.29	0.0001
MODALITY	0.00	1	0.00	0.01	1.0
ERROR	5.68	22	0.26		
STANDARD	0.31	1	0.31	2.71	0.2
SM	0.03	1	0.03	0.23	0.7
ERROR	2.54	22	0.12		

Table [C3.3] Between-pair Difference

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	57.98	1	57.98	64.08	0.0001
MODALITY	2.25	1	2.25	2.49	0.2
ERROR	9.05	10	0.90		
STANDARD	0.07	1	0.07	0.15	0.8
SM	0.80	1	0.80	1.69	0.3
ERROR	4.75	10	0.48		

APPENDIX D: ANOVA TABLES FOR EXPERIMENTS IN CHAPTER SIX

Interaction terms are designated by the initial letter or letters of the factors involved.

EXPERIMENT D1

Table [D1.1] Constant Errors

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	246.14	1	246.14	1.21	0.3
AGE	0.20	1	0.20	0.00	1.0
ERROR	12623.97	62	203.61		
MODALITY	117.24	1	117.24	1.62	0.3
MA	70.51	1	70.51	0.97	0.4
ERROR	4488.82	62	72.40		
VELOCITY	164.26	1	164.26	2.74	0.2
VA	137.16	1	137.16	2.29	0.2
ERROR	3712.65	62	59.88		
MV	775.20	1	775.20	18.42	0.0001
MVA	17.63	1	17.63	0.42	0.6
ERROR	2608.74	62	42.08		
STANDARD	1617.38	1	1617.38	42.72	0.0001
StA	30.52	1	30.52	0.81	0.4
ERROR	2347.41	62	37.86		
MSt	7.03	1	7.03	0.14	0.8
MStA	0.05	1	0.05	0.00	1.0
ERROR	3175.73	62	51.22		
VSt	336.38	1	366.38	8.41	0.0052
VStA	3.13	1	3.13	0.08	0.8
ERROR	2480.81	62	40.01		
MVSt	30.52	1	30.52	0.89	0.4
MVStA	4.88	1	4.88	0.14	0.8
ERROR	2128.66	62	34.33		

Table [D1.2] Between-pair Difference

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	480.27	1	480.27	746.87	0.0001
AGE	8.00	1	8.00	12.44	0.0014
ERROR	19.29	30	0.64		
MODALITY	3.90	1	3.90	15.04	0.0005
MA	0.01	1	0.01	0.05	0.9
ERROR	7.77	30	0.26		
VELOCITY	0.69	1	0.69	1.34	0.3
VA	0.29	1	0.29	0.55	0.5
ERROR	15.49	30	0.52		
MV	0.60	1	0.60	1.15	0.3
MVA	0.29	1	0.29	0.55	0.5
ERROR	15.66	30	0.52		

EXPERIMENT D2

Table [D2.1] Constant Errors
Comparison of Experiments A3 and D2

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	3100.78	1	3100.78	43.01	0.0001
EXPERIMENT	657.03	1	657.03	9.11	0.0031
AGE	122.07	1	122.07	1.69	0.2
MODALITY	4038.38	3	1346.13	18.67	0.0001
EA	56.45	1	56.45	0.78	0.4
EM	431.35	3	143.78	1.99	0.2
AM	74.32	3	24.77	0.34	0.8
EAM	129.00	3	43.00	0.69	0.7
ERROR	8075.00	112	72.10		
STANDARD	943.41	1	943.41	35.07	0.0001
StE	74.27	1	74.27	2.76	0.1
StA	1.22	1	1.22	0.05	0.9
StM	202.39	3	67.46	2.51	0.1
StEA	0.44	1	0.44	0.02	0.9
StEM	179.74	3	59.91	2.23	0.1
StAM	52.98	3	17.66	0.66	0.6
StEAM	135.40	3	45.13	1.68	0.2
ERROR	3013.28	112	26.90		
PAIR	3.13	1	3.13	0.12	0.8
PE	28.13	1	28.13	1.09	0.3
PA	12.50	1	12.50	0.49	0.5
PM	104.79	3	34.93	1.36	0.3
PEA	7.03	1	7.03	0.27	0.7
PEM	124.71	3	41.57	1.61	0.2
PAM	19.43	3	6.48	0.25	0.9
PEAM	51.86	3	17.29	0.67	0.6
ERROR	2882.81	112	25.74		
StP	66.85	1	66.85	3.22	0.1
StPE	0.44	1	0.44	0.02	0.9
StPA	10.99	1	10.99	0.53	0.5
StPM	47.31	3	15.77	0.76	0.6
StPEA	5.91	1	5.91	0.28	0.6
StPEM	30.91	3	10.30	0.50	0.7
StPAM	202.59	3	67.53	3.25	0.0245
StPEAM	112.35	3	37.45	1.80	0.2
ERROR	2325.78	112	20.77		

Table [D2.2] Constant Errors

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	677.34	1	677.34	5.20	0.0264
AGE	1.63	1	1.63	0.01	1.0
MODALITY	4066.02	3	1355.34	10.40	0.0001
AM	26.11	3	8.70	0.07	1.0
ERROR	7295.57	56	130.28		
STANDARD	876.04	1	876.04	31.47	0.0001
StA	3.19	1	3.19	0.11	0.8
StM	640.23	3	213.41	7.67	0.0002
StAM	138.09	3	46.03	1.65	0.2
ERROR	1559.11	56	27.84		
PAIR	25.00	2	12.50	0.44	0.7
PA	25.52	2	12.76	0.45	0.7
PM	172.67	6	28.78	1.02	0.5
PAM	231.51	6	38.59	1.37	0.3
ERROR	3159.90	112	28.21		
StP	68.10	2	34.05	1.30	0.3
StPA	25.91	2	12.96	0.49	0.7
StPM	85.55	6	14.26	0.54	0.8
StPAM	432.42	6	72.07	2.75	0.0158
ERROR	2940.10	112	26.25		

Table [D2.3] Variable Errors

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	274.51	1	274.51	1024.14	0.0001
AGE	0.15	1	0.15	0.56	0.5
MODALITY	0.23	3	0.08	0.29	0.9
AM	1.81	3	0.60	2.26	0.1
ERROR	15.01	56	0.27		
STANDARD	0.06	1	0.06	0.44	0.6
StA	0.15	1	0.15	1.04	0.4
StM	0.24	3	0.08	0.55	0.7
StAM	0.13	3	0.04	0.29	0.9
ERROR	8.06	56	0.14		

Table [D2.4] Between-pair Difference

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	201.82	1	201.82	508.38	0.0001
AGE	0.24	1	0.24	0.60	0.5
MODALITY	2.83	3	0.94	2.37	0.1
AM	2.77	3	0.92	2.33	0.1
ERROR	9.53	24	0.40		
STANDARD	0.12	1	0.12	0.44	0.6
StA	0.23	1	0.23	0.86	0.4
StM	1.61	3	0.54	2.03	0.2
StAM	0.72	3	0.24	0.91	0.5
ERROR	6.34	24	0.26		

EXPERIMENT D3

Table [D3.1] Constant Errors

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	1197.81	1	1197.81	9.25	0.0029
AGE	24.22	1	24.22	0.19	0.7
MODALITY	2908.91	3	969.64	7.49	0.0001
AM	195.38	3	65.13	0.50	0.7
ERROR	15540.09	120	129.50		
VELOCITY	360.41	1	360.41	8.74	0.0038
VA	15.88	1	15.88	0.38	0.6
VM	678.14	3	226.05	5.48	0.0015
VAM	419.55	3	139.85	3.39	0.0203
ERROR	4948.68	120	41.24		
STANDARD	1990.00	1	1990.00	47.28	0.0001
SA	195.56	1	195.56	4.65	0.0331
SM	971.70	3	323.90	7.70	0.0001
SAM	114.96	3	38.32	0.91	0.5
ERROR	5050.44	120	42.09		
VS	11.29	1	11.29	0.51	0.5
VSA	24.22	1	24.22	1.09	0.3
VSM	55.49	3	18.50	0.83	0.5
VSAM	39.03	3	13.01	0.59	0.7
ERROR	2661.38	120	22.18		
PAIR	0.15	1	0.15	0.01	1.0
PA	7.48	1	7.48	0.40	0.6
PM	16.72	3	5.57	0.30	0.9
PAM	40.84	3	13.61	0.72	0.6
ERROR	2263.72	120	18.86		
VP	36.19	1	36.19	1.81	0.2
VPA	246.59	1	246.59	12.37	0.0006
VPM	10.57	3	3.52	0.18	1.0
VPAM	99.19	3	33.06	1.66	0.2
ERROR	2392.63	120	19.94		
SP	13.48	1	13.48	0.68	0.5
SPA	11.29	1	11.29	0.57	0.5
SPM	38.45	3	12.82	0.64	0.6
SPAM	17.40	3	5.80	0.29	0.9
ERROR	2392.04	120	19.93		
VSP	1.37	1	1.37	0.09	0.8
VSPA	46.20	1	46.20	2.93	0.1
VSPM	3.68	3	1.23	0.08	1.0
VSPAM	10.61	3	3.54	0.22	0.9
ERROR	1892.04	120	15.77		

Table [D3.2] Within-subject Deviation

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	1098.79	1	1098.79	1442.74	0.0001
AGE	0.12	1	0.12	0.14	0.8
MODALITY	1.39	3	0.46	0.61	0.7
AM	6.38	3	2.13	2.79	0.0434
ERROR	91.39	120	0.76		
VELOCITY	0.00	1	0.00	0.00	1.0
VA	0.02	1	0.02	0.02	0.9
VM	1.24	3	0.41	0.59	0.7
VAM	1.17	3	0.39	0.56	0.7
ERROR	83.81	120	0.70		
STANDARD	0.36	1	0.36	0.55	0.5
StA	0.25	1	0.25	0.38	0.6
StM	0.64	3	0.21	0.33	0.9
StAM	0.63	3	0.21	0.32	0.9
ERROR	78.07	120	0.65		
VSt	0.17	1	0.17	0.32	0.6
VStA	0.00	1	0.00	0.00	1.0
VStM	0.20	3	0.07	0.12	1.0
VStAM	1.72	3	0.57	1.06	0.4
ERROR	64.88	120	0.54		

Table [D3.3] Between-pair Difference

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	336.47	1	336.47	597.84	0.0001
AGE	0.67	1	0.67	1.19	0.3
MODALITY	2.61	3	0.87	1.54	0.3
AM	1.36	3	0.45	0.81	0.5
ERROR	31.52	56	0.56		
VELOCITY	0.08	1	0.08	0.17	0.7
VA	0.19	1	0.19	0.38	0.6
VM	0.87	3	0.29	0.59	0.7
VAM	0.11	3	0.04	0.08	1.0
ERROR	27.30	56	0.49		

APPENDIX E : ANOVA AND MEAN TABLES FOR ANALYSES OF ABSOLUTE ERRORS

Interaction terms are designated by the initial letter or letters of the factors involved.

Table [E.1] Experiment Ala (Analysis One) - ANOVA Table

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	13355.91	1	13355.91	431.66	0.0001
AGE	1564.50	1	1564.50	50.56	0.0001
MODALITY	30.52	1	30.52	0.99	0.4
SEX	10.99	1	10.99	0.36	0.6
AM	35.60	1	35.60	1.15	0.3
ASe	59.81	1	59.81	1.93	0.2
MSe	74.27	1	74.27	2.40	0.2
AMSe	246.14	1	246.14	7.96	0.0095
ERROR	742.58	24	30.94		
STANDARD	2.39	1	2.39	0.07	0.8
StA	1.22	1	1.22	0.04	0.9
StM	35.60	1	35.60	1.06	0.4
StSe	74.27	1	74.27	2.22	0.2
StAM	169.97	1	169.97	5.08	0.0337
StASe	0.05	1	0.05	0.00	1.0
StMSe	158.64	1	158.64	4.74	0.0396
StAMSe	5.91	1	5.91	0.18	0.7
ERROR	803.52	24	33.48		
PROCEDURE	8.25	1	8.25	0.12	0.8
PA	74.27	1	74.27	1.04	0.4
PM	17.63	1	17.63	0.25	0.7
PSe	74.27	1	74.27	1.04	0.4
PAM	8.25	1	8.25	0.12	0.8
PASe	74.27	1	74.27	1.04	0.4
PMSe	0.44	1	0.44	0.01	1.0
PAMSe	117.24	1	117.24	1.64	0.3
ERROR	1714.45	24	71.44		
StP	30.52	1	30.52	0.74	0.4
StPA	3.96	1	3.96	0.10	0.8
StPM	46.92	1	46.92	1.14	0.3
StPSe	30.52	1	30.52	0.74	0.4
StPAM	21.53	1	21.53	0.52	0.5
StPASe	137.16	1	137.16	3.33	0.1
StPMSe	35.60	1	35.60	0.86	0.4
StPAMSe	1.22	1	1.22	0.03	0.9
ERROR	987.89	24	41.16		

Table [E.2] Experiment Ala (Analysis One) - Table of Means

		Standard Procedure	10 deg. 1	10 deg. 2	40 deg. 1	40 deg. 2
6 years	VV	M	13.13	11.88	8.13	9.38
		F	18.13	18.75	18.13	12.50
	KK	M	10.63	12.50	15.00	21.25
		F	12.50	12.50	18.13	6.88
Adult	VV	M	5.00	8.75	5.63	7.50
		F	6.25	1.25	4.38	6.88
	KK	M	4.38	8.13	9.38	8.13
		F	6.88	15.00	3.75	6.25

Table [E.3] Experiment Ala (Analysis Two) - ANOVA Table

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	13355.91	1	13355.91	431.66	0.0001
AGE	1564.50	1	1564.50	50.56	0.0001
MODALITY	30.52	1	30.52	0.99	0.4
SEX	10.99	1	10.99	0.36	0.6
AM	35.60	1	35.60	1.15	0.3
ASe	59.81	1	59.81	1.93	0.2
MSe	74.27	1	74.27	2.40	0.2
AMSe	246.14	1	246.14	7.96	0.0095
ERROR	742.58	24	30.94		
CUE	2.39	1	2.39	0.06	0.9
CA	5.91	1	5.91	0.16	0.7
CM	117.24	1	117.24	3.10	0.1
CSe	21.53	1	21.53	0.57	0.5
CAM	10.99	1	10.99	0.29	0.6
CASe	137.16	1	137.16	3.62	0.1
CMSe	74.27	1	74.27	1.96	0.2
CAMSe	17.63	1	17.63	0.47	0.6
ERROR	908.20	24	37.84		
PROCEDURE	8.25	1	8.25	0.12	0.8
PA	74.27	1	74.27	1.04	0.4
PM	17.63	1	17.63	0.25	0.7
PSe	74.27	1	74.27	1.04	0.4
PAM	8.25	1	8.25	0.12	0.8
PASe	74.27	1	74.27	1.04	0.4
PMSe	0.44	1	0.44	0.01	1.0
PAMSe	117.24	1	117.24	1.64	0.3
ERROR	1714.45	24	71.44		
CP	0.44	1	0.44	0.01	1.0
CPA	8.25	1	8.25	0.19	0.7
CPM	46.92	1	46.92	1.09	0.4
CPSe	46.92	1	46.92	1.09	0.4
CPAM	35.60	1	35.60	0.83	0.4
CPASe	5.91	1	5.91	0.14	0.8
CPMSe	0.05	1	0.05	0.00	1.0
CPAMSe	74.27	1	74.27	1.73	0.3
ERROR	1033.20	24	43.05		

Table [E.4] Experiment Ala (Analysis Two) - Table of Means

Age	Modality	Cue Procedure	Length 1	Length 2	Procedure 1	Procedure 2
6	VV	M	11.25	10.00	10.00	11.25
		F	16.25	18.75	20.00	12.50
	KK	M	10.63	11.88	15.00	21.88
		F	18.13	10.00	12.50	9.38
Adult	VV	M	6.88	11.25	3.75	5.00
		F	5.63	4.38	5.00	3.75
	KK	M	6.88	6.25	6.88	10.00
		F	2.50	10.63	8.13	10.63

Table [E.5] Experiment Ala (Analysis Three) - ANOVA Table

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	13355.91	1	13355.91	431.66	0.0001
AGE	1564.50	1	1564.50	50.56	0.0001
MODALITY	30.52	1	30.52	0.99	0.4
SEX	10.99	1	10.99	0.36	0.6
AM	35.60	1	35.60	1.15	0.3
ASe	59.81	1	59.81	1.93	0.2
MSe	74.27	1	74.27	2.40	0.2
AMSe	246.14	1	246.14	7.96	0.0095
ERROR	742.58	24	30.94		
CUE	2.39	1	2.39	0.06	0.9
CA	5.91	1	5.91	0.16	0.7
CM	117.24	1	117.24	3.10	0.1
CSe	21.53	1	21.53	0.57	0.5
CAM	10.99	1	10.99	0.29	0.6
CASe	137.16	1	137.16	3.62	0.1
CMSe	74.27	1	74.27	1.96	0.2
CAMSe	17.63	1	17.63	0.47	0.6
ERROR	908.20	24	37.84		
STANDARD	2.39	1	2.39	0.07	0.8
StA	1.22	1	1.22	0.04	0.9
StM	35.60	1	35.60	1.06	0.4
StSe	74.27	1	74.27	2.22	0.2
StAM	169.97	1	169.97	5.08	0.0337
StASe	0.05	1	0.05	0.00	1.0
StMSe	158.64	1	158.64	4.74	0.1
StAMSe	5.91	1	5.91	0.18	0.7
ERROR	803.52	24	33.48		
CSt	352.78	1	352.78	7.42	0.0118
CStA	41.06	1	41.06	0.86	0.4
CStM	35.60	1	35.60	0.75	0.4
CStSe	137.16	1	137.16	2.88	0.2
CStAM	46.92	1	46.92	0.99	0.4
CStASe	260.21	1	260.21	5.47	0.0280
CStMSe	0.05	1	0.05	0.00	1.0
CStAMSe	74.27	1	74.27	1.56	0.3
ERROR	1141.02	24	47.54		

Table [E.6] Experiments Ala and Alb - ANOVA Table

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	17754.35	1	17754.35	335.33	0.0001
EXPERIMENT	478.56	1	478.56	9.04	0.0061
MODALITY	53.17	1	53.17	1.00	0.4
SEX	74.27	1	74.27	1.40	0.3
EM	59.81	1	59.81	1.13	0.3
ESe	5.91	1	5.91	0.11	0.8
MSe	0.05	1	0.05	0.00	1.0
EMSe	601.61	1	601.61	11.36	0.0025
ERROR	1270.70	24	52.95		
CUE	2.39	1	2.39	0.05	0.9
CE	30.52	1	30.52	0.62	0.5
CM	46.92	1	46.92	0.96	0.4
CSe	127.00	1	127.00	2.59	0.2
CEM	0.44	1	0.44	0.01	1.0
CESe	25.83	1	25.83	0.53	0.5
CMSe	10.99	1	10.99	0.22	0.7
CEMSe	260.21	1	260.21	5.31	0.0301
ERROR	1175.39	24	48.97		
STANDARD	41.06	1	41.06	1.23	0.3
StE	35.60	1	35.60	1.07	0.4
StM	98.88	1	98.88	2.96	0.1
StSe	8.25	1	8.25	0.25	0.7
StEM	82.08	1	82.08	2.46	0.2
StESe	30.52	1	30.52	0.92	0.4
StMSe	107.86	1	107.86	3.23	0.1
StEMSe	0.05	1	0.05	0.00	1.0
ERROR	800.39	24	33.35		
CSt	459.42	1	459.42	6.87	0.0150
CStE	14.11	1	14.11	0.21	0.7
CStM	59.81	1	59.81	0.89	0.4
CStSe	538.33	1	538.33	8.05	0.0091
CStEM	25.83	1	25.83	0.39	0.6
CStESe	21.53	1	21.53	0.32	0.6
CStMSe	25.83	1	25.83	0.39	0.6
CStEMSe	10.99	1	10.99	0.16	0.7
ERROR	1605.08	24	66.88		

Table [E.7] Experiment Ala (Analysis Three) and Experiment Alb - Table of Means

			Cue:	Length		Position	
			Standard:	10 deg.	40 deg.	10 deg.	40 deg.
Experiment	Age	Modality	Sex				
Ala	6	VV	M	11.88	9.38	13.13	8.13
			F	21.88	13.13	15.00	17.50
		KK	M	8.13	14.38	15.00	21.88
			F	20.63	7.50	4.38	17.50
	Adult	VV	M	10.00	8.13	3.75	5.00
			F	5.63	4.38	1.88	6.88
		KK	M	7.50	5.63	5.00	11.88
			F	9.38	3.75	12.50	6.25
Alb	6	VV	M	11.25	6.88	13.75	8.75
			F	13.13	7.50	0.63	6.25
		KK	M	10.00	9.38	7.50	6.88
			F	18.13	7.50	13.13	16.88

Table [E.8] Experiment A2a - Table of Means (in degrees)

	Standard:	20				40			
Response	Movement:	30		70		30		70	
	Session:	1	2	1	2	1	2	1	2
Modality	Sex								
VV	M	4.38	4.38	6.88	3.13	9.38	10.63	11.88	6.25
	F	1.88	3.75	8.75	8.13	10.00	9.38	11.25	9.38
KK	M	6.25	4.38	6.25	5.63	6.88	1.88	10.63	4.38
	F	1.88	1.88	6.88	5.00	1.88	5.00	13.75	10.63

Table [E.9] Experiment A2a - ANOVA Table

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	5644.51	1	5644.51	229.06	0.0001
MODALITY	86.13	1	86.13	3.50	0.1
SEX	4.88	1	4.88	0.20	0.7
MSe	3.12	1	3.12	0.13	0.8
ERROR	295.70	12	24.64		
STANDARD	361.13	1	361.13	24.06	0.0001
StM	50.00	1	50.00	3.33	0.1
StSe	19.53	1	19.53	1.30	0.3
StMSe	33.01	1	33.01	2.20	0.2
ERROR	180.08	12	15.01		
RESP.MOVE	253.12	1	253.12	5.09	0.043
RM	56.45	1	56.45	1.14	0.4
RSe	122.07	1	122.07	2.46	0.2
RMSe	3.13	1	3.13	0.06	0.9
ERROR	596.48	12	49.71		
StR	0.20	1	0.20	0.01	1.0
StRM	94.53	1	94.53	4.15	0.1
StRSe	0.78	1	0.78	0.03	0.9
StRMSe	15.82	1	15.82	0.70	0.5
ERROR	273.05	12	22.75		
SESSION	78.12	1	78.12	4.39	0.06
SsM	4.88	1	4.88	0.27	0.7
SsSe	43.95	1	43.95	2.47	0.2
SsMSe	3.12	1	3.12	0.18	0.7
ERROR	213.67	12	17.81		
StSs	15.82	1	15.82	0.82	0.4
StSsM	0.78	1	0.78	0.04	0.9
StSsSe	7.03	1	7.03	0.37	0.6
StSsMSe	23.63	1	23.63	1.23	0.3
ERROR	230.86	12	19.24		
RSs	63.28	1	63.28	1.64	0.3
RSsM	4.88	1	4.88	0.13	0.8
RSsSe	0.20	1	0.20	0.01	1.0
RSsMSe	28.12	1	28.12	0.73	0.5
ERROR	462.89	12	38.57		
StRSs	9.57	1	9.57	0.50	0.5
StRSsM	3.12	1	3.12	0.16	0.7
StRSsSe	0.78	1	0.78	0.04	0.9
StRSsMSe	4.88	1	4.88	0.25	0.7
ERROR	231.64	12	19.30		

Table [E.10] **Experiment** A2b - ANOVA Table

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	10613.31	1	10613.31	129.24	0.0001
MODALITY	1092.64	2	546.32	6.65	0.0058
ERROR	1724.51	21	82.12		
STANDARD	3.94	1	3.94	0.06	0.9
StM	31.32	2	15.66	0.25	0.8
ERROR	1307.71	21	62.27		
RESP.MOVE	2.64	1	2.64	0.11	0.8
RM	31.84	2	15.92	0.65	0.6
ERROR	517.87	21	24.66		
StR	84.67	1	84.67	5.08	0.0351
StRM	4.88	2	2.44	0.15	0.9
ERROR	350.29	21	16.68		
SESSION	113.31	1	113.31	0.98	0.4
SsM	44.21	2	22.10	0.19	0.9
ERROR	2426.07	21	115.53		
StSs	20.35	1	20.35	1.67	0.3
StSsM	82.10	2	41.05	3.37	0.1
ERROR	256.15	21	12.20		
RSs	54.72	1	54.72	2.78	0.2
RSsM	30.53	2	15.27	0.77	0.5
ERROR	413.96	21	19.71		
StRSs	9.41	1	9.41	0.51	0.5
StRSsM	104.36	2	52.18	2.84	0.1
ERROR	385.45	21	18.35		

Table [E.11] Experiment A2b - Table of Means (in degrees)

Standard:	20				40			
Response Movement:	30		70		30		70	
Session:	1	2	1	2	1	2	1	2
Modality								
VV-HI	1.88	3.44	5.63	3.75	5.63	4.38	2.81	7.50
VV-LI	7.81	5.94	6.56	11.56	8.44	7.81	6.25	7.50
KK	7.50	12.50	7.81	13.13	11.25	11.25	8.44	9.69

Table [E.12] Experiment A3 - ANOVA Table

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	7439.06	1	7439.06	345.81	0.0001
AGE	6.25	1	6.25	0.29	0.6
MODALITY	525.78	3	175.26	8.15	0.0001
AM	74.22	3	24.74	1.15	0.4
ERROR	1204.69	56	21.51		
STANDARD	100.00	1	100.00	5.73	0.0201
StA	14.06	1	14.06	0.81	0.4
StM	65.63	3	21.88	1.25	0.3
StAM	17.97	3	5.99	0.34	0.8
ERROR	977.34	56	17.45		
SESSION	47.27	1	47.27	3.87	0.06
SsA	0.39	1	0.39	0.03	0.9
SsM	19.92	3	6.64	0.54	0.7
SsAM	30.08	3	10.03	0.82	0.5
ERROR	683.59	56	12.21		
StSs	6.25	1	6.25	0.46	0.6
StSsA	6.25	1	6.25	0.46	0.6
StSsM	25.78	3	8.59	0.63	0.6
StSsAM	39.84	3	13.28	0.97	0.5
ERROR	765.63	56	13.67		

Table [E.13] Experiment A3 - Table of Means

	Standard:	20		40	
	Session:	1	2	1	2
Age	Modality				
6	VV	4.06	5.63	4.69	5.31
	KK	5.31	5.94	5.94	10.94
	KV	4.38	5.00	4.06	3.13
	KK	3.75	4.69	5.94	5.00
9	VV	1.88	2.81	3.75	5.63
	VK	7.50	7.50	9.38	10.00
	KV	5.00	4.06	3.75	6.25
	KK	4.06	4.69	5.94	6.56

Table [E.14] Experiment A4 - ANOVA Table

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	18378.04	1	18378.04	437.45	0.0001
AGE	13.29	1	13.29	0.32	0.6
MODALITY	2515.47	3	838.49	19.96	0.0001
AM	56.97	3	18.99	0.45	0.8
ERROR	2352.64	56	42.01		
DELAY	2.75	1	2.75	0.12	0.8
DA	3.53	1	3.53	0.15	0.8
DM	13.81	3	4.60	0.19	1.0
DAM	31.97	3	10.66	0.45	0.8
ERROR	1329.98	56	23.75		
STANDARD	209.48	1	209.48	10.88	0.0017
StA	0.99	1	0.99	0.05	0.9
StM	264.88	3	88.29	4.59	0.0061
StAM	119.27	3	39.76	2.07	0.2
ERROR	1078.03	56	19.25		
DSt	11.73	1	11.73	0.77	0.4
DStA	13.29	1	13.29	0.87	0.4
DStM	95.06	3	31.68	2.08	0.2
DStAM	69.86	3	23.29	1.53	0.3
ERROR	854.59	56	15.26		
PAIR	16.71	1	16.71	1.52	0.3
PA	0.31	1	0.31	0.03	0.9
PM	31.29	3	10.43	0.95	0.5
PAM	13.12	3	4.37	0.40	0.8
ERROR	617.48	56	11.03		
DP	42.49	1	42.49	3.46	0.1
DPA	2.75	1	2.75	0.22	0.7
DPM	23.87	3	7.95	0.65	0.6
DPAM	7.17	3	2.39	0.19	0.9
ERROR	687.01	56	12.29		
StP	14.95	1	14.95	1.12	0.3
StPA	58.15	1	58.15	4.36	0.0414
StPM	5.31	3	1.77	0.13	1
StPAM	122.50	3	40.83	3.06	0.0354
ERROR	746.78	56	13.34		
DStP	8.90	1	8.90	0.64	0.5
DStPA	0.60	1	0.60	0.04	0.9
DStPM	14.10	3	4.70	0.34	0.8
DStPAM	47.60	3	15.87	1.14	0.4
ERROR	779.59	56	13.92		

Table [E.15] Experiment A4 - Table of Means

Delay:		Immediate				Delayed			
Standard:		20		40		20		40	
Pair of Trials:		1	2	1	2	1	2	1	2
Age	Modality								
6	VV	4.69	4.69	4.69	5.00	3.13	2.81	5.31	4.06
	VK	6.25	5.00	12.50	14.69	8.44	7.19	11.25	9.38
	KV	5.63	5.31	5.31	6.25	8.44	6.25	3.75	5.94
	KK	6.88	4.06	4.06	5.94	5.63	3.13	5.00	6.25
9	VV	4.69	3.13	1.88	5.94	2.81	2.50	4.38	4.38
	VK	7.81	9.38	10.94	10.31	10.63	8.44	12.19	10.31
	KV	5.31	6.56	5.63	6.88	5.00	5.31	6.56	4.06
	KK	2.50	2.50	6.56	3.13	3.44	3.75	6.25	3.44

Table [E.16] Experiment A5 - Table of Means

Standard:		20 deg.				40 deg.			
Session:		1		2		1		2	
Pair of Trials:		1	2	1	2	1	2	1	2
Age									
6		2.81	3.44	4.38	3.13	1.56	3.75	3.13	2.50
		5.00	5.31	4.69	5.31	4.38	8.44	4.69	4.38
		6.25	6.25	7.81	11.88	8.13	8.44	10.00	10.00
		6.88	5.94	5.63	8.75	4.06	5.94	3.75	4.06
9		1.88	2.50	3.13	2.81	3.75	2.19	1.56	2.50
		7.50	8.75	5.94	4.69	9.38	10.94	6.88	5.94
		4.38	6.56	5.63	7.81	7.19	7.50	8.44	6.56
		4.69	5.31	5.00	6.56	2.50	4.38	5.94	5.63

Table [E.17] Experiment A5 - ANOVA Table

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	15753.13	1	15753.13	295.10	0.0001
AGE	4.88	1	4.88	0.09	0.8
MODALITY	1634.47	3	544.82	10.21	0.0001
AM	283.69	3	94.56	1.77	0.2
ERROR	2989.45	56	53.38		
STANDARD	0.44	1	0.44	0.02	0.9
StA	25.83	1	25.83	1.42	0.3
StM	160.01	3	53.34	2.93	0.0416
StAM	8.25	3	2.75	0.15	1.0
ERROR	1021.09	56	18.23		
SESSION	1.22	1	1.22	0.05	0.9
SsA	17.63	1	17.63	0.68	0.5
SsM	253.37	3	84.46	3.25	0.0284
SsAM	79.35	3	26.45	1.02	0.4
ERROR	1454.69	56	25.98		
StSs	33.01	1	33.01	2.51	0.2
StSsA	7.03	1	7.03	0.54	0.5
StSsM	8.50	3	2.83	0.22	0.9
StSsAM	22.17	3	7.39	0.56	0.7
ERROR	735.55	56	13.13		
PAIR	56.45	1	56.45	4.45	0.0394
PA	7.03	1	7.03	0.55	0.5
PM	16.70	3	5.57	0.44	0.8
PAM	3.42	3	1.14	0.09	1.0
ERROR	710.16	56	12.68		
StP	3.96	1	3.96	0.25	0.7
StPA	8.25	1	8.25	0.52	0.5
StPM	49.85	3	16.62	1.04	0.4
StPAM	1.61	3	0.54	0.03	1.0
ERROR	892.58	56	15.94		
SsP	10.99	1	10.99	1.11	0.3
SsPA	2.39	1	2.39	0.24	0.7
SsPM	36.96	3	12.32	1.24	0.4
SsPAM	42.24	3	14.08	1.42	0.3
ERROR	554.30	56	9.90		
StSsP	38.28	1	38.28	3.19	0.1
StSsPA	23.63	1	23.63	1.97	0.2
StSsPM	35.64	3	11.88	0.99	0.5
StSsPAM	2.44	3	0.81	0.07	1.0
ERROR	671.88	56	12.00		

Table [E.18] Experiment B2 - ANOVA Table

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	5729.89	1	5729.89	148.95	0.0001
ERROR	653.96	17	38.47		
RESISTANCE	9.78	2	4.89	0.19	0.9
ERROR	864.18	34	25.42		
STANDARD	31.51	1	31.51	3.29	0.1
ERROR	162.76	17	9.57		
RSt	8.51	2	4.25	0.33	0.8
ERROR	442.53	34	13.02		
SESSION	12.76	1	12.76	0.47	0.6
ERROR	462.76	17	27.22		
RSs	48.09	2	24.05	2.00	0.2
ERROR	409.20	34	12.04		
StSs	75.26	1	75.26	3.55	0.1
ERROR	360.68	17	21.22		
RStSs	17.88	2	8.94	0.59	0.6
ERROR	516.49	34	15.19		

Table [E.19] Experiment B2 - Table of Means

Standard:	20 deg.	40 deg.	20 deg.	40 deg.
Session:	1	2	1	2
Resistance Condition				
Control:	4.86	5.42	5.14	4.58
Resistance:	5.14	6.53	5.42	4.72
Stop:	5.56	5.69	6.25	2.50

Table [E.20] Experiment C1 - ANOVA Table

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	3454.88	1	3454.88	181.36	0.0001
ERROR	285.74	15	19.05		
PAIR	7.03	1	7.03	0.48	0.5
ERROR	217.97	15	14.53		
STANDARD	7.03	1	7.03	0.26	0.7
ERROR	408.59	15	14.47		
PS	1.76	1	1.76	0.12	0.8
ERROR	216.99	15	14.47		
LENGTH	112.50	1	112.50	7.40	0.0158
ERROR	228.13	15	15.21		
PL	0.20	1	0.20	0.01	1.0
ERROR	231.05	15	15.40		
SL	1.76	1	1.76	0.07	0.8
ERROR	388.87	15	25.92		
PSL	0.00	1	0.00	0.00	1.0
ERROR	162.50	15	10.83		

Table [E.21] Experiment C1 - Table of Means

Standard:	20 deg.				40 deg.			
Movement Length:	30 deg.		70 deg.		30 deg.		70 deg.	
Pair:	1	2	1	2	1	2	1	2
	3.75	4.53	5.47	6.09	4.22	4.53	6.41	6.56

Table [E.22] Experiment C2 - Tables of Means

Standard:	20 deg.				40 deg.			
Movement Length:	30 deg.		70 deg.		30 deg.		70 deg.	
Pair:	1	2	1	2	1	2	1	2
	8.13	6.56	10.94	7.97	5.78	9.69	6.09	5.63

Table [E.23] Experiment C2 - ANOVA Table

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	7388.72	1	7388.72	153.96	0.0001
ERROR	719.87	15	47.99		
STANDARD	82.08	1	82.08	1.89	0.2
ERROR	651.51	15	43.43		
LENGTH	0.44	1	0.44	0.02	0.9
ERROR	298.78	15	19.92		
StL	127.00	1	127.00	3.98	0.1
ERROR	478.47	15	31.90		
SESSION	2.39	1	2.39	0.06	0.9
ERROR	578.08	15	38.54		
StSs	127.00	1	127.00	7.16	0.0173
ERROR	265.97	15	17.73		
LSs	66.85	1	66.85	1.41	0.3
ERROR	710.50	15	47.37		
StLSs	17.63	1	17.63	0.91	0.4
ERROR	290.97	15	19.40		

Table [E.24] Experiment C3 - ANOVA Table

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	2413.20	1	2413.20	57.05	0.0001
MODALITY	0.11	1	0.11	0.00	1.0
ERROR	549.93	13	42.30		
STANDARD	4.98	1	4.98	0.19	0.7
SM	31.08	1	31.08	1.18	0.3
ERROR	342.70	13	26.36		
PAIR	73.38	3	24.46	2.02	0.2
PM	1.40	3	0.43	0.04	1.0
ERROR	471.07	39	12.08		
SP	55.84	3	18.61	1.18	0.4
SPM	9.29	3	3.10	0.20	0.9
ERROR	616.56	39	15.81		

Table [E.25] Experiment C3 - Table of Means

Standard:	20 deg.				40 deg.			
Pair of Trials:	1	2	1	2	1	2	1	2
Modality Condition								
V	2.45	5.31	5.31	6.25	4.06	5.00	4.06	3.75
V_I	1.43	3.93	5.36	4.29	4.64	6.07	4.64	5.36

Table [E.26] Experiment D1 - ANOVA Table

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	21076.61	1	21076.61	503.36	0.0001
AGE	657.03	1	657.03	15.69	0.0002
ERROR	2596.04	62	41.87		
MODALITY	260.21	1	260.21	7.94	0.0065
MA	0.78	1	0.78	0.02	0.9
ERROR	2031.20	62	32.76		
VELOCITY	0.20	1	0.20	0.00	1.0
VA	35.60	1	35.60	0.91	0.4
ERROR	2437.65	62	39.32		
MV	0.20	1	0.20	0.01	1.0
MVA	14.11	1	14.11	0.53	0.5
ERROR	1652.88	62	26.66		
STANDARD	903.13	1	903.13	38.93	0.0001
StA	25.83	1	25.83	1.11	0.3
ERROR	1438.23	62	23.20		
MSt	33.01	1	33.01	1.32	1.3
MStA	8.25	1	8.25	0.33	0.6
ERROR	1544.68	62	24.91		
VSt	17.63	1	17.63	0.96	0.4
VStA	7.03	1	7.03	0.38	0.6
ERROR	1142.53	62	18.43		
MVSt	41.06	1	41.06	2.12	0.2
MVStA	0.20	1	0.20	0.01	1.0
ERROR	1200.93	62	19.37		

Table [E.27] Experiment D1 - Table of Means

Modality:	KK				VV			
Velocity:	Fast		Slow		Fast		Slow	
Standard:	20	40	20	40	20	40	20	40
Age								
6	6.09	10.70	6.64	9.77	6.33	8.20	4.92	7.73
9	4.06	7.66	5.39	6.72	3.20	5.23	4.06	5.94

Table [E.28] Experiment D2 - ANOVA Table

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	14259.38	1	14259.38	368.95	0.0001
AGE	40.69	1	40.69	1.05	0.4
MODALITY	211.07	3	70.36	1.82	0.2
AM	37.04	3	12.35	0.32	0.9
ERROR	2164.32	56	38.65		
STANDARD	283.59	1	283.59	12.15	0.001
StA	1.63	1	1.63	0.07	0.8
StM	273.31	3	91.10	3.90	0.0133
StAM	38.61	3	12.87	0.55	0.7
ERROR	1307.03	56	23.34		
PAIR	32.81	2	16.41	1.16	0.4
PA	3.65	2	1.82	0.13	0.9
PM	52.60	6	8.77	0.62	0.8
PAM	191.67	6	31.94	2.25	0.0432
ERROR	1588.02	112	14.18		
StP	63.67	2	31.84	2.10	0.2
StPA	2.47	2	1.24	0.08	1.0
StPM	109.51	6	18.25	1.20	0.4
StPAM	90.49	6	15.08	0.99	0.5
ERROR	1698.44	112	15.16		

Table [E.29] Experiment D2 - Table of Means

	Standard:	20 deg.			40 deg.		
	Pair of Trials:	1	2	3	1	2	3
Age	Modality						
6	VV	4.06	7.81	5.31	4.06	5.00	6.25
	VK	5.31	5.31	6.56	7.81	11.25	8.75
	KV	5.94	5.31	5.63	6.56	5.94	4.38
	KK	7.19	5.63	3.44	6.88	8.44	11.25
9	VV	4.69	4.06	3.13	4.06	6.56	5.63
	VK	7.19	4.38	4.69	8.44	6.56	8.13
	KV	4.69	6.56	7.50	3.75	6.56	6.88
	KK	4.69	5.00	1.56	7.19	9.38	7.19

Table [E.30] Experiment D3 - ANOVA Table

Source	Sum of Squares	d.f.	Mean Square	F	p<
MEAN	28450.20	1	28450.20	940.72	0.0001
AGE	55.08	1	55.08	1.82	0.2
MODALITY	951.04	3	317.01	10.48	0.0001
AM	137.18	3	45.73	1.51	0.3
ERROR	3629.15	120	30.24		
VELOCITY	0.15	1	0.15	0.01	1.0
VA	0.30	1	0.30	0.01	1.0
VM	40.74	3	13.58	0.63	0.6
VAM	146.84	3	48.95	2.27	0.1
ERROR	2590.87	120	21.59		
STANDARD	42.05	1	42.05	1.77	0.2
StA	18.46	1	18.46	0.78	0.4
StM	402.85	3	134.28	5.64	0.0012
StAM	71.75	3	23.92	1.00	0.4
ERROR	2856.30	120	23.80		
VSt	3.23	1	3.23	0.23	0.7
VStA	0.49	1	0.49	0.03	0.9
VStM	31.12	3	10.37	0.73	0.6
VStAM	99.48	3	33.16	2.34	0.1
ERROR	1700.83	120	14.17		
PAIR	24.22	1	24.22	2.02	0.2
PA	1.03	1	1.03	0.09	0.8
PM	51.63	3	17.21	1.44	0.3
PAM	53.92	3	17.97	1.50	0.3
ERROR	1435.60	120	11.96		
VP	5.13	1	5.13	0.44	0.6
VPA	15.88	1	15.88	1.36	0.3
VPM	47.87	3	15.96	1.37	0.3
VPAM	89.28	3	29.76	2.55	0.1
ERROR	1402.00	120	11.68		
StP	1.37	1	1.37	0.12	0.8
StPA	3.81	1	3.81	0.34	0.6
StPM	24.77	3	8.26	0.74	0.6
StPAM	18.23	3	6.08	0.55	0.7
ERROR	1336.96	120	11.14		
VStP	17.14	1	17.14	2.03	0.2
VStPA	1.76	1	1.76	0.21	0.7
VStPM	3.14	3	1.05	0.12	1.0
VStPAM	169.11	3	56.37	6.68	0.0003
ERROR	1012.74	120	8.44		

Table [E.31] Experiment D3 - Table of Means

Velocity:		Equal				Fast			
Standard:		20 deg.		40 deg.		20 deg.		40 deg.	
Pair of Trials:		1	2	1	2	1	2	1	2
Age	Modality								
6	VV	4.84	4.53	3.75	5.78	4.22	5.00	3.28	3.59
	VK	5.47	5.47	7.97	9.53	4.38	7.03	8.75	7.34
	KV	5.63	5.16	5.63	4.22	5.00	6.56	4.69	5.63
	KK	5.16	5.31	5.00	4.53	5.47	3.44	6.88	6.88
9	VV	2.50	3.91	2.50	3.44	3.44	3.59	3.91	4.38
	VK	4.38	5.16	6.88	5.47	5.63	6.72	5.94	8.28
	KV	8.75	8.13	4.84	5.94	7.03	4.69	5.63	3.91
	KK	3.91	3.91	3.75	7.66	3.48	4.38	5.16	4.06

APPENDIX F: THE EFFECT OF USING AN ARC MOVEMENT

INTRODUCTION

In the cross-modal matching experiments that are reported in this thesis, it is a consistent finding that undershooting is greater in the VK condition than in the KV condition. Except when differential biasing factors were deliberately introduced, VV does not differ from KK, and both are intermediate between VK and KV.

Attempts to explain this result were reported in Section [4.B]. This appendix gives a more detailed account of one possible explanation which was dealt with briefly in that section. Specifically, the following hypothesis is to be tested:

(i) When an arc movement is perceived kinaesthetically, the length of the movement is judged to be the distance moved around the arc.

(ii) When an arc movement is perceived visually, the length of the movement is judged to be the linear distance between the start and end-points of the movement.

(iii) Therefore a given arc movement is judged to be longer when it is perceived kinaesthetically than when it is perceived visually.

This hypothesis would explain the effect of modality on constant errors in the following way. In the VK condition, the length of the acquisition movement is underestimated, so that the subject does not allow the lever to move far enough during response. In the KV condition, the length of the response movement is underestimated, so that the subject allows the lever to move too far. In the within-modal conditions, errors cancel out. A quite separate source of bias must be hypothesised to account for the result that there is an overall bias to undershoot in all four conditions.

Because the hypothesis predicts no bias in the within-modal conditions, it is possible to predict what the bias should be in each cross-modal condition, relative to the bias in the within-modal conditions. The magnitude of the predicted bias would depend on the subject's precise strategy. Three models are described below, which can be used to predict bias assuming three possible encoding strategies. Models A and B assume that the subject encodes positions relative to his midline, this assumption being supported by the results of the experiments reported. Model C is given for completeness, and is based on a more complex, generally unsupported, hypothesis. Following the description of the three models, their predictions are tested.

MODEL A

Refer to Figure [F.A].

In this model, it is assumed that:

The distance encoded visually = k

The distance encoded kinaesthetically = a

It can be stated that $k = 2r\sin(B/2)$

and that $a = 2\pi rB/360$

$= \pi rB/180$

In the VK condition, the subject encodes the distance k, then during response allows the lever to move a distance k around the arc from the midline.

The angle made by this movement is given by $180k/r$.

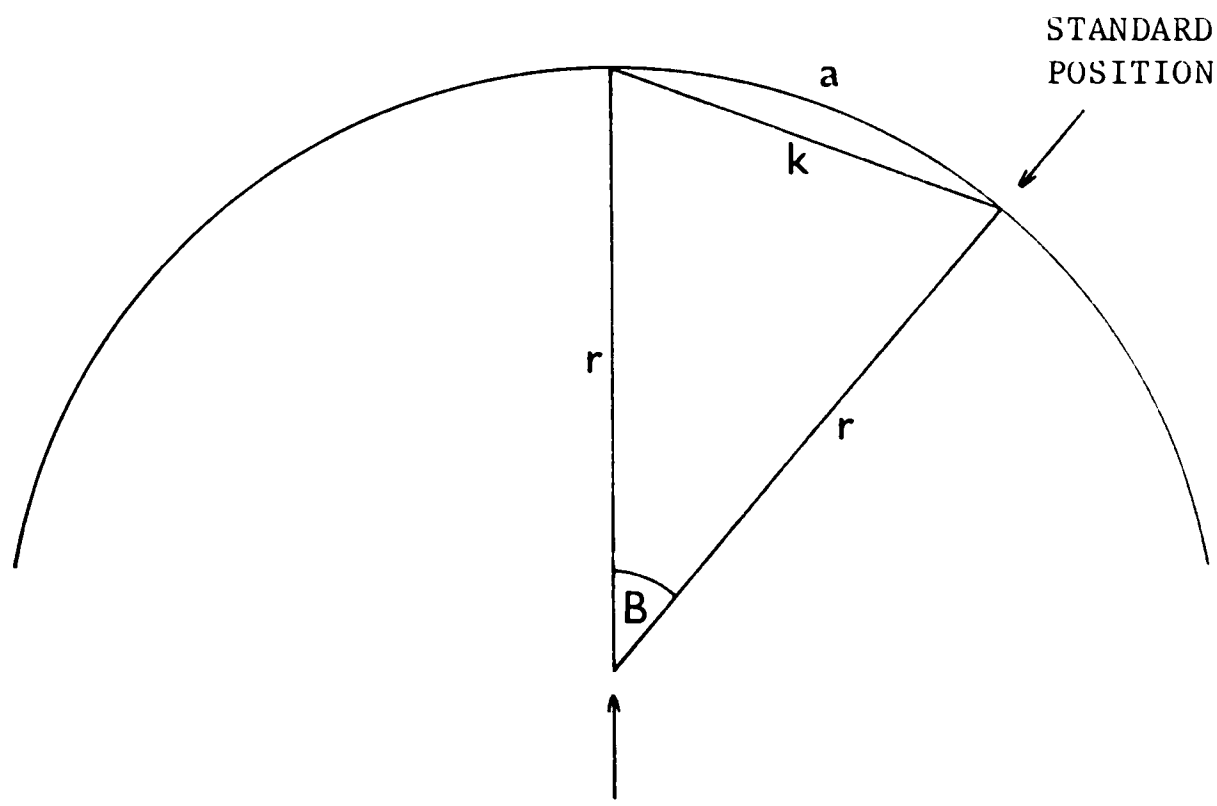
The expected angular bias, b, is therefore given by:

$b = 180k/\pi r - B$

$= 180[2r\sin(B/2)]/\pi r - B$

$= 360\sin(B/2)/\pi - B$

Figure [F.A] Diagram for Model A



In the KV condition, the subject encodes the distance a, then during response allows the lever to move a linear distance a.

The angle made by this movement is given by 2arcsin(a/2r).

The expected angular bias, b, is therefore given by:

$$b = 2\arcsin(a/2r) - B$$
$$= 2\arcsin(\pi rB/360r) - B$$
$$= 2\arcsin(\pi B/360) - B$$

MODEL B

Refer to Figure [F.B].

In this model, it is assumed that:

The distance encode visually = x

The distance encoded kinaesthetically = a

It can be stated that $x = r\sin B$

and that $a = \pi rB/180$

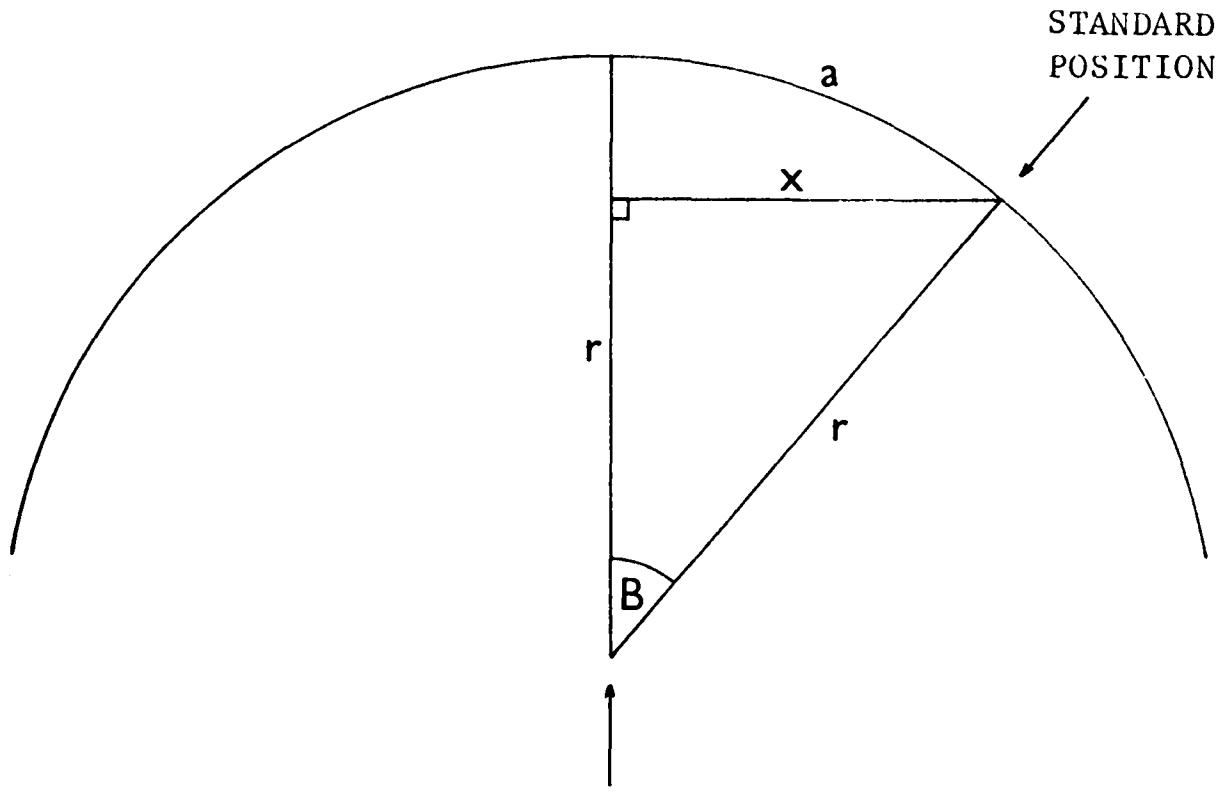
In the VK condition, the subject encodes the distance x, then during response allows the lever to move a distance x around the arc from the midline.

The angle made by this movement is given by $180x/\pi r$.

The expected bias, b, is therefore given by:

$$b = 180x/\pi r - B$$
$$= 180r\sin B/\pi r - B$$
$$= 180\sin B/\pi - B$$

Figure [F.B] Diagram for Model B



MIDLINE

In the KV condition, the subject encodes the distance a, then allows the lever to move a linear distance a.

The angle made by this movement is given by $\arcsin(a/r)$.

The expected bias, b, is therefore given by:

$$b = \arcsin(a/r) - B$$

$$= \arcsin(\pi rB/180r) - B$$

$$= \arcsin(\pi B/180) - B$$

MODEL C

Refer to Figure [F.C]

In this model, it is assumed that

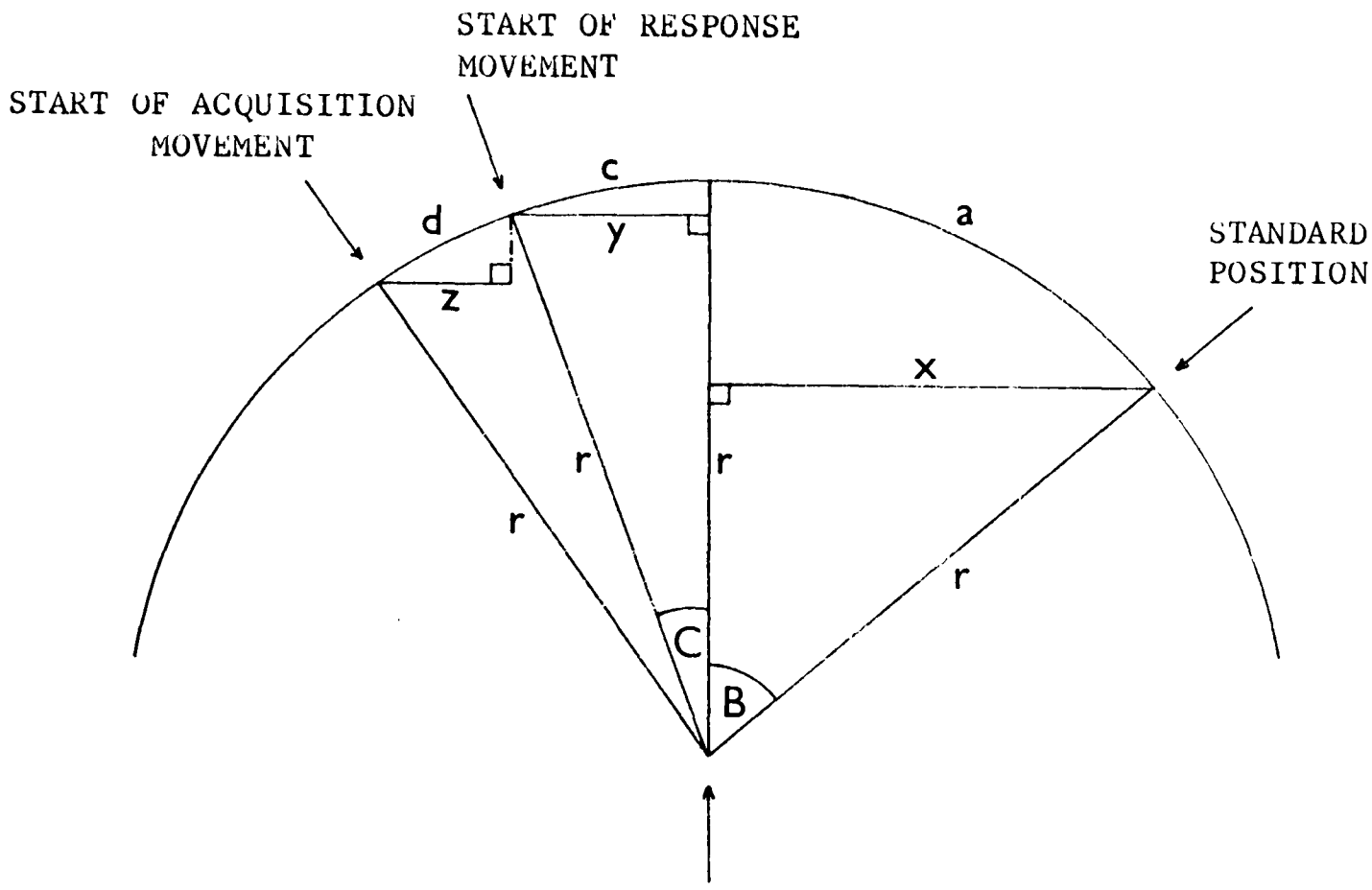
The distance encoded visually = $w = x + y + z$

The distance encoded kinaesthetically = $s = a + c + d$

The basic formulas required to derive formulas for expected bias are the same as those for Model B. The main difference between the two models, is that in Model B the subject is assumed to code the standard position relative to his midline, whereas in Model C it is assumed that coding is relative to the starting position of the acquisition movement.

No hypothesis is offered as to how the subject might code relative to the starting position. The model is presented for the sake of completeness, to show that the distance of the standard position from the midline is relevant, even if the subject does not actually code relative to the midline.

Figure [F.C] Diagram for Model C



MIDLINE

In the VK condition, the subject encodes the distance w, then during response allows the lever to move a distance (w - z) around the arc. The subject must be supposed to correct by an amount z, not d, because to correct by an amount d would require the ability to convert linear distances to equivalent angular distances, and it is an axiom of the model that subjects do not do this.

The angle made by a movement of (w - z) around the arc is given by $180(w - z)/\pi r$.

The expected angular bias, b, is therefore given by:

$$b = 180(w - z)/\pi r - (B + C)$$
$$= 180(x + y)/\pi r - (B + C)$$
$$= 180(r\sin B + r\sin C)/\pi r - (B + C)$$
$$= 180(\sin B + \sin C)/\pi - (B + C)$$

(the angle C must be given a negative sign if it is on the same side of the midline as B).

In the KV condition, the subject encodes the distance s, then during response allows the lever to move a distance (s - d) from the starting point of the response movement. The distance from the midline will then be (s - d - y) = (a + c - y), and the angle made with the midline will be given by $\arcsin[(a + c - y)/r]$.

The expected angular bias, b, is therefore given by:

$$b = \arcsin[(a + c - y)/r] - B$$
$$= \arcsin[(\pi rB/180 + \pi rC/180 - r\sin C)/r] - B$$
$$= \arcsin(\pi(B + C)/180 - \sin C) - B$$

(as with VK, if the angle C is on the same side of the midline as the angle B, C must be given a negative sign).

DO THE MODELS FIT THE DATA?

Table [F.1] shows the biases predicted by the three models for the two standard positions used (see Section [3.A2a.2]). Two general features of the predictions can be pointed out.

Table [F.1] Predicted bias under models A, B and C (in degrees)

MODALITY	STANDARD	MODEL A	MODEL B	MODEL C
VK	20 deg.	-2.07	-2.29	-3.33
	40 deg.	-4.59	-6.32	-5.29
	Mean	-3.33	-4.31	-4.31
KV	20 deg.	2.34	2.70	3.93
	40 deg.	5.40	9.20	7.61
	Mean	3.87	5.95	5.77

First, the predicted bias is greater for the 40 degree standard than for the 20 degree standard, in all three models. The data from Experiments A3, A4 and D3 ("Equal" condition), give mean biases of 4.17 and 3.47 degrees, with the 20 and 40 degree standards respectively. This is a difference in the opposite direction to the predicted difference. In none of the three experiments is there a significant Modality x Standard interaction, due to a greater difference between VK and KV with the 40 degree standard.

Second, the bias predicted for KV is greater than the bias predicted for VK, in all three models. The data from the experiments noted above gives mean biases of -4.30 and 3.35 degrees for VK and KV respectively. Again, the actual difference is in the opposite direction to the predicted difference.

The evidence is therefore against the hypothesis which was set out in the introduction to this appendix.

APPENDIX G: SUPPLEMENTARY ANALYSIS OF EXPERIMENT B3

The exact numerical predictions about the difference between visual and kinaesthetic psychophysical scaling judgements are not applicable, since the hypothetical constant difference between visual and kinesthetic estimates is only one factor in the determination of the functions. Nevertheless, it is possible to examine a model based only on this factor, in order to assess the relative magnitude of the effect of other factors.

It will be recalled that kinaesthetically perceived lengths are hypothesised to be overestimated by a constant amount k. From the equations and results given in Experiment B3, the value of k is estimated to be 1.5 degrees.

Because the standard length (10 degrees) was given the value 10, according to the hypothesis visual length judgements given by the subject should be numerically equal to the perceived length of the comparison movement, m (i.e. $10m/10 = m$). Kinaesthetic length judgements should take the value $10(m+k)/(10+k)$. If k is assumed to have the value 1.5, then

$$10(m+k)/(10+k) = (10m + 15)/11.5 .$$

For each comparison movement, Table [G1] gives the predicted visual and kinaesthetic estimates, and the difference between these estimates. The difference increases with m slightly nore steeply than expected, and the slope of both the functions is less than predicted (the actual results are shown in Table [4.B3.1], page 217). This confirms that the psychophysical judgements are affected by factors other than the constant error k.

Table [G1] Predicted estimates (in degrees)

Comparison movement	PREDICTED ESTIMATE Visual	Kinaesthetic	Difference
5	5	5.65	-0.65
10	10	10.00	0.00
15	15	14.35	0.65
20	20	18.70	1.30
25	25	23.04	1.96
30	30	27.39	2.61
35	35	31.74	3.26
40	40	36.09	3.91
45	45	40.43	4.57
50	50	44.78	5.22