AID FOR THE TALKING DEAF

by

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ABSTRACT

Part One, containing two chapters, presents a discussion of the educational controversy: Oralism versus Total communication. It is suggested that the educational problems of the deaf cannot be solved satisfactorily by an approach which unduly stresses the need for good articulation. A brief exposition of an original model of thought is included to indicate that speech is not fundamentally important for deaf children, and that sign language will provide the means for acquiring language, thought and sociability. The need for good articulation is given a context by these chapters, and included in Chapter Two is an explanation of the difficulties the deaf have in acquiring articulation.

Part Two of the thesis presents some empirical work, in three chapters. The first of these has associated with it two appendices. The material of Chapter Three and the appendices, working from the proposition that vibrotactile sensations will be more useful to the deaf than visual indications of speech, shows that a novel form of stimulation can be considered for use in a prototype speech training aid. The inadequacy of the previous attempts at providing vibrotactile stimulation is explained, it is suggested, by the lack of considerations of what might constitute 'natural' tactile stimulation.

Chapter Four presents a description of a prototype aid incorporating a novel speech processor and the means of stimulation discussed in the preceeding chapter. In Chapter Five the use of the aid is described and its usefulness is assessed. This chapter also includes criticism of the aid and the conclusion reached is that despite several discovered drawbacks the aid is promising.

Part Three, a single chapter, presents some general conclusions. In particular, the implications of the discussions presented in Part One and the methodological point arising out of the use of only one subject, (Part Two), are discussed.

Chelm is a land which, according to Yiddish folklore, was accidentally peopled with fools and innocents.

Someone saw a Chelmite writing a letter in an unusually large hand and asked, "Why such huge letters?"

"I am writing to my uncle, who is -- may you be spared the like -- very deaf."

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INTRODUCTION

The work reported in this thesis concerns the training of the deaf to speak. Deafness is used here to mean a degree of hearing loss so great that interest in the aural world is absent. The deaf themselves tend to use two terms, 'deaf' and 'partially hearing', which indicate a qualitative difference between people so afflicted. The fact that the difference is qualitative and not quantitative is, in my opinion, at the root of much confusion and controversy in the education of the deaf.

The number of deaf people in Great Britain, in the above sense, is unknown, but from figures issued by the Royal National Institute of the Deaf, (R.N.I.D., 1972), an estimate is possible of the number of people who have too little hearing to enable them to hear speech. The number is approximately 120,000, (the R.N.I.D. figures are themselves estimates). There are in addition 5,000 deaf children in special schools, not counting those in Partially Hearing units.

My interest in this type of work grew out of a few chance encounters with deaf and partially hearing people, and condensed in a resolve to do something to help the deaf with their problem, 'lack of speech'. Like most hearing people who encounter deafness I did not understand the problem, and I now know it to be a great deal more than just 'lack of speech'.

I decided to build a speech training aid utilizing vibrotactile sensations, as I felt this approach to be suitable. Since that decision, a correct one I believe, I have become aware of what I consider to be the real problem for deaf people. This is the lack of provision, in the education system, of the appropriate means for communication and thus personal development. I consider that integration in the hearing world is only partially possible and that it is ridiculous to structure the education of the deaf with this aim of 'integration' in mind. It is surely necessary to educate the deaf as individuals, as people, and to concentrate on the full development of a person and the realisation of the potential of that person.

Equipping a deaf person with speech is a secondary task to be accomplished as quickly and efficiently as possible; it should not be the time-consuming goal of the education of the deaf. Integration with the hearing world will take place, to whatever degree is possible, only when the people involved have a well developed personality and the ability and desire to communicate fluently with others like them. To be sociable a deaf person must be a social being and he cannot become a full social being if he has been brought up in an aural/oral world of little interest or relevance to him. It is not surprising that deaf people so brought up find speech and lip-reading tiring, frustrating and irrelevant, and the aural/oral world a very foreign place.

The first part of this thesis expands this argument formally and contains an exposition of the current controversy surrounding 'oral' versus 'Total communication' as the educational method. This part of the thesis contains a brief exposition of an original model of the perceptual/thought processes in man. It is pointed out that a formal language such as English is not fundamentally important for the development of personality or the acquisition of social skills.

Speech training assumes a diminished role in such an appraisal of the educational needs of the deaf but it is not irrelevant. In Part 2 of the thesis I present the psychophysical work I have done on the development of a novel form of stimulus, (this is an original contribution to the psychophysical study of vibrotactile sensitivity). This stimulus has been incorporated in a prototype speech training aid which is of original design.

The psychophysical work and the work with the training aid have involved one profoundly deaf woman as a subject. Both from a theoretical point of view, (concerning experimental method in psychological work), and a practical point of view, (very young children are physiologically better equipped to acquire speech), this programme is of limited scope.

The third part of the thesis takes up this point and includes a brief discussion of the ethical and methodological problems of developing and testing experimental educational equipment. This part of the thesis contains some general conclusions.

The work I have done is not complete. There is a need for both theoretical and practical development of the various ideas presented, including the development of a prototype instrument for use in schools.

I should warn the reader that this thesis is a personal statement, the 'first person' is used extensively throughout. I have tried to use the 'third person' wherever I feel that the material discussed or presented is to be interpreted as 'objective' or 'scientific'. The 'first person' is used to convey the impression of personal involvement and a degree of subjectivity which would not permit the use of the impersonal pronoun.

PART 1

PART 1

This part of the thesis serves to set the context for my attempt to help the deaf educationally.

Chapter 1 presents the controversy surrounding the use of sign language in education. No discussion of the educational problems of the deaf would be complete without such a discussion. I am in favour of the use of Total communication in the education of the deaf.

The second chapter is largely devoted to a necessarily brief discussion of thought processes and the relation between language and thinking. I introduce a model of perception and thinking which is, I believe, novel. The conclusion in Chapter 1, that a formal language such as English is not essential for intellectual development is supported in Chapter 2, where it is suggested that there are theoretical grounds for considering sign language to be suitable for the acquisition of mental and social skills. This chapter also contains a brief statement of the nature of the problem, for a deaf person, of speech acquisition.

CHAPTER 1

In this first chapter I explain the educational controversy surrounding the use of sign language. The first section, (1 a).), outlines, very briefly, the history of the controversy and presents the oralist approach and criticisms of it. The second section, (1 b).), contains an explanation of Total communication and presents, briefly, the evidence in support of it.

I show that speech acquisition, although important, is not as important as the personal development of the deaf.

1 a).

People often indulge in speculation, either privately or with others, about what it must be like to be deaf or blind. Such speculation is frequently sparked off by a particular incident, such as the sight of a blind person 'coping magnificently' with public transport, perhaps with the aid of his 'blind-dog', (or rather, 'guide-dog'). People marvel at the mobility and courage of the blind, and when they encounter a blind person they are willing to help and offer the odd word of praise.

It is easy to imagine blindness, closing one's eyes is sufficient to provide a dramatic personal illustration 'of what it must be like', although surely an inadequate one. Stumbling around in the darkness of a power-cut gives another illustration, again inadequate because of the possession of prior knowledge acquired through sight, and because complete darkness is very rarely encountered.

With deafness there are no 'earlids' to aid the imagining. Some speculators about sensory handicaps appreciate that deafness must be very isolating but such awareness is rarely profound enough to afford the sort of understanding possessed by some parents, (and teachers), of deaf children.

It is so very difficult to understand deafness because we, as hearing people, cannot divorce ourselves from our personal histories of communication, (i.e. education and society). In his 'keynote' address to a conference Franklin S. Cooper,(1971), remarked that "blindness primarily hinders communication with the environment while deafness hinders communication with people." That this is not the whole story I will show in the remainder of this chapter.

The important thing to note about speculative comparisons between blindness and deafness is that in them blindness is defined by shutting one's eyes. Deafness is not as explicitly defined and is assumed to be 'shutting one's ears' in some strange way. Discussants will perhaps refer to putting fingers in their ears or to the partial and temporary deafness which sometimes accomapnies a heavy cold. What then should be the definition of deafness?

Textbooks which deal with deafness exclusively, (for example Ballantyne, 1970), define deafness in terms of hearing loss in audiometric tests, the most sophisticated of which provide some measure of the ability to understand speech. Texts with a more medical bias, (e.g. Fisch 1964), contain definitions in terms of 'impaired function' or 'prevention of a stimulus reaching the auditory cortex', and discussions of the causes and prevention of 'conductive' and 'perceptive' deafness, (i.e. deafness caused by outer or middle ear malfunction and that caused by inner ear or nerve malfunction).

A socially more profound definition is that offered by Vernon, (1969),

> "A person is educationally and socially deaf when he cannot understand conversational speech in most situations and when the onset of the hearing loss was prelingual or early in life."

This definition brings out the social and educational aspects of deafness, the latter being of fundamental importance and involving the former.

* * * *

The education of the deaf is a complex topic involving a great deal of controversy and emotional judgement. The history of the education of the deaf is fascinating and illustrates the controversy, (see Ballantyne, op. cit., and Denmark, 1973).

Early attempts at teaching the deaf concentrated entirely on teaching speech, the point being that for deaf people to be considered 'normal' they had at least to show that they could speak. Only a few fortunate and rich people benefited from the limited skills available, (there were very few people in the 15th, 16th, and 17th Centuries with sufficient interest and skill to undertake the work of teaching the deaf to speak).

In the Eighteenth Century a French abbé, (Charles de l'Epée), began to teach deaf children language, using a sign language. His argument was that an understanding of language is necessary for speech; language should take priority over speech in the education of the deaf.

These two points of view, 'the deaf should be taught speech' and 'the deaf should be taught language' are at the base of an educational controversy which is still very much alive today.

In 1880 in Milan the International Conference of Teachers of the Deaf convened and passed the following resolution, (see Denmark, op. cit.):-

> "The congress, considering the incontestable superiority of speech over signing in <u>restoring the deaf mute to society</u> and in <u>giving him a more perfect knowledge of language</u>, declares that the oral method ought to be preferred to that of signs for the education of the deaf and dumb. [My emphasis.]"

This resolution is of interest because it serves to illustrate several aspects of the 'oralist' approach, (which is that followed in most of the present day British schools for deaf children).

Firstly, the oralists view their method as being distinct from the use of signs, (implying that signs are used without speech). Thus the oralists see the controversy as being 'oralism' versus 'manualism'. For most people who favour the use of signs the controversy has ceased to be oralism versus manualism because it is not envisaged by these people that signs will be used without speech and/or language. A special sign system has been devised which mirrors English syntax but even without this there is no reason to suppose that someone who uses signs will never use, or want to use, the English language or speech. The arguments now centre on the suitability of Total communication, (see 1 b).).

Secondly, the oralists state that their method gives a 'more perfect knowledge of language'. They make this statement without producing any evidence, and even if 'speech' is substituted for 'language', as it often is, there is still no evidence. Another aspect of this argument is that speech is personally important, without it a person might be something less than human. (The fact is that speech is socially important for <u>hearing</u> people, it is not fundamentally important for the deaf). This argument is merely a less brutal version of the same argument which led to the drowning of deaf children in Caesar's time, and the classification of the deaf as idiots in mediaevial Britain, (see Denmark, op. cit.).

When challenged about the lack of evidence in support of oralism teachers of the deaf may often reply that in any case the use of signs has an adverse effect on the acquisition and use of language, or at best has no beneficial effect, (this is discussed in section 1 b).). The argument is illustrated by Grace Paget, (Paget, 1969), writing about the Paget-Gorman Systematic Sign Language, (a sign language based on the syntax of English). She implies that a point in support of a systematic sign language is the following, (this point had been made to her husband by someone in favour of such a language).

> "..... the use of such a sign language might counter the objection that most teachers of the deaf had to the use, for the purpose of education, of any sign language which was related to events rather than words, and which was therefore of no help in the acquisition of correct language."

Denmark, (op. cit.), says:-

> "The principle argument of those who advocate a purely oral approach to the education of deaf children is that if

children are allowed to use manual communication methods they will do so in preference to speech and lip-reading. They agree that facility in manual communication is easier for deaf children to acquire than are oral skills but do not pause to consider the psychological significance of this. They argue that deaf children will employ the easier method, will not make efforts to speak or to lip-read and will be unable to integrate into hearing society."

The third point illustrated by the resolution quoted above is the need for deaf people to be 'integrated' into society. This need pervades the oralist's arguments and has already been mentioned above. My impression is that the need for integration is usually expressed in terms which leave no doubt that what is really meant is that the 'need' is for the deaf to be inconspicuous. This may seem a harsh comment on what is superficially a sympathetic desire, but the following extracts from a government publication, (Lewis, 1968), show that the more selfish aspects of 'integration' can be manifested in a crass and insensitive manner which indicates a total lack of appreciation of the meaning of deafness.

Para. 13. "In the education of deaf children, the desire <u>for</u> them to achieve the greatest possible degree of normality has become the more urgent.....[My emphasis.]"

Para. 18. "Of major importance to parents of deaf children is the acquisition by their children of intelligible speech."

The committee is not referring to partially hearing children for on page three of the report they give the following definition of 'deaf pupils':

".....deaf pupils, that is to say, pupils with impaired hearing who require education by methods suitable for pupils with little or no naturally acquired speech or language;....."

In paragraph 20 where the aims in education are discussed the following sentence occurs:-

"Without exception the witnesses whose views we sought on this point saw the aims of education as being to fit deaf children as far as possible to take their place in hearing society and to realise a full personal development."

(However, in paragraph 282 the order of priorities is reversed).

This is balanced by paragraph 21 where the committee found that ".....all our witnesses were agreed on the importance for deaf children of proficiency in the use of language....." They also state "There are those who would argue that for some deaf children the aim of integration with the hearing world is incompatible with that of complete personal development."

Whilst acknowledging the need for research into the use of manual media, to establish scientifically the validity of them, (paras. 293 & 294), they were not presented with any scientifically valid evidence in favour of oralism, the method they favoured, (see paras. 3 & 9 of the summarised recommendations). It is clear that oralism was seen by the committee as the method of providing the means for integration.

Contrast this with the opinions of Vernon, (op. cit.), on the "Hearing world versus the Deaf world."

> "This false dichotomy is used to frighten parents into unrealistic programs and to justify outlandish educational endeavours."

He compares deaf people to Rotary Club members, (who are not forced to choose between a Rotarian or a non-Rotarian world), and Greeks, Jews, etc., all of whom form subcultures in America, or as he puts it ".....many ethnic, social, and professional groups prefer primary social interactions with their own." He goes on:-

> "Why should deaf persons not have similar desires and be permitted to exercise them without mystical suggestions on our part that they are leaving 'the hearing world'? It just may be that our egocentric view that everyone would prefer our company is wrong. Deaf people may prefer the company of other deaf people."

This of course does not suggest that the deaf do not want to speak, (any more than the Rotarians relinquish speech).

This view has not gone unchallenged although there is some support for it in Britain, (see Rawson, 1973, for an expression of the need for 'studied withdrawal' for those who suffer from acquired deafness). Reger, (1970), in a letter, criticises Vernon's views and again emphasises the point made earlier about manual communication.

> "The simple fact is that deaf persons cannot communicate with anybody other than deaf people when they can communicate only manually." Later in the same letter:- ".....children should never learn to rely on manual communication, simply because it is useless in the larger society."

When the deaf child has passed through school to enter, as an adult member, 'the larger society' what are his achievements? His potential, in all probability the same as that of a hearing person, will not have been achieved, (for a review of the evidence for these two points see

Vernon, op. cit., where some British, but mostly American, evidence is discussed). Even if he is a fair speaker he will find integration into the hearing world difficult and will seek the company of other deaf people. He will learn sign language if he does not already know it and his parents will suffer the ignominy of having to learn sign language. The psychological stresses in such a family can be great,(Denmark, op. cit.).

It is not surprising that psychiatric opinion, (British), is that

> "The use of manual communication methods for educating deaf children should be extended. .....There should be further research into the extent and type of counselling given to the parents of deaf children." (Denmark & Eldridge, 1969).

Denkark,(1971), writes, of deaf psychiatric patients,

> ".....the large majority of these patients communicate by manual methods, (finger spelling and signing), and it is vital that psychiatrists and other personnel involved in the diagnosis and treatment be trained in these methods. .....Prelingual profound deafness presents such a handicap to total development that every channel of communication should be utilized to the full, especially in the early formative years. It would seem that there is a need for not only the development of specialized psychiatric services for deaf children and their parents, but also for a critical appraisal of educational methodology."

Also, from America, there is the opinion of Moores,(1970).

> "It is interesting to note that while most educators of the deaf prefer oral methods, many psychologists, psychiatrists, and 'outside' educators familiar with the problems of deafness argue for some form of early manual communiation."

What is it about oralism that produces these strong feelings? (I do not pretend that I have not become emotionally involved).

Oralism sets out to provide the deaf person with speech, <u>and therefore</u> with the means for integrating into society, (i.e. language). Thus, firstly speech is to some extent confused with language, a fundamental error. (The relationships between speech, language and thought are more thoroughly explored in Chapter 2).

Secondly, the oralist method teaches the written form of language as it would be taught to hearing children. Thus, the familiar sight of a child reading aloud may well be found in a school for deaf children, although of course the speech may be very poor. It has been shown that for some deaf children reading aloud has a detrimental effect on comprehension, (Conrad, 1971, see also Conrad, 1970 & 1972).

Thirdly, oralism tries to provide the deaf with speech and language by establishing a completely oral/verbal educational environment for the deaf child regardless of his degree of deafness, a sort of total immersion in the hearing world's means of communication. Signing is often forbidden in schools for deaf children, and not only English is taught orally but everything else as well.

The main means of communication available to the deaf, it is presumed, is lip-reading, sometimes hopefully but inaccurately called speech-reading. This skill, or art, is not universally available, many deaf people find it difficult or impossible to learn.

Research in Russia, (Bel'tuykov, 1967), with deaf children has shown that lip-reading skills are correlated with articulatory skills, and that the meaning of this is that a deaf child will only be able to lip-read as well as he can articulate, (this is during the language acquisition phase of a child's development). It is fair to point out here that oralists favour the use of all available technical means for aiding the acquisition of speech skills, provided that they do not interfere with the acquisition of language.

In addition to the above point there are many criticisms, of the exclusive use of lip-reading, based on ambiguity and dependence upon grammatical knowledge. For example, L.J. Fant,(1972), says:-

> "Spoken English appears too ambiguous and unreliable on the lips for the purpose of using it as a medium to learn English as a first language."

Moores,(1970), writes:-

> "A straight oral approach is committed to teaching language through speech and speech-reading although research indicates that a primary requisite for speech-reading is grammatical ability. Deaf people, after years of training in speech-reading often cannot speech-read as well as hard of hearing people because they lack the ability to utilize context and anticipate, integrate and interpret in consistent grammatical patterns those sounds, words and phrases which are difficult to distinguish from the lips. The difficulty lies in the fact that many distinct sounds in English either look like other sounds on the lips, (e.g. 'p', 'b', 'm'), or present very limited visual clues, (e.g. 'k', 'g', 'h')."

He points out in the same article that recognition of this is not new, and says that Alexander Graham Bell was aware of the difficulties. Cornett,(1970), also makes this point and provides a quote from Bell:-

> "A knowledge of language will teach speech reading, but speech reading will not teach a knowledge of language."

Vernon, (op. cit.) reports Lowell,(1957-1958, & 1959), as finding that

> ".....non-deaf college sophomores who never studied speech reading were better at it than deaf children and adults to whom it had been taught for most of their school lives. The reason for the superiority of the sophomores was that they had a solid language base, (syntax and vocabulary), which enabled them to fill in by guessing words they could not speech read. As 40% - 60% or more of the sounds of English are homophenous, that is they look like some other sound on the lips, a person who lacks the language base to fill in the gaps understands very little. In fact, the best speech readers in a one-to-one situation understand only 26% of what is said,(Lowell, 1957-1958, & 1959). Many bright, otherwise capable deaf children grasped less than 5%."

Fourthly, such educational methods may take a great deal of time:-

> "The effort to give them [profoundly deaf children] some degree of speech and a command of language makes a continual demand on the teacher, almost to the exclusion of all else." (Denmark & Eldridge, op. cit.).

These criticisms of the oral method can apply to some children who wear hearing-aids. A very deaf 'partially hearing' child with a 'deaf-aid', (or as teachers prefer, 'hearing-aid'), is handicapped in all the above mentioned ways. Use of residual hearing with a hearing-aid is sometimes of doubtful value because a child will perhaps learn to ignore the noises he hears. He then has to learn to correlate aural sensations with lip-reading, he may not do this automatically,(Neate, 1972). Vandenberg,(1972), found that only extensive out-of-class use of an aid, (with in-class use of amplification), was correlated with written language and academic achievement, but that it was not possible to conclude that constant use of an aid was the 'causal factor' in producing better performance. Use of amplification in the classroom but not outside it did not correlate with academic achievement.

Oralism is thus seen to be well-intentioned, but the view of those who disapprove of oralism is that the oralist approach is not in the best interests of the deaf child because the nature of the problem has been misunderstood and the remedy has been incorrectly chosen. They argue that 'integration' is not the real problem and that in any case oralism is not the best way of achieving integration.

For an educator of deaf children trained in oral methods the outstanding problem is to equip such children with the means for coping with 'the hearing world'. It is assumed that the means lacking is speech. If speech can be intensively taught then a deaf child will acquire language and social skills in the normal way, (i.e. as a hearing child 'effortlessly' does).

A difficulty immediately arises when the meaning of 'deafness' is discussed. To an oralist educator deafness is measured by a 'pointer' along a scale or continuum with 'slightly hearing impaired' at one end, (perhaps not requiring a hearing-aid), and 'profoundly deaf' at the other. This latter condition, it is hastily pointed out by teachers of the deaf, is comparatively rare. It means that even with the most powerful hearing-aid no aural sensation at all is available. Thus a 'deaf child' will be placed somewhere along this continuum according to his hearing loss as recorded on his audiogram. It is unfortunate that measurement science has not produced a method for correlating audiographically measured hearing loss with either a) the nature of the amplification to be achieved with an aid, or b) the importance of the hearing loss for the understanding of spoken speech. (Speech audiometry techniques use speech material to provide some measure of 'speech hearing loss', so this is not based on interpretation of pure tone audiograms). These valid goals seem attainable and the teacher meanwhile works with imperfect data on his children. The assumption is that the effort and time to be expended on making the deaf child aware of speech should be proportional to the degree of hearing loss.

What is not appreciated is that deafness is not 'lack of hearing' for some people. The human being is very flexible and adaptable and whilst oralism will work well for those 'deaf' people who are only slightly deaf and for whom the aural environment is of some interest, there exists a substantial number of children who from a very early age have had no interest in the aural world because their aural experience is so slight, or painful or even non-existent. (Interest in the aural environment is the basis of hearing tests for infants and very young children. This sort of test is potentially very useful but it is difficult, at present, to interpret the results, (see Fisch, 1964)).

If an interest in the aural world is lacking then the measurement continuum does not apply. The measurement ceases to be of hearing loss and becomes one of deafness gain and the prescription for educational method must change to one appropriate for a truly non-aural world. The appreciation of this is clouded by the desire to normalise deaf children, to render them inconspicuous, to 'integrate' them.

The point of view that the deaf must seem as little different as possible from the hearing is responsible for 'flesh coloured' hearing-aid ear-pieces, a mistaken attempt to hide deafness and likely to cause embarrassment and feelings of inadequacy.

Attempting to communicate with deaf people is the way most people discover the deaf in society. Verbal communication is not easy, although it is often made out to be, and it is popular only in the sense of being widespread. When a person encounters another, and initiates a conversation, both put themselves momentarily at great personal risk. Initiating a conversation is a tremendous act of trust and the participants will feel some sense of vulnerability, even if only very briefly. The first word uttered serves usually to dispel the fears and provides information about many things, class, race, emotion, calm, aggression, illness, excitement, haste, leisure; a myriad of impressions settles in the mind of the 'listener', whether he is the first listener or the second. Such impressions are avidly sought and they are of great value in conversation for establishing a working relationship between the two conversants.

Should the first listener be a deaf person, with good or poor speech, lack of understanding is almost guaranteed, and his or her response will greatly unsettle the initiator of the conversation, (assumed to be hearing). It is not surprising that most hearing people feel that communication and speech are the problems of deafness. If they could see a person to be deaf, (just as the white stick enables recognition of blindness to take place visually), then much embarrassment would be avoided. This should not be seen as an attempt to 'label' the deaf, (i.e. 'label' as social outcasts), any more than the blind are so labelled.

The point I am making is that deafness should become acceptable for what it is. It is not lack of hearing, it is deafness, and the absence of aural experience must not be viewed as a handicap so much as an attribute. Moreover, it should be recognised that for the deaf to integrate into the hearing world as teachers and counsellors, (and therefore the deaf child's parents) desire, they must first be social beings. The hearing world per se does not provide the means for the deaf to become social beings; to make the mistake of assuming that it does is rather like considering a caged bird to be as 'alive' as one of his airborn relatives, the milieu is hopelessly and frustratingly inappropriate.

People who argue along similar lines, although perhaps not as extremely as I do, often give as an example the following type of argument, (offered by Scouten,1967, in justification of the Rochester method, see 1 b).).

> "Parents would not deny the orthopedic child his leg braces, nor the blind child his Braille. Logic similarly dictates....."

In the fascinating article by Vernon, already much quoted, is a passage concerned with the need for the 'hearing world' to heed the advice of the deaf themselves, (there is such a need in Great Britain where there are no deaf teachers of the deaf).

> "Yet, aural rehabilitationists who have so much to say about the deaf person's education and status in society almost never consult the deaf or have the contact with them that is required to graps the problems of deafness. An analogous situation existed with blind leaders of organisations for the blind who fought for over a hundred years for Braille so that they might read. Sighted people opposed the idea, ignoring the views of the blind themselves and claiming that the blind must read raised printed letters that were copies of the letters sighted people read. Finally a blind man, Louis Braille, found influential people who would listen to him, and the system of Braille was developed. Thus the blind were enabled to read and now they do well educationally. A breakthrough wherein hearing professionals attend to the ideas that deaf people have for their own welfare has yet to be achieved. The deaf are still forced to 'read raised printed symbols'."

1 b).

I have expressed in the first section of this chapter my opinions of oralism as a means of educating the deaf. I have suggested that for some deaf people a completely different approach should be tried, although I have not yet said what this should be. However, before going on to that I will make clear my opinions concerning the need for the deaf to acquire speech, (but see also Chapter 2).

It could almost be said that the history of the education of the deaf has been a history of attempts to make the deaf speak. I am wholeheartedly in favour of helping deaf people to acquire the skills of articulation and thus the means for expressing themselves in the hearing world. I consider it important to use all means of demonstrating to the deaf the characteristics of the aural world. Aspects of rhythm, music and extraneous noise as well as speech should all be taught to young deaf children to compensate for lack of natural interest in, and perceptiveness of, the aural world.

I do not, however, believe that 'speech' should be the goal of the education system. The goal must be the full development of personality and realisation of potential.

The emphasis must shift from the degree to which society will accept the output of schools for the deaf to an appreciation, within the schools, of what deafness means to a deaf person. Scouten,(1969), arguing in favour of the Rochester method, says:-

> "As long as we teachers and administrators study the child in terms of our own experience as mature hearing persons, we are quite likely never to understand either the child or his deafness.
>
> Perhaps we could assume a totally different perspective of the child and his problem, and attempt to develop a clinically subjective point of view; that is, a kind of empathy with the deaf child and his situation. In doing this we might see, to some extent, what the deaf child 'sees' and what he 'does not see'. Thus we might come closer to understanding the child and the circumstances imposed upon him by deafness."

However, although the argument is a good one his use of it is inappropriate because the Rochester method is fundamentally oralist.

There are two methods of teaching language to the deaf which, although partly manual because gestures made by the hand are used, are oral methods. These are 'cued speech' and the 'Rochester method'. Both of these seek to augment lip-reading in ways which render it less ambiguous. In the case of the Rochester method fingerspelling is used to make clear ambiguous or invisible articulations,(Scouten 1967). In cued speech the hand is used "to provide a visual difference between speech sounds." (Cornett, 1970). "If two sounds look alike, (or very nearly alike), on the lips, they will look different on the hand. If they look clearly different on the lips, they may look the same on the hand."

The fundamental principle behind these two methods is oralist because it is assumed that a language can be acquired from exposure to its visual form, (i.e. the lips), the emphasis being on visual appreciation of sounds in the case of cued speech.

There is a method which answers all the objections to oralism and which shows an underlying acceptance of the deaf for what they are. This method, 'Total communication', has been tried in America but has so far not received approval in Britain. 'Total communication' means, as would be expected, the use of every means of communication available, including sign language and fingerspelling, lip-reading and speech.

The arguments in favour of this method range from general ones concerning the full development of personality and social poise, through to linguistic arguments.

The social argument rests on the pleas of the psychiatrists, (quoted in section 1 a).), and the abilities of those deaf people who have benefited from a 'Total communication' education. This argument is extremely difficult to evaluate but would probably acquire weight if the opinions of the deaf themselves were to be considered.

Another point of view is that neatly expressed by Panara,(1971), who compares progress in educating the deaf to progress in educating hearing children. He points out that only recently have audiovisual aids become popular, (although I add here that they may be being misused), and that:-

> "Mere verbalism in the form of the spoken word is no longer considered sufficient for such important purposes as motivating students to learn and communicate effectively."

He wonders "why can't we include the simultaneous method [i.e. Total communication] as an instructional model in every class of deaf children, in every learning situation involving deaf persons?" He replies, a little later on "Some may object, can we tolerate sign language, gestures, mimicry in the class-room? My answer is, can we afford to neglect them?"

He ends his article with the words "In order to communicate effectively and fluently, people must feel at home in their language, and the deaf are no exception, [his emphasis]."

Research into the use of audiovisual aids in the education of the deaf, as well as some more general theoretical aspects of their education, is being conducted at the University of Sussex,(Watts, 1972).

The linguistic argument in favour of Total communication is very appealing to some educators because it is scientifically supportable. Through the recent burgeoning of linguistics, psycholinguistics, socio-linguistics and contrastive linguistics have all contributed much to the discussion and will continue to do so.

> "Sociolinguistics has much to offer as well as much to learn from the study of the deaf subculture as a language community." (Meadow, 1972).

The present climate of opinion is the result, ironically, of oralist pressure and the use of the argument 'yes, but sign language isn't really a language'. In other words, the oralists view sign language as being inadmissable in education because it cannot fulfil the role filled by the English language. The linguistic argument in favour of Total communication is that sign languages are real languages, but the grammars are not the same as that of English. Sign language can therefore be used instead of English.

In response to the claim that sign language is not suitable for teaching English the Paget-Gorman Systematic Sign Language was developed in Britain. This, as mentioned in the previous section, is based on English syntax. It is being cautiously tried in Britain, the argument being that because it mirrors English grammar it will not impede the acquisition of the English language but help it. It is envisaged that the Systematic Sign Language will only be used in the teaching situation as an aid for the acquisition of language, and as a general communication aid in school. It is not known whether the use of this language permits the natural course of events in language acquisition, (e.g. the use of 'childish grammars'), or whether it makes language acquisition easier.

However, it does make communication easier and research has been, and is being, conducted at the University of Reading into its value, (Wells, 1972). The researchers, for a preliminary evaluation, sought the approval of the parents of the children with whom the 'controversial method' was going to be tried, (some parents were doubtful, having moved to Reading because of the specifically oralist education provided there). The system was tried and quickly found to be a success:-

> ".....subjectively all those associated with this experiment are sure that progress is being made in the development of language using the Paget Systematic Sign Language, and that, in particular, the parents who are most intimately concerned with the progress of their children and are deeply determined to maintain an oral tradition have become convinced of its value."

The Systematic Sign Language also overcomes the problem of dialects, because the teachers of it are themselves carefully taught. Teachers of the deaf sometimes maintain that sign language is of no value because there are so many dialects. Until the advent of the 'systematic' language they did little to encourage the spread of a single sign language, to reduce the dialectical variations.

The Systematic Sign Language has possibly one drawback, and that is that it is conceived of as being merely a tool in the acquisition of language. It is intended, by its devisors, that its use should be restricted to aiding the acquisition of English and "it is believed that it is a tool which should, in time, be discarded when it has served its purpose." (This quote is from a circular issued by the Royal National Institute for the Deaf in London). This attitude is probably unrealistic; it will be interesting to follow the progress of the pilot study group in Reading and to find out whether the sign language is discarded by the children and their families when these children have reached the secondary level of their educational careers.

In America a great deal of research has been done with natural sign language, (the Systematic Sign Language is not a natural sign language), and this and other research indicates that Total communication is beneficial.

For example, oral pre-school training is held to be of great value. If deafness can be diagnosed early in life then pre-school oral training should help with academic skills, so the oralists maintain. Research does not support this.

Vernon and Koh,(1970, 1971), have shown, using genetically deaf children, that early manual communication produced better overall educational achievement including superiority in reading skills and written language.

> "Comparisons on the variables of speech intelligibility and speech-reading indicated no differences despite the fact that the oral group, for the most part, had extensive pre-school oral training whereas the manual group had neither

> this training nor had they parents who could use hearing to aid them in speech. In psychological adjustment no statistically significant differences were found."

All the data are presented in the paper from which this quotation was taken, (Vernon & Koh, 1970). The smallest number of subjects tested was 32 matched pairs: oral/manual.

In their later paper the authors conclude very strongly in favour of the use of sign language. They note that this is despite the extremely unfavourable conditions for the comparison. The oral pre-school group, (40 children), were advantaged in many ways, they had a good home environment and some of the parents have psychotherapy for helping with adjustments to having a deaf child; "the parents represent an above average group educationally and socio-economically."

In contrast were the deaf parents. The manual child, (35 were used), was defined as being born deaf to deaf parents, with the extensive use of signs in the home, (i.e. no oral home background), and the concomitant psychological, sociological and educational problems to be expected in such an environment. These children did not receive oral pre-school training.

> "Yet there were no significant differences in speech intelligibility, a finding noted in many other studies, [they quote here Moores, 1970]."

They conclude:-

> "Reasonable men can disagree over individual research findings and their policy implications. However, as study after study gives evidence indicating the limitations to just oralism as destructive to deaf children, there should be some point at which responsible professionals act accordingly and in the interest of these children."

These two studies were not concerned to show that Total communication was superior to oralism but that oralism was not as useful to the deaf child as was the exclusive use of signs in the home, for children of pre-school age. The superior academic performance of the 'manual' children would suggest that Total communication might well be profitably employed with deaf children from a very early age, (the authors did not mention the educational methods used in the schools from which the pupils for their study were drawn).

In addition to these modern studies which do not concern Total communication, per se, merely the inadequacies of oralism, and the preceding studies quoted by many authors, (e.g. Denmark, 1973, Vernon, 1969), there are other limited findings which widen the discussion and provide a fuller picture of the value of Total communication.

Klopping,(1972), has studied the effectiveness of three methods of communication in a secondary school in an attempt to discover which system results in the highest level of comprehension of language. The Total communication method scored higher than the Rochester method which itself did better than the oral method.

Olson,(1972), reports on the success, (with one child), when using sign language to stimulate language development. The parents were enlightened and realised the importance for their son of acquiring a language when very young.

Another study,(Howse and Fitch, 1972), suggests "that deaf children of hearing parents who have been exposed to manual communication skills make greater gains in expressive skills than a control group."

The linguistic research in America is more extensive than in England and it is now recognised that American Sign Language is a language in the formal sense,(Bergman, 1972, Dalgleish, 1973). There is no reason to suppose that if research were to be conducted into the nature of the English sign language and its dialects this would not be found to be a language, although it might bear no relation to English. (American Sign Language is not based on English grammar). American Sign Language is being studied as a language, with very interesting results,(Bellugi & Fischer, 1972). Cicourel and Boese,(1972), and L.J. Fant, (op. cit.), consider that the deaf must be thought of as bilingual with their first language being sign language. This is a challenging thought for the educationalists and it will lead to some interesting work on linguistic universals,(Cicourel & Boese, op. cit.), some of which has been started, (Schlesinger, 1971, and Dalgleish, 1973).

English as a second language for the deaf is seriously being discussed, as it should be because such discussion recognises that deafness is something special, not just lack of hearing, (see O'Rourke, 1972).

None of what has been written by myself or any other writer on this controversy has shown that speech acquisition is unimportant or that English is unimportant, merely that they are not fundamentally important. The suggestion I am making is that English can be acquired as a second language and that it and speech should be acquired early to give the child the full benefits of bilingualism. This is what Total communication could achieve.

The thought processes of the deaf are of increasing interest to many people and it is relevant to the discussion of Total communication to show that a language such as English is not fundamentally important for thinking. If this is accepted then the use of Total communication in the education of the deaf should be acceptable to all but the most sceptical. Some educators of the deaf might be convinced by the above presentation of the controversy which, I contend, shows that oralism is fundamentally misconceived and that there is a realistic and superior method available. For many it may be necessary to show that Total communication should work theoretically. (This is discussed in the next chapter).

A problem is raised by the advocacy of Total communication, and that is 'Which children should not be exposed to it?' I would suggest that until a reliable method is developed for estimating a child's deafness, (i.e. lack of involvement in the aural environment), it would be better to use Total communication with all deaf children, (i.e. even those who now benefit from an oral education), there is no evidence to suggest that it will harm anyone.

Speech acquisition has not been belittled by arguments in favour of Total communication, rather, it has been put in perspective. It is necessary for the deaf to have good speech if they want to communicate with hearing people, but it is also necessary for the deaf to have the desire to speak, and for this a more comfortable first language seems appropriate.

For speech acquisition there is a need for good and new training methods,(Lach et al., 1970, Nickerson & Stevens, 1973). This will be discussed in the next chapter.

CHAPTER 2

This chapter is divided into two sections. The first deals with the thought processes of the deaf and hearing and presents a speculative model of perception and thinking which is believed to be original. It is suggested how language acquisition can be understood as an extension of the acquisition of perceptual and mental skills. (Section 2 a).).

The second section, (2 b).), of this chapter presents a brief discussion of the problems of speech acquisition and indicates the field within which work must be concentrated if a successful speech training aid is to be developed.

2 a).

Discussions of the thought processes of the deaf have been concerned recently with the question of whether or not they think without language, as Furth,(1966), maintains. This discussion has led to an increased interest on the part of many psychologists in the nature of thought and the role of language in thinking. It may be the case, as Furth,(1971), suggests, "that most deaf American children grow up during the early formative period of life without any systematized conventional language." It is not the case that they grow up to be alinguistic, because American Sign Language is a language, (it is not formal in the sense that its grammar has not been fully elucidated; the term 'formal language' is used in this chapter to mean spoken natural languages). The same is probably true of British deaf children since there is no reason to suppose that natural British sign language is alinguistic.

Thus, although a formal language such as English may not be available to the deaf thinker it cannot be said that they think without language. It is likely that although deaf children are often linguistically impoverished, as regards a formal language, they think in a manner similar to hearing children,(Furth, 1971). Furth writes, (ibid.):-

> "This interesting fact implies that the current emphasis in psychology that attributes the growth of intelligence mainly to verbal factors is unquestionably inadequate and that undue emphasis on linguistic skill, particularly in early education, may not be a sound basis for nourishing intellectual development,(Furth, 1970)."

Furth adheres to the description of cognitive development put forward by Piaget who maintains that the sources of intellectual operations are to be found in the first developmental phase in a child. Piaget views the cognitive development of children as happening in four stages, 1) Sensori-motor, (up to the age of $1\frac{1}{2}$-2), 2) Pre-operational, ($1\frac{1}{2}$/2-7/8), 3) Concrete operational stage, (7/8-11/12), 4) Formal operations. (Piaget 1962).

As Sinclair-de-Zwart,(1969a), explains, "Intellectual operations are actions that have become interiorized and reversible, but they are still actions." For Piaget, he says, "The formation of representational thought is contemporaneous with the acquisition of language; both belong to a more general process, that of the construction of the symbolic function in general."

Piaget's theories are appealing to workers with deaf children who favour a 'de-stressing' of the importance of language, because he supports his theories with the results of some experiments on deaf children and blind children, (for a brief summary of these experiments see Sinclair-de-Zwart, op. cit.). His theories are also popular because they favour mathematics and logic with a high place in intellectual achievement, (see Inhelder and Matalon, 1960).

Bruner, (1964), holds the view that there are three stages in cognitive development and these parallel the three means available to man for modelling his environment. These three, enactive representation, iconic representation, and symbolic representation, occur in that order in the development of the child. Bruner, like Piaget, views each of the stages as being in some way superior to, and superceding, the stage preceding. ".....language shapes, augments, and even supercedes the child's earlier modes of processing information." (Bruner, op. cit.). The last stage, in which language is acquired, is representative, for Bruner, of the essence of man, ".....the growth of symbolic functioning links a unique set of powers to mans' capacity....."

For Piaget language is merely facilitated by the growth of formal operational structures. Within this progression of stages is the growth of the use, (logical), of symbols, this being achieved fully in the last stage. There is thus a contact between the two descriptive theories in that the growth of symbol usage is described in stages, the difference between them being the number of stages and the amount of importance given to language and the role of language in the acquisition and manipulation of signs.

Developmental psychology and studies of cognitive development have been joined by developmental linguistics and psycholinguistics, and it would take an excessive amount of space to present here the growth of these linguistic studies and their interrelation with the more established developmental work. It is sufficient to summarise briefly, and not I hope too violently, the approaches of the Piagetian workers and of Chomsky and his 'linguistic' school.

Chomsky has evolved a theory of linguistic universals. These are rules which prescribe the generation of grammars and languages, and the generation of sentences in a language. A native speaker of any language will possess a set of rules, (which may only be a subset of the set of linguistic universals), which will be used by him when he uses his language. The theory of linguistic universals would specify, if it were completely formalised, which elements could or could not be compounded to form a subset which would specify a natural language. A native speaker's competence, (the possessed knowledge of the language), may well differ from his performance, (actual utterances), and it must somehow be acquired from the performances of other speakers. This, as Chomsky points out, (1968), ".....complicates the problem of language acquisition." He suggests, (ibid.), ".....that the study of language may very well, as was traditionally supposed, provide a remarkably favourable perspective for the study of human mental processes."

Whilst he does not discuss too deeply the problems of language acquisition, (innate linguistic structures are postulated), he expresses the need for 'respect for the facts', (i.e. the richness of natural languages), and an awareness of the creativeness of language, (one of the fundamental points of his work), as it is expressed in the performance of the users, (and is therefore inherent in their language competence). There is an additional problem which is that a generative grammar may describe language, even 'performed' language with ungrammatical anomalies excused by context considerations, but it may not describe the way people actually use the language. Thus an individual's competence may not match a linguist's analysis of what that competence must be, (based on the performance), or of how the competence-performance process proceeds, (Chomsky's transformational grammars are postulated for this), (see also Broadbent, 1970, and Greene, 1972).

Sinclair-de-Zwart,(1969b), in putting forward a possible theory of language acquisition within a Piagetian framework, expresses the disquiet others have felt with the idea of linguistic universals and innate linguistic structures, (see also Morton, 1971).

> "Much of the need for postulating specific, innate linguistic structures seems to vanish if one considers <u>language acquisition within the total cognitive development</u> and, in particular, within the frame of the symbolic function, [my emphasis]."

This, I consider, expresses very succinctly the strength of the cognitive approach to language, but a weakness is introduced in that the developmental theory of Piaget is descriptive, not explanatory, whereas Chomsky's linguistic theory is explanatory, (but not necessarily the correct explanation).

The need for an explanatory theory is illustrated by another 'cognitive' approach to language, that offered by Lenneberg,(1967). He believes that ".....the nature of language is partly the nature of man," and that it is only necessary to postulate a certain 'categorical' innate structure. He writes, (ibid., p374):-

> "Language is the manifestation of species-specific cognitive propensities. It is the consequence of the biological peculiarities that make a human type of cognition possible."

And later:-

> "The cognitive function underlying language consists of an adaptation of a ubiquitous process (among vertebrates) of categorization and extraction of similarities. The perception and production of language may be reduced on all levels to categorization processes, including the subsuming of narrow categories under more comprehensive ones and the subdivision of comprehensive categories into more specific ones. The extraction of similarities does not only operate upon physical stimuli but also upon categories of underlying structural schemata. Words label categorization processes."
>
> "Certain specializations in peripheral anatomy and physiology account for some of the universal features of natural languages, but the description of these human peculiarities does not constitute an explanation for the phylogenetic development of language.....Today, mastery of language by an individual may be accomplished despite several peripheral anomalies, indicating that cerebral function is now the determining factor for language behaviour as we know it in contemporary man."

I feel that Lenneberg is not willing to speculatively add explanatory power to his theory.

*   *   *   *

Having briefly indicated a trend in modern opinions about the processes of language acquisition and the nature of the innate structures underlying language, I will present a very speculative model of my own. This is not intended to displace the descriptive theories mentioned above, but rather, to provide an explanatory framework into which they can fit. It is considered that aspects of these theories become 'obvious' in terms of the 'theory' I provide, and that they become more interrelated.

This model arose almost entirely introspectively, (I have only recently come to learn something about developmental psychology and linguistics), from a consideration of thought and human perception. The informal presentation here is the first presentation of my ideas on the subject and is clearly in need of tidying and reformulation in a formal manner. Nevertheless, I consider that it contributes something to the discussion of language and thought. The formulation presented here indicates the points of contact that I have discovered between existing theories of the nature of language and thought; a fuller discussion encompassing learning, memory, the subconscious, play and other aspects of human mental activity would not be appropriate here. The discussion here is largely confined to language acquisition and the importance for thinking of a formal language.

It is postulated that there exists in man, and to some extent other animals as well, an innate propensity for structuring the perceived world, and that this structuring is the essence of perception, (see Neisser, 1966). This structuring is 'wired in' in the case of certain basic visual mechanisms, for example, (e.g. 'line-detectors, and 'edge-detectors'). At a more complex level it must be present as a built-in scheme for handling data processed at the peripheral level. (Such a scheme may well be a general 'wired in' characteristic of vertebrate perceptual mechanisms, c.f. Lenneberg's "ubiquitous process of categorization and extraction of similarities."). Thus far my thinking differs from Lenneberg's only in that it is much less well informed.

However, my model is extended from this base to a consideration of what is done with the structured data, and how from these simple beginnings an organism like man comes to acquire language. The initial question that I asked myself, for my interest was in the perception of structures such as speech and music, was:- 'What is a feature?'

The answer offered is the following. The structures into which incoming data are processed are clusters, (a less well defined entity than a category), of elemental features extracted peripherally. Each cluster will have a feature, defined, (as are the elemental features), by relationships between the components of the cluster. At the elemental level the clusters are well defined, (it is postulated), and their features are unvarying, (the extraction of them, that is, the relations between the components of the clusters, being innately given). Features of clusters at one level will form the components of clusters at a slightly higher level in a hierarchy, and so on, to ever increasingly complex clusters. The extraction of features in these higher levels is a basic innately given propensity, (as postulated by Lenneberg), but as the hierarchy grows the components are no longer simple and the extraction must proceed more and more by means of acquired rules acting on the representations which are the components of clusters, rather than by means of innately given rules. The basic feature extraction equipment is still the same as for the lower levels of the hierarchy but the instructions or rules governing the extraction are less and less innately given.

I define a feature^1 as some relationships between features^2, the numerals referring to different levels in the hierarchy. The definition shows these levels as being adjacent, but they need not be, components may be combined from many different levels to form a cluster.

Thus far I have offered a somewhat more detailed idea of 'categorization' and 'extraction of similarities' than does Lenneberg. Like him I propose an innate feature extraction mechanism or 'categorization' mechanism, however, I also offer a more detailed idea of the mechanism at the higher levels in the hierarchy.

I have defined a feature as some relationships between features, and I have said that at the elemental, peripheral, level these relationships are 'wired in'. At the higher levels the relationships are extracted 'more and more by means of acquired rules'. The implication must be followed through. Acquired rules must be perceived rules, and to have been perceived the elements of them must have been structured into clusters, and features extracted. This is not quite as circular as it may seem initially. Consider the process developmentally.

A growing perceptual organism such as man will have certain perceptual equipment provided before birth, (e.g. hearing). With this equipment structuring of the perceived world begins even within the womb. After birth, the other senses are more involved and the hierarchy of clusters and features grows, features themselves coming to form the components, or 'elemental features' of yet more clusters. Feature extraction takes place on these clusters and another layer of the hierarchy is formed, and so on. The rules for extraction and clustering are still very physiologically determined but they can be learned, (i.e. the 'wiring in' can proceed after birth, as is suggested by experiments with kittens deprived of certain types of visual clues in their environment, see Blakemore, 1971).

As the perceiving organism develops the hierarchy grows and contains ever more complexly derived clusters and features which begin to operate as rules themselves and to affect and effect the clustering and feature extraction at the higher levels. The features and clusters are, at these levels, not necessarily well defined, there may be an increase in 'fuzziness' as the complexity increases.

Development is thus seen to be the perception and learning of rules and structuring schemes which become more and more complex and which involve levels higher and higher in the hierarchy. Moreover the hierarchy gradually ceases to be an ordered affair of levels and it instead grows 'sideways' and 'downwards' as well, providing the circularity necessary for the process of perception to govern itself, via memory.

The structure thus obtained is a very untidy, idiosyncratic affair administered still by very simple, (in principle), feature extraction and clustering mechanisms operating with a tremendous variety of innate and derived rules for clustering and feature extraction. It is interesting to note that within this model the context for any feature, (i.e. the cluster), is as important as the feature because a feature is defined by relationships. Thus a cluster consisting of perhaps many components will have a feature which is some aspect of some of the relationships between the components and which depends upon the components for its existence. At the higher levels, if the feature has been extracted, it is not necessarily fixed and immutable, (nor is the cluster of which it is the feature). The derivation of the relationships which define the feature is the feature extraction and the clustering which precedes this is the context setting.

These processes, to summarize the preceeding, are innately given for elemental sensory data, or can be wired in soon after birth, but once the elementary sensory data have been processed and reprocessed the clustering and feature extraction operate more and more on internally generated data using internally derived rules, (the rules and the data being of the same form). It is necessary only to suppose the innate ability to use these rules, and to extract them, this ability being a generalisation or adaptation of elementary perceptual mechanisms.

Language and sign usage can be encompassed within the model as an extension of the use of symbols, as suggested by Piaget. (A sign is arbitrary, whereas a symbol is linked more obviously to that which is represented). During the development of the human being symbols and signs, (e.g. names, words, the signs of sign language), will become attached to objects and actions, and later structures will be derived by the clustering and feature extraction processes. Because a feature can be a relationship as well as a thing, it is valid to consider the derivation of grammatical rules, (and rules in general), as feature extraction. It is the case that lexical acquisition proceeds before syntax acquisition, (see for example Dimitrovsky & Almy, 1972, and Sinclair-de-Zwart, 1969b).

Thus just as earlier rules were perceived, (constructed), for affecting and effecting perception then so will the process occur again with symbols and signs, leading to the acquisition of a language.*

As the infant grows, more and more of his perceptual learning will be concerned with the products of society and less with the fundamental elements of the physical environment. The influence of society on the perceptual mechanisms will increase and become very great by the time language is being acquired, but it will not only occur through language, (see also Whorf, 1956).

Time may well be perceived more culturally than space, as Whorf has pointed out, (ibid.). It is not, I think, necessary to postulate that the ability to perceive time independently of real time, (i.e. to plan), is a distinctively human attribute, (see Morton, 1971). Animals surely plan, especially when they use tools; they may not be aware of the plans.

It is to be expected that the time taken to proceed from stage to stage in any developmental model will lengthen because the degree of consolidation required will increase with each stage reached. It is envisaged here that a 'stage' is the copious generation of cluster components, using and extending the feature extraction principles acquired in the previous 'stage'. Gradually clustering will take place, and this will be influenced

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*This language may not be a formal language such as English, it could just as well be a sign language. Moreover, even if it were English there is no implication that the rules extracted, (the grammar), comprise a set of rules equivalent to those derived by a linguistic analysis of the Chomskian type.

It is important to note that the feature extraction and clustering processes used to derive the rules which constitute a person's linguistic competence are not guaranteed to produce a set of rules which is 'tidy' or 'efficient' or 'elegant'. They will be functional. It is also important in this connection not to reject adult performances which are 'incorrect', because, if these are understood then another person's competence must be able to cope with them. Rejection based on a grammarian's intuitive knowledge of his native language is not necessarily valid. The 'errors' are not manifestations of flaws in the functioning of the system, they are manifestations of the working of the system, (language competence and performance).

The feature extraction and rule generation system which it is postulated is part of man's perceptual/mental equipment is ideally suited to model building, (as is suggested by Bruner's itemisation of the three stages of perceptual development in his theory). Model building is what perception is really all about, and model builders must always be wary lest their models only explain a part of what they were intended to explain.

Concepts can be considered to be generalised models, that is models without some of the details specified by much of the context, and in this sense words can be considered to be categorization processes. It is, I consider, more valuable and fruitful to think of the clustering and feature extraction as the basis of mental activity, for from this can be derived explanations of insight, concept formation, intuition and many other aspects of human mental behaviour.

by other members of society, until new features become extracted, and the process begins again. (Lenneberg, 1972, calls these stages, in the process of language acquisition, 'readiness' - 'acceptance' - 'synthesis' - 'readiness' ..... He notes that the same sequence of events is probably that which occurs in cognitive development generally). Clustering itself presupposes some sort of partial feature extraction, and as well as being guided by other people, these will reflect the existence of 'partial insights', (Wason & Johnson-Laird, 1972), or possibly 'potential concepts', (Vygotsky, 1962). Learning when an adult will be a process very akin to that outlined above but without the aspects peculiar to development.

The nature of language is thus seen to be intimately entwined in the perceptual processes by being affected and effected by the processes which are themselves affected and effected by language.

Language, (not necessarily a formal language), and the use of signs and symbols may, it is suggested, be considered as just another aspect of perception. Language can be thought of as a 'sense', but one acquired through membership of a society and thus socially determined. The 'sense data' are socially generated, (and agreed upon), but acquiring the language is no different, in principle, from acquiring the whole gamut of environmental and domestic knowledge that a child copes with, (water, fire, electricity, doorhandles, switches, toys, etc.). What language provides sensory experience of are the minds and sensory experiences of other people.

Some examples are appropriate here, and I will give three, all relying entirely on the native experience of the reader as a perceptual being. The examples are intended to show the clustering and feature extraction nature of perception; to show that features can usefully be described as relationships between features, (i.e. that features exist on two levels).

It is implied, intentionally, that human beings put out their 'descriptions' of the world in a format which reflects their internal perceptual processes. This is done, I suggest,because the 'descriptions', and the language used for expressing them, must be suitable for interpretation and assimilation by other humans.

Firstly, consider a house. An image may form of a detached, 'three up and three down' house, with a neat garden and smoke curling away from the chimney pot. It may be situated in a quiet road with other dissimilar establishments. The road is in a quiet part of a town which is ..... which is somewhere in England.

The house as a single thing, a feature, can be turned into a cluster of features by entering it, the mode of entry being determined by your relationship to it, guest, burglar, owner, etc. Similarly, once inside your definition of features will depend upon whether you are the proud possessor showing it to a friend, an electrician called to mend some fault in the wiring, a decorator, a window cleaner, a thief, etc. For each of these people different things will constitute features but for the user and some of the intruders listed above, the rooms may well be features, within which will be features, (cupboards, beds, pictures, boxes, etc.), within which will be features, ...

It should be noted that the clusters are not necessarily well defined, the boundaries may not be clear. For example, is the wall of the house part of it or not? Perhaps it is only part of the house, perceptually, conceptually, some of the time. To which room does a dividing wall belong, or is it ignored and excluded from the clusters of components which constitute the 'rooms' on either side. It may instead be a component of a cluster called 'interior structure'.

The second example is also visual but it is concerned much more with symbols and signs, as manifested in a particular description of man's environment.

An atlas contains an extraordinary amount of information, in a very structured way. There will be 'economic' maps, 'demographic' maps, 'political' maps, and so on. Each of these will be to different scales and will probably show different cities, and they may or may not show rivers or mountains, for example, depending on how 'integrated' a view the cartographer had in mind. These maps may well be at the back of the atlas. At the front of the atlas (just after a 'projection' of the world perhaps), will be a map of Great Britain, (assuming the atlas to be British). Following this will be maps of greater detail showing England and Wales, Scotland, and Eire.

As the area covered by any one map decreases the amount of 'detail' increases, (i.e. the amount of material to which a person can easily relate). Cities cease to be mere dots on maps with a scale of one inch to one mile, roads and churches and ponds are all marked. The countryside becomes full of life on maps with this scale. The green and brown shades and patches of the smaller scale maps become fields and contours. Streams can be followed to their sources and it is even possible to tell whether a road has fences or boarders directly onto fields. Bridges can be recognised, public houses and telephones can be located, and pylons carrying electricity cables look like ruler-straight lines with representations of the pylons along them, but not in the right places. (I refer here to the ordinance survey maps of Gt. Britain).

By the time '2½ inch maps' have been discarded in favour of maps with a scale of, say, six inches to the mile the detail available is truly astonishing. (There may be no more 'information' on these maps than on the maps with the smaller scales).

This illustration shows that at any scale signs and symbols are used which can either be 'expanded' by going to a 'more detailed' map, or condensed and subsumed under more comprehensive signs and symbols on a 'less detailed' map. Nevertheless, the relationships between the symbols are as complicated on the six inches to the mile scale as they are on the 60 miles to the inch scale.

The third example is linguistic and demonstrates how language is entangled with perception, (see also Kolers, 1972). Consider a description of something, contained in a book, to which one chapter of the book is devoted. There will be other chapters on other topics, and the book as a whole will have a title, which to some extent will indicate the contents of the book. The title and author will enable you to find it in a library where its location will depend upon the author's name and the book's contents, (an important point).

Within the book there will be chapters, dealing with different topics, and within each chapter will be different sections, each of which will contain paragraphs, each of which will contain sentences, ... It will be possible in this hierarchy to recognise the features at any of the levels, key words, sentences, paragraph topics, etc. The word ordering, and sentence ordering, (and paragraph ordering .....), will not be random, rules will govern these orders, socially agreed rules and idiosyncratic rules.

Within the words will be symbols which are also socially agreed, as are the rules governing their ordering. The shape of the symbols will be governed by rules, (the font will have certain characteristics, 'sans serif', etc.). If a person looks continuously at a word, for example, KITCHEN, for a while, the word may start to fragment, other words may appear as subunits, and finally a sense of unease about the spelling may be felt. (A similar effect is available aurally, see Warren & Warren, 1970). Continuous inspection of a word 'demands' that it be considered in isolation, rather than in a sequence of words, and it becomes appraised for what it is instead of its relation to other words. Its structure will be probed, its meaning investigated, it will cease to be a feature as a thing and become a feature as a set of relationships, (either itself or the thing signified).

As Cherry,(1966), points out the meaning of any one word is not well defined, "when does a bush become a tree?" Dictionaries, as he also points out, are almost completely closed systems, as every word in them is defined by the other words presented. The dictionary is only of use if you already know the language to some extent. This also illustrates the two-fold nature of words and features. They can be explained or serve to explain. Thus, I feel, Lenneberg's,(1967), definition of words as being categorization processes is only half of the story.

Print is in principle no different from any other aspect of language, except that it is a poor transcript of spoken speech and is not as visually distinctive as is speech aurally distinctive, (see Conrad, 1972). It does have the advantage of breaking speech into words. The listener would normally have to do this feature extraction himself. Thus if a listener does not know the rules for the feature extraction, (both at a phonemic level, and a lexical level), a language will sound like a continuous jabbering.

* * * *

One final aspect of this speculative theory is that memory, thought and imagery all become integrated with perception instead of being separated.

For example, the richly interconnected, and rather fuzzy clusters of features, (words, symbols and images), form the structure of memory. This is a vast collection of loosely organised 'lists', some of which might even contain themselves as an item! These clusters, or lists, serve both as elements of other clusters or lists or they may be broken down into elements, (see also Kintsch, 1970). Once again rules are used for extracting information, and these are adaptations of perceptual rules and will be governed by the organization and the nature of the knowledge. Remembering is a sort of reperceiving.

Recall might be formulated as the internal specification of a feature, or cluster (the specification may be incomplete), and the extraction of information from memory using the appropriate rules. Recognition is the same except that a 'template' is provided externally, (this may be misconstrued and lead to incorrect recognition). The template may also be incomplete. Context is important here, in theory, just as it is in practice, ('what was I doing when I last had it?' - an approach to the finding of a lost article).*

The verb FLUMGING, to open a book, you will not have experienced before and you know this immediately because you 'know where it would be stored' if you had previously experienced it. Memory is content addressable, and in a loose sense associative, the contents are loose lists, fuzzy clusters, of features. There is a good reason for this and it is that by having perceptual experiences so organized it is necessary only to provide internally a model, (i.e. a feature in the form of a cluster, 'perceptual set' is an example of this), of what it is expected will be perceived. Perception proceeds on a strong base of anticipation, sensory data are largely used for verification, (not to prove, see also Wason & Johnson-Laird, 1972, for an illustration of the fact that reasoning is not logical and embodies this 'need for verification').†

Using the definition of language formulated earlier, that it is a sense provided by society, (this applies to sign language as much as to formal languages), it is possible to conceive of thought as being a special process of perception. This may be expanded as follows. Language, as an integrated system, becomes partially embedded in ones perceptual mechanism. It is a system of signs and symbols and rules which link, only partially, with the perceptual apparatus which copes with the non-linguistic events. In effect acquiring a language is acquiring a second perceptual mechanism,

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*Neisser,(1966), discusses memory and thought in chapter 11 of his book, where the topics to be covered by any theory of thinking and memory are discussed. I do not agree with him that there are only the records of prior constructions in memory, this seems an unnecessary restriction.

Also, time may be much more culturally dependent than he suggests, (as mentioned above). Time coding need not seem strange and uniquely human, in my model it is not treated differently from space except for the acknowledgement that there may be a strong cultural influence in the perception of time.

It is also possible that time provides the 'environmental' or 'asocial' perceptual mechanism, (see later on in the text), with imagery based either on action or language, (both being coded in time). As regards actions as images I would point out that I do not consider that developmentally each 'stage' should replace the preceding one, as 'supercedes' suggests. There is no need for suggestions of superiority. The sequences of stages observed do not imply an abandoning of previously acquired clustering and feature extraction schemes.

†Attention may simply be the direction of the perceptual mechanisms by the conscious or subconscious processors, (see later in the text), in order to more fully verify an internal model or to modify or construct one, (i.e. learn). The use of internal models makes attention possible without unduely hindering some other aspect of perception or action.

(the full meaning of the word sense), and this mechanism can operate with linguistic material at the same time as the non-linguistic sense data are operated upon non-linguistically. (The operations performed frequently in one processor may intrude into the workings of the other processor, directly or through the reorganisation of material in the memory). One tends to classify material as language/non-language and this is necessary because the information has to be processed by the appropriate mechanism.

It is also possible that visual information is treated primarily, or preferentially, as non-linguistic, or environmental, or asocial in nature, and that aural information is treated preferentially as verbal, linguistic, or social in nature. The two perceptual systems, (i.e. vision and audition), are surely not equivalent in their value either as environmental monitors or social/communication senses.*

The separability of these two perceptual mechanisms may be unique to man and may well have something to do with the apparently unequal division of responsibilities between the two hemispheres of the brain, (as shown by 'laterality' experiments).

Informally, consciousness can be defined as the 'linguistic' or social perceptual mechanism working with signs, symbols and images, and evoking auditory, (in the case of hearing humans), sensations in the asocial perceptual mechanism.

The degree of separation between the social sense and the asocial, or environmental sense effects the degree to which conscious thoughts may resemble formal linguistic structures, (if a formal language is possessed). Thoughts may be conducted in a 'private' language, and the degree of separation of the two senses influences the degree to which subconsious thoughts influence conscious thinking, (not just the content, but the processes as well). Conscious abstract thinking will be the use of symbols and rules which are completely socially acquired and which have no links with the asocial thinking system. The degree to which this divorce is achieved will govern the logicality of the thinking.

Thinking I would define simply as internal 'perception', ('selection' of features and clusters, including images, using memory, perception of them, leading to 'selection' of more, and so on.....), where perception

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*This may account for the 'believability' of visual material, (photographs, etc.), it being unconsciously assumed that visual material is 'environmental' and therefore not social or the product of another person's perceptual and social experience.

is the application of clustering and feature extraction rules, to data, mostly internally generated, in the form of clusters and features. Thus features and clusters, 'accompanied' by images, direct the extraction and perception of more features and clusters and thinking proceeds in a circular fashion, 'accompanied' by a 'procession' of images. (The images may be essential for some of the direction).

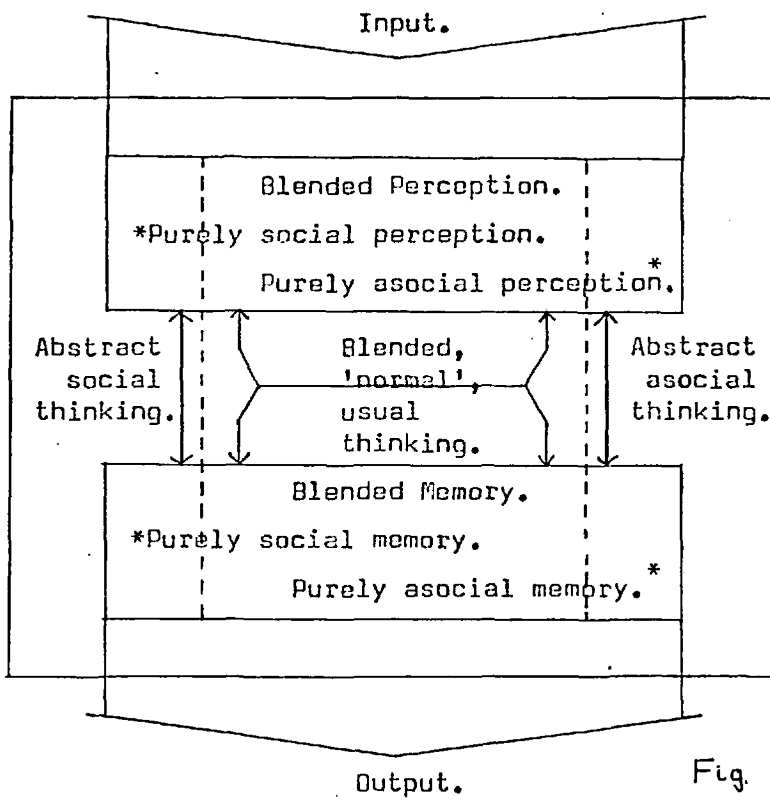
Subconscious thinking is thought, as just defined, carried out with asocial images, symbols and rules. The thinking proceeds unnoticed until its effect on sign and symbol usage becomes noticeable via the social thinking system. It is possible that dreams proceed via internal and often consciously incomprehensible rules and that they constitute perhaps the subconsciousness's abstract thinking. Sleep is after all the most asocial of activities.

For thought to proceed there is a need for very short-term memories which can store a feature or cluster of features while they are interpreted, and a longer term stores, (perhaps like the 'short-term memory' which is the subject of many psychology experiments, see for example Conrad,1970), which can hold the 'directions' or intention of the thinking, again as a feature or cluster. Without these two stores, (which are not necessarily separate from the normal memory), thinking would become aimless and undirected, if not impossible. These same two stores are also required for perception, to store internal models being 'used', (attention would not be possible without some sort of 'working' store, see Reitman,1970), and to enable the incoming data to be interpreted, (see also Neisser, 1966, and Morton, 1970).

Imagery is described by Neisser, (op. cit.), as "a kind of cognitive luxury." This is strange as he also points out that thought is not verbal in nature, (by appealing to the difficulty people have in expressing their thoughts). He also introduces the idea of schemata, (cognitive structures), and says, "It is easy to see why schemata control the fate of stored information; they are themselves information of a similar sort." This is similar to my approach to memory and thought, but his discussion lacks the detail that I have presented.

Imagery is surely not a cognitive luxury but a part of perception, thought, and memory. Images will be, in one 'system', the accompanying appropriate evocations of the other 'system'. Imagery serves, in everyday thinking, to provide for the blend between social and asocial types of thinking and to aid memory. Studies of short-term memory show that the use of imagery improves recall,(Bower, 1972). This is the basis of many memory systems, and can be understood as the incorporation of the to be remembered item in a cluster, (perhaps containing a lot of visual images), or the classification of it as the feature of such a cluster, thus facilitating recall.

Images in one 'system' or mode of thinking can also form the working material of the other, and thus the two types of thinking can blend to form the 'everyday stream' of thought. Awareness is merely an aspect of this, some of the images accompanying the social thinking are, (in hearing people), largely the auditory images of verbal speech, and they are 'perceived', (in the sense 'observed'), in the asocial system. Other images evoked during social thinking will be non-verbal and these are perceived by the asocial system, (in the active sense of perceive), and they can be manipulated in this system. Each of the two mechanisms of thought, operating with appropriate images, symbols, signs, and rules for clustering and extracting features, can thus not only influence the processes going on in the 'other' mechanism, but can also 'observe' these processes.



The arrows indicate the control and use of perceived and internally generated features, clusters and appropriate rules.

The horizontal separation represents the three types of mental activity: social/'linguistic', blended/'normal', asocial/environmental.

* The organism.

Fig. 2/A

A full exposition of this model would contain a more thorough discussion of perception, learning, memory, a discussion of language, consciousness, and subconsciousness, (this would include a discussion of psychoanalysis and hypnotism), and would extend to include a consideration of play, and the relevance of the model for work in the field of artificial intelligence.

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The role of language in thought becomes clearer in the light of the above discussion. Verbal language is not necessary for thought, any language will do and it is likely that for developmental reasons it should be easily acquirable at the appropriate stage.

The reason why verbal languages have developed, in humans, is clear, (I think), from an evolutionary point of view. Attention to the environment can proceed visually whilst social attention can proceed aurally. The signs and symbols can be made by some part of the body other than the hands, and the languages work in the dark, a distinct advantage.

A language such as English is not essential for thought, what is important is that the language used should maximise the potential in early life for social interaction and thus for the development of social thought. In modern human society the evolutionary pressures are relieved and language acquisition can take place through any medium available.

For the deaf speech will be a very poor medium because of the difficulties involved. Sign language, on the other hand, is much more appropriate, (for Standard Average European cultures, see Whorf, 1956), because its imagery is enactive, (and therefore coded in time), and not 'spatial' or 'environmental'. Also, because the 'images', or symbols and signs, are enactive they can be learnt easily and early. There is no reason to suppose that with its extensive use in the home, at school and amongst friends, sign language cannot serve the essential purpose of providing a social sense. Sign language must be made available to the deaf child as would verbal language, the verbal language of the hearing culture can be taught as a second language.

It is simply not enough to try hard for a few years with a verbal language and then switch to another medium, the first language medium must be consistent, efficient, and it must be provided early as otherwise there will be some detrimental effects on thought processes and social awareness.

The Systematic Sign Language is not a natural sign language; its use as a first language for the deaf needs to be researched. Whilst it may show advantages in that it may make the acquisition of English easier, it may also be inappropriate for the workings of the deaf person's mind. It would be better, in my view, to develop natural sign language as a teaching medium.

Before discussing the acquisition of speech, (the next section), it is worth mentioning some of the work that has been done on the acquisition of language in primates. It appears that chimpanzees, (most extensively used as subjects), may have an internally generated second perceptual mechanism/thinking mechanism. Recent research, (Gardner & Gardner, 1969, Premack & Premack, 1972, McNeill, 1973), shows that they can acquire language to some extent. The existence of a second thinking capability, a social one, possibly serves some social purpose, but it is not possible to say what constitutes the natural language of the chimpanzee.

2 b).

Speech, as articulatory skill, is something taken very much for granted, and yet it is not always appreciated that its acquisition depends very extensively on hearing.

The reason for the dependence on hearing is simple. Hearing serves as a monitor for vocal gestures. This point was recognised early in the history of teaching the deaf to speak. Amman, (1693), writes:-

> ".....that therefore the deaf may know that I open my mouth to emit a Voice; not simply to yawn, or to draw forth a mute breath, I put their hand to my throat that they may be made sensible of that tremulous motion, when I utter my voice; then I put the same hand of theirs to their own throat and command them to imitate me; nor am I discouraged if at the beginning their voice is harsh and difficult; for in time it becomes more and more polite."

This principle is behind all efforts to teach the deaf to speak; it is the principle of mimicry and demonstration.

For a hearing child the ears provide a means of comparing an utterance with a demonstration version of it, and also the knowledge of how to correct an inadequate version. The problem for the deaf child is that the facility for mimicry is absent and it has to be provided. The desire for a deaf child to speak should not be equated with a desire for them to be as normal as possible, (i.e. inconspicuous), it must be a desire for them to be given a part of the means for communicating with hearing people, the chance of broadening their experience and social contact.

I have shown in Chapter 1 and this chapter, that deafness is not just a communication problem, that is the way hearing people view it. I have also shown that the educational methods should be broadened to include the idea of Total communication because it is the personal development of the deaf child that must come first. Through this will come social development and the ability to integrate socially.

To help the deaf to acquire speech, and to enable teachers of the deaf to spend more time on more important matters, it is necessary to provide speech training aids, which, to be appropriate, must provide the facility for mimicry and demonstration. Work is expanding in this field, (see for example the March 1968 issue of American Annals of the Deaf, or the largely visual aids discussed by Nickerson & Stevens, 1973). Other forms of training aids should be employed too, as mentioned in Chapter 1, for compensating for a deaf child's aural indifference. A sense of rhythm and a social sense of vocal activity should be taught using training aids, these aspects of the aural world are important for speech. Other aspects are important socially, and they should not be ignored. A personal noise meter would be of use to the deaf, it would help them to adjust the volume of their voice to suit the surroundings. Other environmental noises should be explained, noises made by clothes, breathing, doors slamming, etc.

It is very important to keep in mind, when attempting to help the deaf, just what it is that deafness and the education of the deaf really mean. I have come to this realisation only slowly. I started with a conventional hearing person's view that the deaf person's problem was lack of speech. I have discovered by working with a deaf person something of what it must be like to be deaf.

There is a real need for speech for the deaf, especially for those who have been, and are being, led to believe that integration and normality are achievable by this means. My foray into the field of speech training aids for the deaf is presented in the next part of this thesis.

PART 2

INTRODUCTION

In Part 1 I was concerned to show that there are some general considerations to be borne in mind by anyone wishing to help the profoundly deaf. These concern the need for the technologist and educationalist to understand and respond appropriately to the problems presented.

Verbal communication is widespread but not fundamentally important for the acquisition of language and other mental skills. For the deaf, therefore, the training of articulation should not occupy a disproportionate amount of a child's time.

A deaf child lacks the facility for mimicry which is essential for the acquisition of good articulation. In this part of the thesis I present some empirical work which, I consider, has gone some way towards showing how the facility for mimicry can be provided, thus offering the possibility of condensing articulation training and increasing its efficacy.

In Chapter 3 I present my own psychophysical work on cutaneous sensitivity to vibration and relate it to previous work, both psychophysical and physiological, (this latter is presented in Appendix 1).

Before discussing my own experiments it is necessary to explain why I decided to use tactile stimulation for supplying to a deaf person information for him to use in the comparison of his speech efforts with those of his teacher.

There exist two schools of opinion as to which sense, (sight or touch), will be more appropriate for use in a compensatory manner for the deaf. The arguments tend to rest on intuitive estimates about the suitability of either, but there is a real possibility that sight is not the appropriate sense for time-varying information, (where the 'rate' is high).

There is evidence that the ability of the cutaneous senses, when it comes to processing time-varying stimuli, is superior to that of the visual system. The evidence comes from several workers. Bobrikov and Tsukerman, (1967), quote Smith(1965) as showing that for the Morse code the auditory rate is 175-200 characters per minute, whereas the rate using light flashes is only 50-60 per minute.

Geldard has shown that the cutaneous senses can process coded information at rates (38 w.p.m.) approaching those of good Morse operators using audition, (1957). Bliss and Crane, (1965), quote a figure of 30 w.p.m. as the best achieved using an array of stimulators, (12 x 8 array of air jets or bimorph reeds), and patterns moving across the array. These figures are certainly comparable to the 175-200 characters per minute for heard Morse.

The subjects used in the above experiments were hearing people. With deaf subjects the rates for tactile Morse are poor, (Tsukerman, 1968). This is difficult to understand. The worst interpretation would be that deaf people have a poor sense of rhythm and can't cope with rapidly varying tactile signals of any sort. This is probably an unrealistic interpretation. The rates reported by Tsukerman (20-30 characters per minute) are probably low because of the difficulties the subjects had in coping with the metronomic rhythm of the dots and dashes. The subjects found this difficulty particularly acute when it came to learning to use the key. It may be that deaf people (especially adults) will require 'rhythm' training before they can make full use of any tactile aid, (i.e. to bring their performance up to that of hearing people).

Geldard writes,(1970), "The skin does well, very well, with time, far surpassing the eye in this respect and even rivalling the ear in some circumstances. It also far out-strips audition as a spatial sense but, in this regard, clearly has to knuckle under to vision." This statement, (at least the first part of it), is supported by the fact that the flicker fusion frequency for the eye is very much lower than that for the skin, there being perhaps a ratio of 20/1 or more in favour of the cutaneous sense, (the figures are, approximately, 25 c.p.s. for vision, and 400-500 c.p.s. for the skin, the latter figure being an estimate. See Appendix 1).

I would argue therefore, that despite the possible need for 'rhythm' training, (especially if a rhythmically demanding code is used), deaf people could make better use of speech information if it is presented tactilely than if it is presented visually. Rhythm training should in any case be given to deaf children, (as mentioned in Chapter 2).

Spoken speech at a rate of 150 words per minute, (of two syllables each), is about average, and this gives 5 syllables per second, or 300 syllables per minute. Assuming that the perceptual unit is the syllable and not the phoneme, 300 s.p.m. is a little too fast, (taking Geldard's figure of 38 w.p.m. as a guide, (i.e. 190 perceptual units per minute)), but slower speech, at a rate of about 80-100 w.p.m. would be within the bounds of possibility. If the perceptual unit is the phoneme then possibly speech would have to be spoken very slowly, but with visual presentation of course the situation is much worse.

Arguments put forward by the proponents of vision as the suitable sense would counter that, despite the possibly inadequate rates at which the visual system can work, not enough is known about the skin senses to be able to prescribe a practical system of stimulation. In their view it is better to use a sense about which a great deal more is known.

There are discernable in this viewpoint two hidden and intangible flaws which I will try to reveal. The first is that, because technology has evolved a little lopsidedly there exists a tremendous variety of ingenious means for presenting information visually to human beings. No such technological 'tool-box' is available to those who wish to use some other sense. Therefore, 'not enough is known about the skin senses...' really includes the statement 'we don't have the technology to do it.' (Neither of these statements is really true).

The second flaw is very much less obvious but, in my opinion, none the less real. I consider that in a very general sense human beings in the English speaking Western World, (I cannot extend the generality further for lack of evidence), believe and believe in that which they see more than that which they hear.. I am suggesting that visual perceptions have an aura of trustworthiness about them.

For example:- Visual identification parades for identifying potential criminals. The ease with which one can believe photographs and films to be 'objective' records, (this leads to vigorous debate, amongst historians, on the value of 'archive' film and photographs). Such phrases as, 'Seeing is believing', 'I couldn't believe my eyes', 'The camera never lies', 'Do you see what I'm getting at?'.

On the other hand, verbal communication is untrustworthy. Examples are:- The concept of hearsay in the giving of evidence. Gossip is spoken. Phrases like 'You can't believe all you hear', 'You're only saying that, you don't really mean it', and so on. The lists can be extended but I think my point is clear.

Moreover the believability of visually presented information (the result I suspect, of misconceptions concerning the nature of the perceptual processes, see Chapter 2,) is transferred to the systems used for the presentation. Thus, equipment which presents information visually to a deaf person is probably believed in by its designer more than is warranted by its merits.

Another reason for not choosing the visual sense is that, (according to Chapter 2), it is the environmental sense and not the social sense. Biologically, it may be inappropriate to use vision for rapidly varying information of a social nature as well as spatial information for environmental and security reasons. The diversity of imagery may be hampered, and it is important to note that sign language has a diverse imagery based on physical action, it is gestural. It may be argued therefore that an aid for speech training should aim to extend the imagery available for speech into an area suitable for rapidly varying social information. Touch should serve this function better than sight.

However, the use of the cactile sense also increases attention problems and intensive training may be necessary with vibrotactile stimuli. The possibility of modal interaction and physiological structuring of the brain of very young children both serving to increase the efficacy of the tactile sensations cannot be ruled out.

The above does not amount to a formal argument, but, without sufficient experimental data to work with, it does constitute a basis for taking a decision.

* * * *

In this introduction I have suggested that there are theoretical and experimental reasons for considering the sense of touch to be better than vision as a substitute for hearing, (as regards speech acquisition). In addition a tactile aid might have the advantages of a) not depriving the deaf person of whatever lip-reading skills he or she might have, and b) being useful to the deaf-blind.

In the chapters which follow I present my own psychophysical work on cutaneous sensitivity, (Chapter 3), and relate it to previous work, (Appendix 1). In Chapter 4 I discuss a vibrotactile speech training aid of novel design, and in Chapter 5 the aid is assessed and briefly compared with three others.

CHAPTER 3

The decision to use some form of tactile stimulation having been made I soon found that insufficient was known for me to be able to specify a suitable system. My disinclination to use electrocutaneous stimulation was reinforced by the discovery that a suitable 'scale of sensation' was not available with such stimulation.

In fact the prevalence of codes in tactile communication systems, (usually based on one or two suprathreshold levels of amplitude, 2 or 3 different durations, and perhaps 5 loci), appears to reflect the general lack of a suitable stimulus continuum.

Despite the lack of a suitable stimulus I decided to use a vibrotactile system because I suspected that the lack was due to insufficient examination of the possibilities, (perhaps because of the unsuitability of the stimuli used by many experimenters). I decided to use the hand as the site for stimulation because of the astonishing sensitivity of the fingertips and because the convenience of this site is not approached by that offered with any other location. The forearm is a good candidate but reduced sensitivity, (requiring larger transducers), and slight inconvenience caused by adjustment of clothing, led me to choose the hand.

In section 3 a) I address myself to two questions.

(1) What characteristics should a vibrotactile stimulus have? (3 a) 1).)

(2) How should I set about determining whether or not any stimulus I devise is unsuitable for use? (3 a) 2).)

Included in subsection 3 a) 1) is a discussion of 'fatigue' and 'adaptation'. It is necessary to distinguish carefully between the two terms. In subsection 3 a) 2) the Weber Fraction tracking technique is explained.

In section 3 b) I present the results of the psychophysics experiment I have performed, and in 3 c) I discuss the relevance of these results to the design of any vibrotactile communication system. I also discuss the contribution made to the psychophysical study of the 'sense of vibration'.

Appendix 1 is a review of the previous work on the 'sense of vibration'. The conceptual content of the Appendix 1, the introduction to Part Two of the thesis, and the subsection (3 a) 1).) which follows, is faithful to my knowledge, impressions and intuition at the time that I designed and conducted the experiments. The exposition of these opinions, facts, and decisions in this thesis is more cogent, thorough, formal and up-to-date than would have been possible at that time. This explains the presence of references to which I would not have had access at the time the experiments were designed and carried out. The inclusion of such references indicates that my sampling of the literature was sufficient to give me a good impression of 'the state of the art' at the time. The development of my arguments has not involved any radical change of opinion. I include this statement here as an explanation of the feeling the reader will have that the exposition is all too up-to-date, and without order in time. It is also more honest than to write the exposition so as to create the impression of a linear progression of ideas and work over the course of the last three years.

3 a).

In this section two topics are discussed. In subsection 3 a).1), the nature of what could constitute the 'best' stimulus is discussed. In subsection 3 a).2), are explanations of the experiments needed to ascertain suitable stimulus conditions and the unsuitability of a novel type of stimulation.

Also included in subsection 3 a).1) is a discussion of adaptation and fatigue.

3 a) 1).

Most workers when faced with the problem of specifying the 'best' stimulus have come to the conclusion that the frequency at which the 'skin' is most sensitive is the optimum.

For example, Cummings,(1938), investigates the effect of local anaesthesia on tactile and vibration thresholds at a frequency of 256 c.p.s. because he considers that this is the frequency at which the sensitivity is greatest.

Pickett,(1963a), reports on a tactual vocoder which uses ten channels, (one for each of the fingers and thumbs), the vibrators of which are all driven at 300 c.p.s. (See Chapter 5 for discussion of this aid).

Rogers,(1970), in a paper called "Choice of Stimulator Frequency for Tactile Arrays", concludes that a frequency of 200-250 c.p.s. is suitable. This work has been taken up, and stimuli at 250 c.p.s. are used in a practical device, the 'Optacon',(Bach-Y-Rita,1972).

Aston and Weed,(1971), use a tactile vocoder in which vibrators are driven with 300 c.p.s. signals.

However, against the use of the 'optimum' frequency of around 200-300 c.p.s. is the fact that the sensations produced are numbing and uncomfortable, and leave a tingling 'afterimage'. This was why work on the vocoder described by Pickett was considered not very promising, (personal communication). This is probably due to fatigue.

Work by Hahn,(1968), on 'adaptation', (i.e. the effect of prolonged vibrotactile stimulation), at two frequencies, 10 c.p.s. and 200 c.p.s., shows that the 'adaptation' follows the same course, despite the fact that different receptor populations are involved. Other work by Hahn,(1966), shows that, for the frequencies 50, 100, 200, 400 and 800 c.p.s., recovery from 'adaptation' for both the threshold and subjective magnitude is almost independent of frequency. The only effect noticed was that at higher frequencies the effect of the 'adapting' stimulus was slightly greater.

This effect had been shown in 1938,(Wedell and Cummings, 1938). They showed, using frequencies of 64, 256, 700 and 1024 c.p.s., that

> "1) Sensitivity to vibratory stimulation applied to the palm of the hand is reduced 5-15dB after three minutes continuous stimulation.
>
> 2) The loss of sensitivity is greater the higher the frequency and the greater the intensity of the fatiguing tone.
>
> 3) After stimulation for three minutes at a certain frequency, the loss of sensitivity is the same whether

measured at a frequency equal to or higher than the fatiguing frequency, but it is less if measured at a lower frequency."

Before discussing the importance of this it is worth commenting on the terms 'fatigue' and 'adaptation'. They are used interchangeably and this leads to confusion.

Hahn discusses the term 'adaptation' in his paper in the book 'The Skin Senses',(Hahn, 1966). In the same book there is a note, by Geldard, on 'Adaptation and allied terms'. Hahn wrote "I should like to use adaptation as a general term referring to changes in the properties of a sensory system resulting from a changed stimulus." Although he is distinguishing his use of the term from the meaning 'fatigue', often put on it, he fails to make this distinction clear in his experiment. The use of the term 'fatigue' by Wedell and Cummings further illustrates the confusion. I think that this confusion can be resolved, as follows.

The term 'adaptation' in its most general sense has a very specific meaning. The Shorter Oxford English Dictionary gives:- "The process of modifying so as to suit new conditions." This quite clearly conveys the sense of coping with a transition from one 'state' to another. This applies to complex psychological adaptation, such as coping with death, a new house, etc., just as well as to adaptation in the eye to cope with new light levels. One very important aspect of adaptation is that the new level or state is simply <u>different</u> from the previously experienced state, and moreover, is different to the state which will follow it. This is entirely in accord with what Hahn wrote. He was aware of the fact that, for example, light levels can become either greater or lesser than ambient and the eye will adapt to the new level.

He fails to note that as regards tactile sensitivity it is incumbent upon him to demonstrate that the 'resting' sensitivity is at some 'midpoint' in the operating range of the sense. Unless he demonstrates this, (which he does not), he cannot use the term adaptation. He does note that his data "confirm the existence of the difference in recovery rates of threshold and subjective magnitude, found earlier....." (he is referring to some earlier work of his).

The situation is not clear however, because he quotes an early paper by Geldard in which it is reported that this difference in recovery rates exists for vision, (i.e. difference between threshold recovery and subjective magnitude recovery). Hahn suggests that as the eye is adaptive, and has this difference, so the skin must be adaptive, because the difference is found. This rests on the questionable assumption that the continua of sensation, physical and subjective, are the same for the two senses.

The only valid use of the term adaptation in all this is the example concerning the eye. Until it is shown that an increase in sensitivity can be measured, in response to stimulation with vibrations, the phenomenon which Hahn calls adaptation is more properly called fatigue, (cutaneous receptor adaptation in response to sustained deformation is well known and is discussed in Appendix 1). I would suggest that the experimental differentiation between the two effects (i.e. fatigue and adaptation), depends upon the demonstration of threshold reduction, in the presence of vibrotactile stimulation, especially when the sense concerned is so poorly understood that extrapolation, or argument by generalisation, is not justifiable. Thus I would say that until evidence to the contrary is forthcoming, (see section 3 c).), the phenomenon of the elevation of threshold, resulting from prolonged vibrotactile stimulation, is the product of fatigue.

The term fatigue can also, of course, have a psychological meaning, as prolonged stimulation can be boring or fatiguing without fatiguing the receptors involved.

More recently, work has been done which investigates the effect of duration of stimulation on the level of fatigue. However, this work (Berglund & Berglund, 1970), does not alter the above argument as the authors use the word 'adaptation' and yet only detect a decrease in perceived intensity as the result of prolonged stimulation, (the rate of decrease being exponential). Working with vibrations of 250 c.p.s. they found, in addition to the above mentioned point, that the greater the stimulation intensity the greater the decrease in perceived intensity, (and recovery times from such stimulation were correspondingly longer) and that recovery times were "fast, requiring only 2 or 3 minutes".

Although the work of Wedell & Cummings, Hahn, and Berglund & Berglund was all carried out with suprathreshold stimuli it still remains valid to object to the use of the word adaptation as all the demonstrated alterations to sensitivity have been such that they could have been caused by fatigue.

As regards the specification of a suitable stimulus the implications of the above work are that;

1) The higher the intensity of stimulation used the greater is the risk of fatigue.

2) The higher the frequency used for stimulation the greater is the risk of fatigue.

3) The effects of fatigue are smaller at frequencies lower than the frequency of the fatiguing stimulus.
4) The perceived intensity decreases exponentially with increased time of stimulation.

It should be noted that all this work was done with stimuli of unchanging frequency, (although different frequencies were used), at suprathreshold intensities. In view of this, and the criticisms expressed in Appendix 1 of the physicists' approach to vibrotactile sensitivity, (i.e. the separation of amplitude and frequency as two parameters), there would appear to be a pressing need to try vibrotactile stimulation of varying frequency.

Moreover, if, as I suggest, a suitable stimulus is one in which frequency and amplitude increase together, (and decrease together), then the evidence above suggests that the most intense 'end' of the scale of sensation will become fatigued first, (provided that the stimulus is at such intensity for the same amount of time that it is at any other intensity). Such a stimulus might well reduce fatigue to manageable levels, although there is insufficient evidence to make a confident prediction.

A reason for suggesting such a scale of sensation is the following. The data presented by Verrillo,(see Fig. A1/7), indicate that if the contactor area is suitably large, a slight increase in amplitude, accompanied by an increase in frequency, will produce a subjectively larger increase in intensity, (perceived amplitude). This subjective increase in intensity is larger than would be experienced as the result of increasing the amplitude by the same small amount without changing the frequency. The convenience, (from an engineering point of view), of obtaining a large subjective scale by combining increases in amplitude with increases in frequency, leads one to hope that such a stimulus is suitable for use in a communication system.

Also, the stimulus is not so artificial as one in which amplitude and frequency are considered to be separate variables. The subjective scale referred to above is not an intensity scale nor a frequency scale, but possibly a sort of synthesised 'texture' scale.

The effect reported by Békésy, (see Appendix 1), and earlier, but in less detail, by Sherrick,(1954), that an increase in amplitude is perceived as a drop in pitch is probably not important, (see also Supplement to Appendix 1), the effect of the stimulus being of the opposite nature. The resultant scale of sensation may be not as large as that indicated by the figures, (Fig. A1/7), but there is no reason to suppose that the stimulus suggested will create any problems.

I use the figures presented by Verrillo for the thenar eminence because the equivalent data for the fingertip do not exist. I am assuming that the subjective magnitude functions, (i.e. the plots of constant subjective magnitude versus frequency), have the same shape for the fingertip. In view of the larger population of Pacinian corpuscles in the fingertip,(Verrillo, 1971), it may be found that the difference between thresholds at 25 c.p.s. and 250 c.p.s. is larger for the fingertip than for the thenar eminence. If this were so my argument would be strengthened.

Some other important characteristics of any vibrotactile stimulus are more easily specified. The area stimulated, and its location are both clearly of importance when deciding the form of a vibrotactile system. The area should be as large as possible consistent with the locality. In addition, travelling waves in the skin should be prevented, where their occurrence would prove embarrassing,(Verrillo & Chamberlain, 1972).

Verrillo,(1971), has obtained data for the finger equivalent to that shown in Fig. A1/8, for the thenar eminence. The displacement magnitudes involved, and the shape of the graphs are similar, (both show a threshold decrease of 3dB/Octave with increasing area), but the values for the area, along the horizontal axis, are very different. Fig. 3/1.

Other factors to be taken into account are discussed by Verrillo, (1966). They include contactor height above the surround, (and thus contactor pressure). As this increases the threshold decreases, (the steepest rate of decrease is 6dB/millimeter at 320 c.p.s.). This effect is not large, and Wagemann,(1969), suggests that allowing subjects to adjust the pressure by manipulating their fingers produces the best, (i.e. most consistent), results. This would not appear to be too important unless it were necessary to use the lowest achievable values for threshold. Configuration appears not to be of importance although an annular configuration might yield lower thresholds. Carmon,(1968), finds an indication that an annular contactor is better and quotes an early paper of Verrillo's,(1962), as support. However in his 1966 paper, where he discusses configuration, Verrillo does not mention this. The effect is small.

One remaining parameter of interest is the 'waveform' of the mechanical displacement. In Appendix 1, I point out that most experimenters have used sinusoidally varying displacement as the stimulus. As the rate of deformation varies with frequency, when such a stimulus is used, it is difficult to decide what will be the effect of a different form of vibrating displacement.

In view of the uncertainty concerning both the waveform to be used and the problem of fatigue, it seemed wise to conduct some preliminary

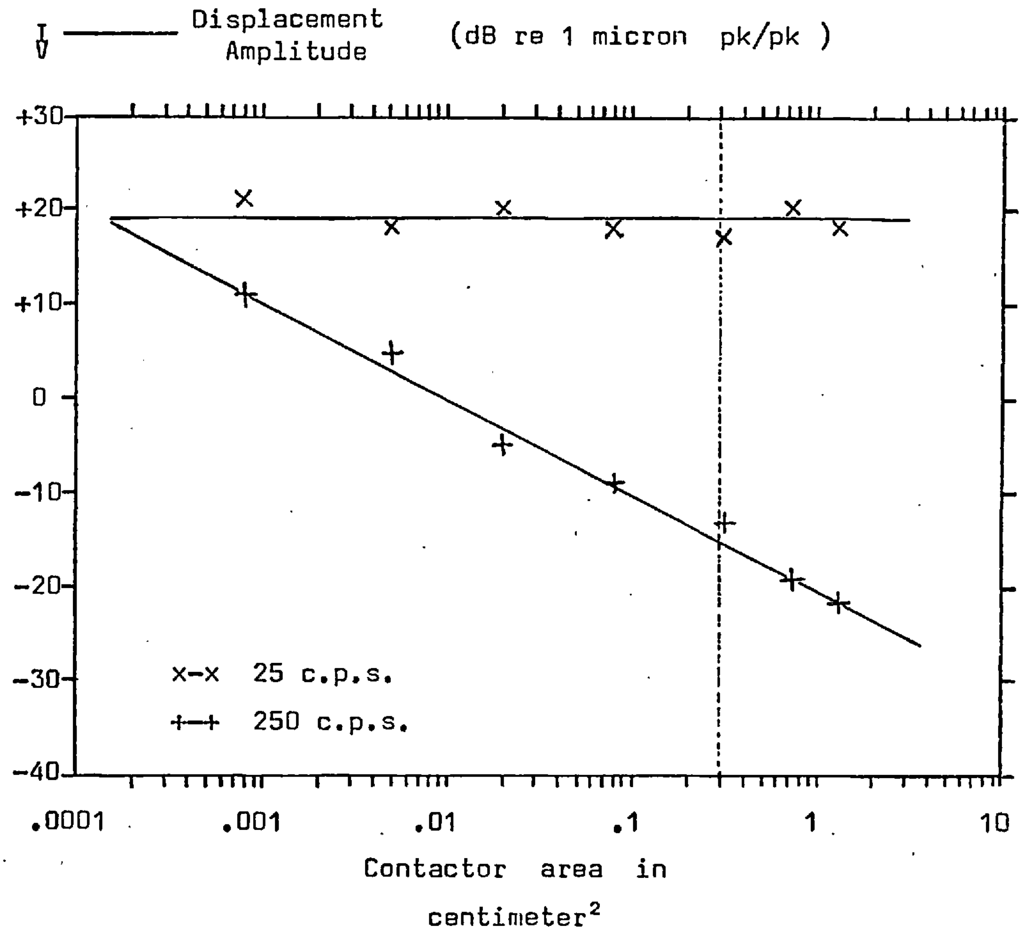


Fig. 3/1

Thresholds for the tip of the middle finger, at two frequencies, as a function of vibrator contactor area. (From Verrillo, 1971) The dotted line indicates the contactor area most extensively employed in my experiments.

psychophysical experiments. Accordingly, I designed some apparatus for generating the sort of stimulus I had in mind, and for measuring both the threshold and the Weber Fraction for this stimulus.

The experiments also served the important function of acclimatising the subject to a laboratory environment. They were time-consuming but not arduous, and continued for several months. This was done to establish the usable range for the stimulus, observe the consistency or otherwise of the subject's performance, and provide time for the building of the training aid itself, based on data from the experiments.

I felt confident in assuming that it was worth proceeding as if I would not have to unravel complicated side-effects resulting from my stimulus. I therefore approached the experiments as being necessary to show that a stimulus operating along a scale synthesised by increasing (and decreasing) amplitude and frequency simultaneously, was not unsuitable. I feel this approach to be justified as I was not directly concerned with the psychophysics for its own sake, having instead a utilitarian attitude to the findings of others.

Despite this, I suggest, (in section 3 c).), that some of my findings are of interest to those working solely on the elucidation of the psychophysical phenomena. These findings concern the existence of adaptation, in the sense that I defined, when the stimulus is of varying frequency, and the nature of 'fatigue' in the experiments conducted by Hahn, and Wedell & Cummings.

3 a) 2).

In order to assess the unsuitability of the stimulus I had devised, I decided to measure the Weber Fraction.

This quantity is commonly used in psychophysics despite its unsound background. Weber proposed a psychophysical law of the form:-

Delta A / A = Constant.

That is, that the smallest noticeable change in the stimulus, Delta A, is a constant fraction of the stimulus magnitude,(D'Amato, 1970). Many experiments have been done and it is found that the law is invalid. However, the ratio is a useful measure and is called the Weber Fraction. It is found that at low amplitudes above threshold the law is completely inadequate, but that at higher amplitudes it is often a fair approximation.

It is of interest to note that the Weber Fraction is strictly only meaningful for stimuli on a continuum of quantity, but it is applied without reservation to stimuli differing in kind or position. Thus one finds reports of the measurement of Weber Fraction for frequency in hearing. It is difficult to know what the ratio means when A does not have quantity but quality, unless it indicates that, subjectively, frequency does have quantitative attributes. The transfer of a measure dealing with amplitude changes, however incorrect the law that gave rise to the measure, to changes in quality, is not a trivial step. The term is still referred to in its restricted sense,(Baird, 1970).

Knudsen,(1928), reports Weber Fraction measurements for tactile frequency discrimination, and his results are shown in the table below.

The results are for two subjects, and the amplitudes were maintained at "50 times the threshold."

Frequency (c.p.s.)	Delta F / F
64	0.12 & 0.15
128	0.22 & 0.31
256	0.08 & 0.10
512	0.20 & 0.35

Goff,(1967), suggested that all previous experiments were difficult to evaluate as the techniques used were not fully reported and because no attempts had been made to control amplitude, (Knudsen was, however, thorough). She conducted experiments in which changes in intensity due to changes in frequency were ruled out. This she did by measuring equal intensity curves, at two intensities, instead of calculating them. Her results are shown in Fig. 3/2.

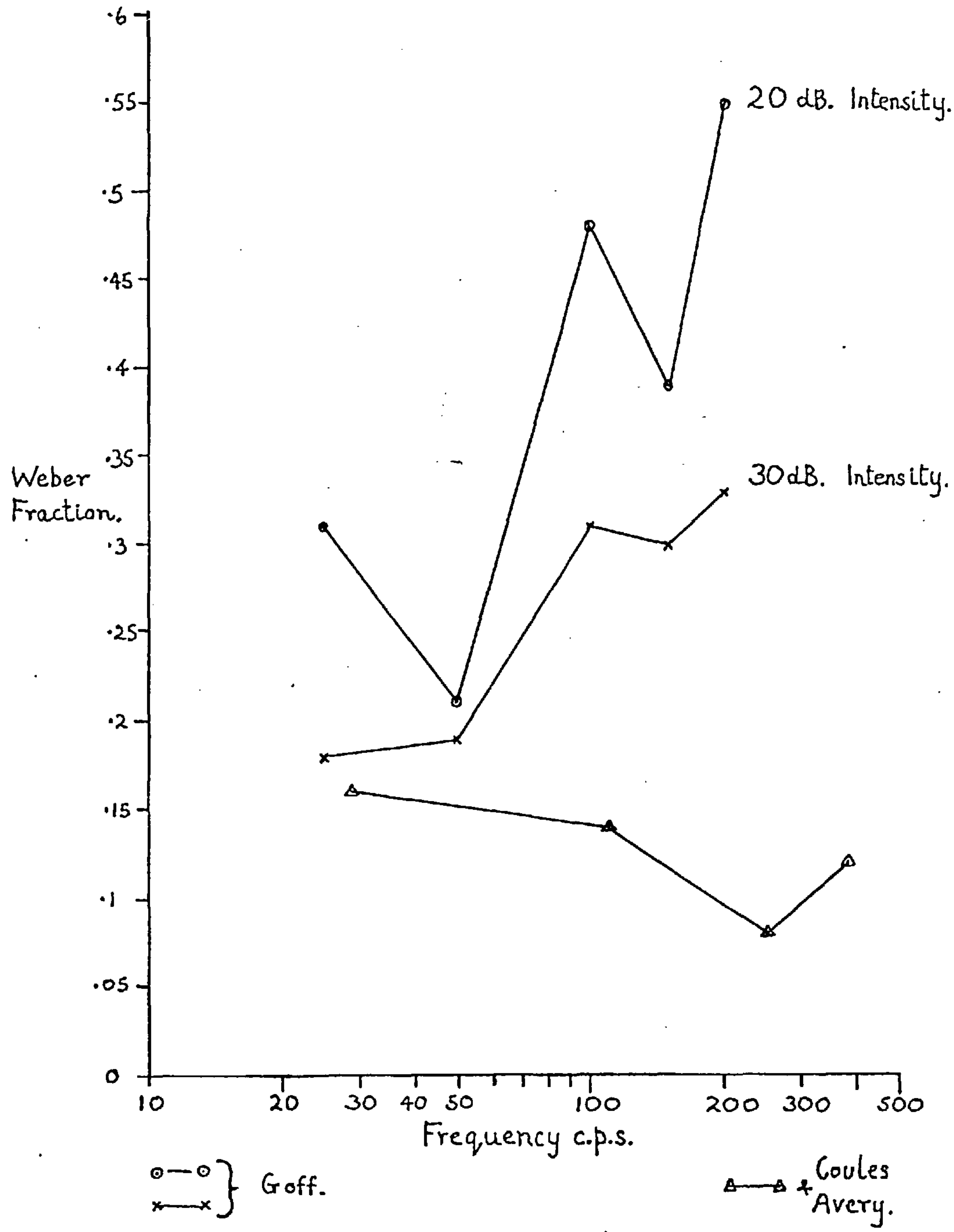


Fig. 3/2

Graphs of Weber Fraction as a function of frequency. The data come from Goff, (1967), and Coules and Avery, (1965). (The intensity was not controlled by Coules and Avery).

Of interest is the finding that, with meticulous attention paid to ensuring that intensity cues are not available to subjects, the Weber Fraction increases with frequency. She also showed that as the intensity increases the ratio decreases. The previous work, she reports, shows the same trend, despite its questionable quality.

An experiment of which she was not aware agrees with her finding, according to the authors, although no attempt was made to control the amplitude of the stimulus,(Coules and Avery, 1965). Their data are also shown in Fig. 3/2. They consider the similarity in shape between their curve for Delta F against F and her's is "... consistently lower but in general agreement with previous findings, [Goff's]".

She notes that energy differences, where they existed, did not aid discrimination, and that discrimination was good where such differences did not exist. She concludes that such differences were not serving as cues in her experiments.

Weber Fractions for vibrotactile amplitude do not appear to have been extensively measured. Geldard,(1957), quotes a few figures from an unpublished thesis. These are for 60 c.p.s. vibrations; they are around 20%.

The measurement of the Weber Fraction appears to be an acceptable way of ascertaining the unsuitability of the stimulus I devised. 'Unsuitable' would be characterised by high values, possibly increasing with frequency and intensity. Moreover, measurements of the Weber Fractions for a stimulus of constant frequency as well as for a stimulus of varying frequency enable a comparison to be made between a stimulus of known value and one of unknown value.

*   *   *   *

As the stimulus was intended for a system in which rapidly varying signals would be 'transmitted' I measured the Weber Fraction in a novel manner.

Conventional methods require the presentation of a steady stimulus, (the standard), and a variable stimulus, (simultaneously or sequentially), and the exaction of a response, usually of the form 'same' or 'different', from the subject. The variable is increased if the response is 'same', decreased if it is 'different', (for determination of the positive increment), and another response called for, and so on for many trials at different frequencies. The positive increment is commonly measured.

My apparatus was designed to permit the measurement of positive or negative increments, although the latter were not extensively investigated. The incremented value and the standard value were presented alternately in rapid succession, at 5 c.p.s., (corresponding to average syllable rate, approximately). Thus 100 msecs. of 'standard' stimulus was followed by 100 msecs. of the incremented stimulus and the two stimuli alternated.

The subject controlled the incremented stimulus. On feeling the 5 c.p.s. modulation of the sensation, the subject pressed a switch, causing the size of the increment to decrease. When the 5 c.p.s. modulation could no longer be felt the switch was released, causing the increment to increase. This resulted in alternating trials, ascending-descending-ascending- and so on. In addition, as the subject was controlling the increment, the standard stimulus continuously varied. The amplitude of the standard slowly increased and decreased, (taking 2 to 3 minutes for this), and the frequency could be tied to the amplitude so that it too varied, or it could be fixed at some value. The apparatus made it possible to 'ring the changes' completely and combine different increments in frequency and amplitude and to have the frequency decreasing as the amplitude increased. This provision was made in case the 'first choice' for the stimulus proved unsuitable.

The subject's performance was recorded continuously on an X-Y plotter, the resultant tracks giving rise to the name of the method, the 'tracking' method.

It was intended that the signal driving the transducer would be a square-wave, (for electronic convenience). I therefore measured the threshold for vibration at different frequencies, using the tracking method. This was done to ensure that there were no anomalies resulting from the choice of stimulus, and to determine suitable values for the minimum amplitude of the stimulus. In this situation the subject controlled the magnitude of the stimulus directly, and the 5 c.p.s. signal was not present. As for the Weber Fraction tracks, the frequency could be varied or fixed at some value of interest.

The tracking method for measuring threshold was used by Békésy, (1958), and by Gescheider & Niblette, (1962). The tracking method is a modification of the method of limits, and Hall, Fucci & Arnst, (1972), have shown that such a method is suitable for the measurement of vibrotactile thresholds.

3 b).

In this section I present the results of my experiments on the tactile sensitivity of one person.

The apparatus is briefly discussed in 3 b) 1). (It is fully discussed in Appendix 2).

In 3 b) 2) I present the results of the experiments in which threshold was measured, and in 3 b) 3) I present the results of the experiments in which the Weber Fraction was measured.

3 b) 1).

The values for the parameters mentioned in subsection 3 a) 1) were chosen arbitrarily, or for reasons of desirability and compromise.

The stimulus was delivered by a small solid plastic peg 6 mm. in diameter protruding through a hole 8 mm. in diameter in the plastic surround. (See Figs 3/1 & 3/3 and Appendix 2). For some experiments the peg was 3 mm. across and the hole in the surround had a diameter of 5 mm. The vibrators were made from small Japanese loudspeakers, and it was difficult to ensure that the projection of the peg through the hole in the surround was a constant value. Usage tended to deform the mechanism a little and the peg of the vibrator used extensively became flush with the surround. This may have resulted in slightly elevated thresholds, but I do not think the uncertainty about this point is of much significance.

The surround had a small ridge 2 mms. high and 2 mms. wide, at a radius of 1 cm. from the centre of the hole. This was provided, initially, to reduce travelling waves, as it was thought that the forearm might be used as the site for stimulation. The transducers were not modified when it was decided to use the fingers and the ridge provides cues which facilitate the positioning of the fingertips.

The finger was not attached in any way to the vibrator, and the subject could press as hard as she liked on the peg. She was told that she could move the finger around on the vibrator, if she wished, to help with the detection of the stimulus. It is not possible to ascertain how much effect such freedom of movement had on the results obtained, but as mentioned in 3 a) 1), Wagemann,(1969), suggests that such freedom yields more consistent results.

The stimulus was provided by driving the vibrator with signals ranging in amplitude from a few millivolts to a few volts, and at frequencies from 22 c.p.s. to 600 c.p.s. The amplitude range was determined at the lower end by an estimate of what technically was possible, (design solutions imposed some difficulties in measuring and generating very small alternating voltages), and at the higher end by the maximum current the coil of the vibrator could pass, (see Appendix 2). The frequency range was chosen in the light of the psychophysical data. The lower limit is determined by the frequency below which 'pokes' or 'jabs' are discriminated rather than felt as a continuous vibration. This frequency is around 15 c.p.s. and so a choice of 22 c.p.s. seemed reasonable. The upper limit was chosen as likely to be of the order of, or above, the flicker fusion frequency, and yet still within the range of usefully low thresholds, (this latter was estimated from

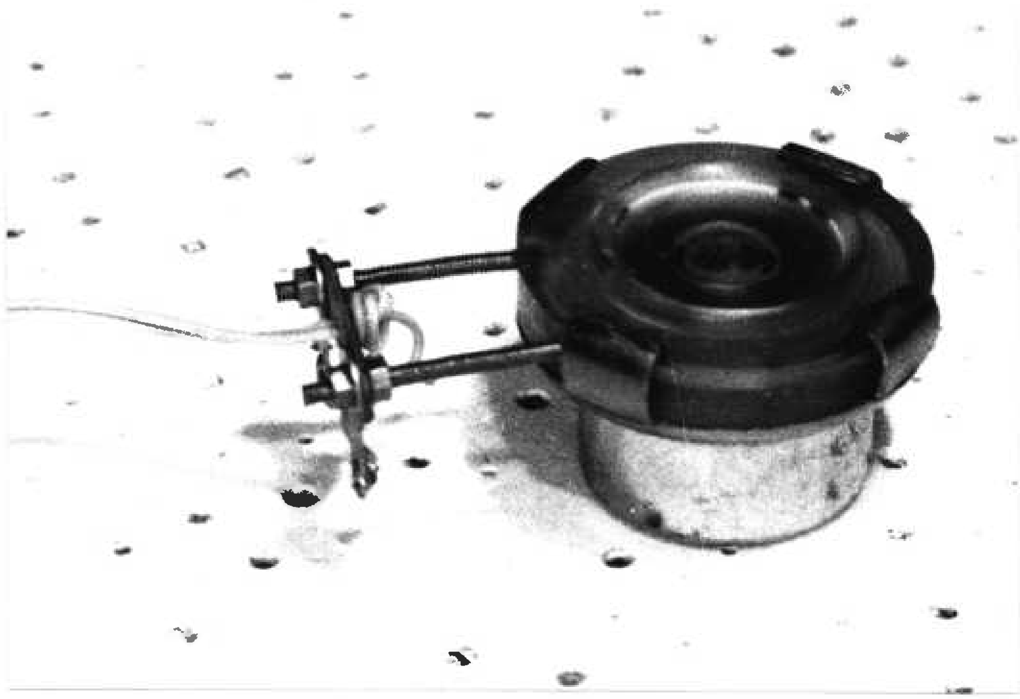


Fig. 3/3

The vibrator employed for most of the experiments. The diameter of the peg is 6 mm., and the vibrator is shown approximately twice life-size. Further details of the construction can be seen in Appendix 2.

the data of Verrillo). To choose a lower frequency might have resulted in the loss of a useful part of the sensation range. The precise values of the voltages and frequencies are of course arbitrary.

The subject used a finger of her left hand, (often the index finger), with the fingertip resting on the plastic peg of the vibrator. The left hand was used for convenience as the subject is right-handed. Weinstein & Sersen,(1961), and Ghent,(1961), have discussed the relationship between handedness, laterality and tactual sensitivity. The left hand in dextrals appears to be slightly more sensitive, although more recent work does not confirm this,(Carmon, Bilstrom & Benton, 1969).

The subject's right hand was used to operate the control switch. The subject is totally deaf, (meningitis deprived her of most of her hearing when she was 27 months old. She became totally deaf at 14 and is now 31), and so no masking sound was needed.

As mentioned in the previous subsection, the tracking method was employed to measure the subject's threshold and the Weber Fractions for various types of stimuli.

One important property was built into the apparatus. I decided to arrange, (having found during the course of electronic experimentation that this was possible), that the amplitude and frequency of the signal driving the transducer should vary 'logarithmically' when a linear control voltage was supplied. I made this decision on the grounds that it was likely that some sort of logarithmic relation, of the type employed by Weber's law, would be found.

The relative constancy of the Weber Fraction at suprathreshold values of amplitude for hearing,(see D'Amato, 1970), is probably a typical sensory performance, although for some sensations there is a slight increase in the ratio at large 'amplitudes'. Previous measurements of the Weber Fraction for vibrotactile frequency discrimination, (with amplitude uncontrolled), suggest that nothing unusual will be found. Weber Fractions for vibrotactile amplitude do not appear to have been measured, although Geldard(1957) does quote a few figures from an unpublished thesis. These are for 60 c.p.s. and are around 20%.

This decision made it possible to employ simple circuitry for producing 'triangle wave' oscillations and these waveforms provided the control voltages.

The diagram below shows, schematically, the interconnections between the three sections of the equipment.

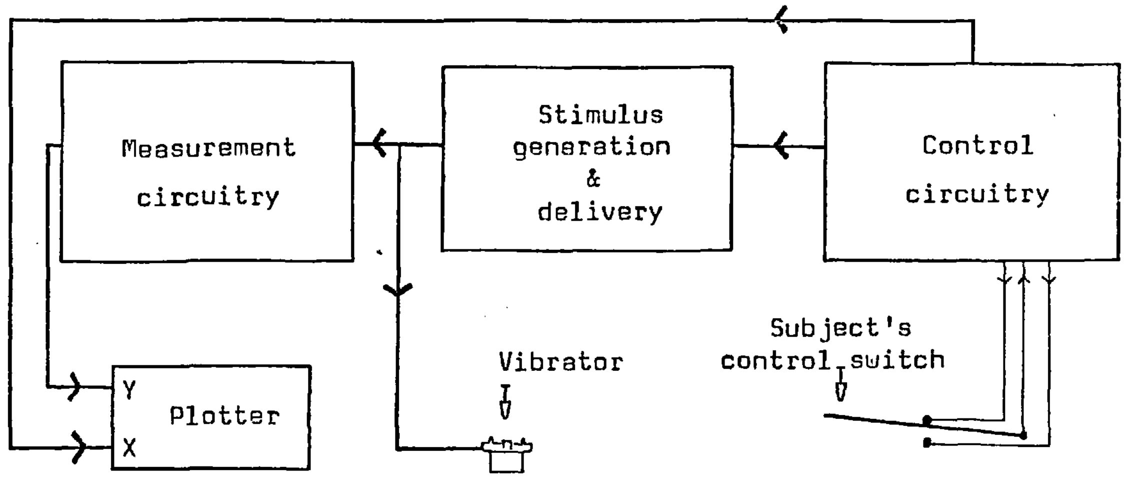


Fig. 3/A

The stimulus generator provides a stimulus 'logarithmically' related to the control voltage. The details are given in Appendix 2.

The control signal generator contains three oscillators. An oscillator with a very long period provides a signal which is used to control the 'X' of the X-Y plotter, and which can be used to gradually change the stimulus amplitude and/or frequency. The period of this oscillator was adjustable, but the most used period provided for a track which lasted about 1¼ minutes.

The subject's control switch controls a second oscillator. This is, like the first, a 'triangle-wave' oscillator. At a suitable point in the circuit a voltage can be tapped which is the 'triangle-wave'. The oscillator is not a resonant circuit type, but a bi-stable type, and therefore it can be triggered, by the application of a suitable pulse, into changing its state. The subject's control switch provides such pulses in a strictly alternating sequence. There exists the possibility with such a system of the oscillator changing state 'naturally' and thus confusing the subject, but with careful adjustment the likelihood of this happening can be reduced.

The output from this oscillator was used to change the amplitude of the stimulus when the subject was tracking her threshold. For measurements of the Weber Fraction the output of the oscillator controlled the amplitude of the third oscillator.

This oscillator was, for convenience, electrically similar to the stimulus generation oscillator, and thus responded 'logarithmically' to linear ramps from the second oscillator. The third oscillator produced the 'modulation' signal, (square-wave), at 5 c.p.s., at an amplitude controlled by the second oscillator. Thus a 'logarithmically' increasing, or decreasing, signal with a frequency of 5 c.p.s. was applied to the stimulus generator which itself responded 'logarithmically'. This was convenient and does not appear to have any disadvantages for the tracking method.

There is conceivably, an advantage in having this type of control 'loop' containing the subject and the 'logarithmic' control of a 'logarithmic' response oscillator. This accrues from the fact that the subject's control of the amplitude of the modulation signal results in any ascending trial commencing at the level at which the previous descending trial finished, and vice versa. This would not be sound practice with the conventional method of limits, although the Békésy type of audiometer works in this manner, (this was the apparatus he used for his experiments with threshold tracking), and the normal practice is to start at some level different from that which caused the subject's response to change. This is done to avoid the possibility that the subject, by responding with 'different' (say) every seventh (say) time a stimulus is presented, will render meaningless the results.

With the tracking method a linear control voltage might allow the subject to respond in an apparently meaningful fashion by in fact responding at regular intervals in time. This possibility exists in the equipment configuration used for tracking threshold, (it also existed for the subjects doing Békésy's experiment but he does not mention any problems). However, in the Weber Fraction experiments slight errors in estimating the time at which a response should be made, (assuming conscious or subconscious perversity of the subject) will quickly lead to changes in the incremented stimulus level, and conversely, (assuming co-operativeness in the subject), if the Weber Fraction is only slightly changing with time, (and therefore with amplitude and/or frequency), the subject's task is perhaps made easier by the rhythm of the response altering quite markedly, for a small change in the ratio. It was decided that, unless the results indicated that the subject was being perverse, (by being metronomic with the control switch), it would be interesting to try both tracking methods.

With the abscissa of the plotter controlled by the long period oscillator the ordinate was controlled by the output of the 'measurement' circuit. This circuit provided a D.C. voltage equal to the peak-to-peak voltage driving the vibrator when threshold was being tracked, or a D.C. voltage equal to the peak-to-peak voltage of the modulation signal when the

Weber Fraction was being tracked. The modulation signal was asymmetric to allow the alternation of standard and incremented values of the stimulus. A symmetric modulation signal would have resulted in the positive and negative incremented values alternating.

For the Weber Fraction measurements the data are easily read from the graphs, calibration graphs taken each day the equipment was used enabled the ordinate to be directly calibrated in percentage change. In fact the equipment performed consistently enough for the same scale to be used for all the tracks. The error involved in this is not great, the error involved in assuming the response of the stimulus oscillator to be truly 'logarithmic' is probably greater. (See Appendix 2).

In the threshold tracking experiments the ordinate is calibrated in an indirect manner. (This is described fully in Appendix 2). Measurements were made, using myself as well as the subject, of the acceleration and displacement of the vibrator with a finger on the plastic peg. The measurements were made at a larger number of frequencies and amplitudes for myself as subject, and a reduced range with the subject taking part because of the time taken to make the measurements and the fact that the vibrator began to deteriorate after months of use. The attaching and removing of the small accelerometer used for the measurements appeared to accelerate the deterioration of the vibrator.

As a consequence the calibration data for the threshold measurements is based on data for myself. The subject's data was not ignored but because of its variable quality it was judged and used according to its agreement with the data for myself. I assume that it is valid to use these data for the subject, but the accuracy is probably not very good.

To have left the calibration of the vibrator for so long, (several months to give the subject time to accustom herself to the stimulus and the experimental situation), appears to have been an error of judgement. However, to have carried out the calibration early in the course of the experiments might well have resulted in a damaged vibrator with reduced 'life-time'. This would have necessitated the use of a second vibrator (and calibration procedure to go with it). The amount of time taken to obtain the calibration data using myself as the subject, (I was very conscious of the difficulties of using an accelerometer), was about 12 hours. Two hours of work with the subject produced a fraction of the data, and it was of poorer quality. She had no comprehension of the importance of physical stillness, necessary for some of the measurements and to have attempted to persuade her to sit still enough for long enough would have been unfair if not impossible, (she had an uncomfortable and persistent skin complaint).

The threshold data is therefore of less value than it might have been, as regards the accuracy of the absolute values of peak-to-peak displacement. The inaccuracies do not affect the value of the data for making comparisons not involving a need for precise, absolute threshold values.

3 b) 2).

In this subsection I present the results of the psychophysical experiments in which threshold was tracked.

Because of the inaccuracies involved in converting the threshold tracks, (these produce values for the threshold in millivolts), into curves showing the threshold in microns, this has not been extensively done.

Fig. 3/4 shows plots of threshold, at the tip of the subject's index finger, from four widely spaced days (18/10/'71 - 12/6/'72). The points are the points halfway between the subject's 'decision points', (i.e. halfway between the 'peaks' and 'valleys' in the tracks, caused by the subject's use of the control switch to increase and decrease the stimulus amplitude). The threshold tracks chosen for this graph are the 'best' obtained on each day, (i.e. the track with the lowest threshold, in millivolts, in the region 200-400 c.p.s.). The conversion from millivolts to microns was done using the curve in Appendix 2, (the accelerometer was not attached). This curve is an estimate of the conversion factor, based on data for myself, the subject, a knowledge of the transducer's behaviour and of the performance of the accelerometer. (The errors are difficult to estimate but could be as high as 50%, see Appendix 2).

Some data for a vibrating rod held between two fingers is shown for comparison, (taken from Békésy, 1939). It is the threshold curve showing the highest sensitivity that is reported in the literature.

Fig. 3/5 shows the upper and lower limits within which lie 5 plots of the type illustrated in Fig. 3/4. The tracks from which the plots were taken, however, were made with the accelerometer attached. The two Figures reveal the effect of loading the vibrator with the accelerometer. Despite the errors I consider the difference between the two Figures to be a genuine reflection of the loading effect. This is because the threshold tracks themselves show a large difference in threshold at higher frequencies. Although the absolute threshold values are subject to unknown errors they are nevertheless quite plausible; the square-wave driving signal does not appear to have any adverse effects, and the difference between the threshold values at 25 c.p.s. and 250 c.p.s. appears to be no greater than the values given by other workers, (see Appendix 1).

Fig. 3/6 shows the effects on the threshold of vibrator contactor size. The values obtained with the smaller contactor are 3-5 times the values obtained with the larger contactor, (this depends on the frequency).

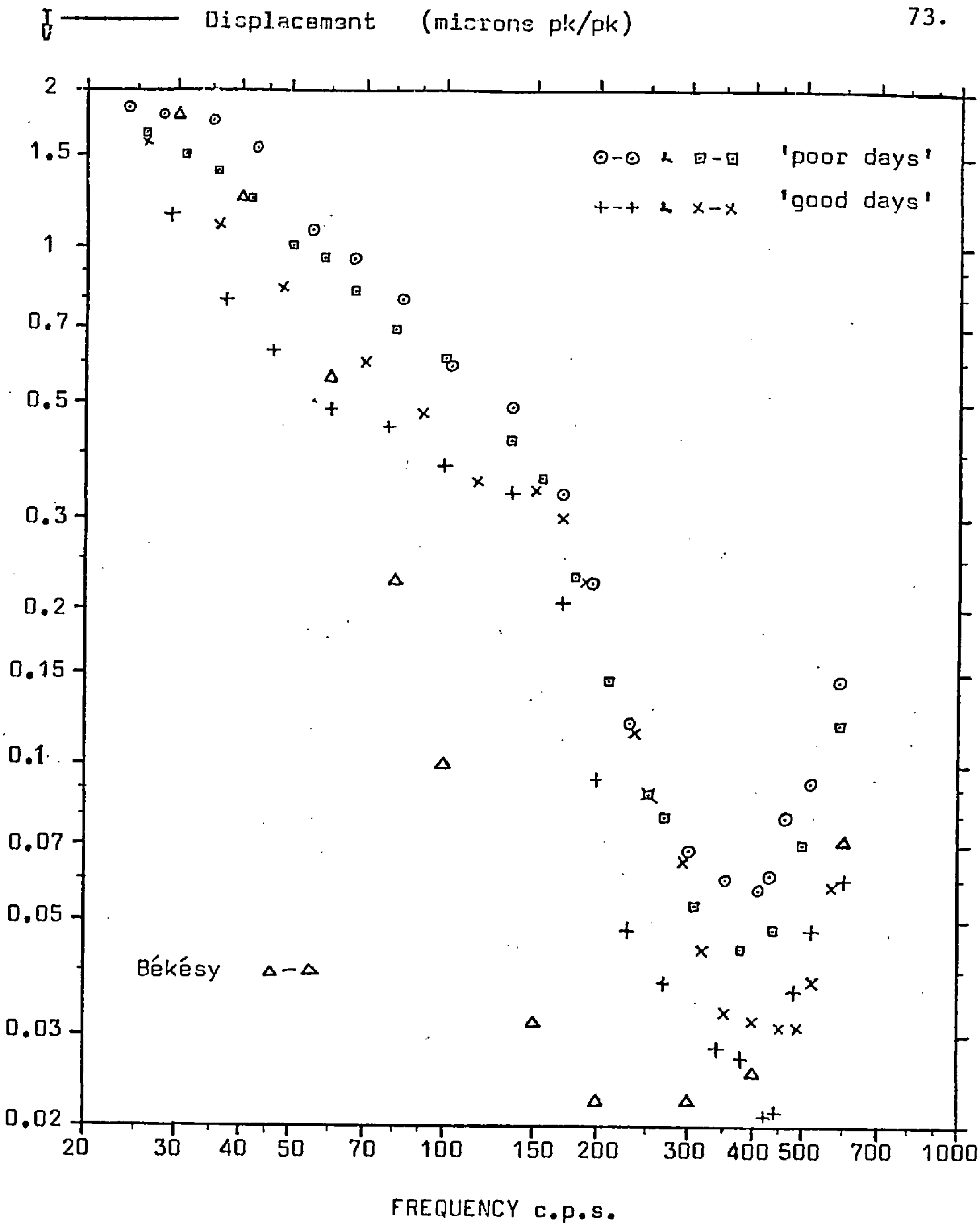


Fig. 3/4

Thresholds at the tip of the subject's index finger as a function of frequency. The data from four widely separated days are shown, two being 'good days' and two being 'poor days'. The points plotted are the points half way between the upper and lower 'decision' points on the tracks. (The accelerometer was not attached, and the data were converted from millivolts to microns using the graph shown in Appendix 2). Data from a paper by Bekesy, (1939), are also shown.

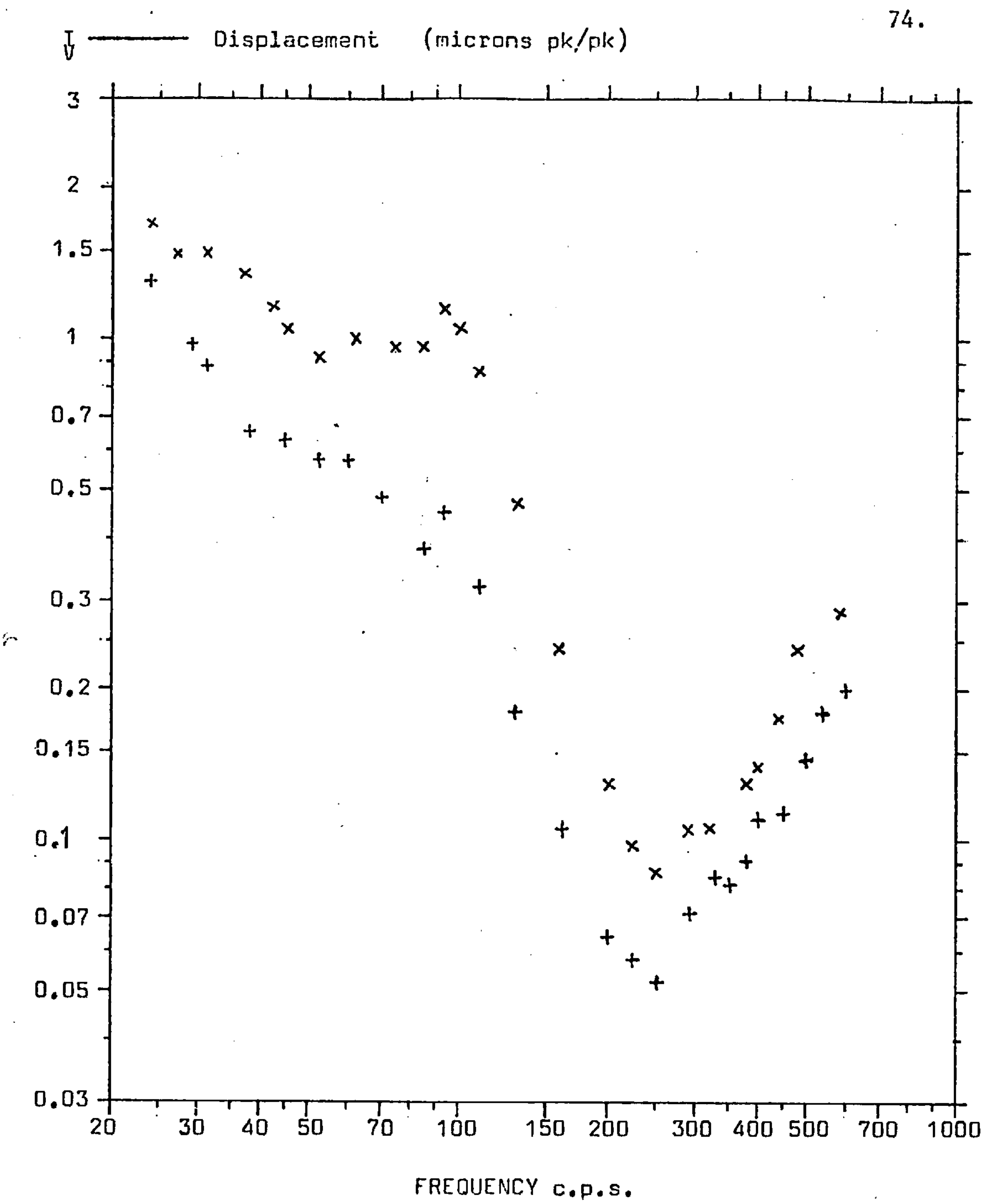


Fig. 3/5

Threshold with accelerometer attached. The site stimulated was the subject's index finger-tip. The two sets of points show the upper and lower limits between which lie 5 graphs of the type shown in Fig. 3/4. (The data were converted from millivolts to microns using the graph shown in Appendix 2).

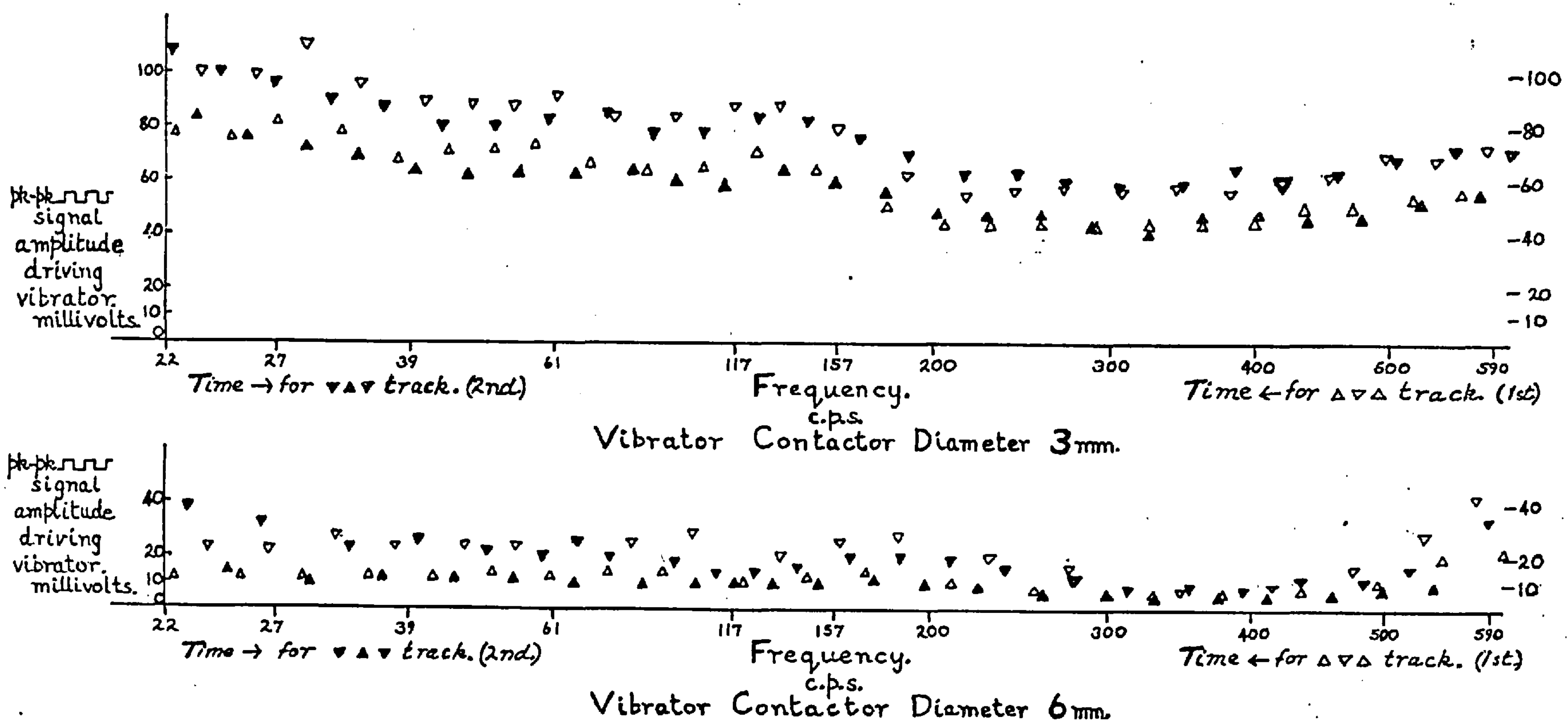


Fig. 3/6

Threshold tracks for the index finger-tip, (showing only the 'decision' points), for two contactor sizes. The tracks also show that there is no appreciable difference between tracks which start at low frequencies and tracks which start at high frequencies. (Unless otherwise stated each of the tracks shown in this chapter took approximately 75 secs. to plot.)

The smaller contactor was not used extensively and it is therefore difficult to generalise about the differences in threshold. However these differences are larger than would be expected, (an elevation of 6dB. would be expected and 12dB. (or more, at higher frequencies), is found). This is possibly due to the fact that a different vibrator was employed. It is to be expected that there will be differences between the vibrators used, although the difference found is perhaps a little surprising.

Fig. 3/6 also shows that there is no appreciable difference between tracks made with the stimulus increasing in frequency and those made with frequency decreasing. (Open and filled triangles). A fault developed in the circuitry, (see Appendix 2), which made difficult repeated reversal of the direction of threshold tracking. Thus all the subsequent threshold tracks were made with the frequency of the stimulus increasing.

The decision to use the larger vibrator contactor area, and the brief demonstration of the unimportance of the 'direction' of the track, depend on one day's work. I do not consider that the value of the subsequent work is undermined by these decisions.

The examples which follow, (both of threshold tracks and Weber Fraction tracks), are usually not unique. In nearly all cases two or more examples illustrative of the same point could be found in the data recorded, (this amounts to three or four times the data presented in this thesis). The decision as to which examples to show was made largely on the ease with which the data could be redrawn or photographed for presentation in the thesis. Factors affecting this decision were cleanliness, number of tracks on the sheets of paper under the pen of the plotter (this alters the number of photographs needed), amount of 'jitter' on the tracks, (see Appendix 2), and to some extent the actual 'content' of the tracks.

Fig. 3/7 shows two tracks from a series. These show very clearly the phenomenon of adaptation of the cutaneous sensitivity to vibrotactile stimulation. The thresholds get progressively lower over a period of time. Moreover this is not an unusual set of tracks. The process of adaptation can be seen in the first few tracks on several different days. On the other hand, it is sometimes not obvious, and occasionally the first track will indicate a lower threshold than is indicated by the subsequent tracks. (See, for example, Figs. 3/11 & 12). This variation is probably explained by day-to-day differences in the subject's threshold.

Fig. 3/8 illustrates that the process of adaptation can be quite prolonged. In this Figure the tracks have been reduced to plots by taking points half way between the decision points. The tracks are from the series which shows the most prolonged adaptation process.

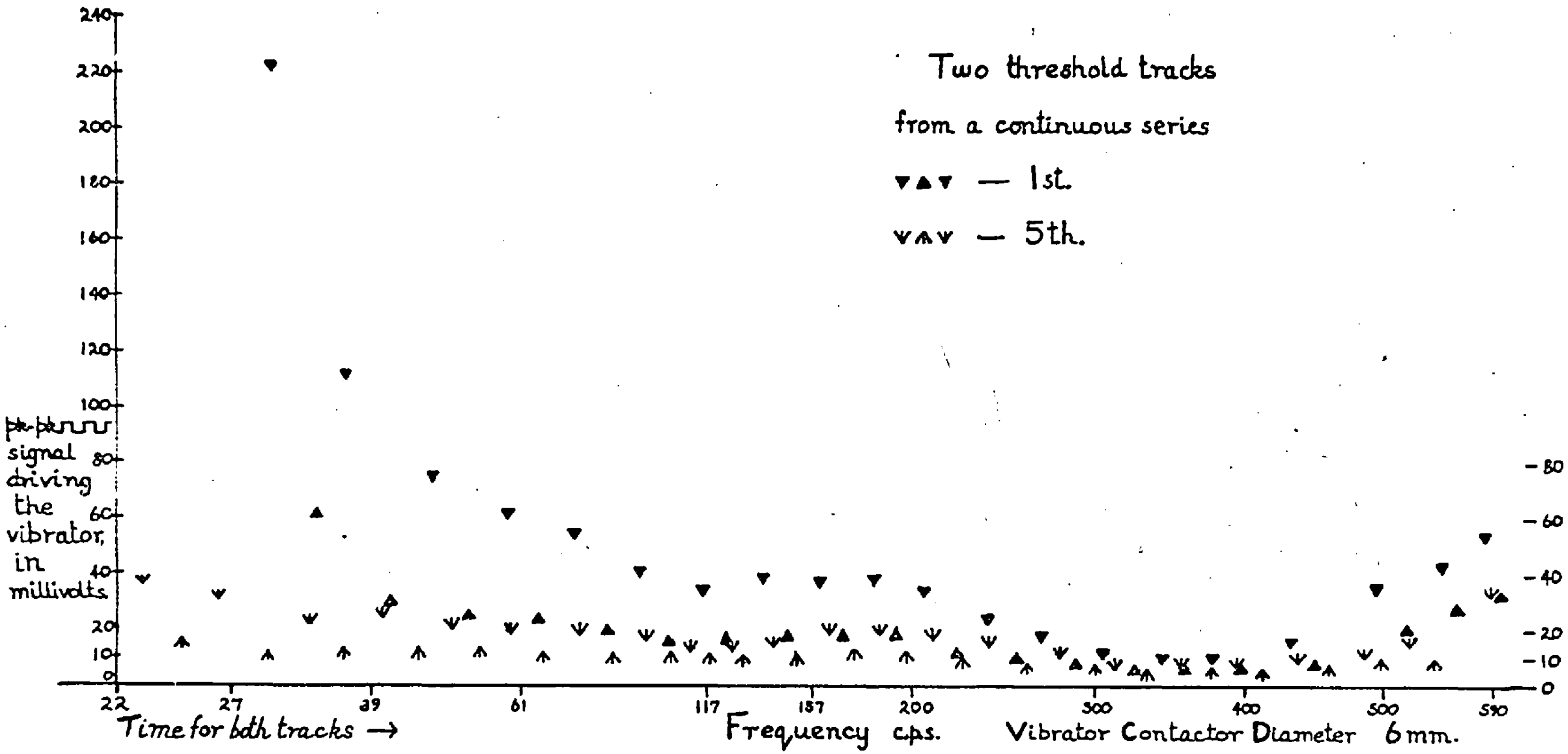


Fig. 3/7

Threshold tracks illustrating the existence of adaptation of the subject's index finger-tip, (the 'decision' points are shown). The five tracks, (of which two are shown), were made without interrupting the subject.

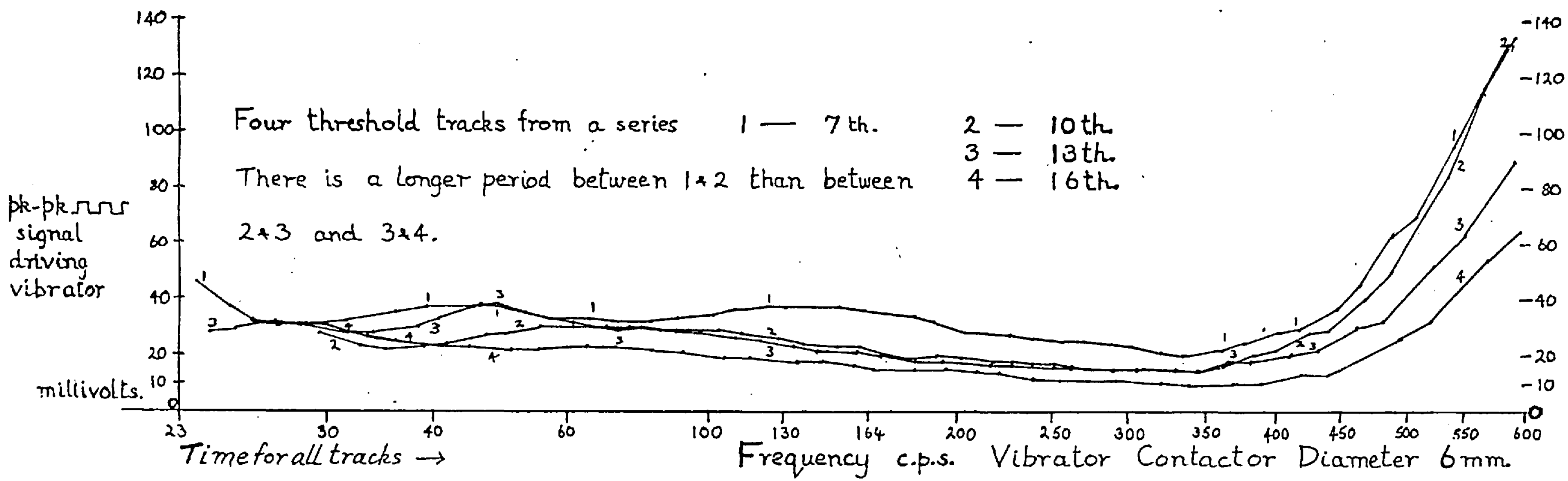


Fig. 3/8

Four threshold tracks, (from a long series with one interruption, between tracks 9 and 10), showing adaptation of the index finger-tip. (The points are those half way between the 'decision' points).

The remaining Figures illustrate various aspects of fatigue. The effects on the threshold of a fatiguing stimulus at 22 c.p.s. are shown in Fig. 3/9. Four plots using points half way between the decision points are shown, the first recorded before the fatiguing stimulus was applied, the remainder after it, (No. 2 starting immediately after the end of the fatiguing period).

The effects appear to be:-

1. General elevation of threshold, (with slow recovery to near normal values).

2. After the fatiguing period the tracking exhibits an erratic quality.

Fig. 3/10 shows three threshold tracks. (The points plotted are the decision points). The first track is a record of the threshold before delivery of the fatiguing stimulus. The second track was made immediately after the fatiguing period, and at the fatiguing frequency of 360 c.p.s., whilst the last track, which immediately followed the second, was made with the frequency varying. The elevation of threshold and the erraticness of the tracks, after the fatiguing stimulus was delivered, are once again shown.

Fig. 3/11 is another illustration of the effects of fatigue, but the effects appear to be different from those detailed above. Seven threshold tracks are shown, using the decision points of the tracks. The first three tracks show the threshold prior to fatiguing the finger. The fatiguing stimulus, at 250 c.p.s. was immediately followed by the tracks numbered 4 - 7, tracks 4 & 6 being made with the frequency constant at 250 c.p.s., and tracks 5 & 7 being made with the frequency varying.

The points of interest are:-

1. The erratic behaviour appears to be confined to the tracks made with the frequency constant, and in this respect track 6 appears to be worse than track 4.

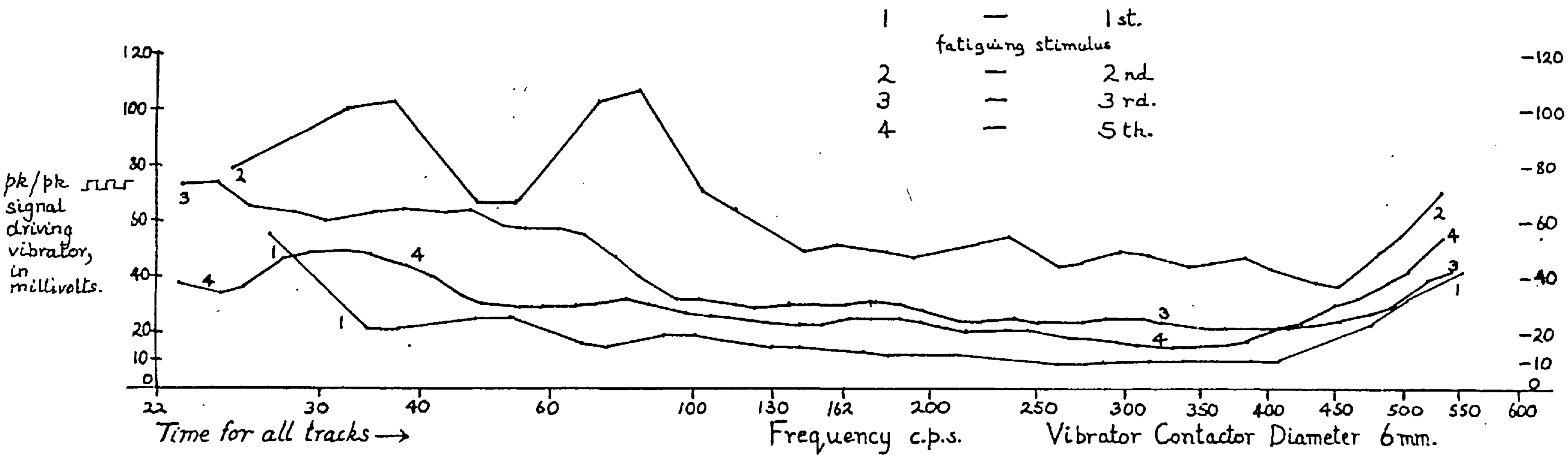
2. The effects on the normal threshold tracks seem to be confined to 200 - 250 c.p.s. and above.

These two observations are not in accord with the observations made earlier, although the findings of Wedell & Cummings,(1938), are perhaps supported by the second point above.

Figs. 3/12-13-14 are photographs of threshold tracks. They provide data which both confuse the overall picture of the effects of fatiguing stimuli and suggest an explanation of them. The 21 tracks shown are divided into three groups, (the tracks are numbered in order of recording).

The first group, (Nos 1 - 9), is shown in Fig. 3/12. The tracks are the thresholds for two fingers, the second finger and the little finger. Thresholds are recorded with the frequency fixed at 300 c.p.s. and with the

Threshold tracks from a series with a fatiguing stimulus between the 1st. & 2nd.



Fatiguing stimulus used:- 1 volt pk-pk at 22 c.p.s. for 1 minute.

Fig. 3/9

Threshold tracks for the index finger-tip showing the elevation of threshold caused by a fatiguing stimulus.

Threshold tracks for the index finger-tip showing the effects of fatigue. The 'decision' points are plotted.

Fig. 3/10

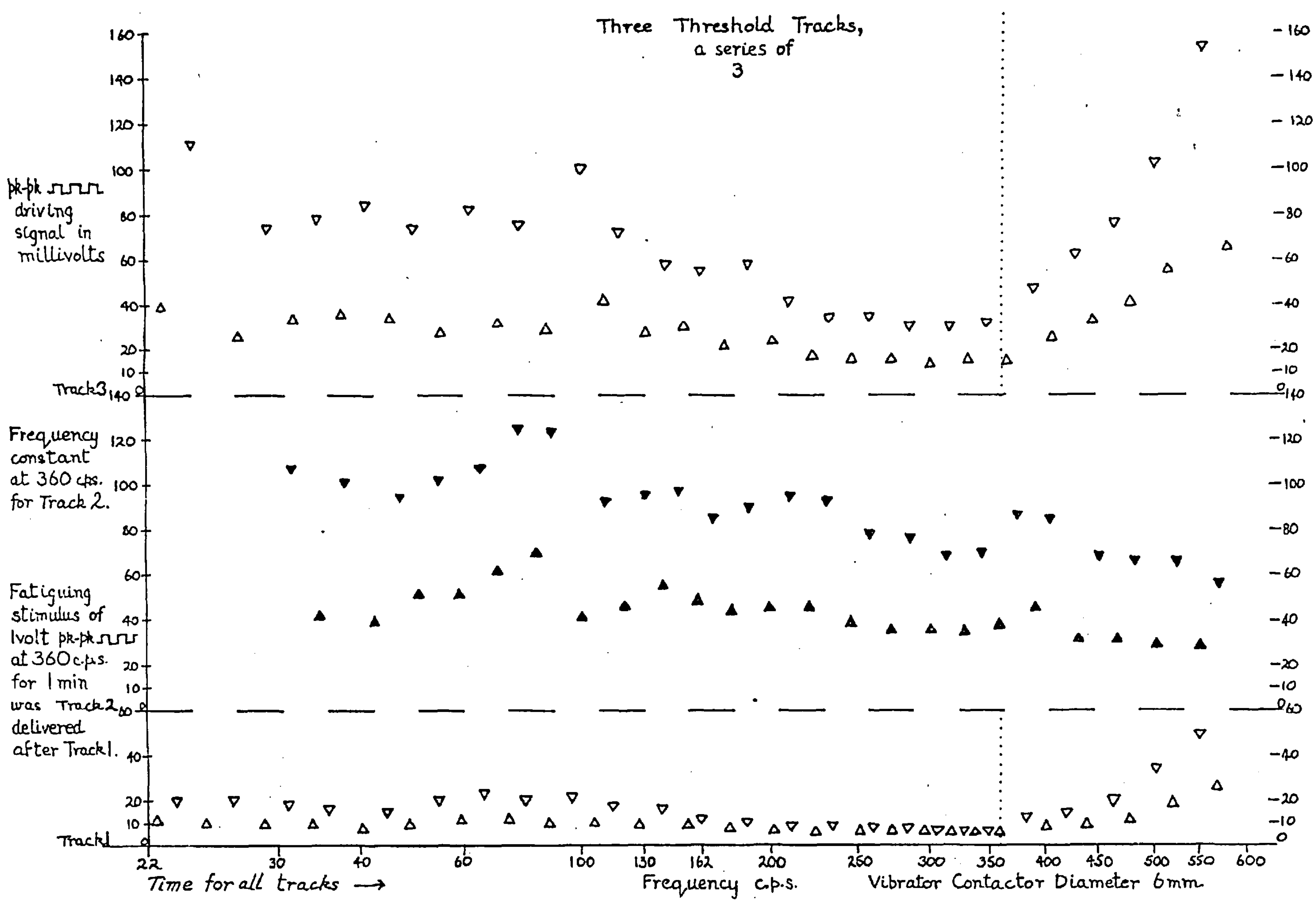


Fig 3/10

Thresholds

Threshold tracks for the index finger-tip showing the effects of fatigue. The 'decision' points are plotted.

Fig. 3/11

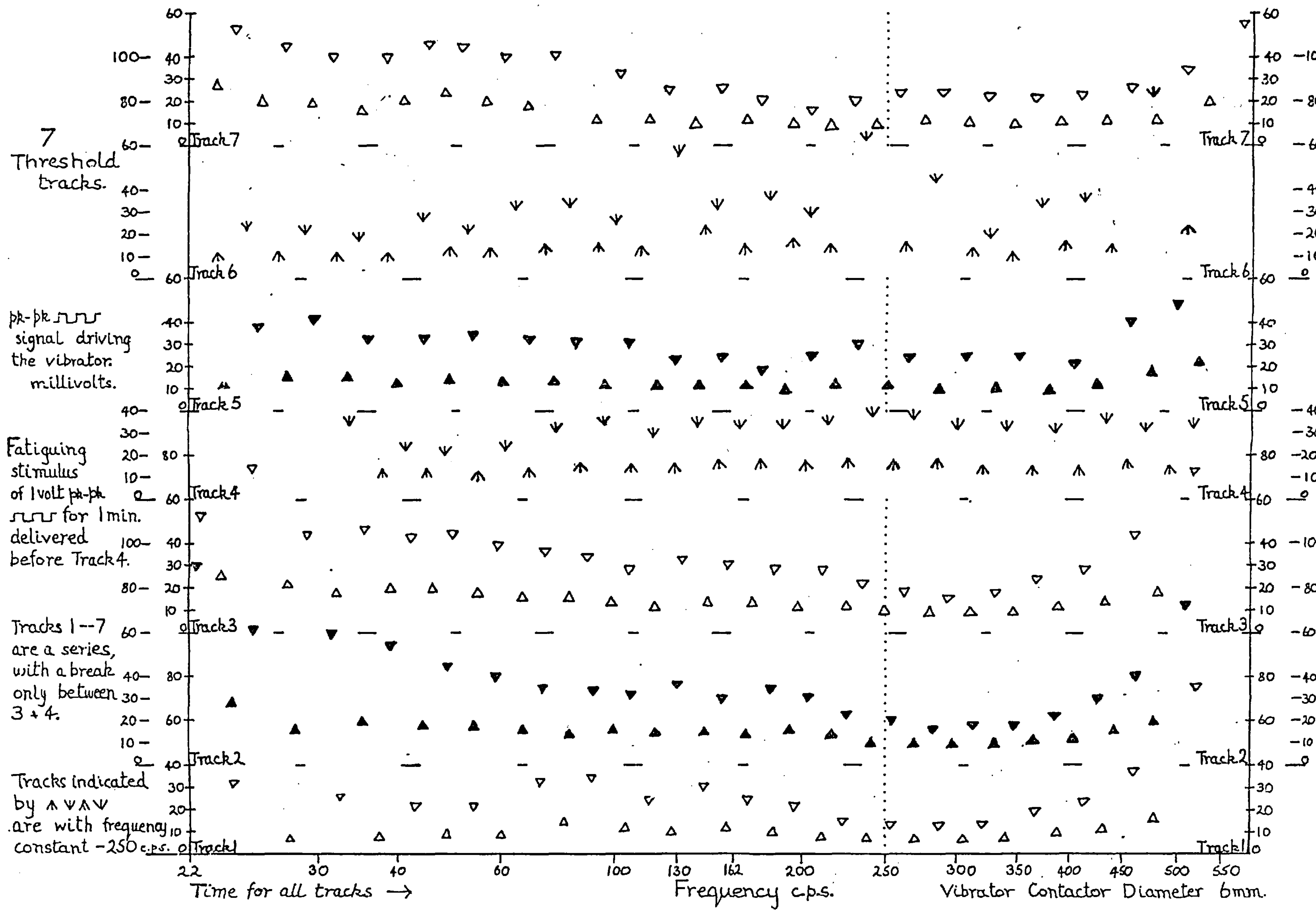
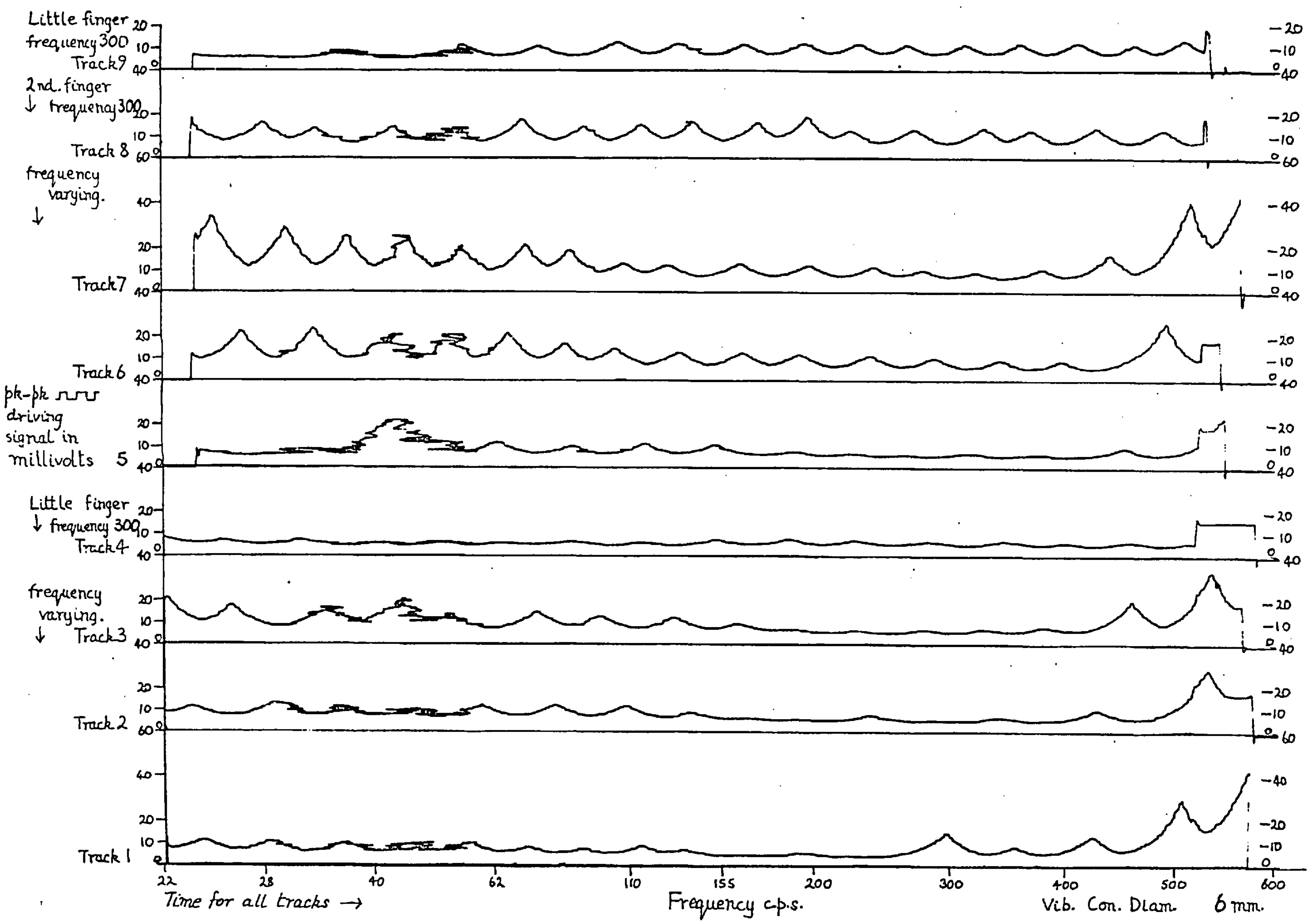


Fig 3/11

Thresholds

Photograph of threshold tracks for two fingers, recorded before delivery of a fatiguing stimulus to the tip of the index finger. The 'peaks' and 'valleys', or local maxima and minima, are the 'decision' points. The horizontal 'jitter' is artifactual. For details see text.

Fig. 3/12



83

Fig 3/12

Thresholds

Photograph of threshold tracks for the second finger, recorded immediately after the delivery of a fatiguing stimulus to the index finger-tip. The 'peaks' and 'valleys', or local maxima and minima, are the 'decision' points. The horizontal 'jitter' is artifactual. For details see text.

Fig. 3/13

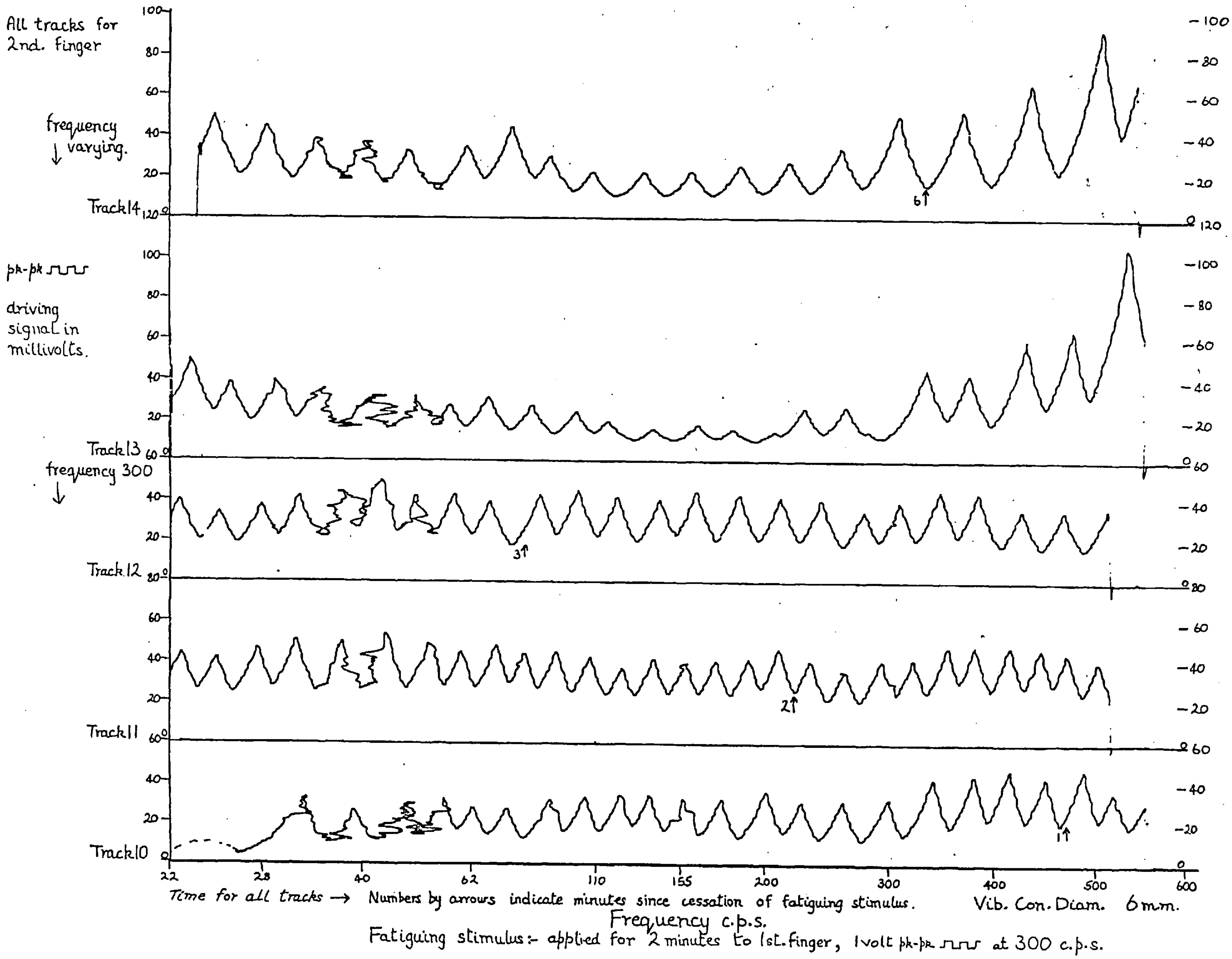


Fig 3/13

Thresholds

Photograph of threshold tracks for the Little finger, recorded immediately after the delivery of a fatiguing stimulus to the index finger-tip. The 'peaks' and 'valleys', or local maxima and minima, are the 'decision' points. The horizontal 'jitter' is artifactual. For details see text.

Fig. 3/14

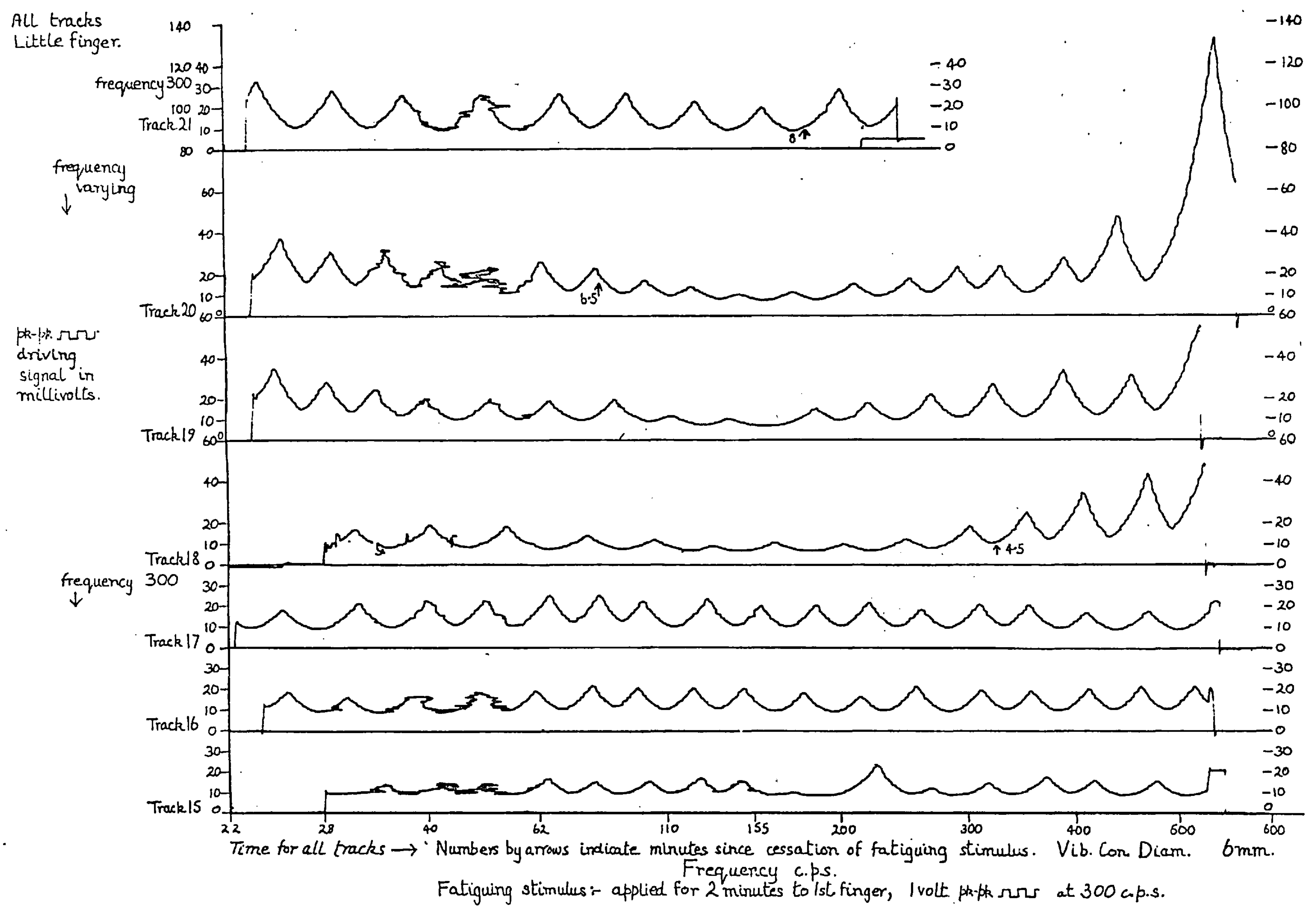


Fig 3/14

Thresholds

frequency varying. The jitter between 28 and 62 c.p.s. is due to dirt or oxide on the slide-wire potentiometer of the X-Y plotter, (see Appendix 2).

The tracks show low thresholds for the two fingers, with perhaps a little fatigue/adaptation occurring in the second finger. They also show that thresholds measured at 300 c.p.s. are low and that the tracks of these thresholds exhibit little or no erraticness.

After track 9 there was a brief break and the subject's index finger was then fatigued for 2 minutes with a 300 c.p.s. stimulus of 1 volt pk-to-pk amplitude. Immediately this finished tracks 10-14 were recorded. (Fig. 3/13). No fatiguing stimulus was given to the second finger, but the threshold tracks show the thresholds for the second finger. There is exhibited in them exactly the sort of behaviour shown in Fig. 3/10, where the fatiguing stimulus was delivered to the same finger as that used to measure thresholds.

The similarity is impressive. The erratic quality is largely confined to the tracks made with the stimulus at 300 c.p.s., (and is not as great as shown in Fig. 3/10). The two tracks made with frequency varying show generally elevated thresholds.

Fig. 3/14 shows the thresholds for the little finger, the tracks being recorded immediately after another period of fatiguing the index finger, (again at 300 c.p.s. for 2 minutes at 1 volt pk-to-pk). Although the effects are smaller they are present. In particular tracks 18 - 21 are of interest. (The 'before fatigue' tracks are Nos. 1 - 4 & 9, Fig. 3/12).

The possibility exists that tracking thresholds with the frequency constant is itself fatiguing, (see, perhaps, track 9), and that this accounts for the elevated thresholds shown in tracks 15 - 17. However, this is no explanation for the degree of elevation shown in track 21. (The elevation recorded in tracks 10 - 12, compared to track 8, seems too large for this to be the case). I am inclined to discount this possibility. From a consideration of all the threshold tracks recorded, and the work of Bjerker et al., (1972), which shows a slight decrease in threshold at 400 c.p.s. as a result of repeated measurements, I would say that tracking threshold is not itself fatiguing, even when consecutive tracks are made with the frequency unvarying. (Bjerker et al. avoid the term adaptation for the effect they find. It could be adaptation but it is a very small effect, 1 - 2dB. reduction over 5 minutes of measurement).

* * * *

The data contained in the threshold tracks are very interesting. They show four things:-

1. The existence of adaptation, as defined earlier. There is the possibility that the subject's freedom of movement caused the increasing sensitivity, (by allowing her to feel for the 'best' spot on her finger). This is unlikely as one would expect the searching to produce much more erraticness than is found, (there do appear to be 'best' spots; when trying to detect low level signals it is sometimes easier if the finger is moved about than if it is kept static).

I consider the effect to be genuine, and the demonstrations of adaptation are, I believe, the first real indications of the phenomenon in cutaneous sensitivity to vibration. Such adaptation is not receptor adaptation, (see subsection 3 c).). Bjerker et al.,(op. cit.), claim to find adaptation but their claim is doubtful, because a) instruction eliminated the effect, and b) the effect appeared to be dependent on frequency, (it was present at 800, 400 & 100 c.p.s. but absent at 50 & 200 c.p.s.). This effect is not that mentioned above.

2. Fatigue is illustrated fairly extensively in the data. The graphs have been selected for inclusion in the thesis. The selection conveys the fairly confused picture that is conveyed by the larger body of data. Figs. 3/9 - 10 - 11 show the various effects observed. The foregoing comments on these figures, when taken in conjunction, are contradictory, but they describe the findings.

3. The data concerned with fatigue illustrate one very interesting phenomenon, a 'complete' example of which is presented in Figs. 3/12 - 13 - 14. (The most impressive example). It appears that a fatiguing stimulus delivered to one finger will elevate the threshold of an adjacent finger, and, to some extent, that of a remote finger.

4. The threshold tracks show that the stimulus does not introduce any anomalies. An exploration of the thresholds such as I have carried out is quite sufficient to indicate that the threshold decreases with frequency up to a frequency around 250 - 400 c.p.s., at a rate of approximately 20dB.per octave. The threshold then rises. This is in general agreement with the findings of others, (see Appendix 1, and Verrillo,1971). It is of interest to note that the frequency at which sensitivity is a maximum is different, with the accelerometer attached to the vibrator, to that when the accelerometer is not attached. (See also Appendix 2).

The threshold tracks show one thing of importance technically, and that is the fact that the characteristics of the transducer match the frequency response of the finger, (c.f. Fig. 3/4 and Fig. 3/12). This means that the hypothesised advantage of the stimulus, (the synthesis of a subjective magnitude range from a stimulus amplitude range reduced through combination with a range of frequencies), does not accrue.

The confusion mentioned in '2' above is I think partly explained by the fact that the measurements were made on different days. Other possible causes, such as differences in fatiguing stimulus frequency and intensity, are ruled out on the grounds that, in general, the effects are too large for small differences, the effects quoted by Wedell & Cummings, (1938), being small for large differences. This confusion, and the phenomenon mentioned in '3' above are discussed in full in section 3 c).

The exploration of vibrotactile thresholds, despite the uncertainty concerning the absolute magnitudes involved, has revealed no anomalies. The existence of adaptation has been positively demonstrated. None of the data indicate that the stimulus delivery system devised should not work, although they do indicate that the hypothesised advantage mentioned above is not available.

3 b) 3).

In this subsection I present the results of the psychophysical experiments in which Weber Fraction was tracked. The experiments were not divorced from the threshold experiments, on some days both threshold tracks and Weber Fraction tracks were recorded. Thus the results are extensive and were acquired over many months. They are, however, simpler to interpret than the threshold results.

The scales for the graphs, to which I shall refer, need some explanation. The abscissae are marked in peak-to-peak amplitude, (volts), and, where applicable, frequency, (cycles per second). These scales are derived from calibration data taken after the day's recordings had been made, (and on some days before, as well). The origin for the abscissae shows a value above zero. This value, (70 mvs. approximately), was chosen as being likely never to be too low, (i.e. below threshold).

The ordinates are calibrated in % directly. The percentage is that of the standard which the increment represents. This is possible because of the design of the equipment, but some frequency ~~differences~~ increments are slightly lower than the % scales imply, and so ~~the~~ these scales are only strictly accurate for the changes in amplitude. It was intended that the frequency ~~differences~~ increments would be such that for each doubling of amplitude the frequency would also double. This turned out not to be the case for the higher frequencies, (the lower frequencies follow this law quite well), and it was not considered worthwhile to try to improve the relationship, (see Fig A2/2 in Appendix 2).

It is assumed that the Weber Fraction tracks made with the frequency constant have the same ordinate scale as those made with the frequency varying. The error involved is small. An overall error value of + or - 10% of the scale values is a realistic estimate, but, as with the threshold experiments, errors are not important unless they seriously affect comparisons between tracks. The nature of the errors is such that at higher frequencies and amplitudes, (above 150 - 160 c.p.s.), the ordinate scale gives values which are too small in 'frequency fixed' Weber Fraction tracks, and too large, possibly, in the 'frequency varying' tracks. (It is possible that a real doubling of frequency with doubling of amplitude would yield lower percentages at the higher frequencies than the tracks record).

In comparing the tracks it should be borne in mind that at higher amplitudes and, where applicable, frequencies, (i.e. above 0.5 volts and 150 - 160 c.p.s. and 0.5 volts), the tracks are 'biased' in favour of the 'frequency fixed' stimulus and against the 'frequency varying' stimulus. Estimates of the amount of bias are not possible.

* * * *

Figures 3/15 - 20 show almost all the Weber Fraction tracks obtained with the vibrator at different sites, (there are more recordings for the first and third fingers). The results are quite typical of the magnitudes and variations obtained on other days for the fingerpad of the first finger, the site used extensively. On some occasions the Weber Fractions are lower than is shown in the Figures. Apart from the values for the palm, (where a higher threshold is presumably responsible for the higher values at the lower amplitudes), there is good agreement between all the sites, as regards the form of the tracks. (The erraticness in the last track for the third finger is due to the subject's tiredness).

In some tracks the effect of fatigue is noticeable. This can be seen at the lower amplitudes in the later tracks for the first and second fingers, and for the thumb. It is manifest as an elevation in the track at low amplitudes, as would be expected. This effect was explicitly explored in one series of tracks, shown in Fig. 3/21.

Unlike the threshold tracks the Weber Fraction tracks are done in both directions, i.e. tracks are recorded with the amplitude of the standard decreasing as well as increasing. This is indicated on the graphs by small arrows which show the direction in which the tracks were made. Like the threshold tracks the decision points are the data points, (i.e. the discontinuities), the curves joining the peaks and valleys record the change in the stimulus, as time passes, between the values at which the subject made her decisions.

Figs. 3/22 - 26 show Weber Fraction tracks in which frequency was kept constant, (several different frequencies were used). Also shown are tracks, made on the same day, with frequency varying, (the values are typical of the best obtained).

Figs. 3/27 & 28 show some interesting effects that can be obtained with the equipment. Fig. 3/27 is of Weber Fraction tracks, (both directions superimposed), made with oscillator 1 running four times faster than was used normally, (i.e. approximately 37·secs. per cycle instead of the usual 2½ minutes). (Oscillator 2 was running at twice normal rate, i.e. 10 secs. per cycle as opposed to 20 secs. per cycle). The difference between the two 'tracks' shown is that one was made with the frequency varying (Fig. 3/27(a)), and the other with the frequency constant (Fig. 3/27(b)), at 400 c.p.s. The increased rate at which the subject has to make decisions does not appear to affect the overall shapes of the tracks, (c.f. Fig. 3/25). This method of tracking was not extensively used.

Fig. 3/28 shows the effect of altering the increments and standard, ('ringing the changes' as I expressed it earlier). In this example the

Fig. 3/15

Photograph of Weber Fraction tracks, with stimulation of the site indicated. The ordinate scales are in percentage, and the tracks record, as a function of time, (and therefore of intensity and frequency), the amount by which the stimulus must increase (as a percentage of the 'original' stimulus) for a change to be noticeable. The 'original' stimulus, (i.e. the standard stimulus), and the incremented stimulus are presented alternately, at 5 c.p.s. The 'original' stimulus changes as a function of time, and the sense of the change is indicated on the tracks by small arrows.

The percentage scale is only strictly accurate for amplitude changes, frequency increments being too small at the higher frequencies. (As with the threshold records the horizontal 'jitter' is artifactual.)

For details see text.

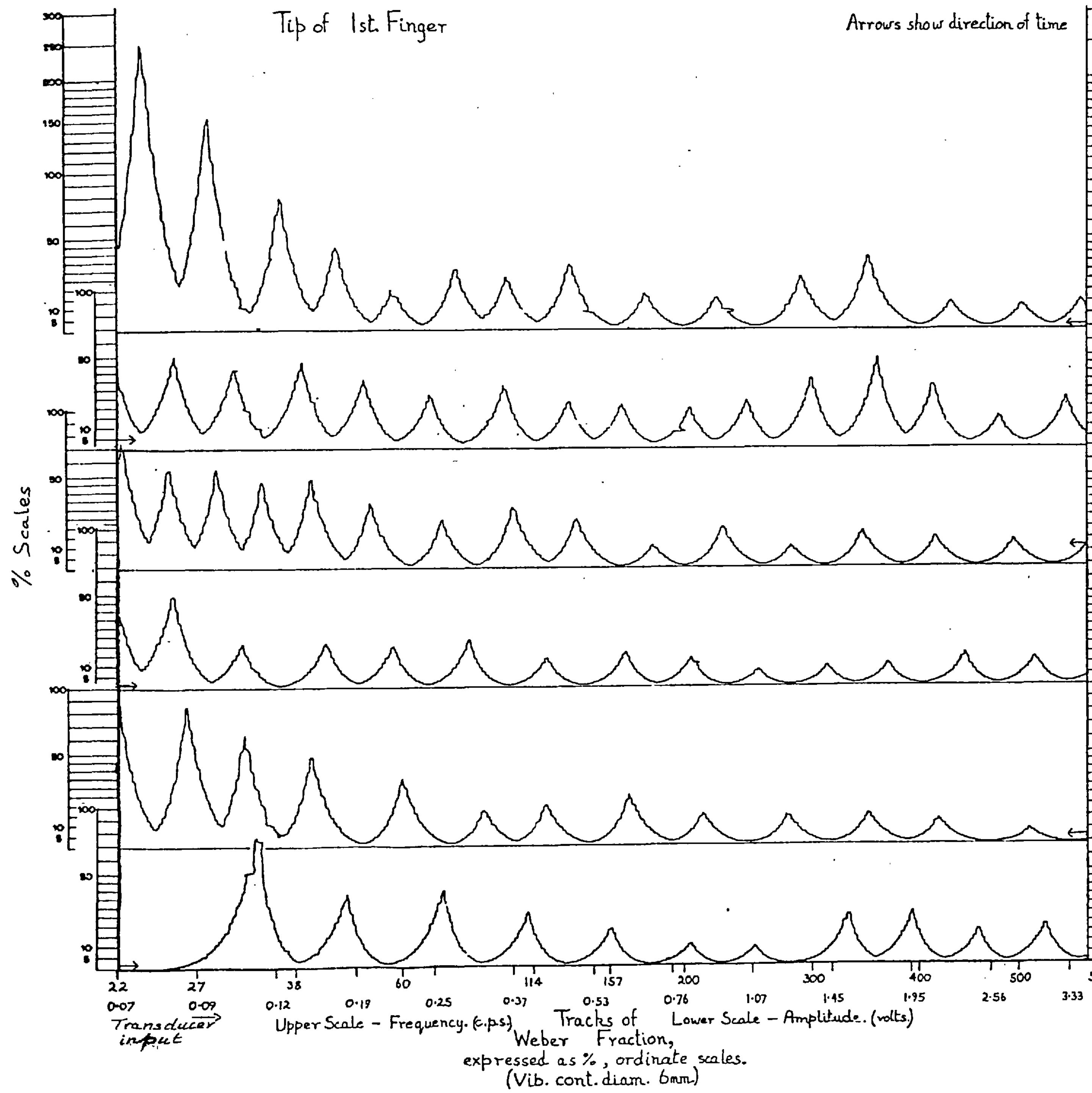
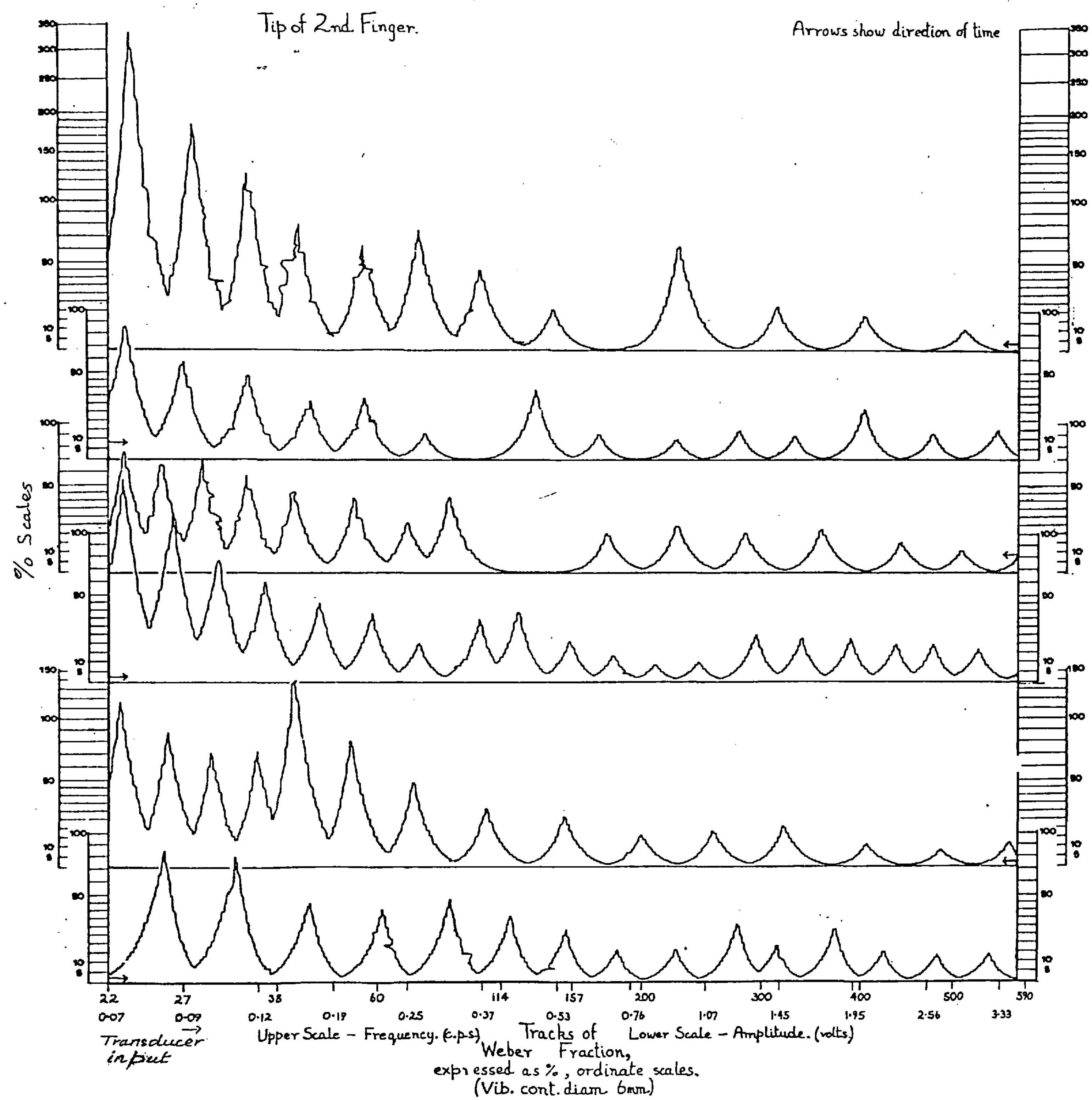


Fig 3/15

Weber Fraction

1st. Finger

Fig. 3/16



Photograph of Weber Fraction tracks, with stimulation of the site indicated. The ordinate scales are in percentage, and the tracks record, as a function of time, (and therefore of intensity and frequency), the amount by which the stimulus must increase (as a percentage of the 'original' stimulus) for a change to be noticeable. The 'original' stimulus, (i.e. the standard stimulus), and the incremented stimulus are presented alternately, at 5 c.p.s. The 'original' stimulus changes as a function of time, and the sense of the change is indicated on the tracks by small arrows.

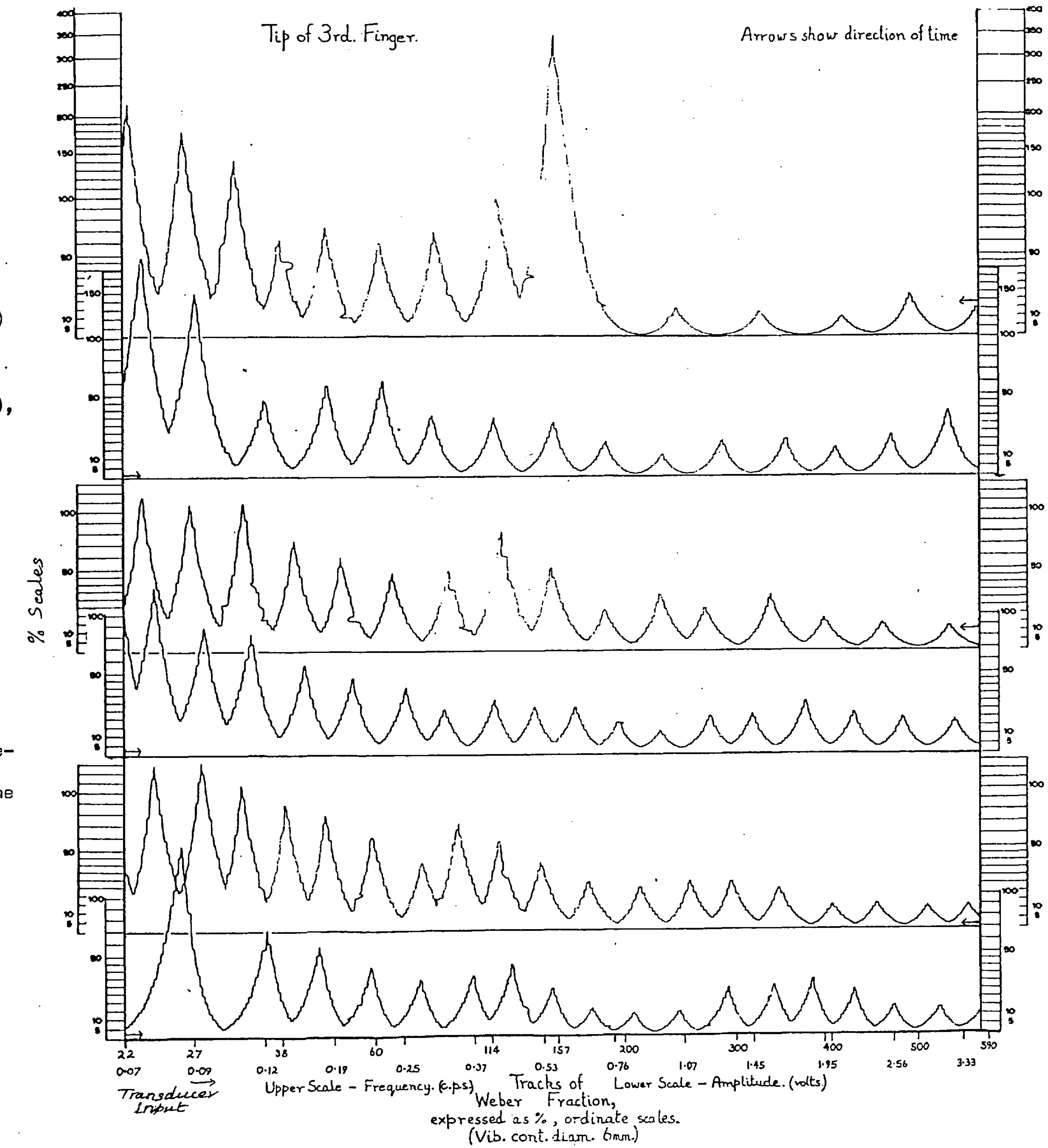
The percentage scale is only strictly accurate for amplitude changes, frequency increments being too small at the higher frequencies. (As with the threshold records the horizontal 'jitter' is artifactual.)

For details see text.

Fig 3/16

Weber Fraction
2nd. Finger

Fig. 3/17



Photograph of Weber Fraction tracks, with stimulation of the site indicated. The ordinate scales are in percentage, and the tracks record, as a function of time, (and therefore of intensity and frequency), the amount by which the stimulus must increase (as a percentage of the 'original' stimulus) for a change to be noticeable. The 'original' stimulus, (i.e. the standard stimulus), and the incremented stimulus are presented alternately, at 5 c.p.s. The 'original' stimulus changes as a function of time, and the sense of the change is indicated on the tracks by small arrows.

The percentage scale is only strictly accurate for amplitude changes, frequency increments being too small at the higher frequencies. (As with the threshold records the horizontal 'jitter' is artifactual.)

For details see text.

Fig 3/17

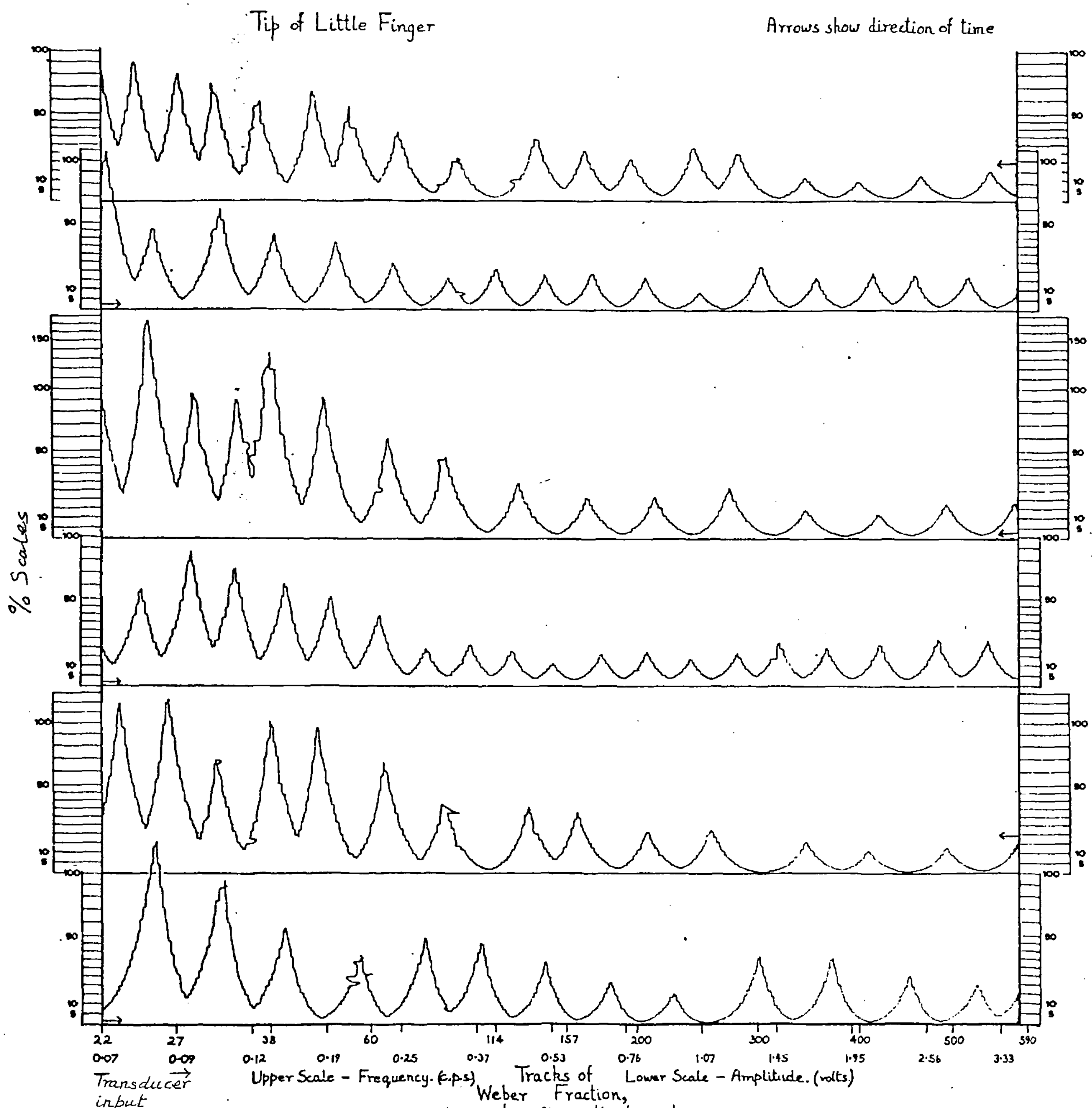
Weber Fraction
3rd Finger.

Fig. 3/18

Photograph of Weber Fraction tracks, with stimulation of the site indicated. The ordinate scales are in percentage, and the tracks record, as a function of time, (and therefore of intensity and frequency), the amount by which the stimulus must increase (as a percentage of the 'original' stimulus) for a change to be noticeable. The 'original' stimulus, (i.e. the standard stimulus), and the incremented stimulus are presented alternately, at 5 c.p.s. The 'original' stimulus changes as a function of time, and the sense of the change is indicated on the tracks by small arrows.

The percentage scale is only strictly accurate for amplitude changes, frequency increments being too small at the higher frequencies. (As with the threshold records the horizontal 'jitter' is artifactual.)

For details see text.



Tracks of Weber Fraction, expressed as %, ordinate scales. (Vib. cont. diam. 6mm)

Fig 3/18

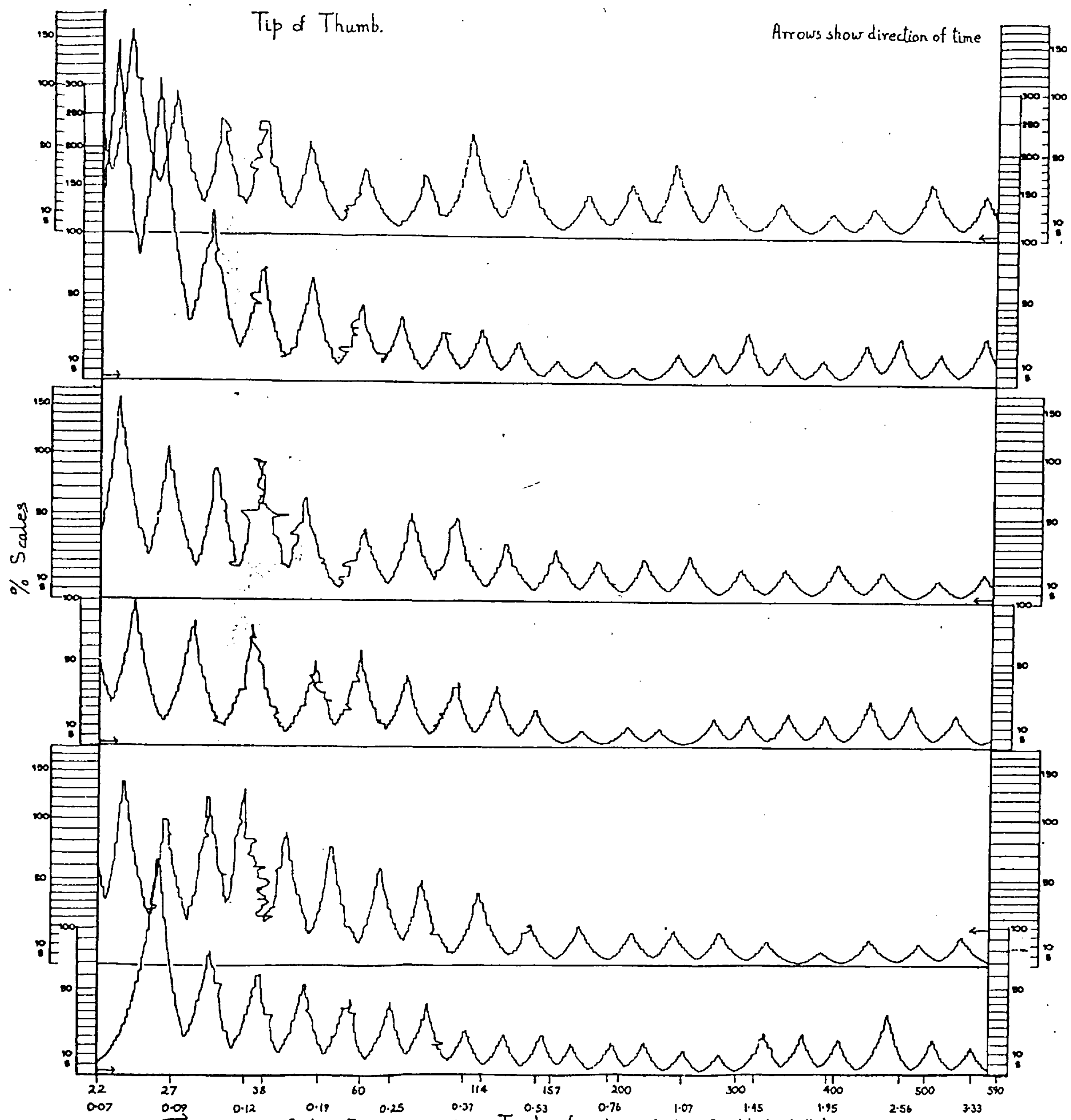
Weber Fraction
Little Finger.

Fig. 3/19

Photograph of Weber Fraction tracks, with stimulation of the site indicated. The ordinate scales are in percentage, and the tracks record, as a function of time, (and therefore of intensity and frequency), the amount by which the stimulus must increase (as a percentage of the 'original' stimulus) for a change to be noticeable. The 'original' stimulus, (i.e. the standard stimulus), and the incremented stimulus are presented alternately, at 5 c.p.s. The 'original' stimulus changes as a function of time, and the sense of the change is indicated on the tracks by small arrows.

The percentage scale is only strictly accurate for amplitude changes, frequency increments being too small at the higher frequencies. (As with the threshold records the horizontal 'jitter' is artifactual.)

For details see text.

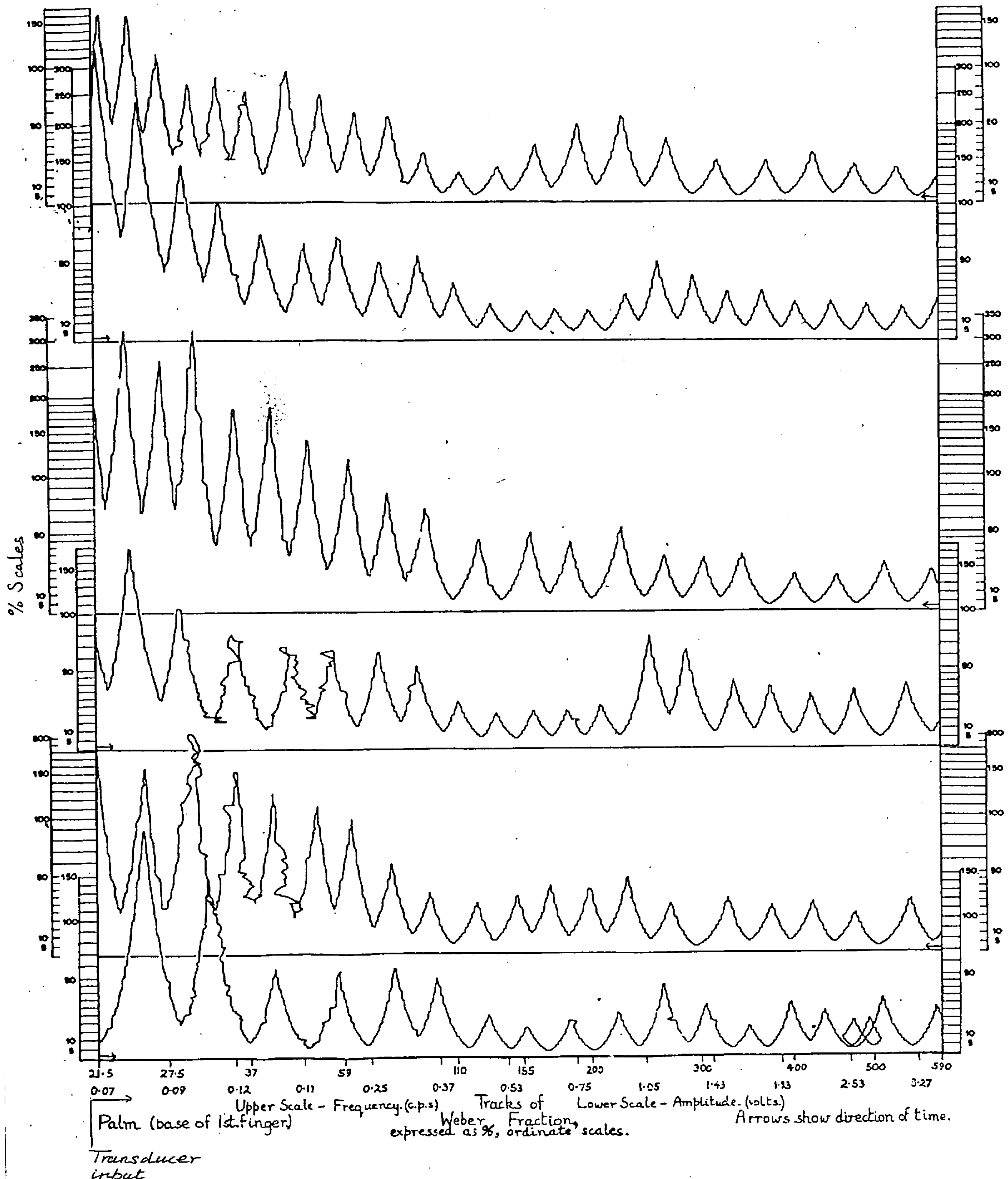


Tracks of Weber Fraction, expressed as %, ordinate scales. (Vib. cont. diam. 6mm.)

Fig 3/19

Weber Fraction
Thumb

Fig. 3/20



Photograph of Weber Fraction tracks, with stimulation of the site indicated. The ordinate scales are in percentage, and the tracks record, as a function of time, (and therefore of intensity and frequency), the amount by which the stimulus must increase (as a percentage of the 'original' stimulus) for a change to be noticeable. The 'original' stimulus, (i.e. the standard stimulus), and the incremented stimulus are presented alternately, at 5 c.p.s. The 'original' stimulus changes as a function of time, and the sense of the change is indicated on the tracks by small arrows.

The percentage scale is only strictly accurate for amplitude changes, frequency increments being too small at the higher frequencies. (As with the threshold records the horizontal 'jitter' is artifactual.)

For details see text.

Fig 3/20

Weber Fraction
Palm

Photograph of Weber Fraction tracks, with stimulation of the tip of the index finger. The ordinate scales are in percentage, and the tracks record, as a function of time, the amount by which the stimulus must increase for a change to be noticeable. (As for Figs. 3/15-20 the percentage scale is only strictly accurate for amplitude changes). The standard stimulus and the incremented stimulus are presented alternately, at 5 c.p.s.

This photograph illustrates the effect of fatigue. For details see text.

Fig. 3/21

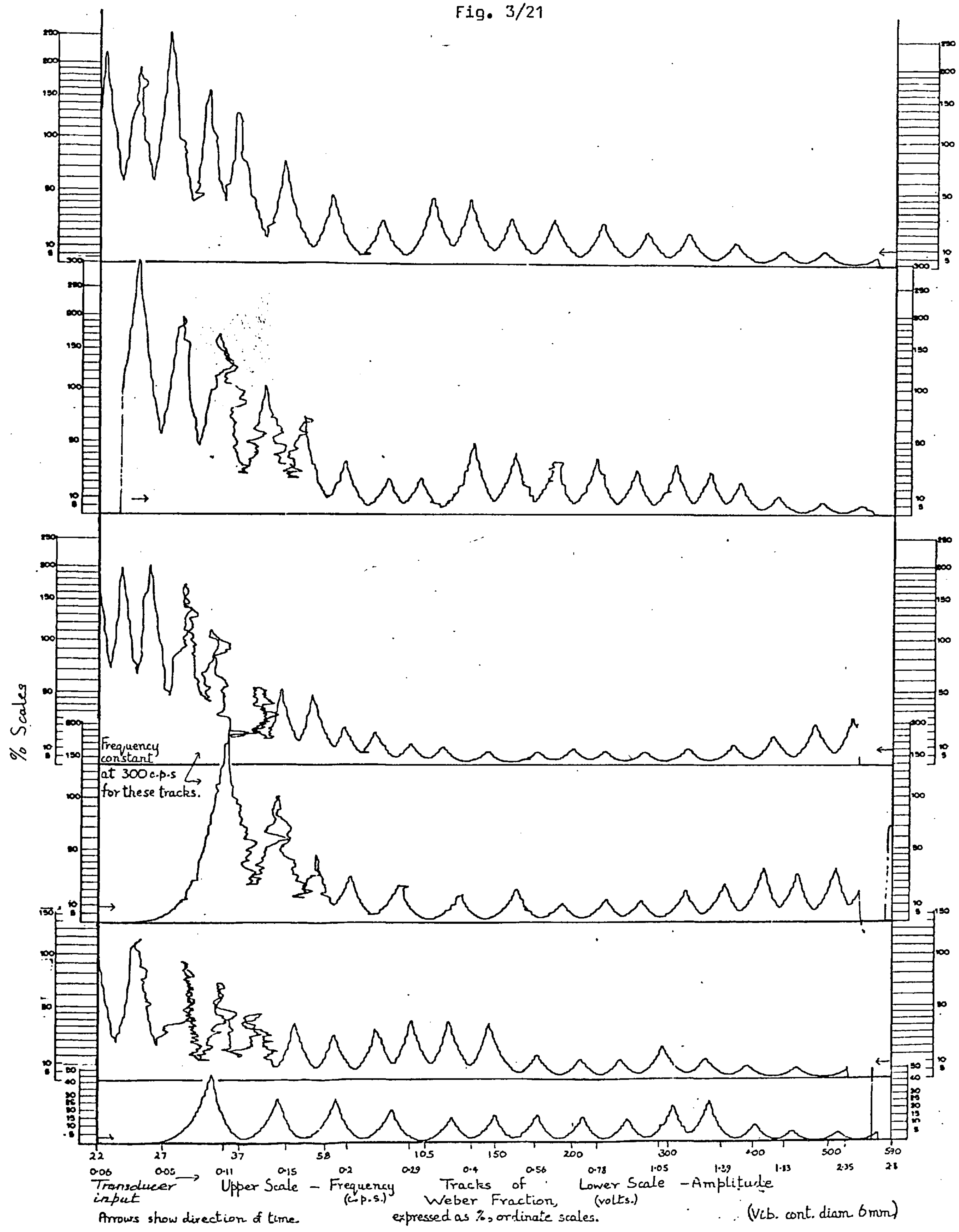


Fig 3/21

Weber Fraction
Fatique.

Photograph of Weber Fraction tracks, with stimulation of the index finger-tip. The ordinate scales are in percentage, and the tracks record, as a function of time, the amount by which the stimulus must increase for a change to be noticeable. (The standard and incremented stimuli alternate at 5 c.p.s.)

The bottom pair of tracks was recorded with the frequency varying, (the same pair of tracks is reproduced in all the Figs. 3/22-26). For the other tracks the frequency was maintained at the value shown for each pair.

Overlapping tracks, as in the bottom set, indicate that the recording was repeated, to give twice the data.

For details see text.

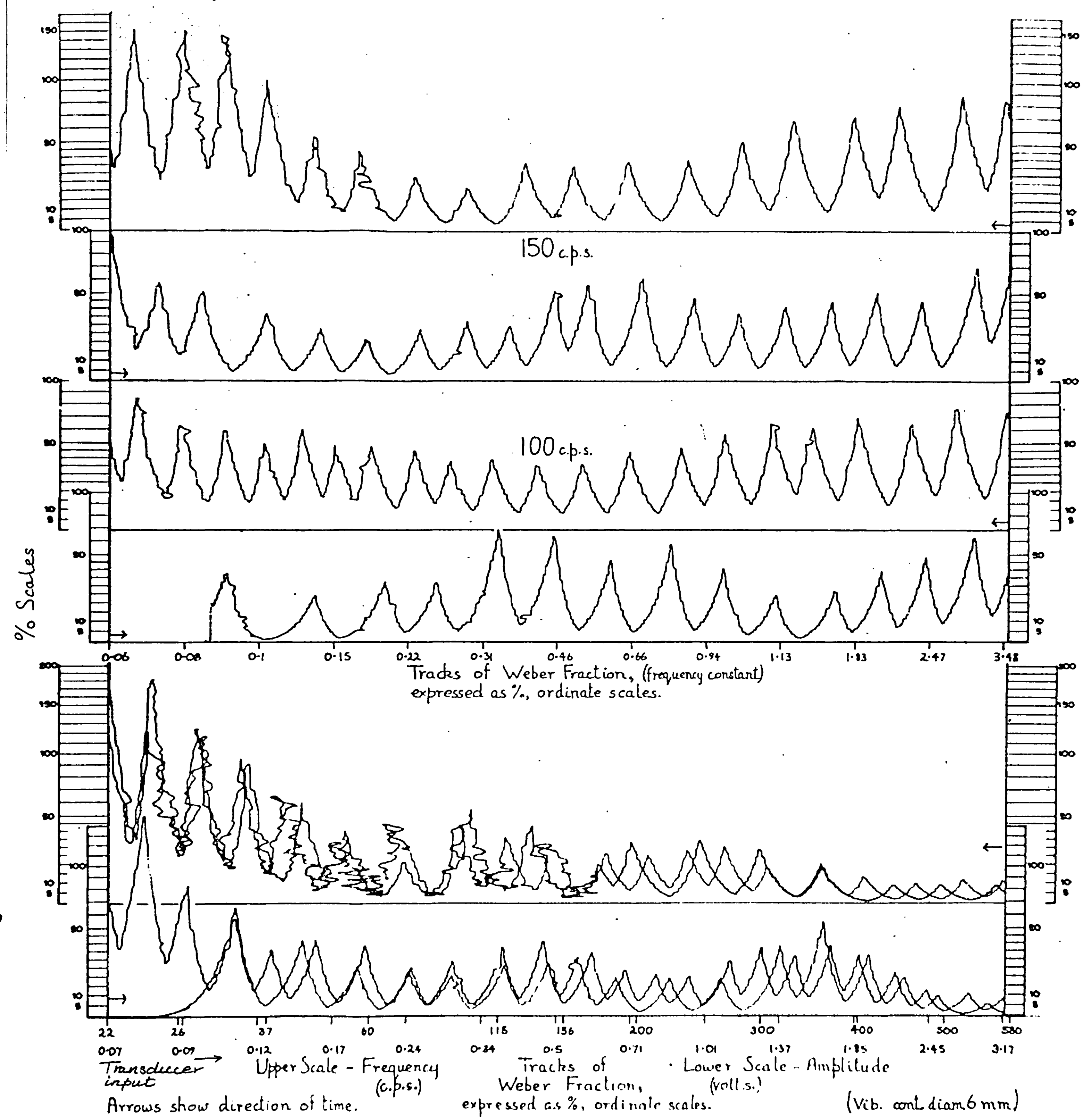


Fig. 3/22

Fig 3/22

Weber Fraction
Constant frequencies
100 cps. & 150 cps.

Photograph of Weber Fraction tracks, with stimulation of the index finger-tip. The ordinate scales are in percentage, and the tracks record, as a function of time, the amount by which the stimulus must increase for a change to be noticeable. (The standard and incremented stimuli alternate at 5 c.p.s.)

The bottom pair of tracks was recorded with the frequency varying, (the same pair of tracks is reproduced in all the Figs. 3/22-26). For the other tracks the frequency was maintained at the value shown for each pair.

Overlapping tracks, as in the bottom set, indicate that the recording was repeated, to give twice the data.

For details see text.

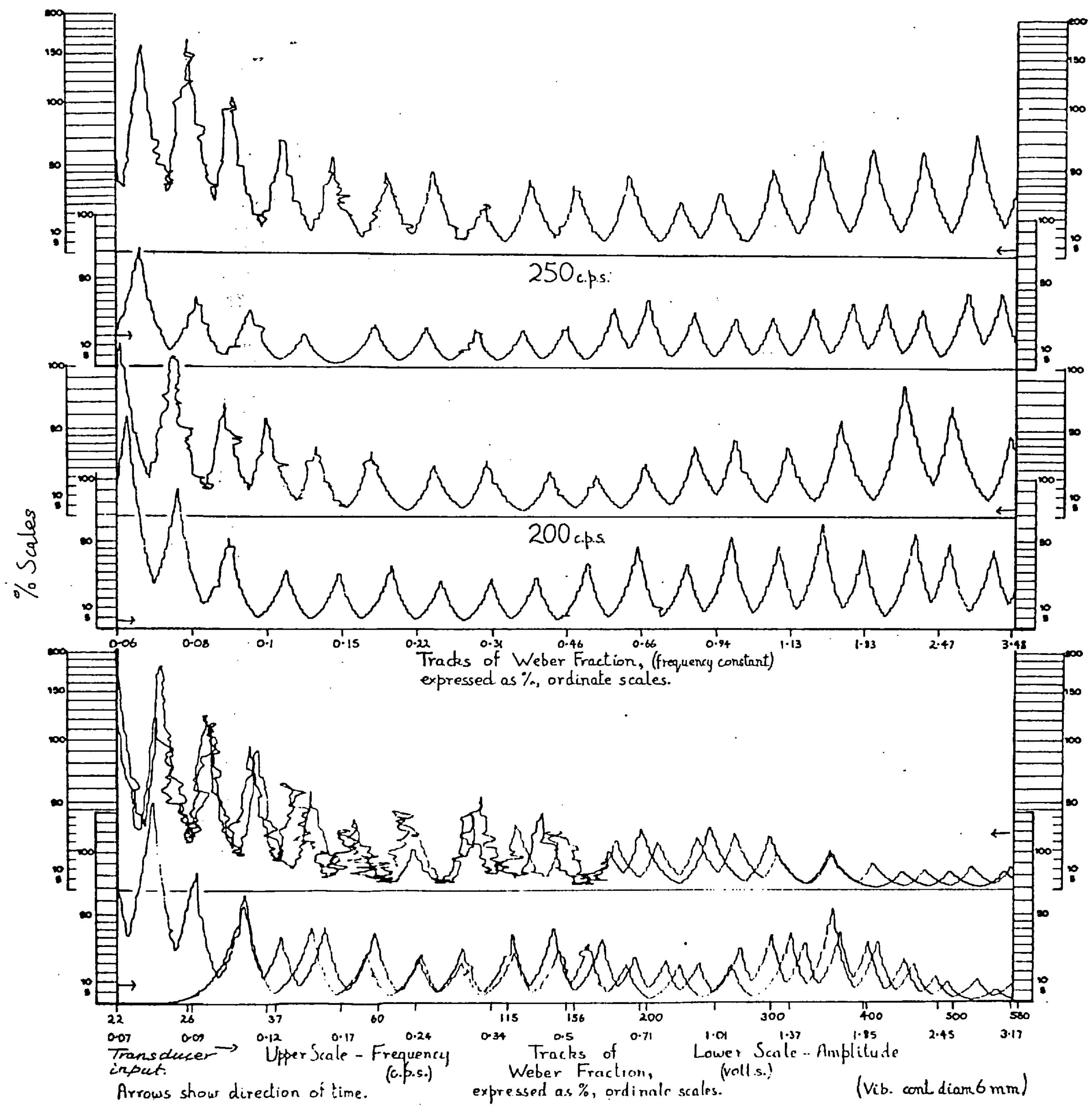


Fig. 3/23

Fig 3/23

Weber Fraction

Constant frequencies
200 cps. & 250 cps.

Photograph of Weber Fraction tracks, with stimulation of the index finger-tip. The ordinate scales are in percentage, and the tracks record, as a function of time, the amount by which the stimulus must increase for a change to be noticeable. (The standard and incremented stimuli alternate at 5 c.p.s.)

The bottom pair of tracks was recorded with the frequency varying, (the same pair of tracks is reproduced in all the Figs. 3/22-26). For the other tracks the frequency was maintained at the value shown for each pair.

Overlapping tracks, as in the bottom set, indicate that the recording was repeated, to give twice the data.

For details see text.

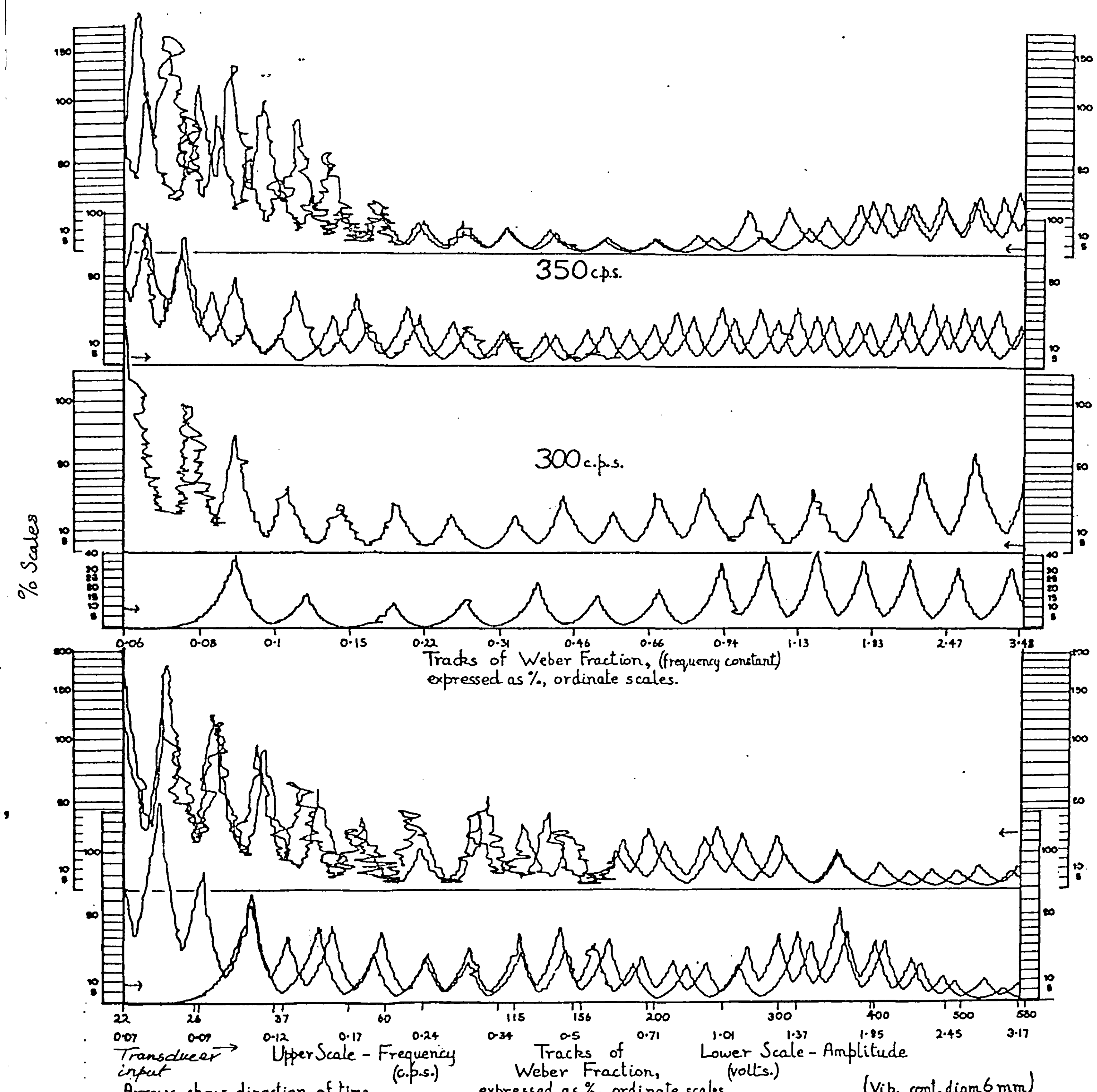


Fig. 3/24

Fig 3/24

Weber Fraction

Constant frequencies
300 cps. & 350 cps.

Photograph of Weber Fraction tracks, with stimulation of the index finger-tip. The ordinate scales are in percentage, and the tracks record, as a function of time, the amount by which the stimulus must increase for a change to be noticeable. (The standard and incremented stimuli alternate at 5 c.p.s.)

The bottom pair of tracks was recorded with the frequency varying, (the same pair of tracks is reproduced in all the Figs. 3/22-26). For the other tracks the frequency was maintained at the value shown for each pair.

Overlapping tracks, as in the bottom set, indicate that the recording was repeated, to give twice the data.

For details see text.

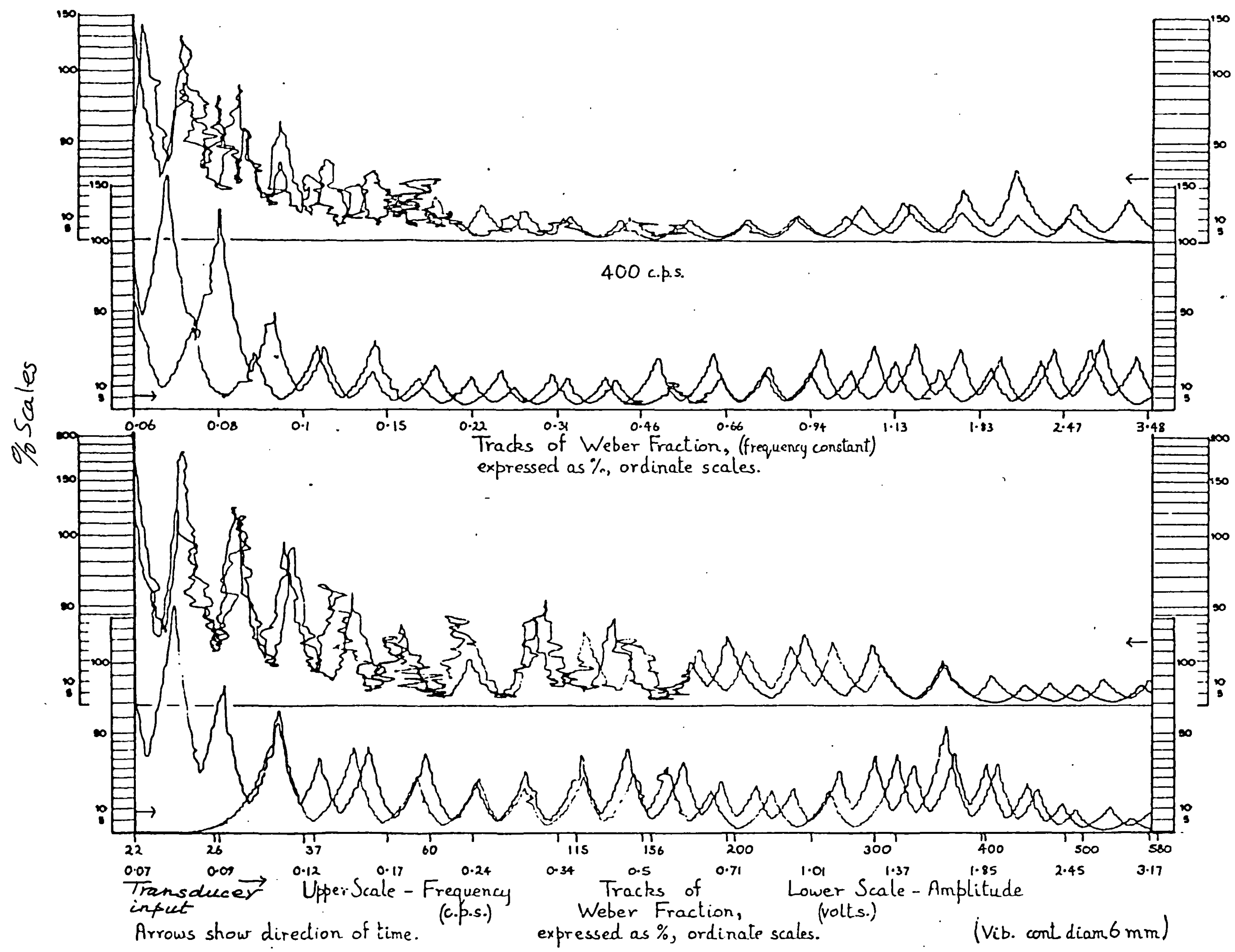


Fig. 3/25

Fig 3/25

Weber Fraction

Constant frequency

400 c.p.s.

Photograph of Weber Fraction tracks, with stimulation of the index finger-tip. The ordinate scales are in percentage, and the tracks record, as a function of time, the amount by which the stimulus must increase for a change to be noticeable. (The standard and incremented stimuli alternate at 5 c.p.s.)

The bottom pair of tracks was recorded with the frequency varying, (the same pair of tracks is reproduced in all the Figs. 3/22-26). For the other tracks the frequency was maintained at the value shown for each pair.

Overlapping tracks, as in the bottom set, indicate that the recording was repeated, to give twice the data.

For details see text.

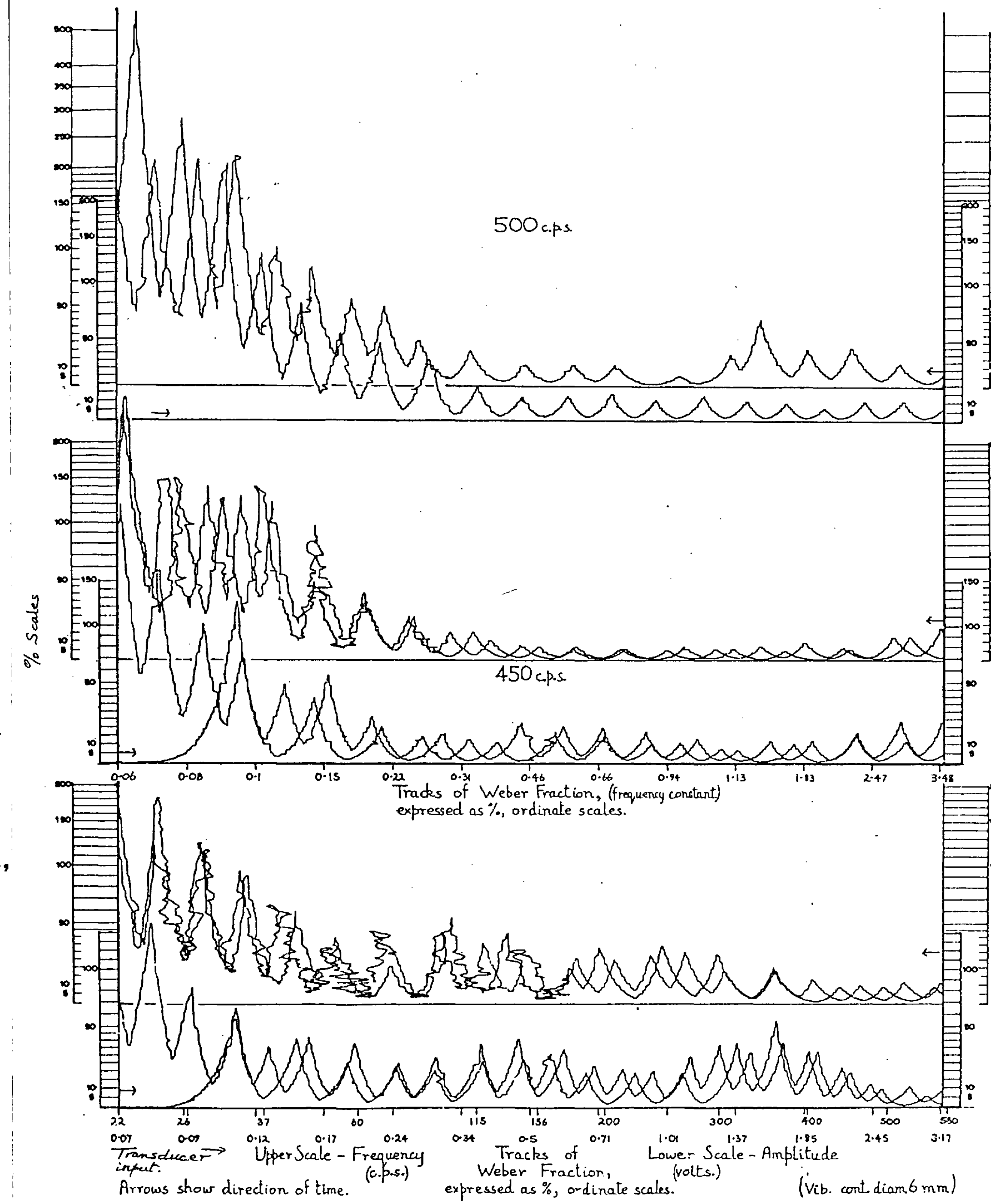


Fig. 3/26

Fig 3/26

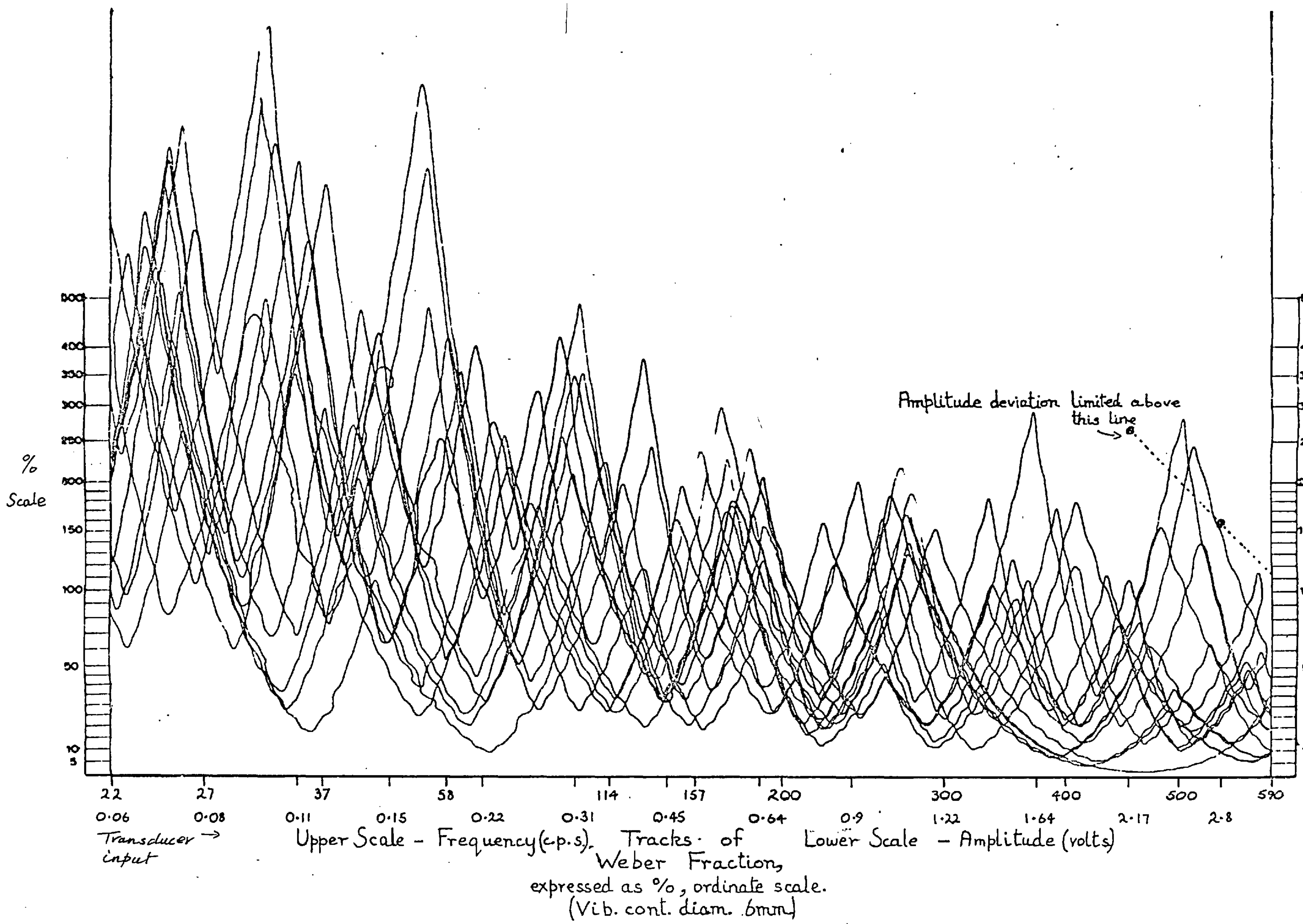
Weber Fraction

Constant frequencies

450 c.p.s. & 500 c.p.s.

Photograph of Weber Fraction track made with the rate of change of the standard four times the rate usually employed. The site of the stimulation was the finger-tip of the first finger. For details see text.

Fig. 3/27a



Weber Fraction

Photograph of Weber Fraction track made with the rate of change of the standard four times the rate usually employed. The site of the stimulation was the finger-tip of the first finger. For details see text.

Fig. 3/27b

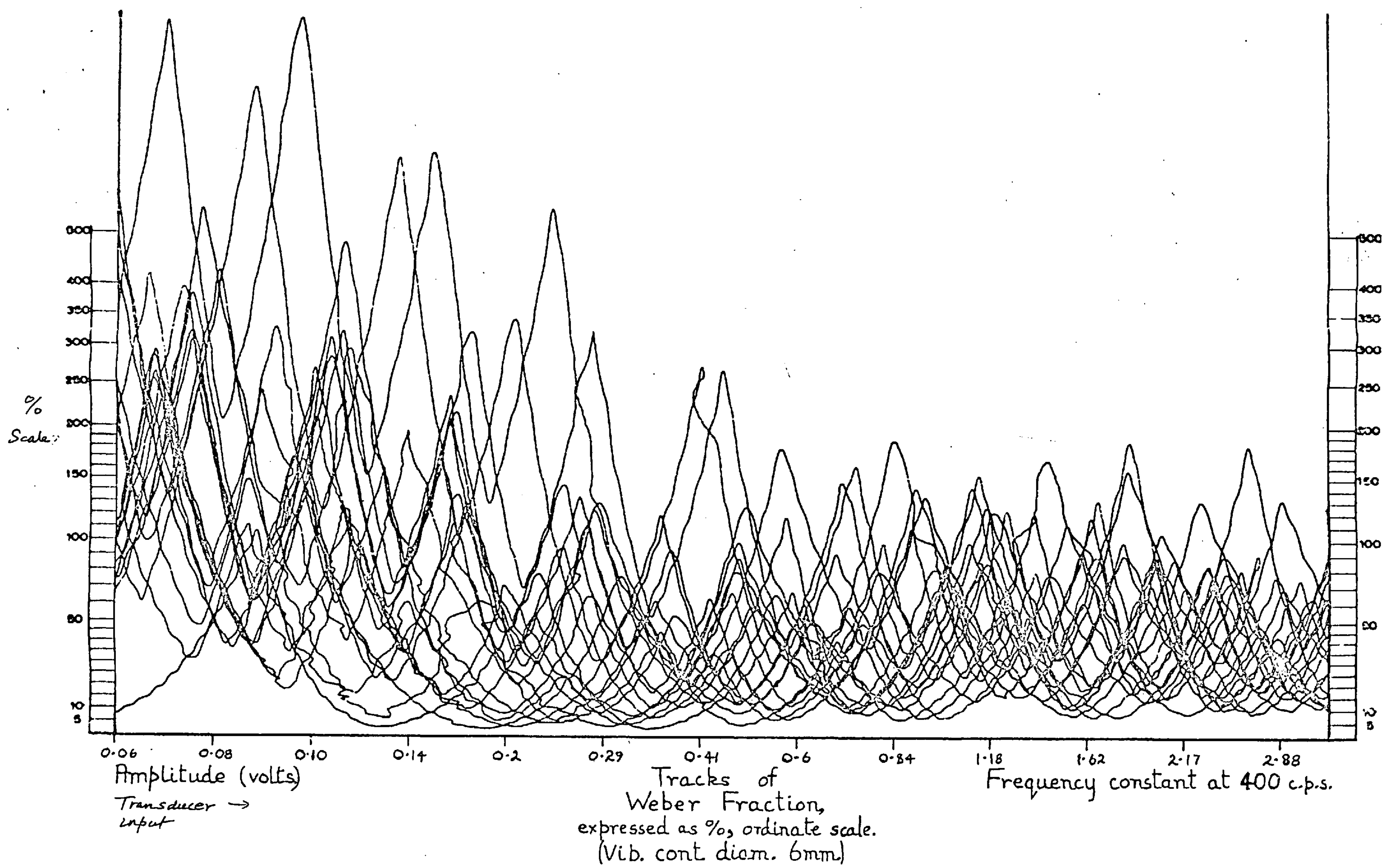
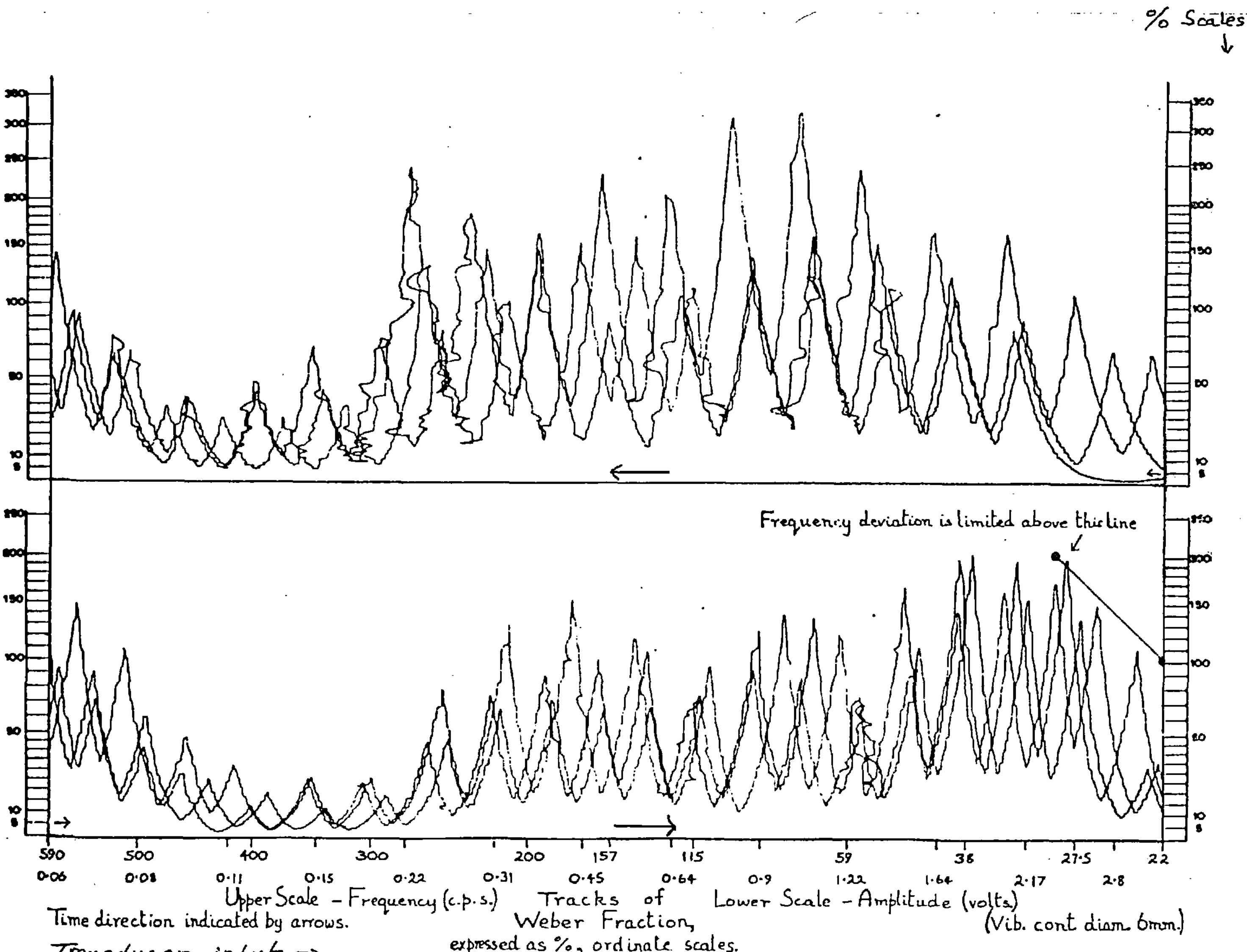


Fig 3/27b

Weber Fraction

Photograph of Weber Fraction tracks, with the stimulus being delivered to the tip of the index finger. The standard and incremented stimuli alternated at 5 c.p.s. The amplitude increments are positive and are accompanied by 'negative' increments in frequency. (Whilst tracking the amplitude slowly increased at the same time as the frequency slowly decreased.) The ordinate scale values in % should be divided by two to obtain the frequency decrement values; the amplitude increments are as indicated.

Fig. 3/28



frequency is highest at the lowest amplitude, (and vice versa), and the increment in the frequency is negative. (The ordinate scales do not apply to frequency changes, the percentage frequency changes being approximately half the scale values).

* * * *

The data suggest that there is no reason to suppose that the 'synthesised texture' stimulus should not work at least as well as the fixed frequency stimulus. There are clear, consistent, but small, differences between those tracks made with the frequency varying and those made with fixed frequency.

There are also small differences between the tracks recorded in one direction and those recorded in the other direction. These are possibly due to fatigue and/or adaptation. Without further experimentation it is not possible to be more certain of the cause.

The comments of the subject are worth noting. After one session in which she had tracked the Weber Fraction with the frequency constant at 300 c.p.s., for 12 minutes without break, she spontaneously offered her opinion of that particular type of stimulus. She said she found it boring, and that it was difficult to concentrate on the stimulus.

The Weber Fraction tracking experiments therefore provide no real reason for not using the fixed frequency stimulus. Despite the arguments concerning 'naturalness', the lack of success of other systems in which such a stimulus was employed, and the comments of the subject, it is necessary to try both stimuli in any communication system devised.

The question remains, 'Is the Weber Fraction suitable as a basis for comparison?' Without trials, using both types of stimulus in a communication system, controls, and all the statistical paraphernalia of experimental psychology, it would appear to be the only way of attempting to ascertain, (before actually trying it), the unsuitability of either stimulus, compared to the other. It was forseen that the results of such a comparison might be inconclusive.

The suitability of any stimulus could only be ascertained by trying it. The 'weaker' procedure of looking for reasons for classifying a stimulus as unsuitable is, I maintain, the most that can be done without actually using a stimulus in a complete system. There can be no alternative to actually trying something; predicting 'general' results from 'special' experiments is very unwise.

3 c).

The relevance of the findings, presented in subsections 3 b) 1), and 3 b) 2), to the design of any vibrotactile communication system are simply stated.

1. The experiments in which threshold was tracked show that the system for delivering the stimulus is acceptable. It does not appear to introduce any anomalies. The use of a square-wave driving signal is acceptable.

2. The performance of the transducer is such that a conjectured advantage of using a stimulus in which the frequency increased with the amplitude, (namely a reduced driving signal amplitude range), does not accrue.

3. The Weber Fraction tracking experiments provide no data which enable a choice between the stimuli to be made.

4. The consistency of the subject's performance over many months is good. The variations which do exist appear (apart from the effects ascribed to adaptation and fatigue), to be quantitative not qualitative.

The above four points enable a communication system to be devised, but there are aspects of the results which need further discussion. In particular the discussion of results of the experiments which show adaptation and fatigue needs some elaboration.

The selection of the hand, as the site for the vibrators, was made for reasons of convenience and sensitivity. Widely spaced sites are suggested by Geldard & Sherrick,(1965), as being necessary to reduce errors. It is therefore to be expected that using the hand as the site for 5 or 6 vibrators will introduce some errors. These will arise, perhaps from lack of variation of the overall sensation, (this is linked to 'pattern communality' and is discussed in Chapter 4), but probably from interference between the sensations aroused at the different sites, (see also Chapter 4).

The results of the threshold experiments shed some light on the likely source of errors. As previously mentioned, there has been much confusion as to what to call the elevation of threshold resulting from prolonged stimulation at a fixed frequency, my opinion being that, until evidence is forthcoming, it should be called fatigue and not adaptation.

The unequivocal demonstration of the existence of adaptation, provided by my experiments, does not solve the problem of what to call the <u>elevation</u> of threshold. However, a way out of this dilemma exists. The elevation of threshold is perhaps a mixture of adaptation and fatigue.

(Fatigue is a receptor phenomenon, but in response to vibration any adaptation which takes place must be a neural phenomenon, i.e. it must be occurring somewhere in the nervous system).

Gescheider & Wright,(1969), have conjectured that 'adaptation', (the observed elevation of threshold), is the result of such a mixture, but there were too many assumptions involved for the conjecture to be anything more than just that. The experiments, reported in subsection 3 b) 2), which show that 'fatiguing' one finger affects the threshold levels of another finger of the same hand, provide a means for sorting out the contributions of the two effects.

The 'fatigue' exhibited by the unfatigued finger can only result from neural adaptation somewhere in the system. A measure of the fatigue of the fatigued site, simultaneously recorded with a measure of the 'fatigue' of the site of the adjacent finger, would provide, I suggest, the proportions of the two effects constituting the fatigue.

It is possible to say a little about the outcome of such an experiment before it is performed. The experiments of Hahn, (using fatigue to show the existence of two receptor populations), and Wedell & Cummings, cited earlier, indicate that fatigue will not be independent of frequency, (both the fatiguing frequency and the frequency at which measurements are subsequently made). The results of such an experiment will probably show that the higher the frequency the greater the effect. Such a conjecture is supported by general physiological considerations, (these concern the effect of fatigue on the refractory period of a receptor, see Catton,1970).

Adaptation, being partly a neural effect, may possibly turn out to be less dependent on frequency, (assuming that the nerve bundles have a better 'frequency characteristic' than the receptors, see e.g. Figs. 3/8 & 3/11).

These conjectures go some way towards explaining the results obtained in the fatigue experiments. The interdigital 'fatiguing' experiments support the suggestion that adaptation is independent of frequency. Consideration of the results of the straightforward fatigue experiments, with the above point in mind, reveals the possibility that if the threshold is low a fatiguing stimulus will cause adaptation as well as fatigue, whereas if the threshold is high, before the delivery of the fatiguing stimulus, the effects observed will be largely due to fatigue of the receptors. This is, admittedly, conjecture on a grand scale, the experiments upon which it is based being sparse. It is nevertheless quite plausible.

It might be expected, on the strength of the above conjecture, that interdigital adaptation may well affect the usefulness of the hand as the site for stimulation in a communication system. However the conditions which cause the effect, (a prolonged, constant frequency, fatiguing stimulus of considerable intensity) would not obtain in a communication system using varying frequencies of vibrotactile stimulation. The data I have obtained support the suggestion that, with a frequency varying system, adaptation would not take place. Figs. 3/15 - 3/19 are all from one day's work.

CHAPTER 4

In this chapter I present and discuss the prototype speech training aid. In Part One some general problems of speech training were mentioned and it was pointed out that any aid for this should enable speech training to be carried out in a shorter period of time than by present methods. It was also pointed out in the introduction to Part Two that if a vibrotactile stimulus could be devised which evoked useable sensations then this would be preferable to, and more appropriate than, any visual system of information presentation.

The simple psychophysical experiments that I have performed show, (see Chapter 3), that the stimulus system I have devised, ("synthetic texture"), has no apparent drawbacks and can therefore be considered for use in an aid. However, as no justification was found for not considering the simpler stimulus system, it is necessary to try such a system as well.

In section 4 a) I describe the concepts employed in the prototype aid.

In section 4 b) I explain how the aid works and how it is used.

The aid discussed, although it contains novel processing ideas, was built primarily for the purpose of testing the feasibility of using any vibrotactile articulation training aid which incorporated the "synthetic texture" stimuli. The realisation of the novel concepts, (thought to be necessary for any vibrotactile aid), is far from perfect. I considered it important to get something useable working reliably enough to be tried for a long period of time. Although 'useable' begs the question to some extent I felt, nevertheless, that technical improvements to a 'working' realisation, (i.e. the first attempt at realisation), would have to wait. Expertise and time were adverse pressures, (an insufficiency of both), and some time was wasted on trying to build a working automatic gain control system before the solution was conceived, (i.e. to do something altogether different).

4 a).

In this section I set forth the basic concepts employed in the aid and give some idea of how they are realised in the apparatus. I do not elaborate circuit details as they are irrelevant to the exposition. Any number of ways could be found for realising the concepts and those actually employed are by no means optimum. (Designing a piece of equipment is almost a continuous process of making compromise decisions). In the next chapter, where I evaluate the aid, I also present criticisms of the realisation, but they are general ones and in effect constitute quidelines for future realisations rather than detailed instructions for alteration to the present apparatus.

The apparatus is divisible into different sections and these are listed below.

4 a) 1). This subsection covers the general design philosophy. The three sections of the equipment are outlined and labelled as follows:-

i) Microphone-processor interface. (Subsection 4 a) 2).).

ii) Processor. (Subsection 4 a) 3).).

iii) Processor-subject interface. (Subsection 4 a) 4).).

4 a) 1).

The problem to be tackled by the designer of any speech-training aid is simply stated:- 'How can a profoundly deaf person be presented with as much information as possible about the movements of the mouth and vocal organs, as they take place in speech?'

This formulation of the problem is significantly different from that implicitly or explicitly formulated by the devisors of other equipment. For example, in the design I propose there is no suggestion of feature recognition. A machine which indicated nothing more than the presence or absence of '<u>s</u>' or '<u>sh</u>' in a word or phrase is explicitly excluded from consideration by the formulation above, and I make the exclusion because the consideration of such a machine implies a judgement as to which mouth movements are more important for a deaf person. The designer cannot know what will be useful to any given deaf person, and besides this there is the fact that deaf people will vary as to their needs. I consider that any device for aiding the deaf to acquire good articulation must be as general as possible and involve as few preconceptions as practicable of what a deaf person needs.

What other guideline can be given to the potential inventor of a device? The aid should, from the user's point of view as well as the teacher's, be simple to use. It should not involve any inconvenience on the part of either or there will be a disincentive to use it.

An aid should preferably not involve the use of special locations in a building, nor should it place more than a minimum of constraints upon the teacher or pupil, whether these concern behaviour or teaching style.

However, for a prototype aid certain of these conditions can be waived, if necessary, to enable a machine to be constructed and used for a while. Recognition of a great many, (if not all), of the requirements to be met by the finished article does not imply that an 'interim' realisation is not allowed if it does not meet some of these requirements. In addition there is the likelihood that the specification of the article will change as the result of the use of the 'interim' version.

For convenience I elected to design and construct a prototype which had only a limited dynamic range. This decision eased the problems, (e.g. automatic gain control, noise, d.c. offset voltages, etc.), posed by other parts of the equipment. Also, the experimental work with the machine would be conducted in an acoustically quiet, (not anechoic), room. This meant that it was not necessary to give great attention to the requirement that the machine be able to cope with a normal acoustic environment. These two limitations are further discussed in Chapter 5.

The design approach, (having a clarity in retrospect which was not obvious at the time), was:- 'How can the poor dynamic sensory range available at the fingertips be used for information which, at the microphone, has a large dynamic range?'

The solution adopted was to separate the frequency/amplitude variations present in the initial signal, (considered important), from the general amplitude level of the original signal, (not the result of vocal organ configuration and therefore much less important). The important variations will be the complex ones arising from formant movements, presence or absence of frication and so on. This was as much prejudgement of what would be useful to a deaf person as I would allow myself.

Throughout the development of the aid, (much of which took place concurrently with the psychophysical experiments reported in Chapter 3), compromise decisions had to be made to avoid delaying completion of the aid. The overall compromise criterion involved estimating the degree to which the aid would be rendered ineffective by the adoption of a compromise technical solution, (bearing in mind previous departures from the ideal). This, as I have mentioned, is a normal part of engineering design. The amount of compromise involved varies according to timetable pressures and expertise, to mention just two very important factors.

As such a design procedure is applied the designer learns more about what he is doing and consequently comes to view earlier decisions with a mixture of pleasure in their originality and shame at their inadequacy. Setting down, explaining and justifying engineering with a heavy proportion of compromise and intuition is difficult therefore, and the policy I have adopted is to firstly state as simply as possible the conceptual structure of the aid, and its realisation where this is important.

This is done in the three following subsections where, in:-

4 a) 2), the microphone-processor interface is described,

4 a) 3), the processor is explained,

4 a) 4), the processor-vibrator interface is described.

Following this, (in subsection 4b)), I present the reasons for many of the apparently arbitrary choices made and presented as 'fait accompli' in the three subsections listed above.

4 a) 2).

The section of the apparatus which takes the microphone signal and amplifies it, (with optional filtering to mimic the ear's frequency response), and also 'measures' it, (i.e. subjects it to rectification and smoothing, providing from this latter signal a 'voice switching' signal as well as a 'voice intensity', or 'amplitude' signal), I have called the microphone-processor interface. This part of the equipment is shown schematically below, (as are its relations to the other two sections of the machine).

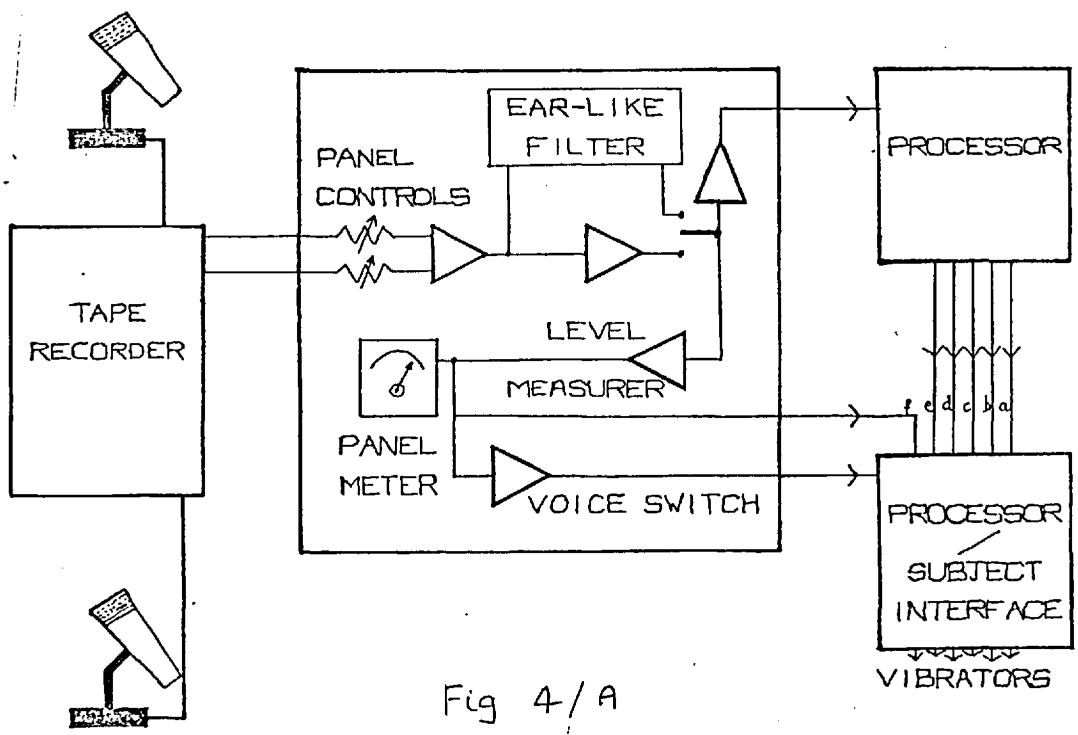


Fig 4/A

The microphone signals are passed via the first amplifier, (a stereo tape-recorder), to the prototype aid where they are mixed. The 'teacher', (i.e. myself), sets the controls so that the amplitudes fall well within the range of the instrument. The mixed microphone signal is supplied to a filter (the characteristics of which are shown in Fig. 4/1), which, given the limited dynamic range of the apparatus, (not more than approximately 30dB.), is intended to 'mimic' the characteristics of the human ear. As there is some disagreement over the precise shape of this response two versons are used. Either of these versions can be selected, or neither, and in the latter case an amplifier with a flat response, (also shown in Fig. 4/1), passes on the signal. The ear's 'response' is shown in Fig. 4/2, (also shown is a modern authority's favoured response).

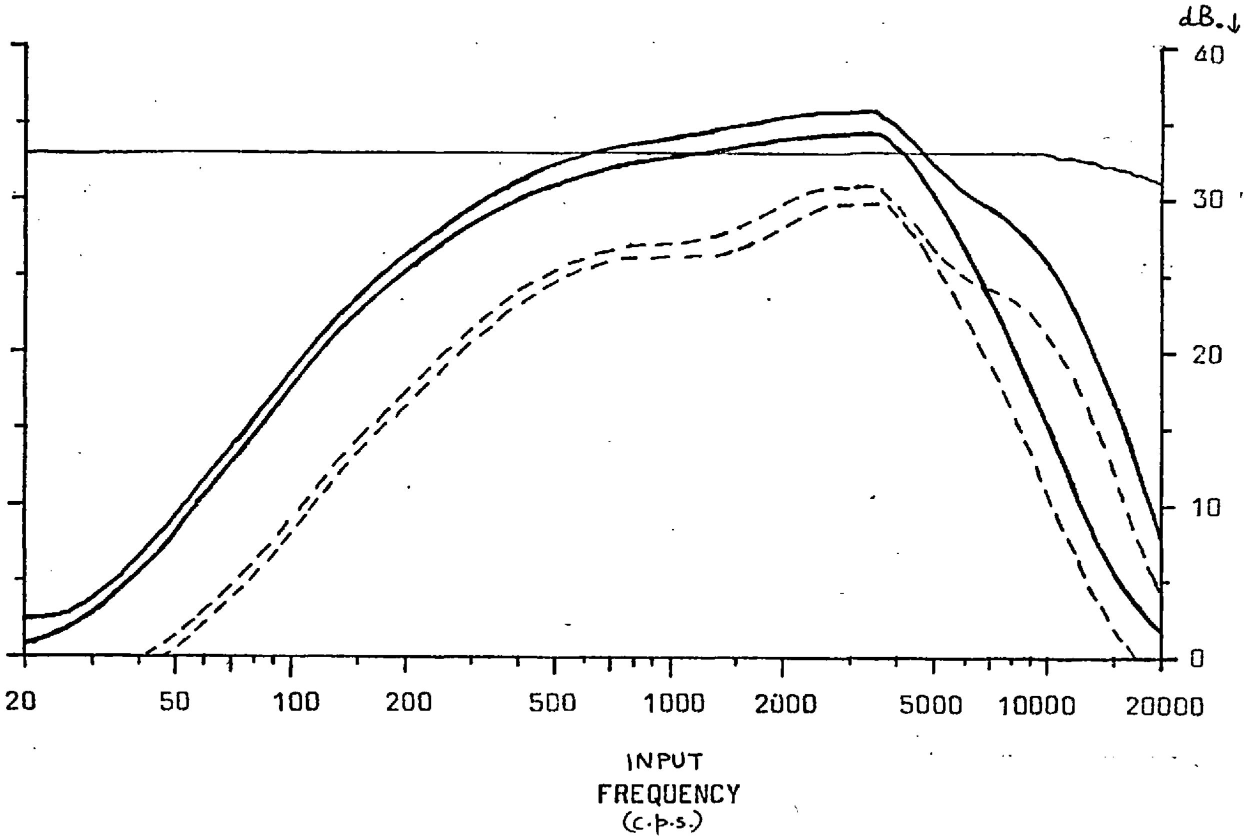
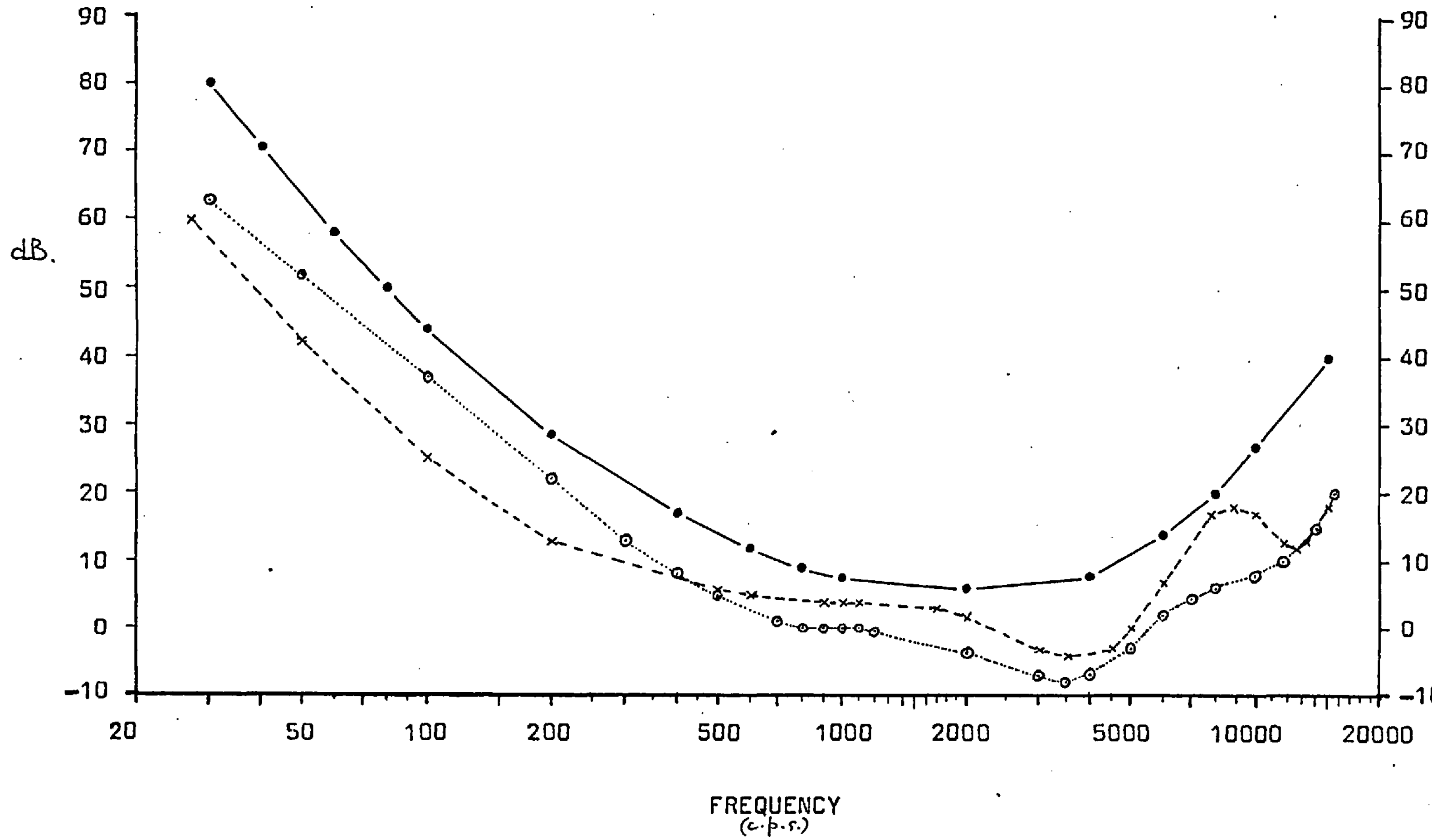


Figure 4/1

The figure shows the response of the filter designed to mimic the ear's response. The ordinate scale is in dB., (the zero level is 10 millivolts).

The 'flat' response shows the response of the amplifier in the first stage of the equipment, without the 'ear-like' filter switched in and with the input intensity 80 on the panel meter. The solid curves show the 'ear-like' responses, (there are two available), with the input intensity 100 on the panel meter. The dashed curves show the same responses for an input level of 40 on the panel meter.



The ear's response.

The ordinate scale is intensity in dB., (the zero level is 0.0002 dynes/sq.cm.)
•—• The points describe the curve for minimum audible pressure from Davis,(1936), as published by I.T.T.,(1968). ⊙⋯⋯⊙ The points describe the curve published by I.T.T.,(1956). ×-----× The points describe the curve published by Richards,(1973).

Figure 4/2

Following this initial amplification with optional filtering the signal passes to three separate amplifiers. The first is simply a buffer to deliver the signal at a suitable level, (d.c. as well as a.c.), to the next section of the apparatus, (the processor). The second is a full-wave rectifier feeding into a storage 'capacitor-resistor' smoothing network with a time constant of 5 milliseconds. This signal, (a measure of the overall intensity of the speech), also controls one of the stimulus generators, (see subsection 4 a) 4).). It is largely the full-wave rectifier circuit employed which limits the dynamic range of the apparatus, (although see the discussion of Fig. 4/5 in 4 a) 4), and Chapter 5).

The third amplifier, fed from the second, is a high gain amplifier the output of which is either a large negative voltage or a large positive voltage, according to the presence or absence of microphone signal. This 'voice switching' amplifier is used to control the stimulation generators.

4 a) 3).

The processor is original and very neatly circumvents some of the problems, (e.g. distortion, overloading on transients), which inevitably arise when automatic control of the gain of an audio amplifier is attempted, (when the amplifier is being used for speech signals). Schematically the processor is shown below.

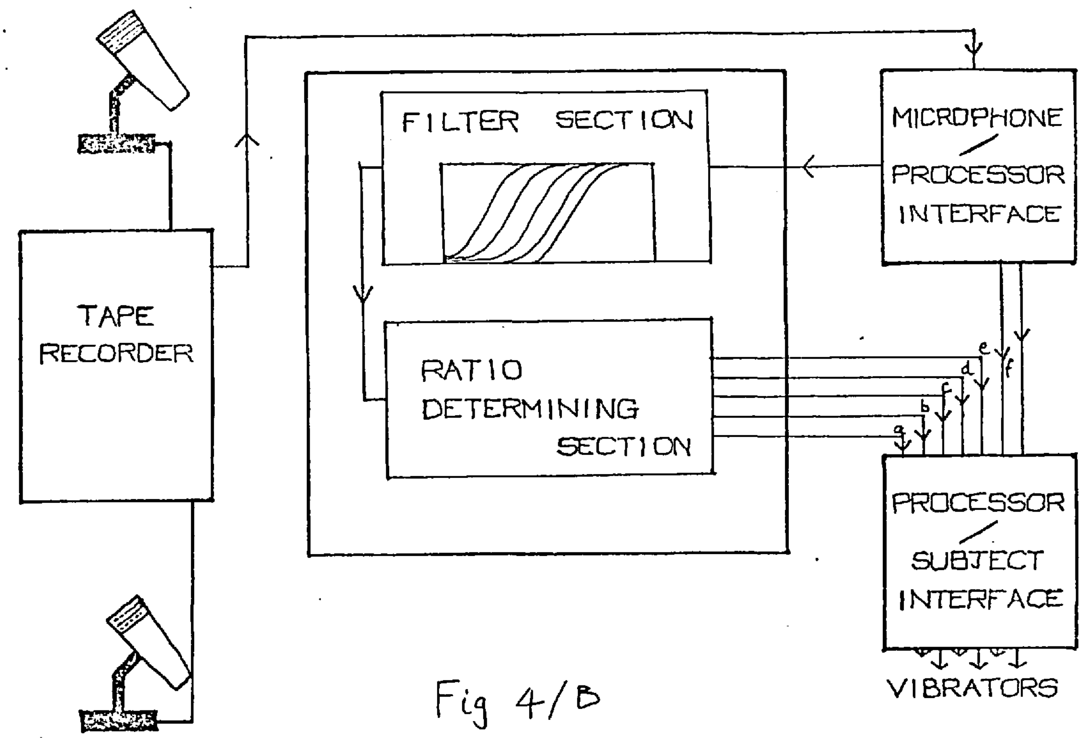


Fig 4/B

The signal from the first stage of the equipment, the 'interface', is fed to five high-pass filters, (the responses are shown in Fig. 4/3). The cut-off frequencies are, with the exception mentioned later, arbitrarily chosen, and it was intended that the curves should not become parallel, (see Chapter 5).

The outputs of the filters are rectified and smoothed in a manner identical to that used in the intensity measuring circuitry. The six 'd.c.' voltages derived from the speech, (intensity level and five filter outputs), are fed to five ratio measuring circuits, the outputs from adjacent filters being compared. Thus the ratio of the overall energy in the speech to the energy in the first filter is obtained; the ratio of the energy in the first filter to the energy in the second; and so on, giving five ratios.

The use of these ratios enables the changes in the overall intensity to be separated from changes in the frequency distribution of the energy. Only the latter will appear as changes in one or more of the ratios whereas an increase or decrease in overall intensity, (with limits set by the elec-

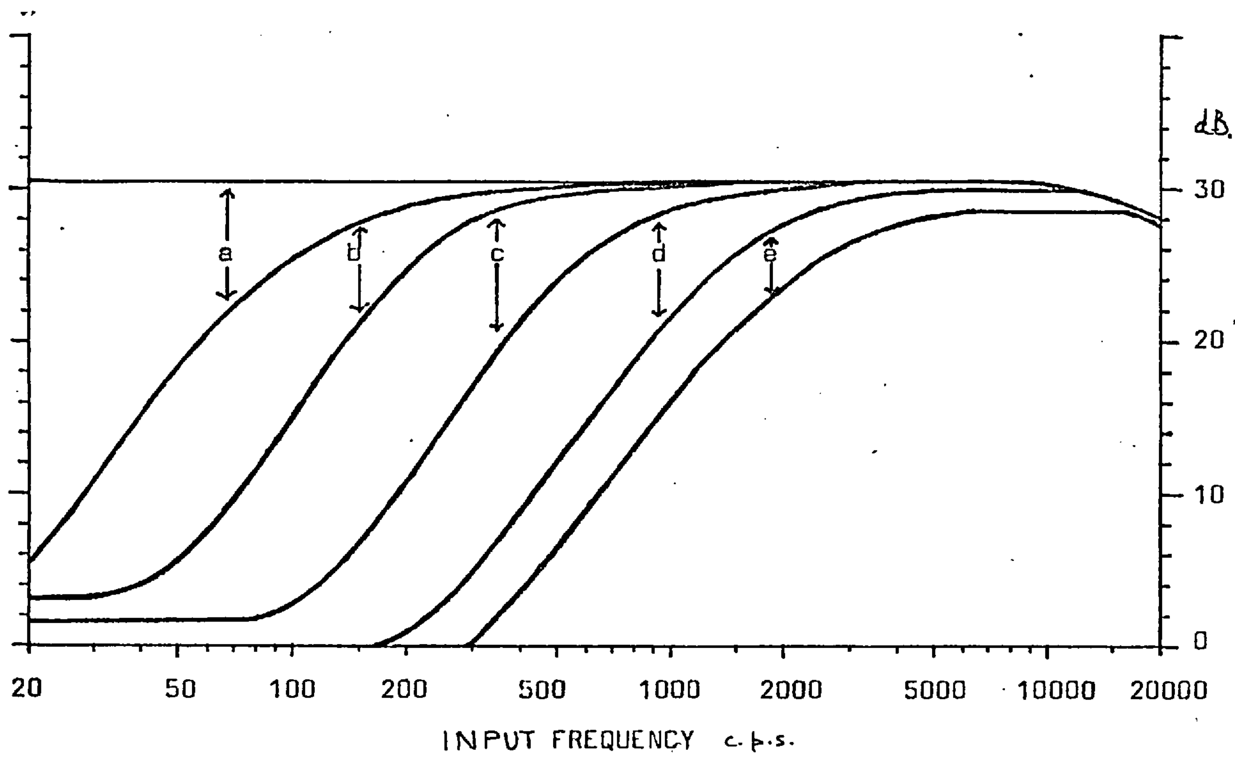
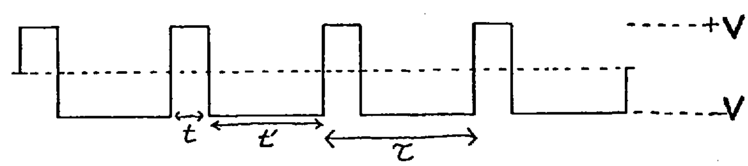


Figure 4/3

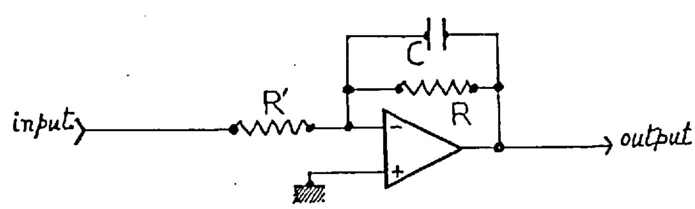
The responses of the five filters used in the processor. Also shown are the filter sources from which the ratios are derived for controlling the stimulus oscillators 'a' -- 'e'. (See also Figure 4/4a -- 4e.) The ordinate scale is in dB., (the zero level is 10 millivolts). The input signal had an intensity of 80 on the panel meter.

tronics), will not affect the ratios and therefore pass unnoticed. The information thus eliminated is provided by the speech intensity measure. Because the voltages output from the rectifier-smoothing circuits all have the same rise times (and fall times) the ratios can be established very rapidly.

The circuit used to measure a ratio, (there are five separate ratio measuring circuits), is based on an oscillator built around two operational amplifiers, (see Appendix 2, Fig. A 2/3). It is possible to make such an oscillator in which the period is controlled by two voltages, (one affecting the period of time for which the output is positive and the other affecting the period of time for which the output is negative). The ratio between the two control voltages is given by the mean d.c. level of the output of the oscillator. This can simply be measured with a leaky integrator circuit using an operational amplifier. With the oscillator output as follows:-

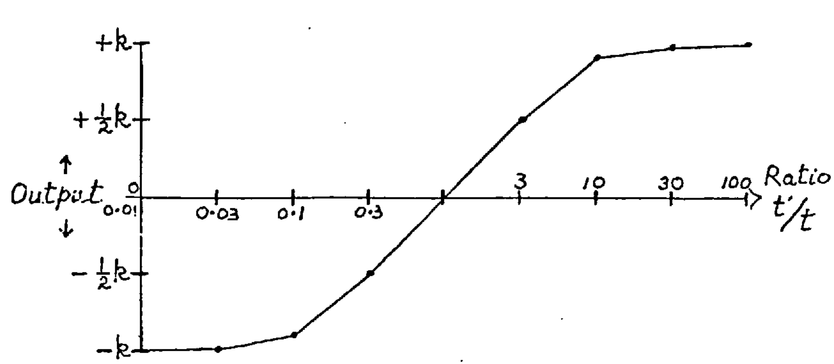


the formula for the output of the leaky integrator,



is $k(2t'/\tau - 1)$ where $k = (R/R')V$ and assuming that the time constant (RC) is long enough to effect smoothing of the signal with frequency $1/\tau$.

This gives a 'logarithmic' measure of the ratio, as illustrated in the sketch below:-



The useful range over which the logarithmic relation holds is approximately 100:1. The variation in the ratio must not occur at anything approaching the frequency $1/\tau$. Engineering compromise dictates that the frequency $1/\tau$ should be about 10 times the frequency of variation which it is desired to 'measure'.

The five voltages, representing the 'logarithms' of the ratios, are supplied to the third section of the apparatus, the processor-subject interface, where they are used to control the oscillators which drive the vibrators.

4 a) 4).

The third section of the apparatus, the processor-subject interface, uses the ratio signals from the processor, (these provide information about the frequency distribution of energy in speech), to control the voltage-controlled oscillators, (similar to that used in the psychophysics experiments), which generate the signals for driving the transducers. This section of the apparatus effects a 'matching' of the signals derived from speech to the stimulus conditions likely to yield useful sensations. The hand is the site used for stimulation.

The relations between this section of the equipment and the other two sections are shown below:-

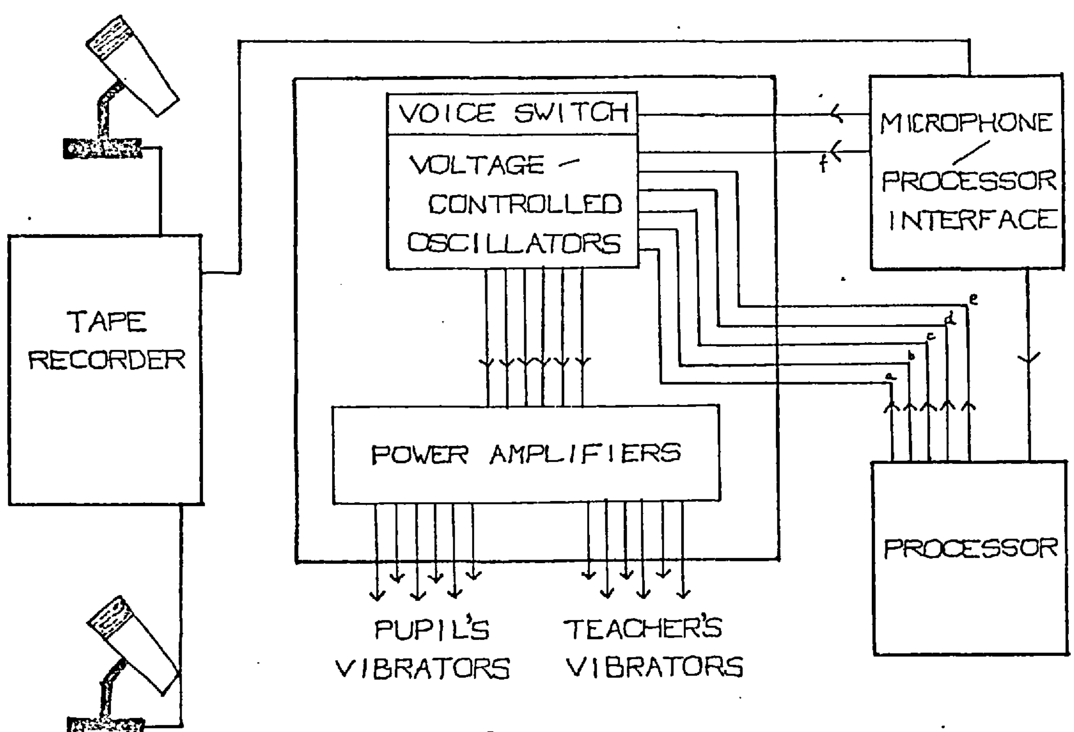


Fig 4/C

The six control voltages, (one of these is the intensity measure), can control the oscillators in two ways. The frequencies can either be kept constant, (the six arbitrarily chosen* frequencies being 315, 350, 400, 360, 320 & 250 c.p.s.), or they can be varied simultaneously with the amplitudes, (as advocated in Chapter 3). The response of the oscillators is such that the logarithmic representation of the ratio of the outputs of two adjacent filters is converted to a scale of amplitude, (and, if chosen, frequency), of vibration well suited to the hand's sensitivity. (The 'logarithmic' response of the oscillators is actually antilogarithmic, see Appendix 2). The oscillator which is controlled by the overall intensity of the speech is not different from the others but it is not controlled by voltage which is the logarithm of the amplitude, (see Chapter 5).

* influenced by performances shown in Figs 3/22-26

The outputs of the five oscillators which are used to convey information about the frequency distribution of the energy in speech are shown in Figs. 4/4a - 4e. The input signal used to obtain data for these graphs was a simple sine-wave. At each frequency the intensity of the input was adjusted to different values, as registered on the panel meter of the apparatus. The abscissae are labelled with the numerals given on the panel meter; these are arbitrary and are not calibrated in terms of sound level, (the meter has a scale of 0 - 100).

The vertical scale for each graph shows the output, (the signal fed to the transducer), and the scale indicates the frequency of the output when this frequency is not kept constant. The scale also serves, with a suitable multiplication factor, (indicated on the graphs), as a scale of amplitude, in volts. The values obtained for the amplitudes in the frequency varying case have to be halved to get the amplitudes employed in the frequency constant mode. The graphs show the effectiveness of the overall amplitude 'suppression'.

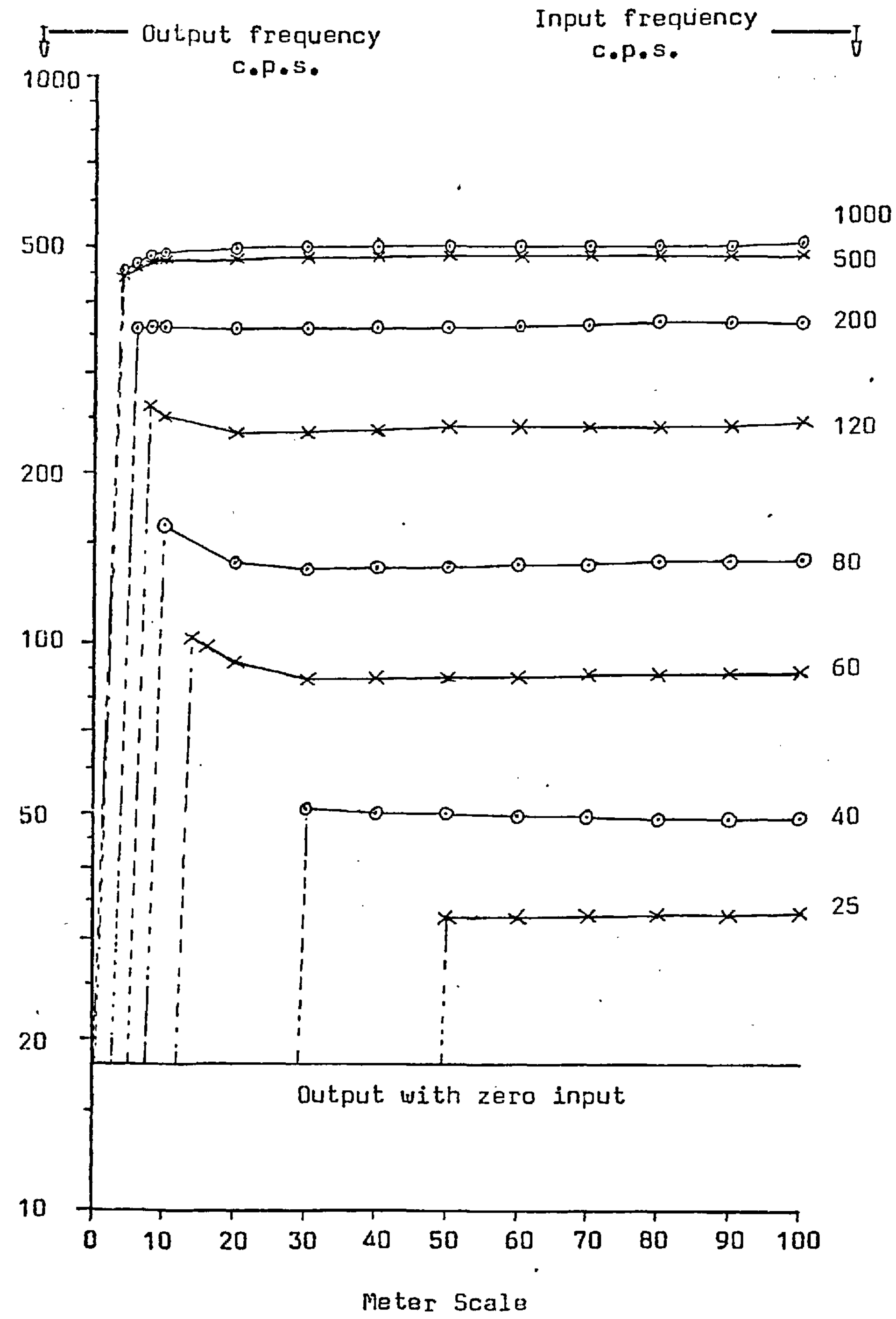
Figures 4/5 and 4/6 show the outputs of the six vibrators, as the input signal frequency is varied, (the signal driving any vibrator is square-wave in waveform). The intensity vibrator output is shown for a constant frequency, and the other outputs are for an input signal of amplitude 60 on the meter. The data for Fig. 4/5 are taken from Figs. 4/4a - 4e, and, as there, the amplitudes can be calculated from the frequencies.

Figure 4/5 shows (in the different shapes of the curves) that there is considerable tolerance available when adjusting the ratio measuring circuits. Comparison with Fig. 4/3 shows that the dynamic range over which the processor operates is not fully utilised; the ratio control signals are governed by the filter responses near their 'cut-offs'. This must be emphasised. The ratio signals employed to control the transducers are only used in those instances where the filter outputs being compared are high enough to ensure that the curved upper sections of the responses (in Fig. 4/3) are operating on the signal. The lower portions of the response curves, where they become parallel, are not involved in controlling the transducers. This is shown in Fig. 4/5, where the frequencies effective in controlling the outputs of the transducers can be seen to be those frequencies around the 'knees' of the curves shown in Fig. 4/3. This point, and the shapes of the curves in Figs. 4/3, 4/4a - 4e, & 4/5, are discussed in Chapter 5, (the high frequency high intensity effects seen in Figs. 4/4d & 4e are explained in 4 b) 1).).

Figures 4/4a - 4e

The graphs show the outputs of the oscillators used to drive the vibrators. The ordinate scale is frequency in cycles per second. The input used was a sinusoidal signal at the frequencies indicated.

The frequencies of the outputs are shown for the frequency varying system. The amplitudes of the signals driving the vibrators can be calculated as follows:- Output frequency/250 = volts pk-to-pk. For the frequency constant system the output level is calculated by means of the expression:- 'Output frequency'/500 = volts pk-to-pk., where the 'output frequency' is read off the ordinate scales.



(Constant frequency, if used, 315 c.p.s.)

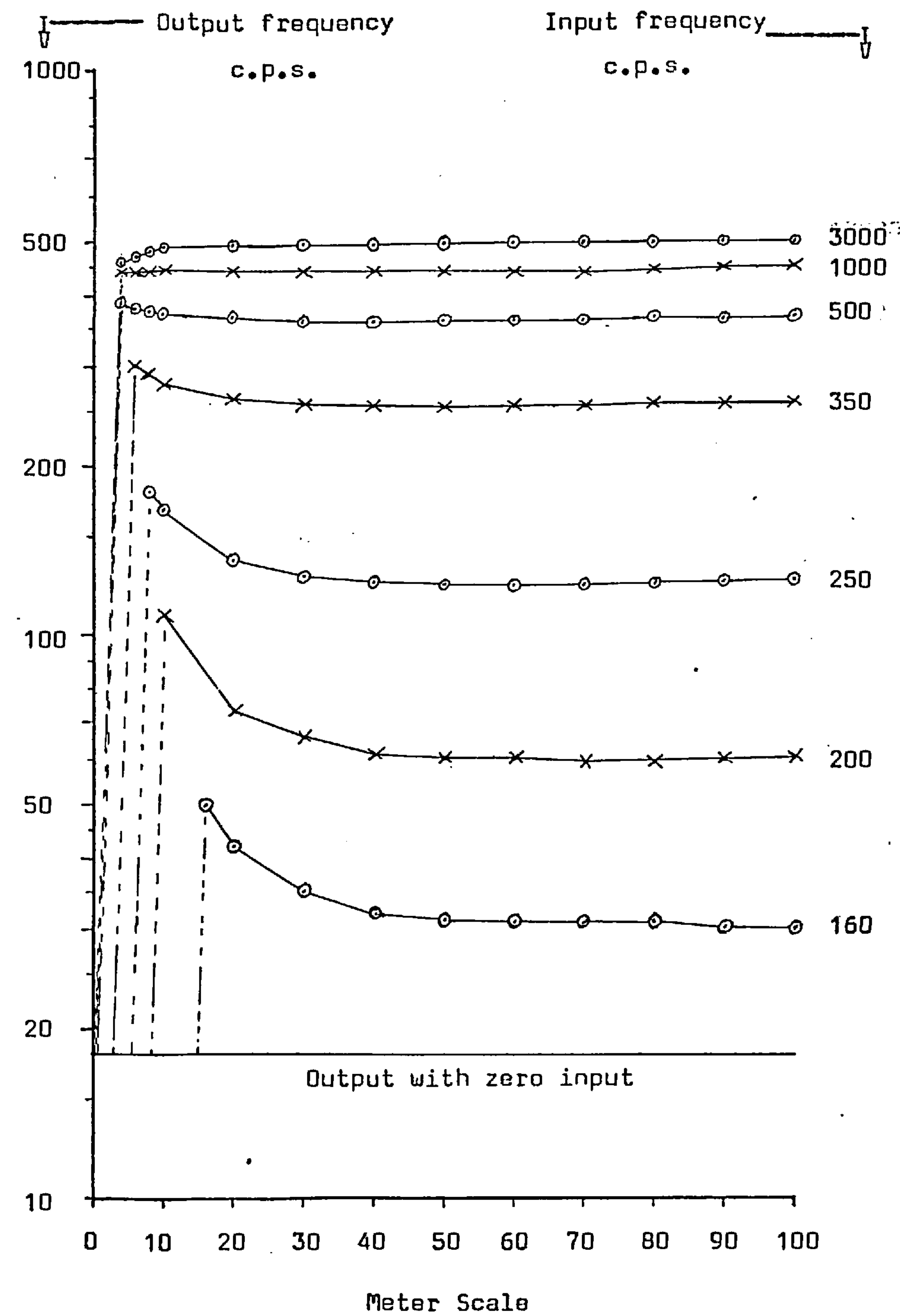
Figure 4/4a

Fig 4/4a

Figures 4/4a - 4e

The graphs show the outputs of the oscillators used to drive the vibrators. The ordinate scale is frequency in cycles per second. The input used was a sinusoidal signal at the frequencies indicated.

The frequencies of the outputs are shown for the frequency varying system. The amplitudes of the signals driving the vibrators can be calculated as follows:- Output frequency/250 = volts pk-to-pk. For the frequency constant system the output level is calculated by means of the expression:- 'Output frequency'/500 = volts pk-to-pk., where the 'output frequency' is read off the ordinate scales.



(Constant frequency, if used, 350 c.p.s.)

Figure 4/4b

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Fig 4/4b

Figures 4/4a – 4e

The graphs show the outputs of the oscillators used to drive the vibrators. The ordinate scale is frequency in cycles per second. The input used was a sinusoidal signal at the frequencies indicated.

The frequencies of the outputs are shown for the frequency varying system. The amplitudes of the signals driving the vibrators can be calculated as follows:- Output frequency/250 = volts pk-to-pk. For the frequency constant system the output level is calculated by means of the expression:- 'Output frequency'/500 = volts pk-to-pk., where the 'output frequency' is read off the ordinate scales.

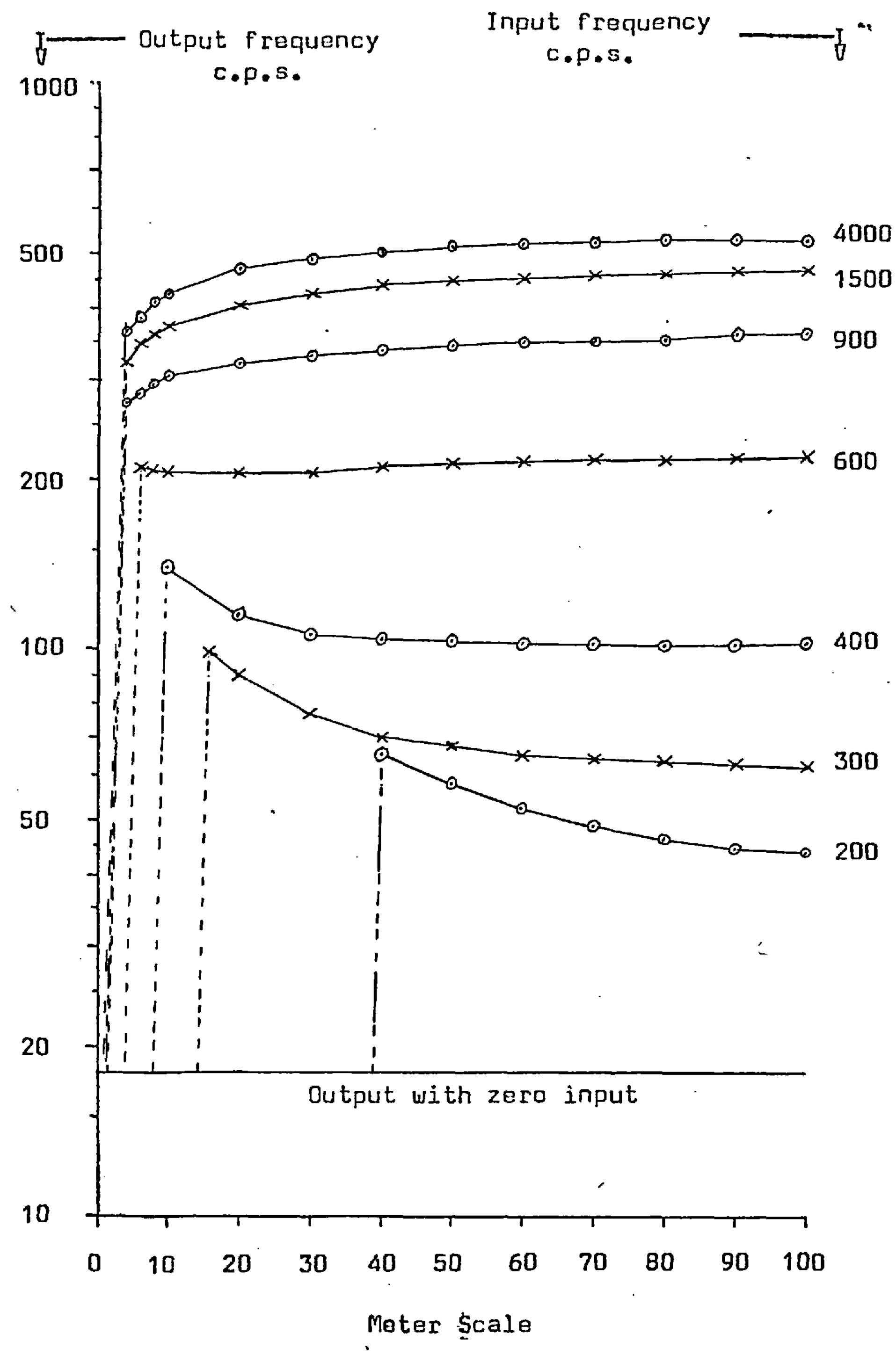

Figure 4/4c

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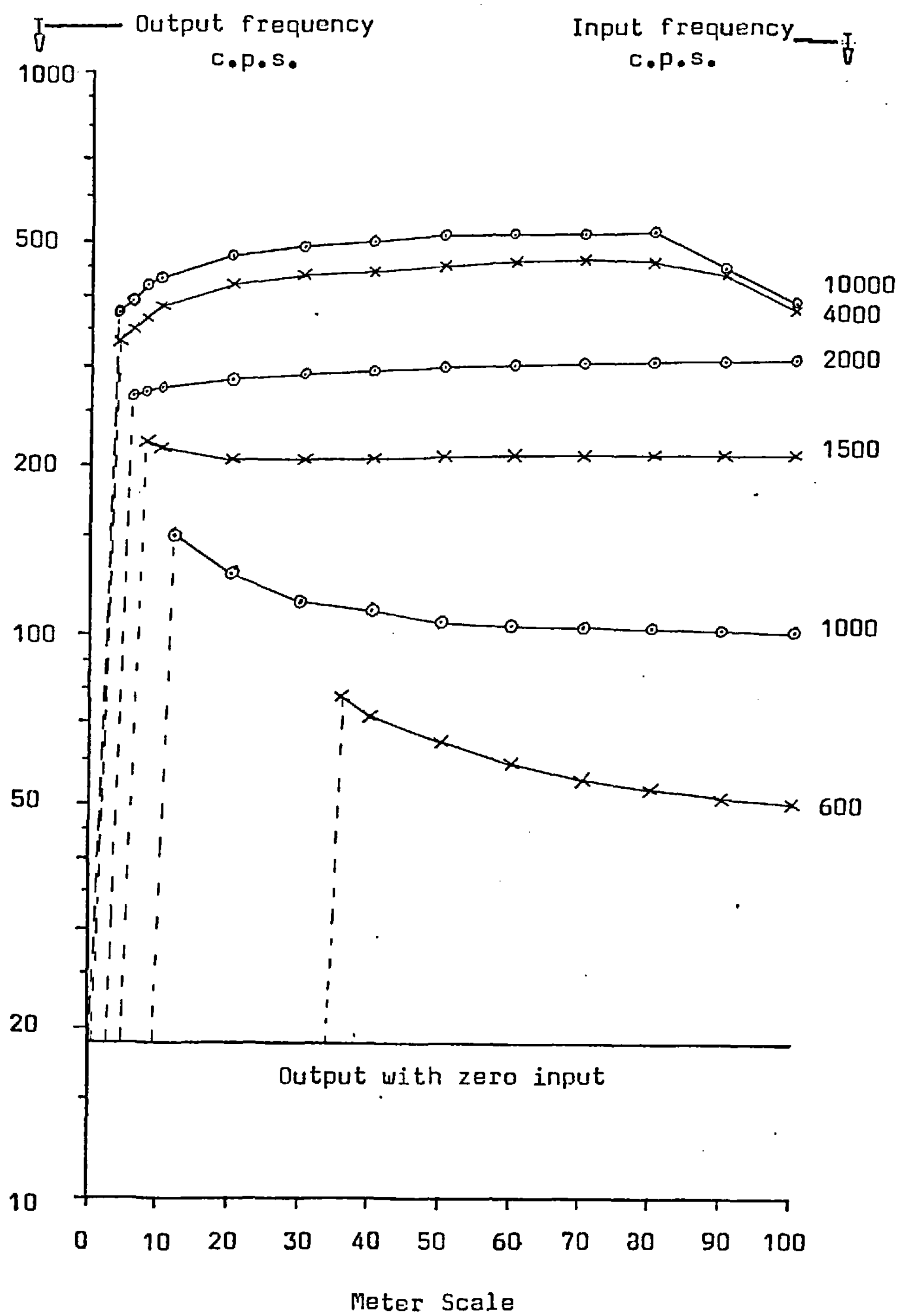
Fig 4/4c

Figures 4/4a – 4e

The graphs show the outputs of the oscillators used to drive the vibrators. The ordinate scale is frequency in cycles per second. The input used was a sinusoidal signal at the frequencies indicated.

The frequencies of the outputs are shown for the frequency varying system. The amplitudes of the signals driving the vibrators can be calculated as follows:- Output frequency/250 = volts pk-to-pk. For the frequency constant system the output level is calculated by means of the expression:- 'Output frequency'/500 = volts pk-to-pk., where the 'output frequency' is read off the ordinate scales.



(Constant frequency, if used, 360 c.p.s.)

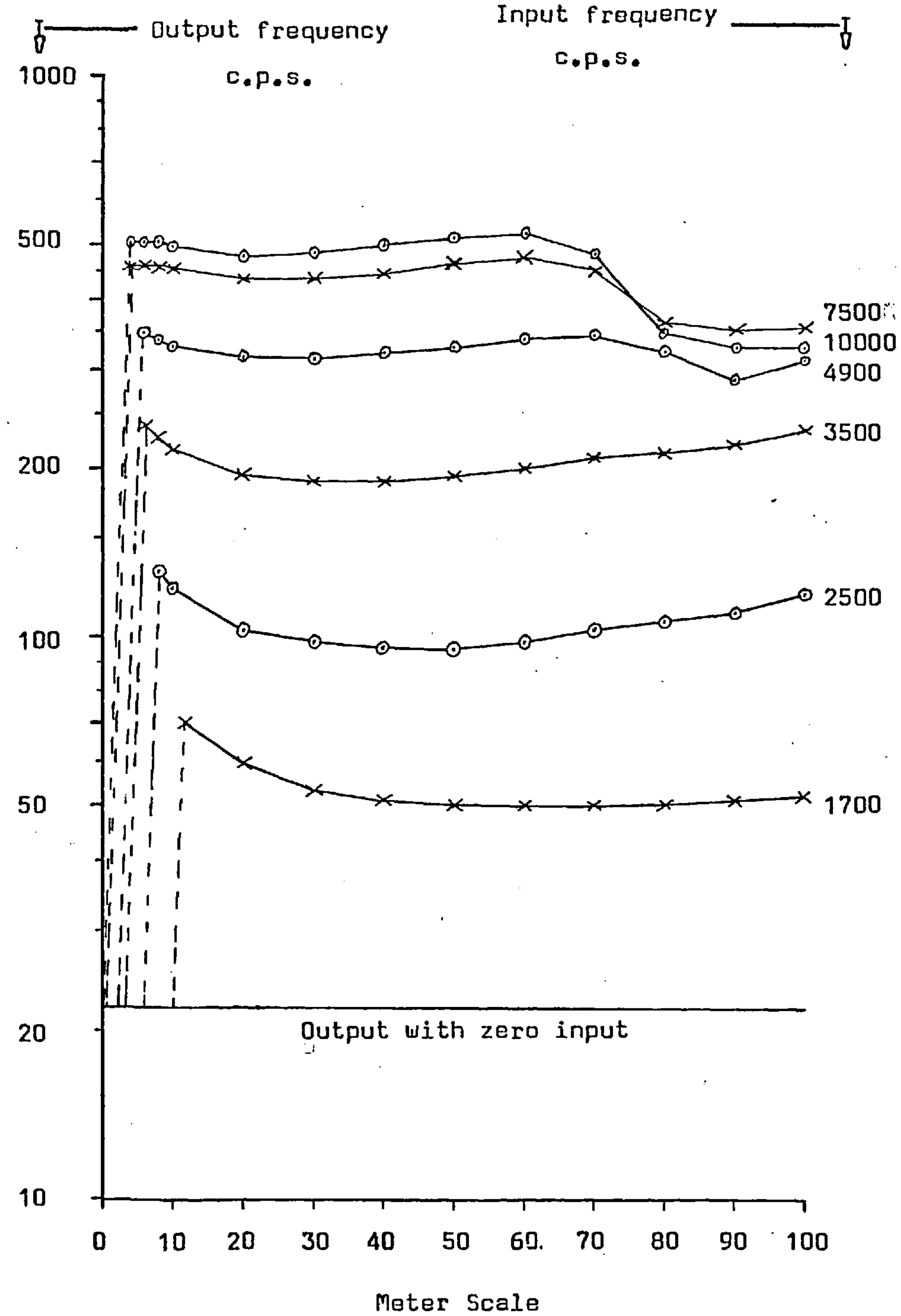
Figure 4/4d

Fig 4/4d

Figures 4/4a – 4e

The graphs show the outputs of the oscillators used to drive the vibrators. The ordinate scale is frequency in cycles per second. The input used was a sinusoidal signal at the frequencies indicated.

The frequencies of the outputs are shown for the frequency varying system. The amplitudes of the signals driving the vibrators can be calculated as follows:- Output frequency/250 = volts pk-to-pk. For the frequency constant system the output level is calculated by means of the expression:- 'Output frequency'/500 = volts pk-to-pk., where the 'output frequency' is read off the ordinate scales.



(Constant frequency, if used, 320 c.p.s.)

Figure 4/4e

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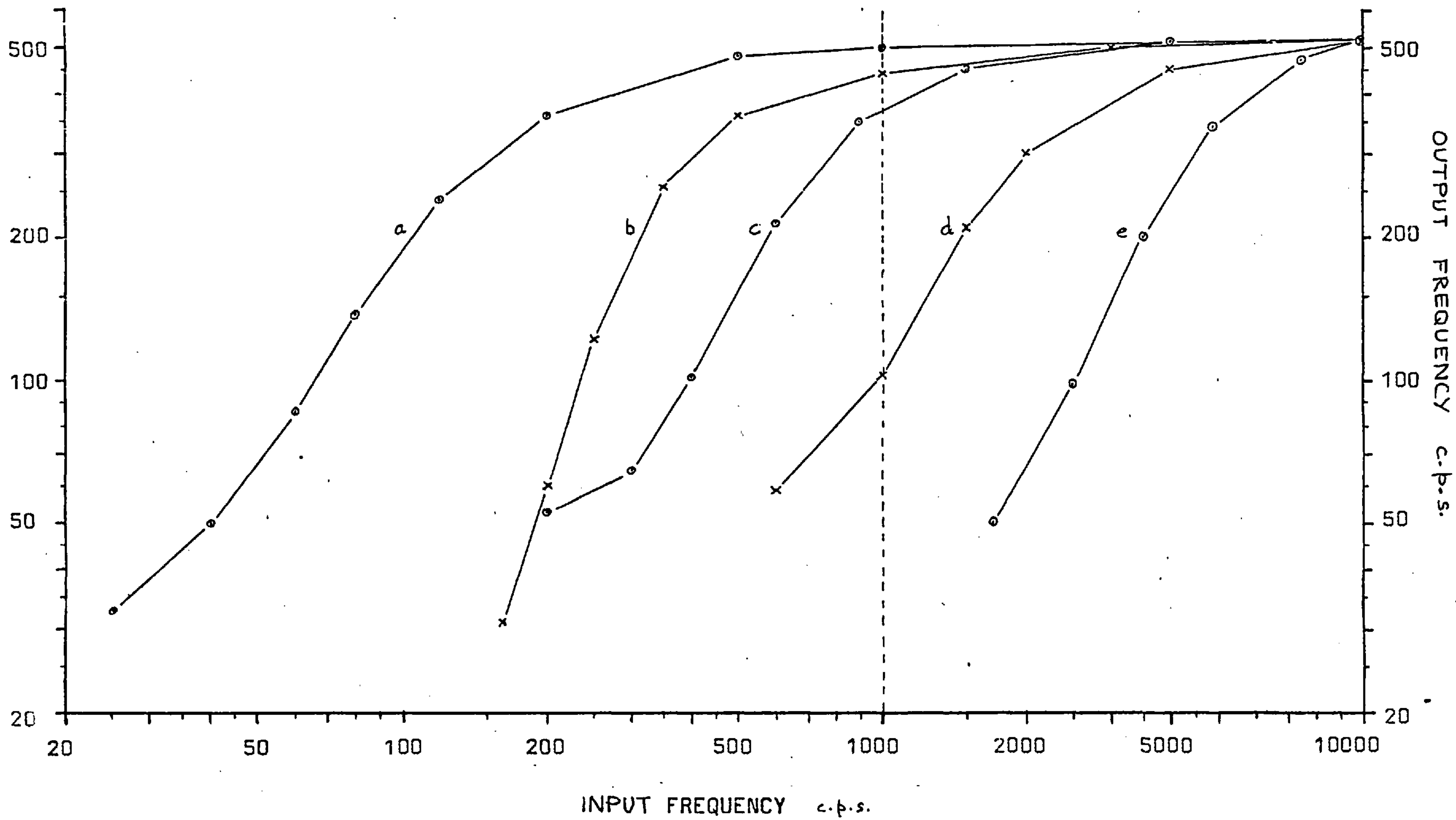
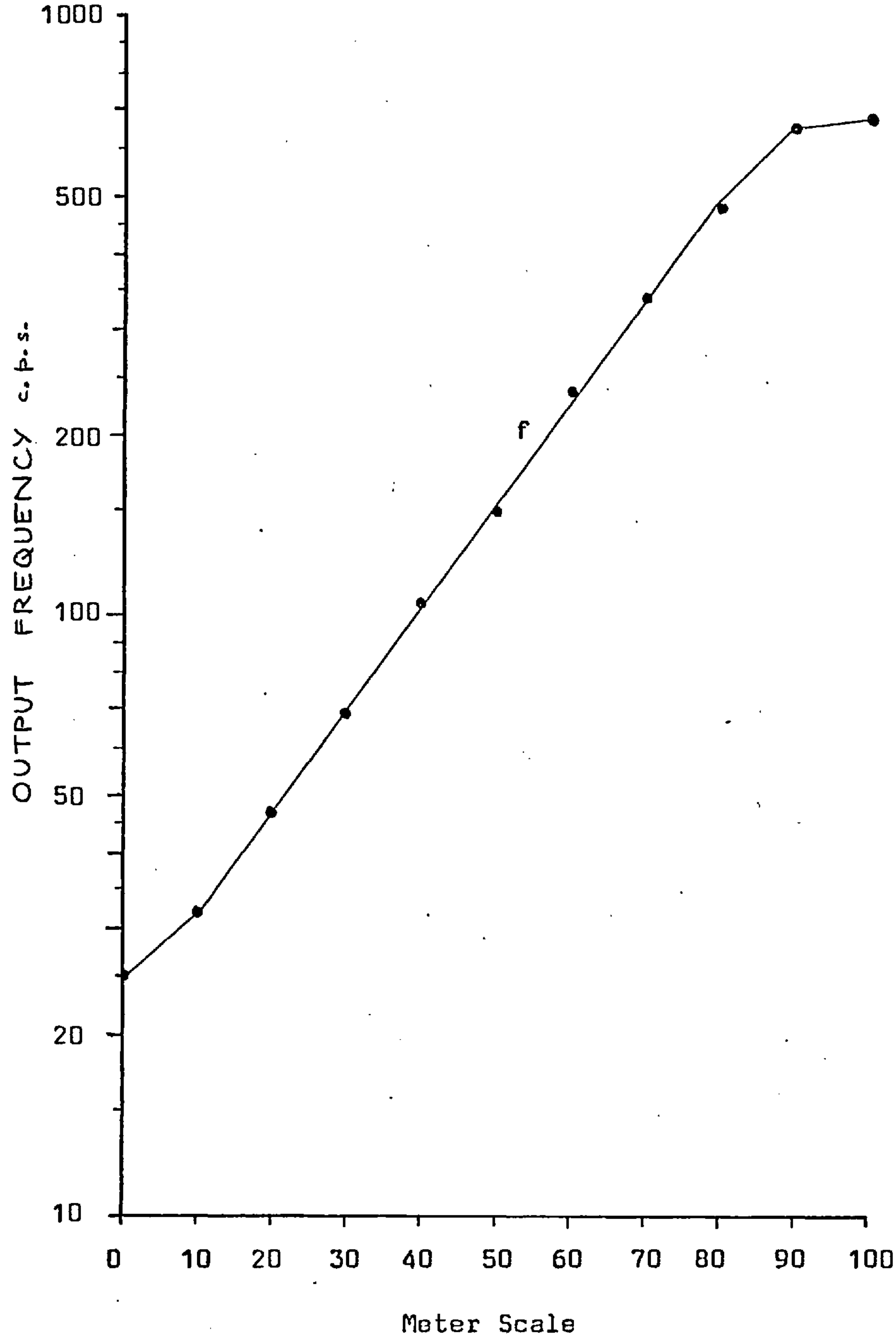
Fig 4/4e

Figure 4/5



This figure shows, as a function of the input frequency, the output frequencies of the five oscillators used to drive the transducers, 'a' – 'e'. The Input sinusoidal signal was maintained throughout at an intensity level on the panel meter of 60.



(Constant frequency, if used, 250 c.p.s.)

Figure 4/6

This graph shows the output of the oscillator used to drive the vibrator which conveys speech intensity information. The ordinate scale is frequency in cycles per second. The voltage of the signal driving the transducer can be calculated using the expression Output frequency/170 = volts pk-to-pk. If the frequency is constant the output voltage, peak-to-peak, is given by the expression Ordinate value/340.

The input signal used had a frequency of 1000 c.p.s.

4 b).

In this section the working of the aid is described in general, and details are given of the reasons for opting for particular design solutions or expressions of ideas, (subsection 4 b).1).). Included in the first subsection is a discussion of the selection of the hand as the site for stimulation.

In subsection 4 b).2). I set out the general principles of the use of the aid. I include in this subsection an assessment of how the sensations feel and how relevant the work reported in Chapter 3 is to the design of the stimulus delivery system employed in the aid.

4 b) 1).

The manner in which the aid operates is conveniently illustrated by a consideration of the stimulus delivered to the hand of the subject as a result of a single sinusoidal frequency, (e.g. 1000 c.p.s.), reaching the microphone.

Provided that the amplitude is of the correct order of magnitude, (this is ensured by the 'teacher' using the meter and the volume controls available), the signal is passed to the filter-bank. The earlike response, when selected, operates before the intensity level is determined and thus the meter indicates the intensity of the signal passed to the filter-bank.

The signal representing the intensity is passed, at a suitable amplitude, to one of the voltage controlled oscillators. It is also amplified and used for switching all six oscillators to their lowest amplitude when the signal falls below a certain threshold, (about 5% of full-scale deflection).

The filter-bank operates on the speech signal in 'parallel', that is, each filter in the bank has as its input the speech signal from the first stage of the equipment. The outputs from the filters are rectified and smoothed. For the output from the two filters with the highest cut-off frequencies additional amplification is provided before rectification to compensate for very low signal levels, (this causes overloading and explains the high frequency high intensity effects seen on Figs. 4/4d & 4e). The rectified and smoothed signals from adjacent pairs of filters are passed to the ratio measuring circuits and hence control the output of the oscillators.

As the signal frequency is 1000 c.p.s. in this instance the two filters with the highest cut-off frequencies pass only a little energy, (see Figs. 4/3 & 4/5). The oscillator controlled by the ratio of the outputs of these two filters will be switched to its lowest amplitude; a separate amplitude switch for each oscillator is provided for the case when the output of the 'higher' filter of any pair falls too low. This ensures that when the outputs of two adjacent filters are both very low the control voltage produced by their ratio is not the same as when the outputs of the filters are both high, (in both cases the ratio might be close to 1:1 and the two cases must be distinguished).

The oscillator controlled by the third and fourth filters will have a frequency of about 100 c.p.s. (see Fig. 4/5). The oscillator controlled by the second and third filters will have a frequency of about 375 c.p.s. and the other two oscillators will have even higher frequencies.

As the signal frequency changes, the amplitudes, (and perhaps frequencies as well), of the stimulus generation signals will change. If a signal of 10 kilocycles per second is used all five oscillators, ('a' - 'e'), will have their maximum output. As the signal frequency is reduced the oscillators, and thus the vibrators as well, will reduce their output, one by one until at a frequency of perhaps 100 c.p.s. only oscillator 'a' will have a sensible output, with a frequency of about 200 c.p.s., (assuming the frequency is not kept constant).

With a more complex input signal the 'pattern' created at the vibrators will be more complex. With speech signals from a microphone the pattern will vary markedly according to the presence or absence of high frequency sounds such as those present in '<u>s</u>' and '<u>sh</u>'. Even if voicing is present with frication, as in the sound '<u>zh</u>' in vision for example, the pattern will be different from that experienced for '<u>s</u>' or '<u>a</u>'. If a phoneme is strongly voiced and without frication then vibrators 'a', 'b', and 'c' will be active to various extents whilst 'e' and perhaps 'd' will not change much, if at all, from their 'resting state'. Vocing will also be indicated by the intensity vibrator, 'f', but when there is no voicing there will be a bigger contrast between the sensation from 'f' and those from 'a', 'b', and 'c'.

The scheme is that the 'patterns' will prove distinctive enough for the sensations to be usefully discriminable.

* * * *

Referring to the above and to the three components described in subsections 4 a) 2)., 4 a) 3)., & 4 a) 4)., some of the design points which need expanding are the following:-

a) In the <u>microphone-processor section</u> of the equipment the 'earlike' response is provided as an optional filter. This provision is made because the characteristics of speech 'match' the ear's frequency response, (see Fig. 4/7). To ignore the fact that certain frequencies in speech are perhaps perceptually more important than others would be unsound.

This consideration also led to an unsuccessful attempt to place the filters 'c', 'd', and 'e' such that the mobility and Q, (shape), of the second formant were taken into account. Figure 4/5 shows that the attempt to give 'greater frequency selectivity' to the system at the frequencies around 2000 c.p.s. was not successful. The second formant is not the <u>most</u> important, no one formant is more important than the others, strictly speaking. However, taking into account the match between the ear's characteristics and the properties of speech it is clearly very important to convey the information contained in the second formant movements, (the second

Figure 4/7

This figure shows some of the characteristics of the speech channel. Solid lines represent speech production, dashed lines represent speech reception. Only the round symbols refer to the left ordinate scale.

●---● Describes the ear's response favoured by Richards, (1973).

⊙---⊙ Describes the ear's response as published by I.T.T., (1956).

○—○ ⊗—⊗ Show the long term averages for the energy in speech, (males and females pooled), measured at 25 mms. from the lips, (upper curve, after Richards,1973), and 30 cms. from the lips, (lower curve, after Dunn & White,1940).

■—■ ▪—▪ Show the 'peak to mean ratio' of energies in speech, (from I.T.T.,1956). The figures on the right ordinate scale are in dB. for these curves. Octave bandwidth filters were used for frequencies below 500 c.p.s., half-octave bandwidth filters for those above 500 c.p.s. The upper curve shows the peak to mean ratio when the mean is established over intervals of 75 seconds, the lower curve is for when the mean is established over intervals of 1/8th. second. (The values for unfiltered speech are, approximately, 21dB. for the long intervals and 11dB. for the short intervals.)

▲—▲ Gives an indication of formant bandwidths, (with the right ordinate scale showing Q values). The figures used for this curve, and the formant positions indication at the top of the graph, are after Fant,(1960), and Flanaghan,(1972).

The points plotted to describe the above curves are not data points from the sources quoted but are points selected from printed graphs so as to enable the curves to be drawn.

△---△ This curve shows data from Kryter,(1962). The points plotted are the centres of bands of equal contribution to speech intelligability. The ordinate scale on the right is for ratios of 'frequency/bandwidth'.

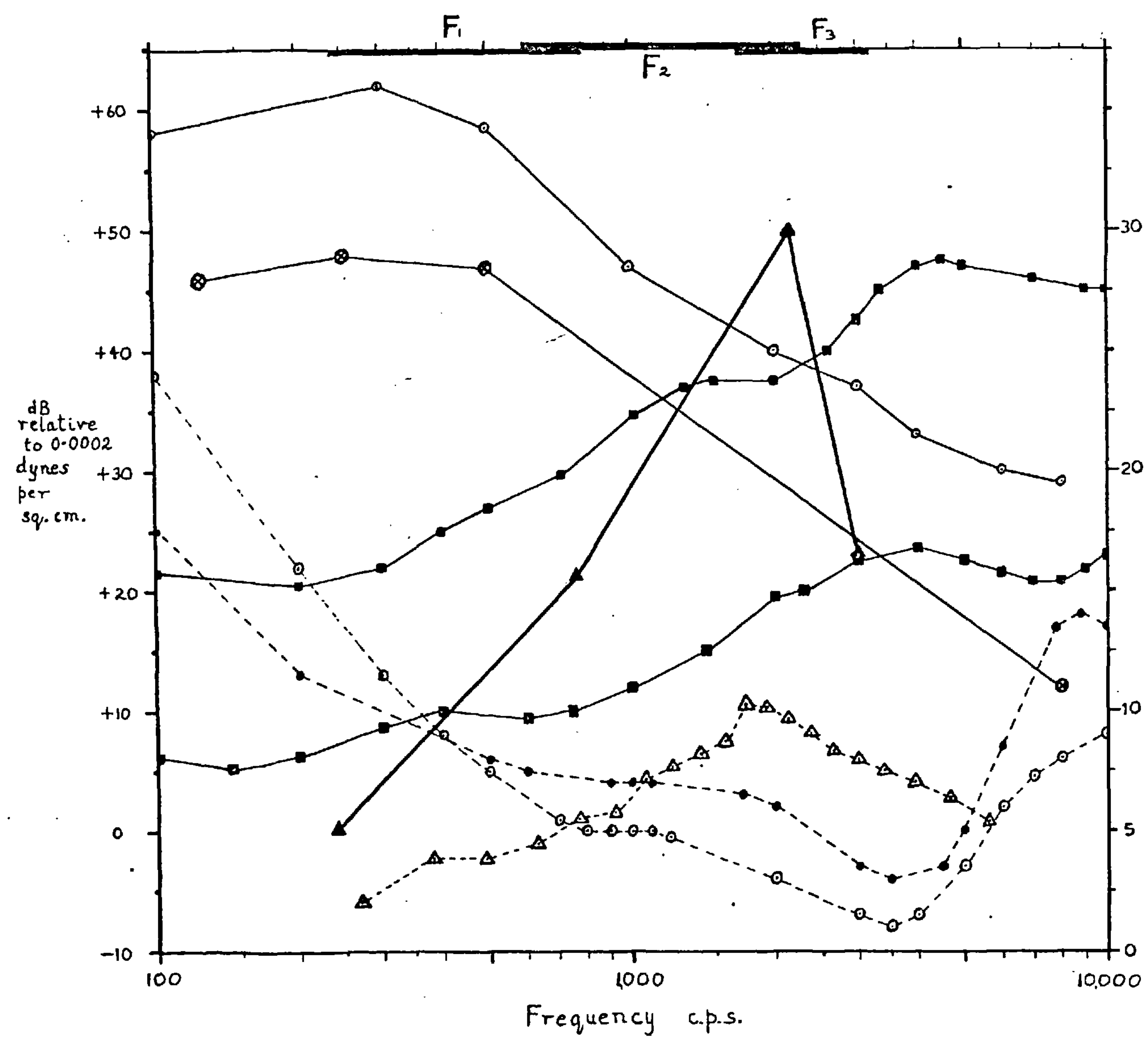


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Fig 4/7

formant is very mobile). In view of this relative importance of the second formant the shortcoming is not trivial, however the attempt to account for the importance was informal. The details of the failure and the suggested improvements are to be found in Chapter 5.

The lack of amplitude compression in this section of the equipment, (preceding the intensity 'measuring' circuitry and the processor), is the result of considerable practical investigation into the problems of automatic gain control. (For a modern review of these problems see Nabelek's, (1972 & 1973), discussions of a.g.c. in hearing aids). It was decided to attempt to accomplish 'amplitude variation exclusion' in the processor; the possibility of doing this was only slowly appreciated. As originally conceived the processor's function was that of 'spectrum analyser', (I considered it to be superior to a conventional narrow-band filter bank for this section of the equipment). This leads onto the next point.

b) A conventional analyser based on a narrow-band filter bank, (a set of adjacent sharply tuned filters), will yield as output a set of voltages the absolute values of which will be governed by the amplitude of the input signal. If the amplitude changes rapidly at any given frequency the ability of the appropriate filter, (a resonant circuit), to respond to the change will depend on the rapidity of the change and the Q of the filter. Thus, although taking the ratios of the outputs of adjacent filters would yield a frequency 'analysis' system insensitive to overall amplitude, the ability of such a system to respond to transients becomes more and more limited, as the number of filters employed, (and hence their Q and the frequency discrimination), increases. This is because the responses of the two adjacent filters to any given transient will not be the same and therefore the ratio of their outputs will not be established immediately, (i.e. there will be some 'settling' time).

The idea of using high-pass filters with overlapping bands, (all the filters having the same high frequency cut-off), was conceived originally as solving the problem, (transient response), inherent in resonant systems. The formal viability of the idea has not been established; I have not produced any mathematical arguments to substantiate my claim. However, I consider that even an intuitive reason for taking a design decision is better than no reason at all, and there would have been no better reason for opting for the narrow-band filter system. The narrow-band filter bank concept is extensively employed in speech processing and analysis. This is because, in 'physical' terms, such a procedure is obvious, (i.e. 'frequency' and 'amplitude' are considered mathematically and a narrow-band filter bank enables 'frequency' analysis of the signal to be carried out by measuring the 'amplitude' as a function of 'frequency'). However, 'extensive use' of a principle does not amount to acceptance of it as 'optimum'. The advantage of the narrow-band filter bank system is its ease of construction, but it is not as sophisticated as the system I have devised because it provides 'too much' information.

The additional advantage of using ratios, (the extraction of information about the frequency distribution of the energy in a signal regardless of the amplitude of the signal), is a general one, as mentioned above, and the practical usefulness of this was not at first appreciated. The system I have devised depends upon ratio measurement, (which is technically difficult), whereas narrow-band filter banks can operate without employing ratio measurement.

With high pass filters having overlapping responses/cut-offs at low frequencies and all having the same high frequency cut-off, the frequency information is not so immediately useful, (the discussion can be generalised so that 'low frequencies' means 'around the frequencies of interest'). If the ratio between two adjacent filters is usefully measurable then there must be more than a certain minimum amount of energy in the filter of the two with the higher 'low-frequency' of cut-off. (In the system I have devised the output of the filter with the lower 'low-frequency' cut-off is never the smaller of the two outputs and I have assigned it to the numerator of the ratio. Adjustments can be made to the actual voltages, as I have done, to make full use of the ratio measuring circuit described in 4 a) 3).).

If the ratio is measurable and not one-to-one then there must be a difference in the energy content of the two filters, and this can only be at the frequencies around the two 'low-frequency' cut-offs. The energy distribution at still higher frequencies can be ascertained from the ratios of the outputs of filters with higher 'low-frequency' cut-offs, (see Fig. 4/3).

There are two main disadvantages with this system. The first is that the 'detail' available, (in terms of the frequency distribution of the energy), at 'low-frequencies' which are low in the band of interest is potentially not as good as that available at high frequencies, (in the band of interest). This is because, (assuming a fairly even frequency distribution of energy), two adjacent filters with their cut-off frequencies low in the band of interest will pass all the energy at higher frequencies and this will mask to some extent the variations occurring at the cut-off frequencies of the filters concerned. At the higher frequencies in the band of interest the amount of masking will be smaller. This non-linearity of frequency sensitivity, (i.e. sensitivity to changes in the frequency distribution of the energy of the signal), can be compensated for by emphasising the low frequencies of interest, and in speech this low frequency emphasis occurs naturally, (see Figs. 4/3 & 4/7).

The second disadvantage with the high-pass filter system is that the ratio of the outputs of two adjacent filters will be one-to-one when they both contain very little energy, (this can be sensed and the ratio ignored, as is done in the present system), as well as when they both contain the same amount of significant energy. Thus the ratio is only significant if it is greater than 1:1 and usefully measurable. In a system such as that employed this means that if a signal contains exclusively high frequencies, (such as '<u>s</u>' or '<u>sh</u>' in speech), all the ratios except for the one or two which are informative about the higher frequencies will have values close to 1:1, or equal to 1:1. As these ratios control the intensity of the sensations in the prototype aid the result will be intense stimulation of almost all the fingers whenever '<u>s</u>' or '<u>sh</u>' occurs, (for signals without a predominant high frequency component the number of fingers stimulated decreases accordingly). A more sophisticated processing of the ratio voltages could yield an alternative set of control signals which would result in sensations such as would be caused if a narrow-band filter bank had been employed, (assuming the transient problem to be negligible), that is, each finger would only be stimulated if the signal contained energy at or around a certain frequency. I decided to keep the system simple and not try to mimic the performance, (or at least the 'favourable' aspects), of the narrow-band were filter bank system.

The assumptions made in the processor were therefore to i) assume that the filter system that I had invented was superior to any other without soundly establishing this as fact; ii) assume that the frequency characteristics of speech production matched the non-linearity in the sensitivity of the system; iii) assume that the discriminability of the sensations would be good enough with the unsophisticated system.

Points i) and ii) are further discussed in Chapter 5, point iii) leads on to considerations of pattern communality and the suitability of the hand as a site for stimulation.

c) One possible disadvantage of using the ratio signals from the processor to control the vibrators concerns <u>pattern variability</u>. Geldard & Sherrick,(1965), and Aston & Weed,(1971), have shown, respectively, that for pattern discrimination to be good, (using a code system with 60 c.p.s. vibrations), and for effective vowel discrimination, (using an 8 channel vibrotactile vocoder and 300 c.p.s. vibrations), it is important not to have a high degree of pattern communality. (Communality is defined as the percentage of two sequential patterns which is subjectively considered to be common to them). It is difficult to assess the communality of the sensations evoked with the system I have devised. Some fingers are stimulated almost continuously, (but not necessarily unchangingly), whereas others are much less involved. In the present realisation the variability of the sensations may be artificially restricted.

The choice of the hand as the site for stimulation is difficult to better. It is very sensitive and easy to use. The fingers can be applied to the vibrators instead of strapping the vibrators to the body, (if the deaf person wishes to cut himself off from the system the action of withdrawing the hand suffices). A large number of sites is available in a small space, and in connection with this Aston & Weed, (op. cit.), showed that the hand was a suitable site for vibrotactile stimulation, (they effectively showed that site separation, favoured by Geldard & Sherrick, is not important when the hand is used, if separate fingers are used as separate sites).

Franzen,(1965, 1969), has found that for vibratory sensations on the first three fingers, perceived vibratory intensity was adequately described by the principle of vector summation. That is, using vibrations at a frequency of 300 c.p.s. and stimulating one, two, or three fingers simultaneously, the perceived <u>overall</u> intensity can be expressed by Steven's 'power law'

$$\Psi = c(\phi - \alpha)^{\beta}$$

where Ψ is the sensation magnitude,

ϕ is the physical magnitude,

c is a constant related to the arbitrary unit of magnitude,

α is a constant related to the threshold,

β is the exponent.

Franzen notes that the implication is that interaction between the channels takes place. The psychophysics reported in chapter 3 also shows, although in a completely different way, interaction between the 'signals' from each digit. There is clearly a need for more extensive work on the perception of sensations generated by varying frequencies of vibration applied to many sites on the hand. As mentioned in Appendix 1 a great deal of psychophysics has been done with 'unnatural' stimuli. It should not be surprising that the 'signals' from the digits interact, a holistic approach to 'naturalistic' tactile sensations on the hand is clearly needed.

The hand is normally called upon to function as a tactile unit, to integrate the sensations from the various fingers into a perceptual whole. Such integration is important for a tactile speech aid and it is likely that with practice, (and a definite attempt at learning), the sensations will become integrated. (It may be necessary to take steps to reduce the communality of the sensations created by the present system, i.e. to mimic the favourable aspects of the narrow-band filter bank). It will always be possible to focus attention onto the performance of a single vibrator, if desired, but the hope is that subconscious appreciation of the whole pattern of events will become the normal way in which the sensations are interpreted by people. (This may be like the experience Morse Code users go through when learning to receive messages).

The above considerations lead me to contend that any alternative to the hand as a site for the vibrotactile stimulators will have to have strong attractions.

4 b) 2).

In use the aid is connected to two sets of six vibrators, (one for the teacher and one for the pupil). The vibrators stimulate the five fingers of the left hand and a point on the palm at the base of the little finger, (suggested as a suitably sensitive spot by F.A. Geldard, personal communication). The intensity of the vibrations delivered to this latter site is higher than the intensities used for the other sites, (the threshold is higher, see also below). The low frequency, low intensity vibrations presented by all the vibrators when no signal is present at the input serve to 'guide' the fingers to the correct locations, (i.e. a 'standby' sensation is available to the user of the machine and serves to indicate that the hand is suitably placed on the vibrators).

The vibrators on the five fingertips are driven by the oscillators 'a' - 'e'. In the configuration used, (maintaining, it is thought, the integrity of the 'patterns'), the vibrator stimulating the little finger is driven by oscillator 'e', oscillator 'a' drives the vibrator for the thumb, and the others are in order across the hand. The oscillator 'f', (controlled by the intensity of the speech), stimulates the site on the palm.

This arrangement, (others are possible), preserves the integrity of the individual 'patterns' and provides a flow of sensation back and forth across the hand. The position of the vibrator which indicates the intensity of the speech serves to draw attention to changes in the intensity which occur at syllable rate. Thus when the word '<u>sigh</u>' is said the '<u>s</u>' is distinctly indicated, and when the vowel starts, (i.e. voicing is introduced), the intensity vibrator indicates this as well as the five vibrators on the fingers. The sensation on the fingers is very lively and flows away from the little finger at the termination of the '<u>s</u>' and on to the thumb and first two fingers at the start of the dipthong. At the end of the dipthong the vowel has changed to '<u>ee</u>' and the sensation flows back to the third finger, (the second formant for the vowel '<u>ee</u>' has quite a high frequency and so is detectable on the third finger).

The frequency range available for the vibratory stimulus, (about 20 c.p.s. to about 600 c.p.s.), was initially used but almost immediately it became obvious that if the vibrations increased much above 500 c.p.s., (in the case where amplitude and frequency vary together), a slightly lowered subjective impression of sensation 'loudness' was experienced. It was thought that this might be confusing and so the maximum frequency was lowered to be about 530 c.p.s. This is a compromise figure and allows almost 30 dB. of change in stimulus level. The reason for the strange subjective effect is not known but it may well be related to the fact that the threshold curve indicates that at high frequencies the threshold rapidly increases. It may also have something to do with the sensation of vibration becoming instead a sensation of pressure, (for both these points see Appendix 1).

Another aspect of the stimulus system was quickly changed when the aid was first tried. The constant frequency stimuli were found to be too intense and numbing and the signals driving the vibrators were accordingly changed. The figure adopted for the reduced amplitude of vibration was, as shown in Figs. 4/4a - 4e, half the amplitude used with the frequency varying system. This was arbitrarily decided. However, even with the decreased amplitude it was very noticeable that the constant frequency sensations were very dull and still numbing, (it is not known whether the dullness is due to the reduced stimulus variability caused by the reduced amplitude range used). After another attempt at using the frequency constant stimulus work with it ceased, (the subject so much preferred the alternative that she no longer wanted to try the frequency constant system). Thus, despite the ambiguous results of the work presented in Chapter 3, (which showed that there was no apparent advantage for either stimulus), the frequency constant stimulus was assessed to be inferior. This assessment is of course very informal and subjective but it nevertheless shows the need for caution when transferring the results of one particular type of investigation to a more general case. (This point is further discussed in Chapter 5).

The 'intensity' vibrator was adjusted so that at about '80' on the meter on the equipment the sensation was strong, (see Fig. 4/6). This more than compensates for the higher threshold at the site used. The reason for this is that the meter was used for adjusting the strength of the signal passed to the rest of the circuitry. It was convenient to adjust for about '80' on the intensity level meter as there would always be the occasional syllable with intensity higher than this. Thus it was ensured that the signal did not overload the circuitry more than very infrequently. Any sluggishness of the meter movement would also tend to yield readings lower than actually present, as voltage levels, in the equipment. It is likely that distortion caused by overloading in the processor was kept to a minimum.

*   *   *   *

The pupil and the teacher, (the latter wearing earphones supplied with signal from only the pupil's microphone), keep their hands on the transducers continuously throughout a session, (and this might last as long as 1 hour). The teacher's earphones serve to concentrate attention onto the pupil's voice and to block out, to some extent, his own voice. This enables the teacher to attend more to the sensations on the hand and to correlate these with the pupil's voice, his own and his knowledge of articulation.

The vibrators, being fairly noisy, are placed in a small sound-shielding box, (one for each person), and in use they remain in the box and the hand is inserted through a flap in one side. Each set of vibrators is thus a fairly bulky affair. It is necessary to exclude from the microphone the sounds made by vibrators as otherwise positive feedback occurs; and also the teacher becomes distracted by the noise of the vibrators.

The minimum requirement of any vibrotactile aid is that discriminations should be possible without requiring the pupil to learn the sensations for different phonemes. Thus it is essential that the aid should enable the pupil to distinguish between '<u>ship</u>' and '<u>sip</u>' or '<u>shack</u>' and '<u>sack</u>' for example, (the distinction being made purely tactilely). If such distinctions were not possible with an aid then it would be worthless. The strong requirement that all the sensations be memorable in the long term is not necessary for articulation training or lip-reading. Such a requirement would only be necessary if it was envisaged that the equipment would be used to make possible telephonic communication between the deaf, or in some other situation where lip-reading was not possible.

For articulation training, as mentioned above, the minimum requirement is that the sensations should enable some phonemes to be discriminated. If the goal is 'an aid for lip-reading' then the requirement is that some of the sensations should be memorable in the sense that they can serve to disambiguate articulations which are ambiguous to the eye. The voiced '<u>b</u>' and the unvoiced '<u>p</u>', for example, are visually indistinguishable.

Thus any aid for articulation training, (the least ambitious goal), must provide for the teacher and the pupil the means of demonstration. For the pupil mimicry is the facility lacking, (i.e. self-demonstration). The sensations only need to be remembered for as long as it takes the pupil to utter a copy of the utterance first given by the teacher. Hopefully the pupil will learn to sense whether a version produced by him, of a word first given by the teacher, is correct, (i.e. the same), or not. This increased awareness will also, hopefully, be integrated with the deaf person's overall perceptual knowledge of the word, especially his kinaesthetic awareness of his own vocal gesture production. For this latter, descriptions offered by the teacher of correct vocal organ movements may also help.

This general discussion of what is to be expected of an aid applies as much to rhythm errors as to those examples previously cited. Such rhythm errors might be, for example, introduction of a vowel where there should be silence. '<u>Six</u>' might be rendered '<u>sickus</u>' due to the inability to say '<u>ks</u>'. Rhythm is also important in that the duration of a phoneme is sometimes sufficient to disambiguate two words and is in any case an indicator of stress and thus important for the overall rhythm of any sentence in which the particular word occurs. If the deaf person's knowledge of such points increases then so will his lip-reading ability, and so improvement in speech production will yield dividends for speech reception.

If discriminable tactile sensations can be created for use by the teacher to demonstrate, and for use by the pupil to enable him to mimic, then an articulation training aid has been provided. I consider that the aid I have developed provides useful tactile sensations; I do not think that as it exists it could be of use for speech reception. (This point is taken up in the next chapter). I decided that for the initial feasibility study the aid should be assessed as a training aid, improved versions, if built, could be assessed in more ambitious circumstances.

CHAPTER 5

In this chapter I give my assessment of the aid, and criticisms of it, from a technical standpoint. I also compare the aid with other tactile aids. (Section 5 b).).

Before that, however, I present a detailed account of how it has been used, and two evaluations of its usefulness as a speech-training aid. This section, (5 a).), is entirely non-technical.

The one subject used, Lydia, is the profoundly deaf woman who took part in the psychophysical experiments.

5 a).

This section is divided into the following subsections:-

5 a) 1). Description of the use to which the aid has been put. This subsection continues, in greater detail, the exposition in 4 b) 2).

5 a) 2). In this subsection I present an evaluation of the aid.

5 a) 3). The speech therapist's evaluation of the aid, and her comments on it, are presented and discussed in the subsection.

5 a) 1).

The 'room' in which the aid has been used is a small, (approximately 2 x 2 x 2 metres), 'sound-proofed' chamber. Air conditioning was soon seen to be essential and quickly installed, but nevertheless the sound level in the chamber is very low. Another change was made to the cubicle, after some use, and this was to increase the lighting levels. Good lighting is essential for lip-reading. The cubicle was not designed for experiments involving more than one person and a very small amount of equipment.

The subject, with a light source behind her head, sat facing the teacher, (myself), and each had their left hand on a set of vibrators. The equipment was placed to the left of the teacher in such a way that the subject could not see the panel meter indicating the intensity, (I considered that this might be distracting). Both right hands were free for finger spelling using the American single-handed manual alphabet, writing, handling books, or using a hand-mirror. The microphones were suspended close to each person's head but in such a manner as to avoid obstruction of vision or restriction of movement. (The tape-recorder was placed outside the booth).

Before work started with the aid the subject's speech therapist moved to Edinburgh. The subject and her therapist considered that I should take on the role of 'speech therapist'. Although untrained I knew more about the machine than anyone else and the subject, who had been visiting the department for the psychophysical experiments, trusted me. For a speech therapist to have to cope with an unknown patient and an experimental situation in a laboratory, involving considerable travel once a week, would have been too much to ask, (the 'practice' being at the University College Hospital, London), even if the replacement therapist had been interested. The replacement therapist was not approached, and the subject has not consulted any other therapist.

Trustworthiness is important to the subject and she is nervous when meeting people for the first time or when in the company of people she does not know very well. A new speech therapist would have introduced difficulties and would have made evaluation of the subject's speech progress impossible.

*   *   *   *

The material used reflects my lack of training and the week-to-week desires of the subject. The words discussed vary from 'Rex', (the name of a dog), 'five', 'pet', to 'voluntary', 'government', 'difficult'.

These are a very few of the words discussed and serve to show that simple words are not easier than long words. We also used as material short phrases that she often required and found difficult, for example, asking for a railway ticket. Similarly, we used as material phrases or words that she found difficult when talking to me.

In addition I sometimes asked her to read from a short story, (Steele, 1938), and any word that she did not understand or had difficulty with we discussed at length, usually in isolation as this was the way she tended to view her difficulties. (The effect of considering the words in isolation is discussed in 5 a) 3).). Apart from her mother and father, (who are used to her speech and so do little to help her improve her speech), I was very often the only hearing person she spoke to during the week.

The techniques employed were simple. If I felt that the word causing problems could be usefully demonstrated with the aid I formed my corrective guidance around the discriminable differences and tried to relate them to the mouth movements I considered to be faulty. If I considered that the aid might not be useful I did not mention it until after I had obtained an improved version and then I asked for her opinion of the discriminability of the two versions. This I did by asking her to utter the correct and the incorrect versions of the word alternately. This is a useful exercise and it demonstrates the point of the aid as providing the facility for mimicry. Of course it is most useful when the two versions of a word are tactilely different and it encourages kinaesthetic introspection in the subject. She is good at this and will correct herself spontaneously, although an indication that something was wrong is often needed to spur her into trying again. This indication need not be specific and if she incorrectly assesses her performance and error useful information is obtained about which sounds she finds difficult.

The mirror, an addition to the 'equipment' made after a short time, served to aid introspection concerning mouth movements and to establish that differences to which I was drawing attention were in fact visible. I was very conscious of the need to be convinced that a visible or tactile discrimination was possible; instructions based on false discriminations proved frustrating for both of us, as did faulty instructions.

I did not undergo any tuition in speech training relying instead entirely on my perspicacity, (and self-confidence in my abilities as a teacher). I consider this valid for three reasons.

i) Straightforward speech therapy is not overly concerned with the problems of teaching the deaf to speak, as this is largely the prerogative of 'teachers of the deaf', although speech therapists do concern themselves with such problems if deaf people are referred to them.. I felt that any instruction in text-books on the subject might prove too oralist, (or be based on the assumption that the patient can hear), and that for me to approach a trained therapist for guidance might arouse in such a person professional indignation.

ii) I consider it important to approach a teacher-student situation with the problems and desires of the student uppermost in my, (the teacher's), mind. To view the situation to be one in which I, the teacher, have knowledge to impart, (with the assumption that the student wants or can make use of such knowledge), is unreasonable and a presumption on my part.

iii) It is valuable, in my opinion, to convey to the subject/pupil, in any teaching situation, the feeling that the teacher can learn something from the subject/pupil. I encouraged this attitude because I felt that in the teaching situation in which I was involved it was important for me to learn something about the subject's thought processes and also about what it is like to be deaf. I thought it would be valuable to suggest to her that I did not know all the answers, could not provide infallible help and that she could teach me things that I did not know.

Thus I learnt, as we worked together, how best to help her correct some of the faults in her articulation, and also something of the life of the deaf in the world of the hearing.

*   *   *   *

Before going on to my evaluation of the aid, (section 5 a) 2).), it is appropriate to mention what I discovered of the way Lydia, (the subject), thinks.

It would appear, from many informal observations and from her answers to my questions, that Lydia, when thinking, uses mental symbols not involved in the production of speech, (this does not of course mean that her thinking is either alinguistic or conducted in English), (see Chapter 2).

Visual symbols are obviously important. If I asked her to think of a word that she found difficult, (I did this on many occasions), she would invariably write it with her finger on the table top, or picture it in her mind, (as though on a vertical pane of glass - my description but very appealing to her), whilst staring at the wall, out of the window, or at a plastic 'whiteboard' provided for student use. These discussions usually took place outside the cubicle, whilst we had a cup of tea or coffee.

Whether we were inside the cubicle or not much amusement resulted from another, (often displayed), illustration of her visual approach to speech and words. If she could not recall the correct spelling of a word, (very important for her attempt at uttering it), I would start to write it down. I very rarely got further than the first one or two letters, (if that). Putting a blank piece of paper in front of her and starting to write, (I wrote upside-down for her benefit), was obviously sufficient to help her accurately recall and correctly utter the word in question, even if it were a long word. However, some words seemed never to be so easily recalled and uttered. There may be a correlation with the degree to which the written word is a phonetic transcription, c.f. 'difficult', 'lollipop', with 'appreciate', 'future'.

When Lydia reads to herself she uses the gross sort of articulatory gestures she uses when signing to another deaf person, accompanied by signs, (although these latter appeared sometimes abreviated or condensed - my brief acquaintance with signing was obviously a handicap here), and also by finger spelling. When reading aloud she also uses the signs and finger spelling but she attempts to articulate for a hearing audience. This is unlike her normal speech, it is of better quality, and she does not comprehend as well as when she reads to herself, (see Conrad, 1971). An illustration of this sort of effect for the hearing reader of this passage is provided by an attempt to read the following passage aloud whilst consciously noting whether or not a sound is to be voiced. Another, slightly more remote, example of incomprehension of written material is typing, where the typist need not, and frequently does not, comprehend what is being typed. An appeal to the context of a word is only made if the handwriting is bad or the dictaphone tape noisy. (For another example see Shaffer, 1972).

The abbreviated signs mentioned above also occur when Lydia is trying to avoid the sort of telegraphese she often uses when speaking. This is very marked when the sentence is simple and she is trying to give it good emotional colouring. (Her face is very active when she speaks).

All these factors suggest that speech production is not an important facet of her mental processing, and possibly language is not much used either, (the 'function' words are the words she omits when she speaks 'telegraphese', i.e. words like 'for', 'but', 'and', 'in', 'we'.....). She says that she speaks telegraphese because she has to speak quickly, otherwise she loses her train of thought, but there are additional familial complications here.

I feel that the frustration she experiences with speech is to some extent apprehensible as the result of the lack of language structure, (perhaps generally, more certainly as regards delivery), in her thought processes and the need for a conscious translation of her thoughts for speech delivery. She sees little point in speech for her, (or amongst deaf people), and it does not come naturally to her, (despite her natural communicativeness). It is a little as though a western hearing person were to find himself in, say, China, with little knowledge of the language, and little prospect of learning it; when visiting a foreign country the language of which is unfamiliar most hearing people experience feelings of isolation. Although the deaf must suffer isolation and frustration more severely than hearing people can apprehend, (comprehension being absolutely impossible), feelings of being 'cut off', evoked by the above example, serve to hint at what deafness means.

For Lydia speech is both important and unimportant. It is important because with good speech delivery she could get along very well in the hearing world, (and she knows this); it is unimportant because without it she is not deprived of thought, she has deaf friends, and in any case she finds speech a tremendous effort. Regardless of the effort she does like talking to hearing people and being with them; the unimportance of speech is slightly academic and theoretical for her, she would very much like to be a hearing person.

The foregoing is intended to convey above all the impression that Lydia must be a very unusual person. She has a great strength of character and much self-awareness coupled with an infectious sense of humour. She undertook to come to College once a week, health and family permitting, to take part in some experimental speech training of unknown value to her.

The improvements in speech reported in the following two sections might not have occurred with a different subject but nevertheless I feel that in the context of a feasibility study the conclusions I reach are valid and valuable for the process of progressing to field trials.

5 a) 2).

The prototype aid very quickly gave me the impression of having potential. This was gratifying, but certain drawbacks were equally obvious and in some respects the aid was disappointing.

The prototype aid can be assessed for suitability as an articulation training aid on the following points:-

a) Indication of voicing and of larynx pitch.

b) Indication of the nature and quality of vowels.

c) Indication of frication.

d) Indication of word rhythms.

In this subsection I first give my subjective survey of the usefulness of the aid, taking the above set of four points as a guideline, (5 a) 2). i).). Following this I explain the objective measurement I have made of Lydia's speech improvement using read material with laboratory personnel and secretaries as listeners, (5 a) 2). ii).).

5 a) 2) i).

a) Indication of voicing and of larynx pitch.

The performance of the prototype aid in this area was a little disappointing. As explained in subsection 4 b) 1) indications of voicing should be provided by the intensity vibrator, (voiced sounds having more energy than unvoiced ones), and by the patterning of the sensations. The sensations evoked by voiced sounds should not involve the two vibrators 'd' and 'e', (except for perhaps the occasional stimulation from 'd'), and larynx pitch should be conveyed, (it was intended), by the vibrator stimulating the thumb. The prototype fulfilled fairly well my expectations as regards voicing indication, although with less clarity than desired, but as far as pitch indication is concerned it performed abysmally.

I quickly formed the impression that the alternation between unvoiced and voiced sounds, an important guide to the rhythm of words, would be reasonably well conveyed and would be usefully demonstrated with the aid. The lack of pitch indication was not too surprising, in retrospect, in view of the fact that the fundamental component of the larynx frequency does not vary greatly and does not, by itself, have much energy. The lack of pitch indication was not a hindrance for work with the prototype aid. (An alternative proposal for pitch indication is discussed in section 5 b).).

b) Indication of the nature and quality of vowels.

With vowel sounds the aid did not provide easily discriminable sensations. This was a serious disappointment and in views of this I explored in more detail its possibilities. I found that with two exceptions sustained vowels were not discriminable tactilely. The exceptions were 'oo', (as in boot), and 'ee', (as in beet), (these two vowels have very different second formant frequencies). Each of these vowels was to some extent identifiable and a transition or glide from one to the other was quite distinctive. The sound 'ee', even if quite short, such as in 'real', was noticeable, and in word pairs such as 'tub-tube', where the softened initial consonant for 'tube' introduces a very short 'ee' sound, (followed by 'oo'), the presence or absence of the 'ee' was often noticeable, (especially if the words were said a little slowly).

This vowel identification/discrimination was all that was possible, but it proved sufficient for use to be made of the aid in demonstrating errors of articulation. Any developed version of the aid would have to do better in this area of speech production.

c) Indication of frication.

Frication was generally well indicated. In particular 's' and 'sh' were strongly indicated and easily discriminated. The limited dynamic range of the instrument showed up most in this facet of articulation training and demonstration.

d) Indication of word rhythms.

Indication of word rhythm was very clear, (this is a combination of a) and c) above). One unexpected benefit deriving from the reduced dynamic range of the instrument was that I was very motivated to ensure that a suitable balance existed between the intensities of voiced and unvoiced sounds, (this required, with Lydia, a reduction in the intensity of the voiced sounds and an increase in the intensity of frication). Without such efforts on my part Lydia's unvoiced sounds tended to be too quiet and her voiced sounds too loud. This meant that the word rhythm could not be reliably transmitted unless a special effort was made on my part and hers. This lack of balance, I came to realise, was definitely a rhythmic flaw, and thus I was accidentally prompted to correct one of the failures in Lydia's speech.

Provided that the intensities were suitably balanced word rhythms were easily sensed as lively variations across the fingers. The demonstration of rhythm was thorough and included voicing and frication patterns, relative durations and intensities, (not of vowels but between voicing and frication), the existence of small segments of silence and the existence of sound where the subject believed there to be silence. The performance of the aid was most impressive in this area of speech training and Lydia found the vibrators useful. For example, the introduction or omission of vowels and or consonants was easily demonstrated and the correct rhythm could be copied and recognised. Examples might be 'intersing' instead of 'interesting', 'sickus' instead of 'six', 'habist' instead of 'habit', 'denetist' instead of 'dentist'. As a rhythm indicator and demonstration aid the machine seems to have definite potential.

* * * *

Despite the drawbacks I felt that the aid had potential and that I had to leave it unaltered for a long period of time, (that is, apart from maintenance), so as to enable an assessment of its usefulness to be made.

No attempt was made to use the aid as a lip-reading aid, (Lydia found that for discrimination of sounds she had to concentrate on the tactile sensations and that she could do this better if she closed her eyes). However, one or two simple experiments on word discrimination were tried. It was found that word pairs such as PET-BET, and PAT-BAT could just be discriminated on the basis of the onset of voicing being delayed for 'P', but some exaggeration in pronunciation was necessary for this, (the time difference is only a few tens of milliseconds). PET-PEST and similar contrasts were tactilely discriminable, (some visual confusion exists for this type of word).

I quickly formed a distinct impression as to how useful the aid would be, and for what. Consequently I tended to concentrate on words which were suitable for use with the aid. As time went by I worried less about whether or not a word or sound was suitable for use with the machine and attended more to the problems of discovering, and conveying to Lydia, how I thought she should attempt to produce a sound or word. If the aid was able to help with a demonstration, I used it, if not I did the best I could without it. I think that the ability to demonstrate, (and for Lydia to demonstrate to herself), correct and incorrect versions of a word is very powerful in that it constitutes for Lydia independent evidence. She does not have to rely solely on my word, my assessment, my expression of pleasure or displeasure; she can feel that it is right. To extend the number of articulatory features and gestures for which this is true would certainly be worthwhile. This impression as to the value of demonstration I felt to be a vindication of my theoretical viewpoint at the outset of the work.

Without the aid for demonstration, explanation of correct or incorrect mouth movements was difficult, introspection suggests that they are somehow irrelevant. I found that flexibility of description was vital. For example, 'j' and soft 'g', are to all intents and purposes the same. They are approximated by 'dzh' in conventional advice on articulation. However, if the subject concerned does not comprehend what he or she should do then persisting with one transcription-description is hopeless. Far better to try some other, perhaps less precise, approximation or instruction, for example:- 'a slow 'ch' with the voice on', if this is going to be better understood.

Indeed, the difference between one instruction, (for a hard 'g' say), such as:- ''k' with the voice on' and another:- 'turn the voice on and then let 'k' happen', is quite marked in that the item mentioned first can assume greater importance in the deaf person's mind. Conventional phonetic transcriptions translated into words are not good enough, the existing performance of the deaf speaker must be considered and the instruction phrased according to that and to the feature it is intended to improve.

My initial fear that the aid would prove to be nothing more than an over-sophisticated rhythm indicator was partly dispelled as the months passed. The slight value of the aid for vowels is important and the ability to detect silences and timing differences of only a few milli-seconds is promising. Both these factors need improvement and a develop-ment version of the aid would have to make good the deficiencies found. The aid's forte is definitely rhythm, especially frication-voicing-frication rhythm, but it need not be.

5 a) 2) ii).

To evaluate my ability to correct faults in Lydia's speech, (i.e. to measure her improvement), I devised the following simple experiment. (This is not a test of the aid itself, if such a thing were possible).

Over the period of many months during which Lydia has been coming to the laboratory for 'speech therapy', (since September 1972), I have made eight recordings of her reading a passage from a short story, (Steele, 1938). The recordings were made approximately every 1 to 2 months, about every fourth - sixth time she visited the laboratory. If there was a long gap, (for school holidays or illness) then two recordings might have been made with less than three unrecorded visits in between. I tried not to record her after a spell of absence as her speech tends to be poor on such occasions. No record of her visits was kept, the nature of the training programme was so informal that a precise record of the amount of time spent using the equipment would not mean very much. In general she spent about two hours at the laboratory every week, of which one would be in the cubicle using the aid in some fashion, (very often conversations started outside the booth continued inside, providing useful material on many topics). I would estimate that during the period covered by the tape-recordings Lydia received about 30 hours 'training' with the aid.

I considered that if I tried to be too detailed and precise in recording her visits I would find myself explaining away too much nasality on one day as being due to a cold, or emotional stress, and on other days I would have had to make allowance for good spirits. It seemed wise to ignore these points and to decide, in advance and without her knowledge, on which days I would record her performance. Accordingly I never knew whether she would perform well or poorly until she arrived, (and of course my judgement of her performance would have been coloured by how receptive I was feeling at the time).

She read the passage aloud for practice, whilst I adjusted the controls and then again 'for the recorder'. In fact both passages were recorded, (she did not discover this), yielding a rehearsed and an unrehearsed rendering for each day, (16 versions in all).

I arbitrarily selected from the passage four sentences of different lengths which I considered to be usually well delivered, (without stumbling or some gross articulatory error which might distract listeners). I thus had 64 different utterances, (16 versions of four sentences), which I transferred onto another tape in a special sequence.

As the day on which I had obtained the fifth recording was approximately halfway through the period covered by the eight recordings I chose the unrehearsed renderings from that day as 'standard', and I gave them a 'score' of 50%. This set of four renderings I recorded twice at the beginning of the tape so that the listeners would have a standard against which to rate the subsequent renderings. The remaining 60 utterances were recorded in random order with a different one of the 'standard' utterances inserted between each group of three non-standard utterances. Thus, although they did not know it, the listeners provided a record, in the scores, of their fluctuating internalised 'standard'.

The tape for the listeners therefore had 88 utterances on it with short, variable, gaps between them, the first 8 being the 'standard' 4, repeated. The listeners were told that the standard utterances had a score of 50% and that they had to rate the remaining utterances on the strength of this. They were laboratory workers and secretaries from the department and 15 in number. They were not all native speakers of Southern English but I considered them to be good speakers of the language, (i.e. I judged that they would be able to cope with the test).

If they did not know that they were to rate the utterances of a deaf person I did not enlighten them until after the test was over. Some listeners knew, (my colleagues), and one of my colleagues who did not know the purpose of the test but who has a deaf sister immediately recognised the speech to be that of a deaf woman. I used the subjects in pairs, (except for one), as the testing would otherwise have taken a very long time. (The tape played for 45 minutes and time had to be allowed for explanations, payment and lateness on the part of the listeners).

I instructed the listeners not to compare scores, (I arranged that they could not see each other's score sheets), and to identify each utterance as they heard it. (The utterances, on the instruction sheet, were labelled so that they had to be able to see/hear which of the four was being uttered). The instruction given to them concerning scoring was that they should judge how much better/worse the utterance was, compared to the standards, and that they should not base the judgement on one word but on their overall impression.

The utterances were played to the listeners through a large 'hi-fi' loudspeaker configuration and the subjects sat in the sound-proofed booth where the speech training took place. None of the listeners recognised the standard utterances as they occurred in the sequence of 80, as intended they marked them as if they had never heard them before.

The scores given by each listener to the novel renderings were normalised to the reference of 50% using the values assigned to the standard renderings. This normalisation was made both forward and backward through the list, that is to say, if nine consecutive scores were as follows:-

40 40 40 40 50 60 60 60 40

with the 1st., 5th., and last being the scores for standard utterances, the data used were:-

45 45 45   68 68 68

All the listeners' normalised data were tabulated and for each rendering the following procedure was employed to yield a rough and ready measure of the agreement between the listeners.

Three averages were computed. These were,

a) the average of all the scores without the lowest score for that utterance,

b) the average of all the scores without the highest score given,

c) the average of the thirteen scores obtained by excluding the highest and the lowest values.

The points plotted in Fig. 5/1 are the values of ((a-b) x 100)/((a+b)/2). These values are a measure of the spread of the scores given, each value being normalised to be a percentage of the average scores for the utterance concerned. For example, for the first novel utterance on the tape the three averages are, a = 51.5, b = 46, c = 48, and (a+b)/2 = 48.75, so ((a-b) x 100)/((a+b)/2), the measure of the spread, = 11.

This arithmetic contrivance produces a useful measure of the spread as a percentage of the average, (overall average), score given for each rendering. The spread figure is not in percent on the same scale as that used by the listeners to make their estimation, (for example the highest score given to the first utterance was 100 and the lowest was 17). The listeners were not constantly outlandish in their estimates, but some produced scores with a very large range (217 - 6 was the largest), and some with a very low range, (65 - 38 was the lowest).

Figure 5/1 shows how the spread varies as a function of the position of the utterance along the tape. As one would expect the spread diminishes with position, the later presentations having the lower spread values.

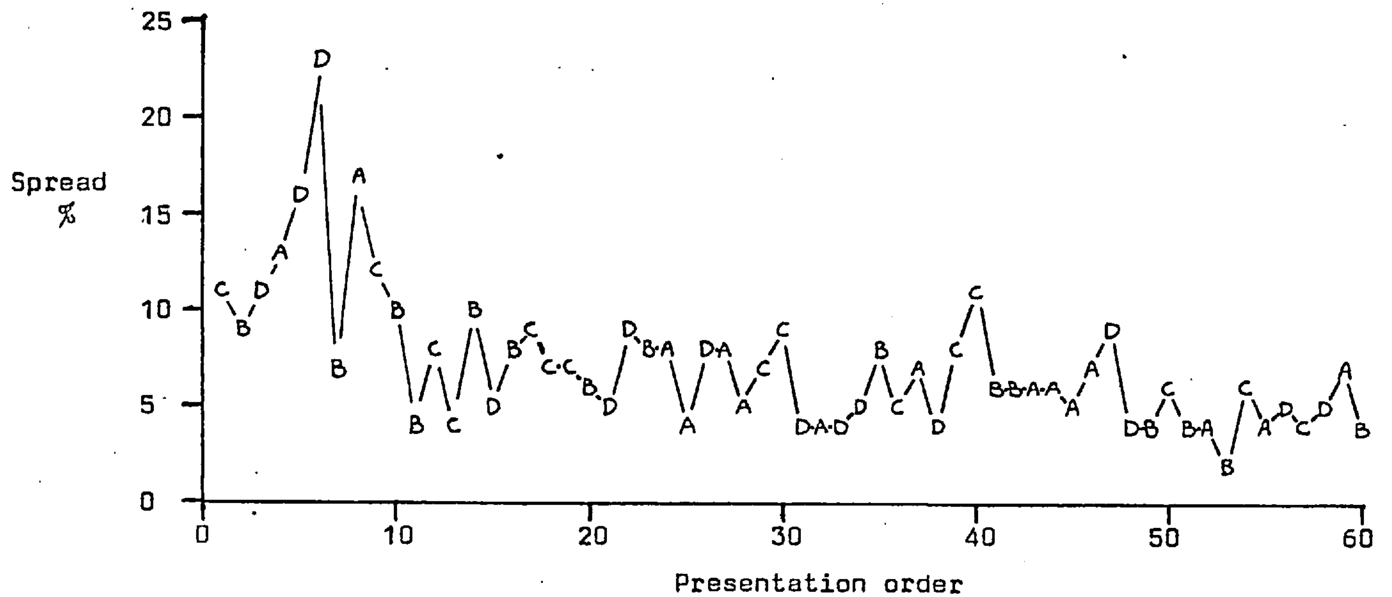


Figure 5/1

This figure shows the calculated spread in the scores as a function of the order of presentation of items in the listening test. The spread is expressed as the percentage of the average score given for each item. (See text for details.)

Figures 5/2 - 5 are plots of the values of the average 'c' above. The bars show the spread scores for the renderings, centred on the values of (a+b)/2, (which values are not always identical to the values for 'c'), indicating how the most extreme scores would effect the averages, and also indicating the amount of spread associated with scores for each utterance. It is interesting to note that for one sentence, (Fig. 5/3), the spreads are low, the scores change little if at all over the 300 days involved, and the unrehearsed utterances did not receive lower scores than the rehearsed utterances. The other three sentences show improvement, though it is not marked, and generally the unrehearsed utterances were rated below the rehearsed utterances.

Figure 5/6 shows the data for utterances pooled and plotted as a function of time, (unrehearsed and rehearsed renderings have their data plotted separately). A definite improvement is indicated and, as would be expected the rehearsed renderings, performed for the recorder, show less improvement and are generally rated higher than the unrehearsed renderings. (Fig. 5/3 shows the data for the sentence for which this is not true).

Without any detailed analysis of the nature of the improvement it is of course not attributable to the work with the aid. It is possible that such improvement would have come about anyway, simply because Lydia was talking to a critical listener for two hours a week. I offer here my suggestion that this is likely to be partly true, but from the experience I have had over the last ten months I would say that without the aid I would have been a very ineffective teacher. I would not have recognised some problems that the aid helped me to recognise and I would not have been able to give very effective instruction for correction of errors, especially those dependent on fine differences in timing and the existence of short silences.

These figures show the scores given to the four sentences listed below, (for details see text).

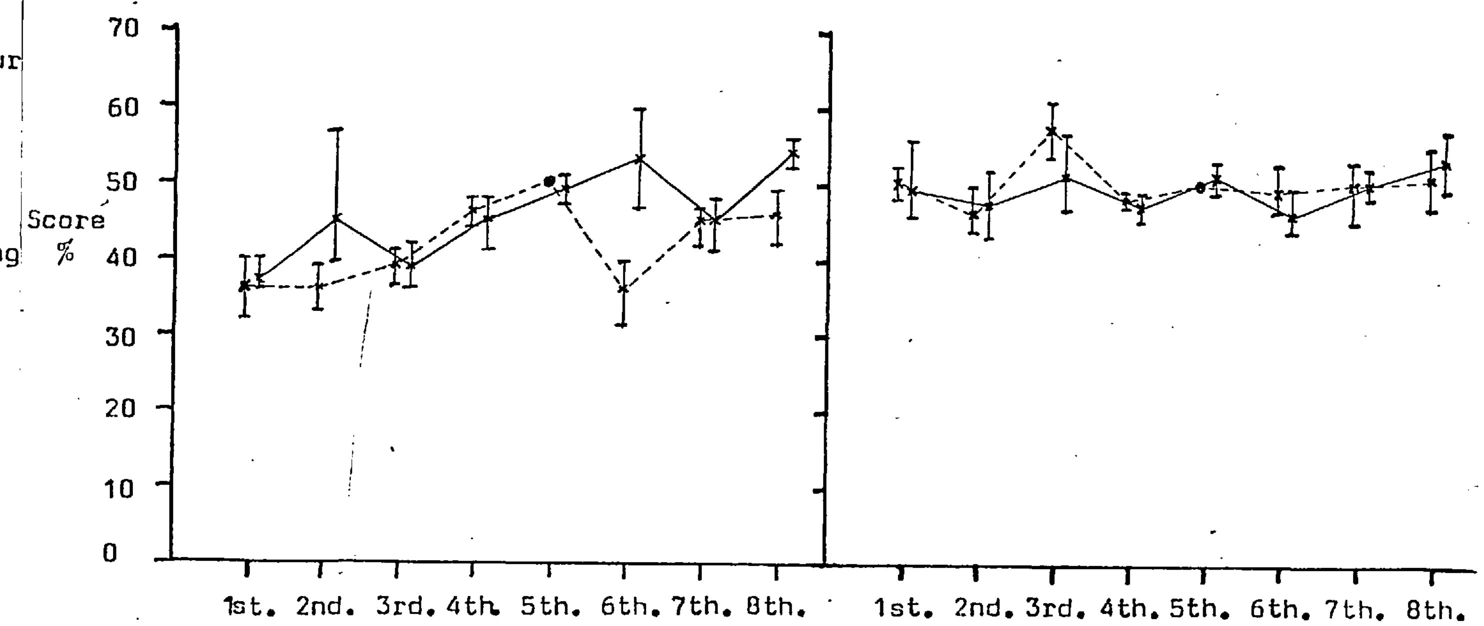
Fig. 5/2. You will tell them probably by seeing a few lousy rags hanging on bushes near; but they are surprisingly well hidden as a rule.

Fig. 5/3. Once inside; more inadequate bits of rag; and tins for cooking, and the remains of the last fire; a shapeless candle-end on a bit of slate, a crust or two perhaps; and a smell of mice.

Fig. 5/4 I felt my way in with both hands, for the night was black enough outside.

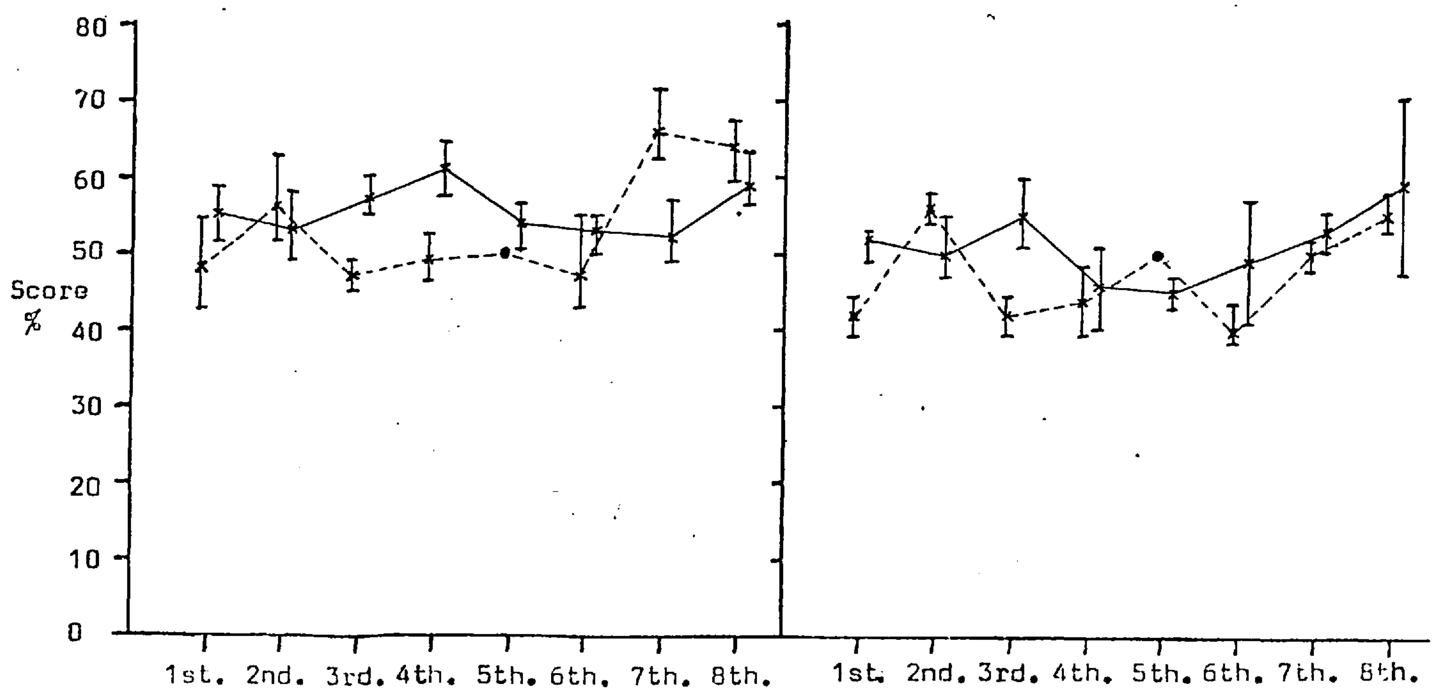
Fig. 5/5 The wind in the trees above roared; and every few moments as they swayed the branches unburdened themselves of rain like a wave breaking.

Figure 5/2 Figure 5/3



The abscissae show the recording sessions from which the test material was extracted. The dashed curves are for unrehearsed material, the solid curves are for rehearsed material.

Figure 5/4 Figure 5/5



159, 160, 161, 162.

Figs 5/2, 5/3, 5/4, 5/5.

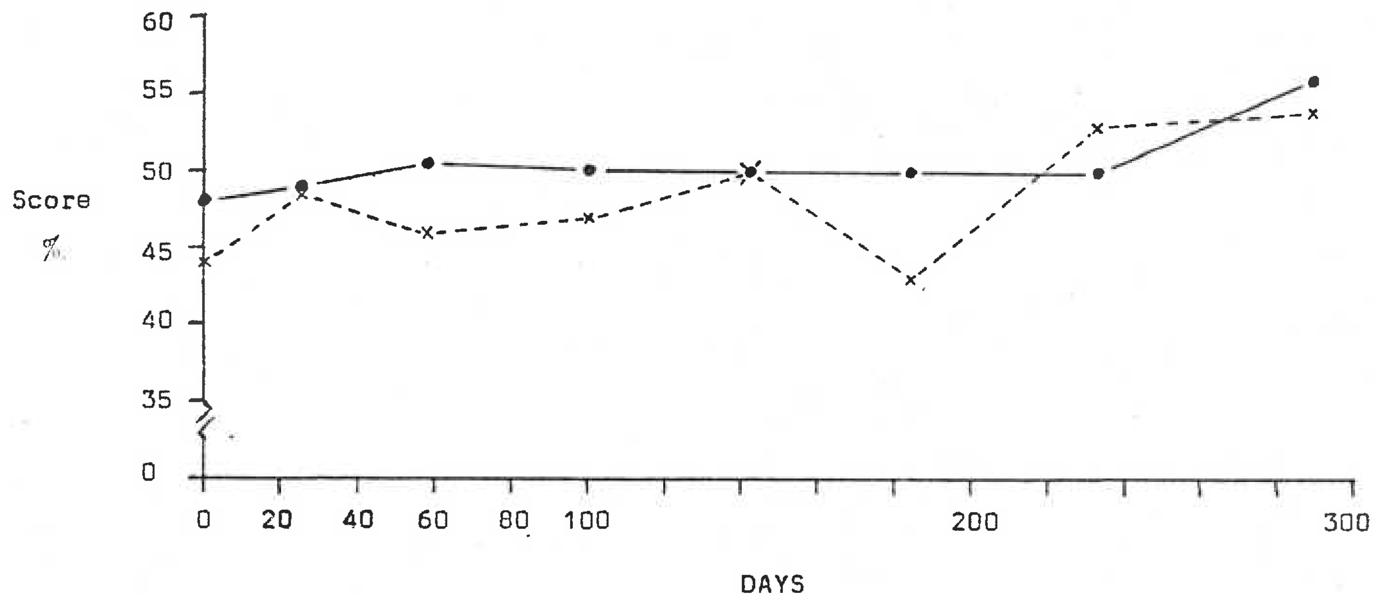


Figure 5/6

This figure shows the scores for the four sentences pooled. The dashed line is for unrehearsed material, the solid line is for rehearsed material. The dates of the recording sessions are shown as the number of days after 11th. September '72.

5 a) 3).

In this subsection the analysis lacking in the previous subsection is provided. I present here the speech therapist's evaluation of the nature of the improvement.

The speech therapist knows Lydia well and is very interested in my work and keen to see something come of it. She based her assessment on tape-recorded material and personal contact with Lydia, travelling down from Edinburgh for this purpose.

Whilst I could not avoid discussion of the aid entirely, (before the speech therapist saw Lydia for the post-training assessment), I was careful not to inflict on her all my thoughts about how I thought Lydia had or had not improved.

The speech therapist's evaluation, and her covering letter to me, form an inclusion in the thesis at the end of this subsection.

I agree with much that the speech therapist says, and as a general conclusion I consider that the aid appears promising in the training situation. However, there are some specific comments that should be made.

1) Lydia is an unusual person and it is impossible to say how any other person would have fared over the same 10 months.

2) Some improvement will surely have taken place simply because I was trying to help her speak more clearly. For example, improvements such as 'better balance of nasal resonance, phonation and pitch level' I could not claim to be the result of work with the aid. They probably result from increased self-confidence and seasonal changes, (winter - summer).

3) There was one factor I tried to improve but did not believe I had. The balance between voiced and unvoiced intensities I had recognised as a problem but did not believe I could permanently remedy, (I viewed the efforts I made to ensure that the processor had satisfactory signals fed to it as being unlikely to last; Lydia has problems with breathing for speech which I have tried to correct without much success, (see also Lenneberg, 1967)). However I believed that I had significantly helped Lydia to produce good '<u>j</u>', '<u>g</u>', '<u>ch</u>', '<u>sh</u>', but there has been apparently no real improvement.

4) I did not expect overall sentence performance to suffer, although I was aware that it might.

5) I would not claim as much for the aid as she is prepared to grant it, however, it is encouraging to see her concluding comment.

Assessment is a tremendously difficult problem, (see Chapter 6), but I believe that between my own opinions and those of the therapist there are sufficient points of agreement for the potential of the machine to be recognised. My conclusions are presented fully in Chapter 6 but there is one point which deserves comment here.

The speech therapist pointed out that Lydia is a very sensitive person, and I too have indicated that she probably cannot be taken as representative of the deaf world. In my opinion it is best to optimise the conditions under which any feasibility study is taken so as to learn as much about the problems, the shortfalls in design and the potential of the equipment before going on to development work. I have been very lucky to work with Lydia and her speech therapist. There remains the problem of generality.

Just as with the psychophysial experiments, where the special situation led to no difference being found between the two stimuli, (Chapter 3), there exists the possibility that with other people, of different ages and temperaments, the aid will be of little use; that is, that the transfer from the particular case to the more general will not prove valid. In favour of the transfer I would offer that young children should be used as subjects as they are better equipped physiologically to learn a language and the means for producing it. In addition, the development version of the equipment will, if it is made, incorporate improved circuitry which should provide for better vowel discrimination, pitch indication and voicing indication. These improvements, (see the next section), are suggested by the present work and will go a long way toward optimising the form of the equipment.

I suggest that the process I have outlined, optimum feasibility study conditions followed by trials with a development version of the aid, forming an optimum route for optimisation, is the only sensible way of developing any aid, (see Chapter 6). (Production versions would be developed from the development version in the same way).

Inclusion

The speech therapist's assessment and the accompanying covering letter.

Lydia - Comparison of speech performance on 29/8/73, samples from dates in 1972.

Improvement is most obvious in controlled speech situations, (i.e. reading, word lists). Not yet much carry-over into conversation except a slight overall clarity created by better balance of nasal resonance, phonation and pitch level.

Comparison of passage read on October 4th. 1972 and August 29th. 1973.

Final syllables and end consonants are more securely established.

Nasality - much reduced, giving clearer performance, (less muffled quality).

Phonation - better balance of vocal cord movement. Less exaggerated voicing.

Articulation - better, and more consistent performance.

Rhythm and intonation - good rhythm in individual words, (i.e. syllable patterns), but definite loss of sentence inflection and sentence rhythm-words separated too much. Presumably this is due to concentration on other elements.

Comparison of word lists from recordings for the same dates as above.

j, sh, ch - not really much improvement in quality but certainly being used more consistently.

r - more fully established especially in the middle of words.

l - much the same, still lacks distinction.

Vowels are still often distorted and would be worth practising.

Conversation.

Much as before, (see comments already made).

Suggestions for future practice.

1) Flow and intonation in connected material (sentences). While basic clarity and intelligibility have now been achieved, the next very important step is to maintain these in a more natural pattern. Lydia can get good intonation and this gives valuable added meaning to her speech.

Inclusion Contd.

2) Continued work on 'l' - perhaps encouraging a firmer closure of tongue tip on palate, and encouraging her to throw air forward onto the sides of the tongue.

3) Vowels. Differentiation of i/e, ay/ia, etc.

4) Encourage her to use full sentences in conversation, not telegrammatic ones with the small words missed out, and also to remember to finish her words, (e.g. final 's' to denote plurality).

General comments.

It would appear that the greatest benefits from the 'vibrators' have been to give

a) a better feel of how much voicing is required, and consequent reduction of forcing,

b) a sense of the syllable content and stress of individual words, especially weak end syllables,

c) sound practice. The most noticeable function has been in the establishing of certain sounds. The tactile stimulation probably produces a more lasting memory of what is required. Many sounds Lydia could already produce with concentration but not maintain. An actual improvement in an individual sound quality is not so apparent.

As the above points are all elements which are extremely difficult to teach in conventional speech teaching of the deaf, it would seem that this machine has very great possibilities.

* * * *

Dear William,

I enclose my comments on Lydia's speech. I hope you don't mind my having put in suggestions for further practice, but I feel that one's first aim - clarity must be backed up by aiming for normal intonation patterns and natural structure and quality. Lydia really has improved in intelligibility and I am sure she can be pushed even further. I think you will find that most teachers of the deaf and speech therapists feel very strongly that words should not be too isolated from the context of the surrounding sentence, so that for the sake of 'selling' your invention I think its probably important to prove that it can help a deaf person to acquire rhythm etc. as well. Does this make sense?

Inclusion Contd.

Are you intending soon to try out other deaf people on the machine? It would be interesting to find out how much basic intelligence and sensitivity are required to do as well as Lydia. I wish I was in a position to be able to work with it myself.

The best of luck for the next stage, and please don't forget to let me know how things progress.

With best wishes,

Alison.

5 b).

In subsection 5 b) 1). I briefly compare three previous aids with the prototype presented in this thesis. This serves to set the aid in an 'external' context.

Subsection 5 b) 2). contains my technical assessment of the prototype aid. This consists of unfavourable as well as favourable comments on the realisation and, where necessary, is extended to cover the effects on the usefulness of the aid.

5 b) 1).

Three tactile aids have been constructed which comply with my formulation of the problem facing a speech training aid designer. The formulation I used in subsection 4 a) 1). was as follows:- 'How can a profoundly deaf person be presented with as much information as possible about the movements of the mouth and vocal organs, as they take place in speech?'

I call any such aid a 'complete speech' aid, because the deaf person is presented with as much information about speech as he or she can use.

Tactile aids have been developed for 'pitch' correction, (Willemain & Lee, 1972), for 's' indication, (Kringlebotn, 1968), and as lipreading aids, indicating just larynx rhythm, (voicing/frication being distinguished in a binary manner and without any attempt at qualifying the nature of either), (Martony & Spens, 1972) and, (although in this case nasality was also indicated, Makkaveyev, 1967 & 1969). All of these have been quite successful in limited trials. In addition there is a fair number of visual aids, some of them 'complete speech' aids usually based on a conventional spectral analysis. None of these aids will be discussed here. A brief but reasonably up-to-date survey is available, (Pickett, 1971), and a more thorough but older survey forms the volume from which comes the Kringlebotn, (1968), reference, (see also Potter, Kopp & Kopp, 1966).

* * * *

The first of the three aids, (chronologically), is that described by Guelke & Huyssen, (1959). This device consisted of 8 sets of steel reeds arranged to stimulate the fingers of one hand. The reeds oscillated at frequencies between 100 and 400 c.p.s., (each set of reeds consisted of 20 reeds each resonating at a slightly different frequency in the 100 - 400 c.p.s. band). The principle of operation was as follows:-

The frequency spectrum was divided into 8 adjacent bands approximately 300 c.p.s. wide. A local oscillator signal, (at a different frequency for each band), was mixed with the signal from a band, and 8 signals with frequencies of 100 - 400 c.p.s. were produced, each signal corresponding to a part of the original spectrum. (For example, a local oscillator at 620 c.p.s. was used to 'beat-down' frequencies of 720 - 1020 c.p.s. yielding frequencies of 100 - 400 c.p.s.).

The mechanical reeds each responded to a slightly different frequency and were arranged to stimulate along a line down a finger, thus coding frequency spatially. For example, along the proximal phalanx of the first finger the frequencies 720 - 1020 c.p.s. were coded by position, (the vibrations being between 100 & 400 c.p.s.), 720 c.p.s. being proximal and 1020 c.p.s. being distal. Thus with their aid Guelke & Huyssen arranged for flowing sensations up and down the fingers, the positions at which stimulation was felt corresponding to the frequencies with high energy. The range covered by this machine was low, (it is possibly a poor contender for the title 'complete speech' aid). The frequencies covered were 410 - 2880 c.p.s.

One deaf person, (profoundly deaf), was used in experiments in which lip-reading was excluded. The authors found that vowel identification was good but that consonant identification was poor. (The material used for the experiments was poorly explained but appears to have been isolated vowels and monosyllables).

In their conclusions the authors recognised the short-comings of their device and suggested that it had potential for correcting vowel articulation and as a lip-reading aid. They noted the problems caused by the restricted frequency range of the apparatus and the large dynamic range of speech. Also mentioned was the opinion that tactile aids are more suitable than visual ones because they liberate vision for lip-reading and because touch is more similar to hearing than is vision.

The second complete speech aid is that reported by Pickett, (1963). This device had ten channels and covered the frequency range 210 - 7700 c.p.s., (with the bands not being equally spaced). The energy in each band, (measured by means of a rectifier and smoothing circuit), controlled the output intensity of a 300 c.p.s. oscillator which drove a bone conduction transducer, (i.e. the type of transducer commonly employed in bone conduction hearing aids). The ten transducers, (each one controlled by the energy in the corresponding filter), were arranged to stimulate all ten digits, the left hand little finger corresponding to the filter tuned to 210 c.p.s. and the right hand finger corresponding to the 7700 c.p.s. filter. The frequencies above 1000 c.p.s. were 'pre-emphasised' before the signal passed to the filter bank.

Hearing and deaf subjects were used, (the latter being taken from classes at a State Deaf School, (in Sweden), with strongly oralist teaching methods). In general the aid appeared useful. Sometimes tactile discrimination of letters or words was better than that achieved with lip-reading, (also alone). On other occasions tactile discrimination was poorer and sometimes there appeared to be no difference. With lip-reading and the vibrotactile aid together there appeared to be no deleterious effects, in fact the tactile aid supplemented the visual reception of speech. (All this was achieved with a comparatively very short training period compared to a life-time's oral training).

Pickett in his discussion of the aid, raised some interesting points. Firstly, an aid such as that he described can be used either to supplement, (i.e. add redundancy to), an already existing speech channel or to complement it, (i.e. extend the range of speech gestures that can be successfully communicated). This assessment, (of interest in its own right), reveals a presumption that the aid will not be used for speech training but only for speech reception. This, as I mentioned in the previous chapter, is the most ambitious (and perhaps least realistic) requirement any communication aid will have to meet. (In order of increasing 'sophistication' are aids for speech training, lip-reading and speech reception without sight).

His second point concerns the "form of the best design for a tactual speech transmitter". He goes on:-

> "The most important task is the development of a tactual speech code having a high compatability with reception characteristics of the skin."

However, he considers that the skin is probably not a very suitable medium, (although he recognises the advantages of releasing the eyes for other work). He goes on to suggest that fewer vibrators might well be employed, (and perhaps not stimulating the finger-tips), because the sensations were not very distinct - ".....it appeared that only one diffuse peak of a momentary pattern could easily be discerned".

He comments finally on the first point again and says that an aid which was flexible in function would be of use, but he does not mention acquisition of articulatory skills as suitable in this context.

The third aid reported is that discussed by Kringlebctn,(op. cit.). (This is a different apparatus to that previously mentioned and it is the major topic of the article cited). This aid is called Tactus, (it is not known whether work with this aid has been discontinued, work with the aids described above has apparently not been continued).

Tactus has five channels and delivers signals, to the finger-tips of one hand, via bone conduction transducers. The signals that drive the transducers are derived from speech by frequency division of the speech signal. The signal from a microphone, via an amplifier with optional automatic gain control, is passed to a trigger circuit which detects peaks above background noise level. The peaks are used to drive one vibrator directly, and after various divisions, the others, (input frequency divided by 2, 4, 8, 16). Thus five frequency bands are effectively converted to frequencies to which the fingers are sensitive. (The response of the transducers is sufficient to provide some sort of 'filtering').

The signals to the transducers are of constant amplitude, (and are square-wave in nature), but have varying frequencies. This aid therefore relies on tactile frequency discrimination. (This is to be compared with the other two systems described where, respectively, spatial discrimination and amplitude discrimination are relied upon).

Using words, (a small set), in an identification test, Kringlebotn obtained promising results. (His subjects were deaf children), he concludes:-

> "Although vibrotactile understanding of connected speech is probably impossible with Tactus, at least at the present stage of development, some useful applications of the apparatus seem to be indicated:-
>
> 1) As a supplement to lip-reading under teaching conditions.
> 2) As an aid for the learning of lip-reading.
> 3) As an aid in speech training and correction.
> 4) As a rhythm indicator".

Kringlebotn's concluding remarks indicate that he, too rates speech reception as more important than articulation training.

* * * *

The three aids described above were clearly conceived as reception elements in the speech communication channel, a set of misconceptions in my view. However, the authors all indicate that vibrotactile aids do offer the deaf some hope; and the diversity of schemes employed suggests a search for what Pickett called ".....a tactual speech code having high compatibility with reception characteristics of the skin".

The approach I have followed, in contradistinction, is to first assess the finger's tactile sensitivity, from a natural point of view, in order to discover what might be done to speech signals to enable speech to be 'matched' to tactual requirements. Such 'matching' I attempt by reducing the speech signal to a set of vocoder-like signals, (i.e. slowly varying signals conveying information about the spectral composition of speech), which do not contain any information about the overall intensity of the speech. This overcomes the attendant problems when automatic gain control is attempted.

My approach is different conceptually in that I consider caution to be prudent, and therefore I view my work as fitting into the teaching situation as a speech training aid primarily (speech reception being untried). This caution has the advantage of preventing an inadequate aid from being tried as an aid for reception, thus causing disappointment. Optimising the aid as an articulation training aid will automatically produce an apparatus optimised for speech reception, (reception and production are interdependent and inseparable functions of speech).

It should be noted in passing that the aid of Guelke & Huyssen relied on spatial discrimination sensitivity, aided by the fact the different sites were stimulated at different frequencies. Whilst there is a variety of reports in the literature on different aspects of tactile spatial acuity, (see for example Carmon,(1968), Sherrick,(1964), Foley & Dewis,(1961)), it is not good enough to substitute knowledge about tactual spatial abilities for lack of knowledge about suitable stimulation; whatever tactual scheme is devised there will be a need for the right sort of stimulation, (as pointed out by Pickett, above).

All the four aids capable of conveying information about complete speech (with the possible exception of Tactus), are too bulky to be portable, at this stage, and so apart from telephonic communication, (a capability which deaf people would greatly value), they have a place almost exclusively in the class-room. In these circumstances it seems a little strange that evaluation of any such aid should ever have been made in terms of its usefulness for speech reception.

5 b) 2).

For this technical assessment the aid is considered in three sections, as in Chapter 4.

i) The microphone-processor link.

ii) The processor.

iii) The processor-vibrators link.

i) The microphone-processor link.

In this part of the apparatus the main weight of my criticism, both favourable and unfavourable, falls on the 'ear's response' filter.

The filter is poorly realised and if a greater amount of signal gain were to be used, (i.e. a greater dynamic range), noise, (electronic), would be a problem. In any development this section of the equipment would have to be redesigned. The use of L-C tuned circuits has one potential advantage, and this is that such filters can have an intensity dependent frequency response, as does the ear. (See Fig. 4/1. This was noted, not explored in detail). However, the use of R-C filters based on operational amplifiers would give a much better all-round performance than the L-C filters and although a different way of reproducing the effect of increased intensity would have to be found this would be preferable, in my opinion, to trying to develop L-C circuits, (because R-C circuits are easier to design and experiment with).

The value of the 'ear's response' built into the prototype has not so far been discussed. Informal evaluation of this option was carried out simply by switching the filter in or out at some point during training sessions. I concluded that with the 'fuller' response, (see Fig. 4/1), the aid was more useful than without it, and also that the sensations were more lively with either than with the 'ear's response' switched out. It was noticeable, for example, that those vowel patterns, ('ee', 'oo'), that were distinctive were more so with the filter switched in. The fricatives were only slightly more noticeable without the filter.

The inclusion of such a filter in the apparatus is a novel step, (most workers, (c.f. Pickett), pre-emphasise the higher frequencies to compensate for low energies in frication). An improved copy of the 'ear's response' and the use of computer simulation and real filters, (connected via an analogue-to-digital converter), would make possible a more formal investigation of the suitability of using any such filter, (see ii).)

The amplitude measuring circuit could be redesigned using compression (without feedback), before rectification and decompression after it. This could be done with amplifiers having logarithmic and antilogarithmic responses. The effect would be to increase the dynamic range. This is at present limited theoretically, not practically, by the rectification system used, (i.e. a different system is required as the present one cannot further be improved). Some development work would be needed on the means for controlling the 'intensity' vibrator to enable the small, important, amplitude changes, (such as vowel energy changes), to be separated from the gross, (i.e. shout/voiced-whisper), changes.

ii) The processor.

The high pass filters employed, the core of the processor, appear to be a useful idea, but once again the realisation is poor.

In particular the curves for any two filters become parallel and then asymptote at some low level, at frequencies well below the cut-off points, (see Fig 5/9 - a copy of Fig. 4/3). Figures 4/4a - 4e and 4/5 (included here as Figs 5/7a-7e & 5/8) illustrate the variety of interrelated effects caused by the shapes of the filter responses and the behaviour of the ratio measuring circuitry. (The figures are included in this chapter for convenience).

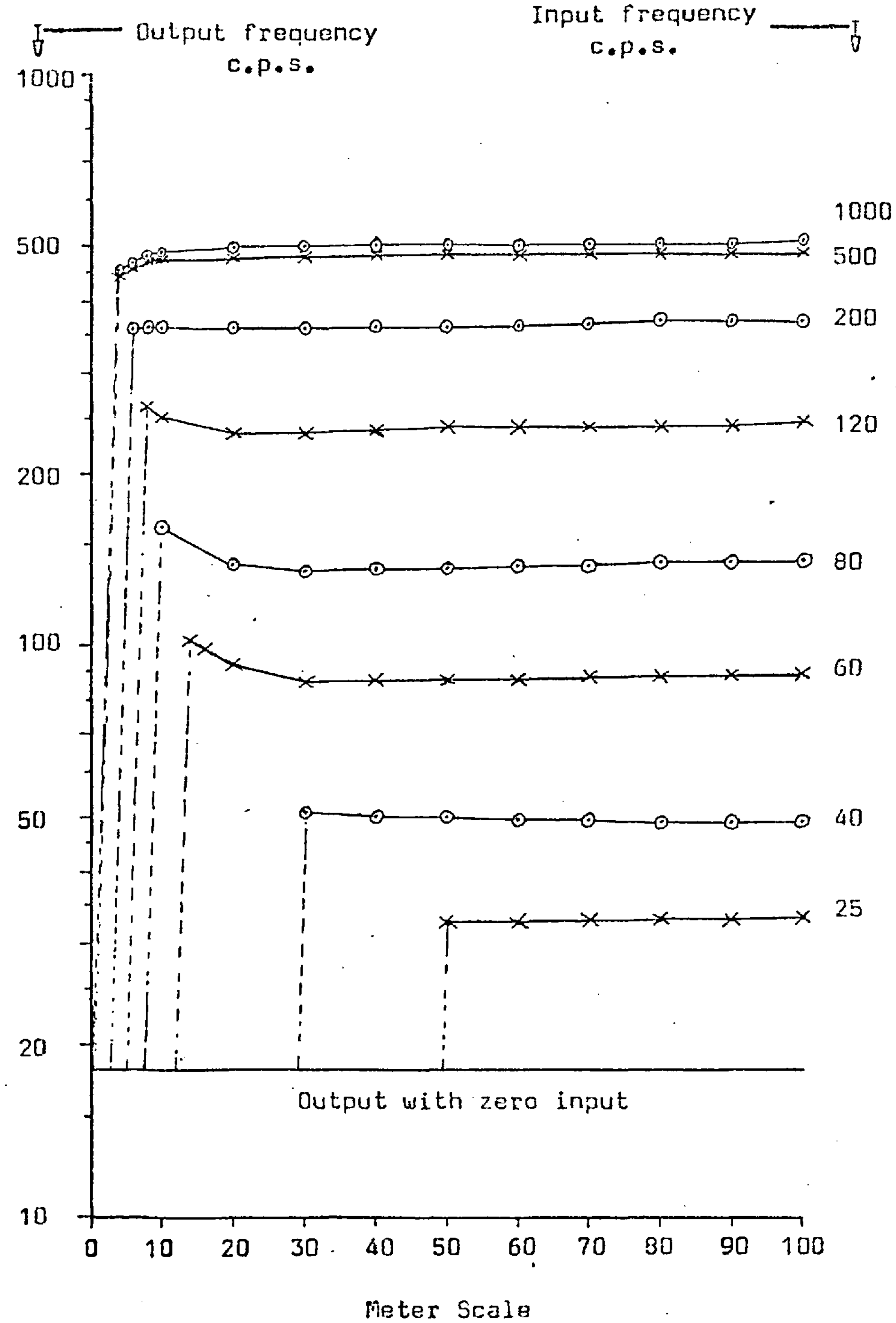
Firstly, as comparison of Fig. 5/9 (i.e. Fig 4/3) with Fig. 5/8 (i.e. Fig 4/5) shows, the ratios are derived almost exclusively from those regions of the filters' outputs where adjacent pairs of filters have non-parallel response curves. This is necessary to prevent the ratio signals remaining constant in value as the input signal changes its frequency, (a single frequency signal was used). My intention that the curves should not become parallel within the dynamic range of interest was not fully realised. If a ratio were to be derived over a greater range of the filters' response curves, (i.e. including a section where the responses of two adjacent filters become parallel), then as the frequency of the input signal changed the ratio would not, because the two signals from which the ratio is derived would decrease or increase by the same amount. Compare in Fig. 5/9 (i.e. Fig 4/3) curve 'a' with curve 'b' and with curve 'c', and then in Fig. 5/8 (i.e. Fig 4/5) curve 'b' with curve 'c'.

In addition the poor dynamic range of the filters means that at low intensities the filter responses do not provide the same useful range, (for the ratio derivation), as at high intensities. The effect of this is that 'amplitude suppression' is not as effective as it should be. Compare 'c' and 'e' in Fig. 5/9 (i.e. Fig 4/3) where the low intensity performance for 'c' is poor, with Figs 5/7c and 7e (i.e. 4/4c & 4e). The low intensity effect at low frequencies seen in Fig. 5/7a (i.e. Fig 4/4a) is probably due to poor adjustment of the voice controlled switch; this switch is employed to prevent the effects of poor amplitude suppression being passed on to the vibrators as well as to prevent the vibrators from delivering a high output in response to no input.

Figs. 5/7a – 7e
copies of
(Figures 4/4a – 4e)

The graphs show the outputs of the oscillators used to drive the vibrators. The ordinate scale is frequency in cycles per second. The input used was a sinusoidal signal at the frequencies indicated.

The frequencies of the outputs are shown for the frequency varying system. The amplitudes of the signals driving the vibrators can be calculated as follows:- Output frequency/250 = volts pk-to-pk. For the frequency constant system the output level is calculated by means of the expression:- 'Output frequency'/500 = volts pk-to-pk., where the 'output frequency' is read off the ordinate scales.



(Constant frequency, if used, 315 c.p.s.)

Fig. 5/7a
(Figure 4/4a)

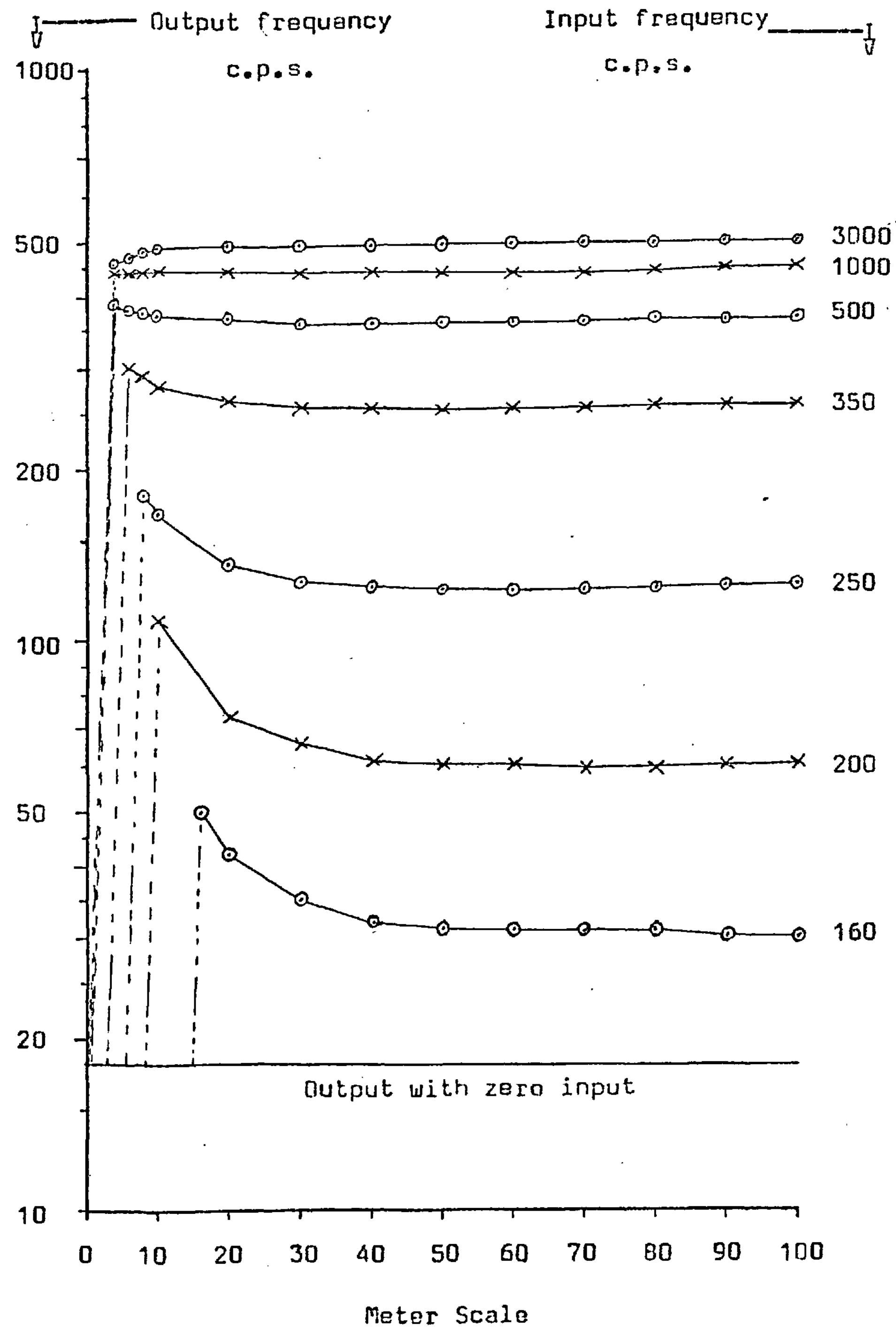
Fig 5/7a
copy of
(Fig 4/4a)

Figs. 5/7a - 7e.

(copies of Figures 4/4a - 4e)

The graphs show the outputs of the oscillators used to drive the vibrators. The ordinate scale is frequency in cycles per second. The input used was a sinusoidal signal at the frequencies indicated.

The frequencies of the outputs are shown for the frequency varying system. The amplitudes of the signals driving the vibrators can be calculated as follows:- Output frequency/250 = volts pk-to-pk. For the frequency constant system the output level is calculated by means of the expression:- 'Output frequency'/500 = volts pk-to-pk., where the 'output frequency' is read off the ordinate scales.



(Constant frequency, if used, 350 c.p.s.)

Fig 5/7b
(Figure 4/4b)

Fig 5/7b

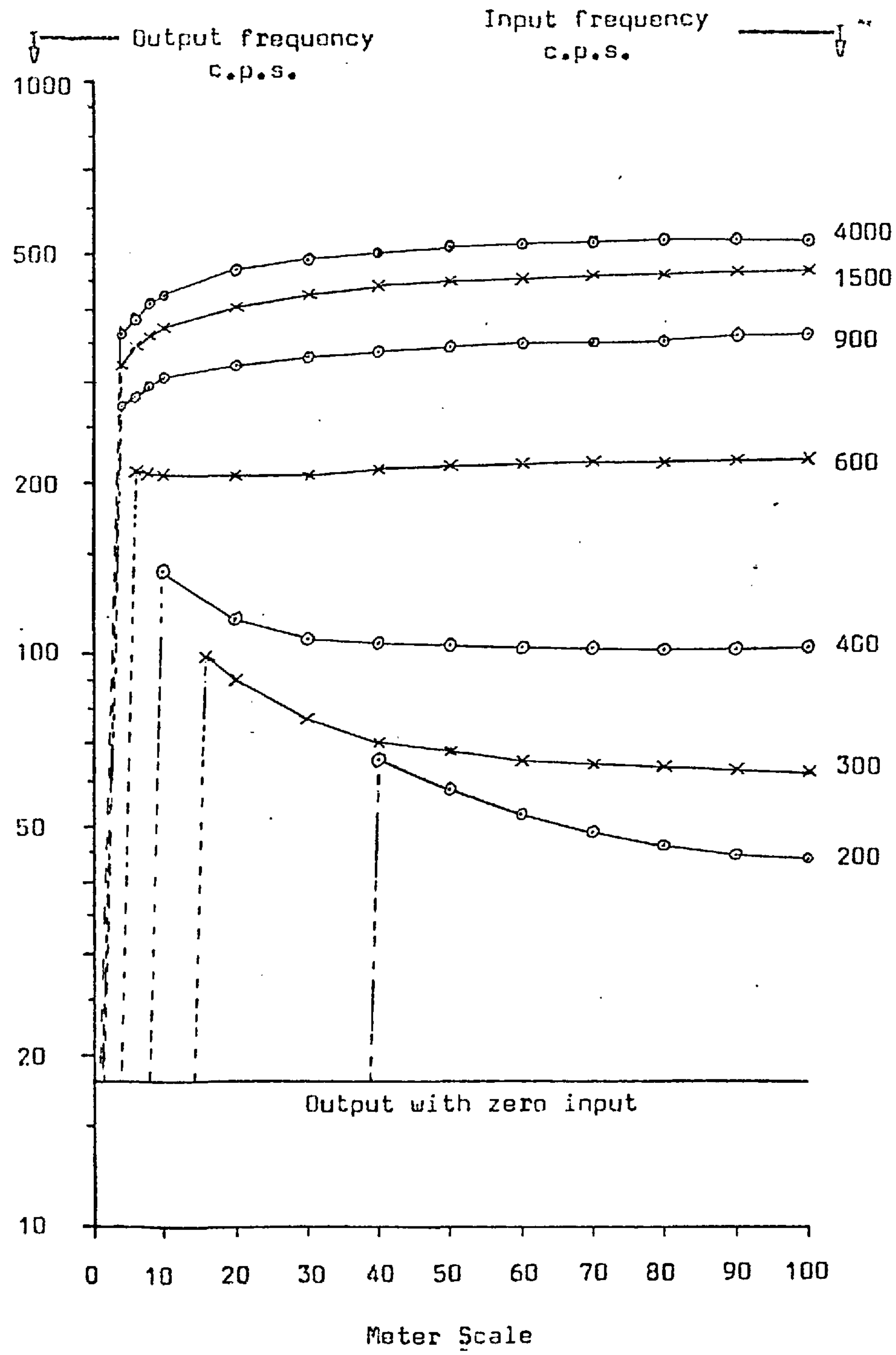
Copy of
(Fig 4/4b)

Figs. 5/7a – 7c.

(Copies of
Figures 4/4a – 4e)

The graphs show the outputs of the oscillators used to drive the vibrators. The ordinate scale is frequency in cycles per second. The input used was a sinusoidal signal at the frequencies indicated.

The frequencies of the outputs are shown for the frequency varying system. The amplitudes of the signals driving the vibrators can be calculated as follows:- Output frequency/250 = volts pk-to-pk. For the frequency constant system the output level is calculated by means of the expression:- 'Output frequency'/500 = volts pk-to-pk., where the 'output frequency' is read off the ordinate scales.



(Constant frequency, if used, 400 c.p.s.)

Fig. 5/7c
(Figure 4/4c)

Fig 5/7c

Copy of

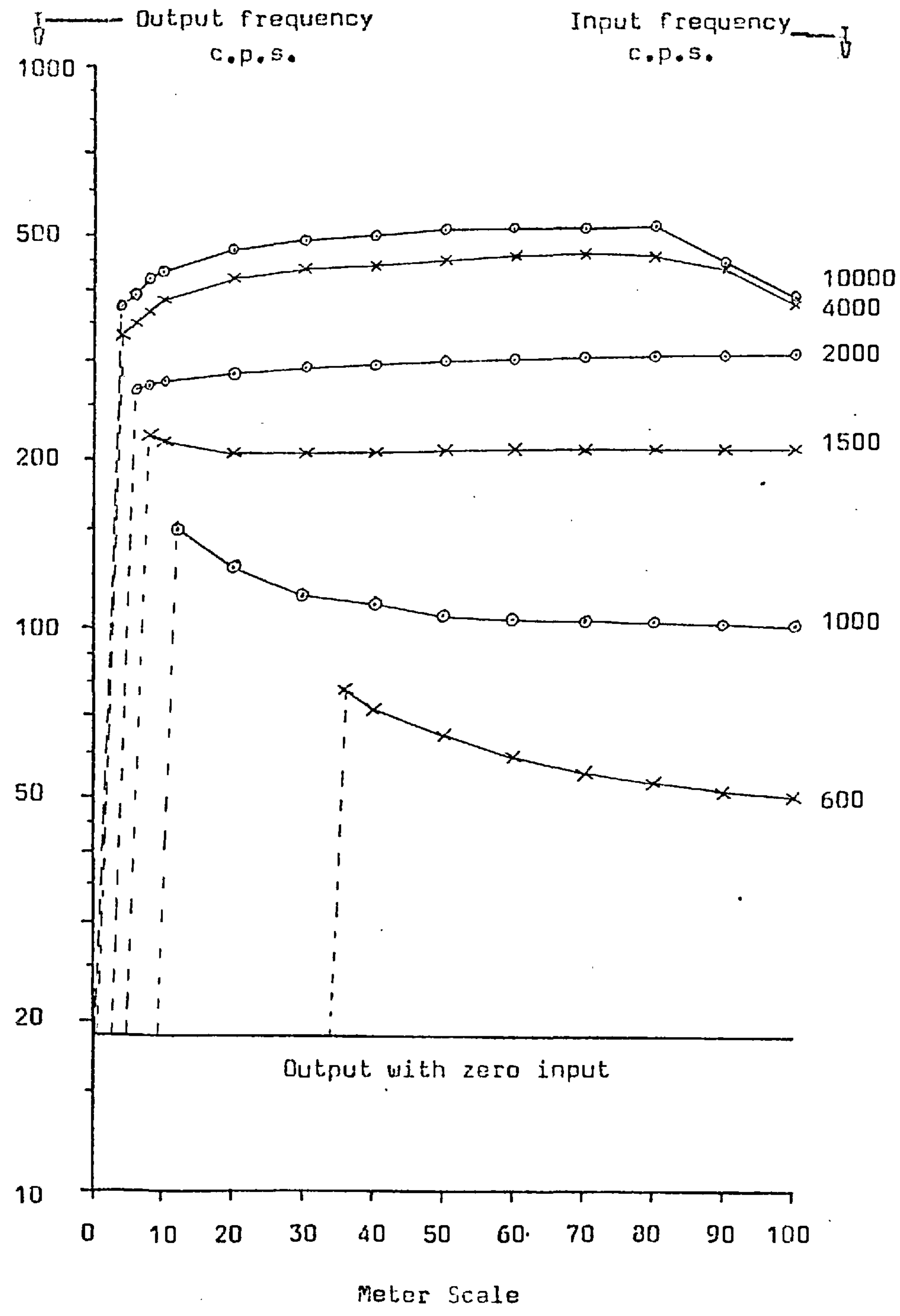
(Fig 4/4c)

Figs. 5/7a – 7e

(Copies of Figures 4/4a – 4e)

The graphs show the outputs of the oscillators used to drive the vibrators. The ordinate scale is frequency in cycles per second. The input used was a sinusoidal signal at the frequencies indicated.

The frequencies of the outputs are shown for the frequency varying system. The amplitudes of the signals driving the vibrators can be calculated as follows:- Output frequency/250 = volts pk-to-pk. For the frequency constant system the output level is calculated by means of the expression:- 'Output frequency'/500 = volts pk-to-pk., where the 'output frequency' is read off the ordinate scales.



(Constant frequency, if used, 360 c.p.s.)

Fig 5/7d

(Figure 4/4d)

Fig 5/7d.

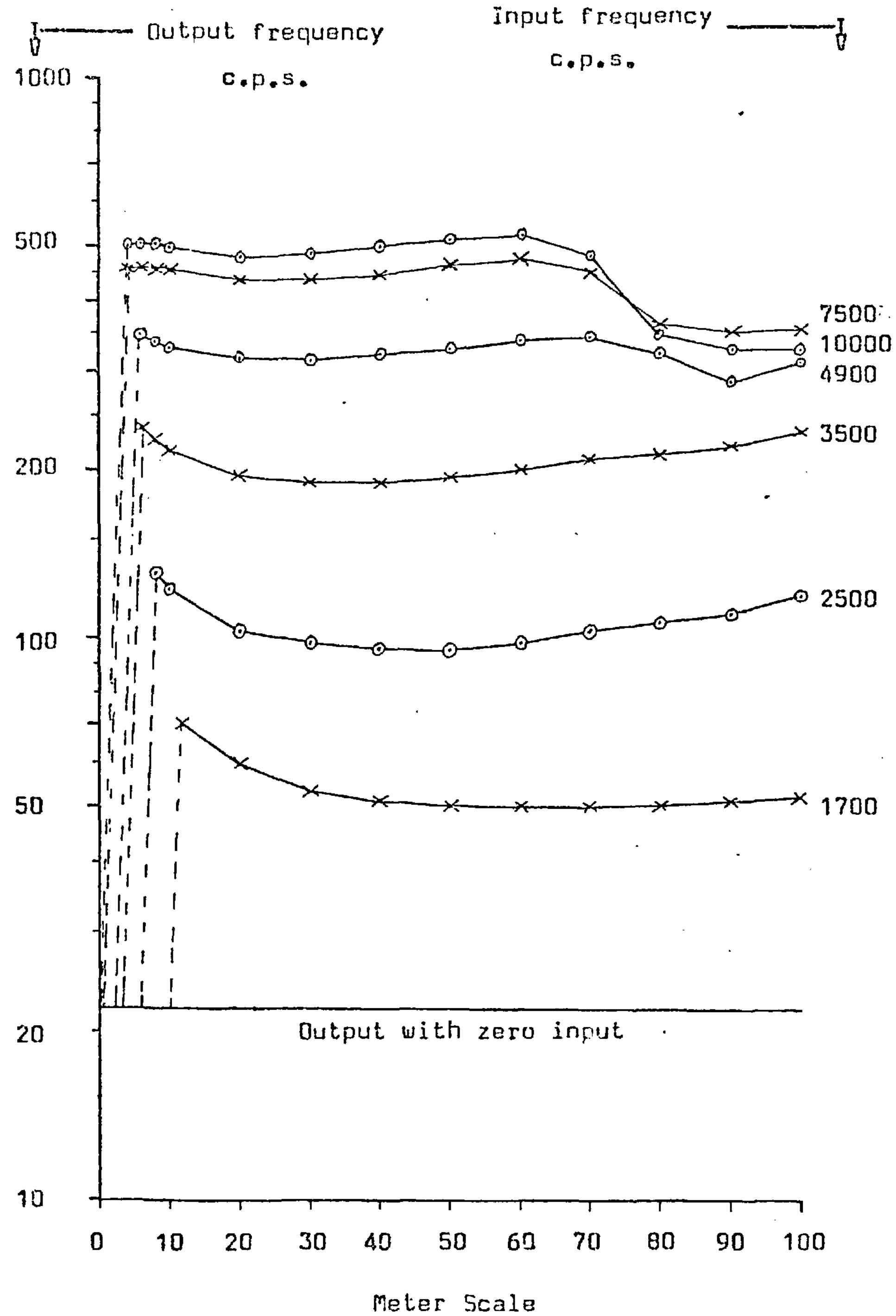
copy of
(Fig 4/4d)

Figs. 5/7a – 7e.

(Copies of Figures 4/4a – 4e)

The graphs show the outputs of the oscillators used to drive the vibrators. The ordinate scale is frequency in cycles per second. The input used was a sinusoidal signal at the frequencies indicated.

The frequencies of the outputs are shown for the frequency varying system. The amplitudes of the signals driving the vibrators can be calculated as follows:- Output frequency/250 = volts pk-to-pk. For the frequency constant system the output level is calculated by means of the expression:- 'Output frequency'/500 = volts pk-to-pk., where the 'output frequency' is read off the ordinate scales.



(Constant frequency, if used, 320 c.p.s.)

Fig. 5/7e
(figure 4/4e)

Fig 5/7e

copy of
(Fig 4/4e)

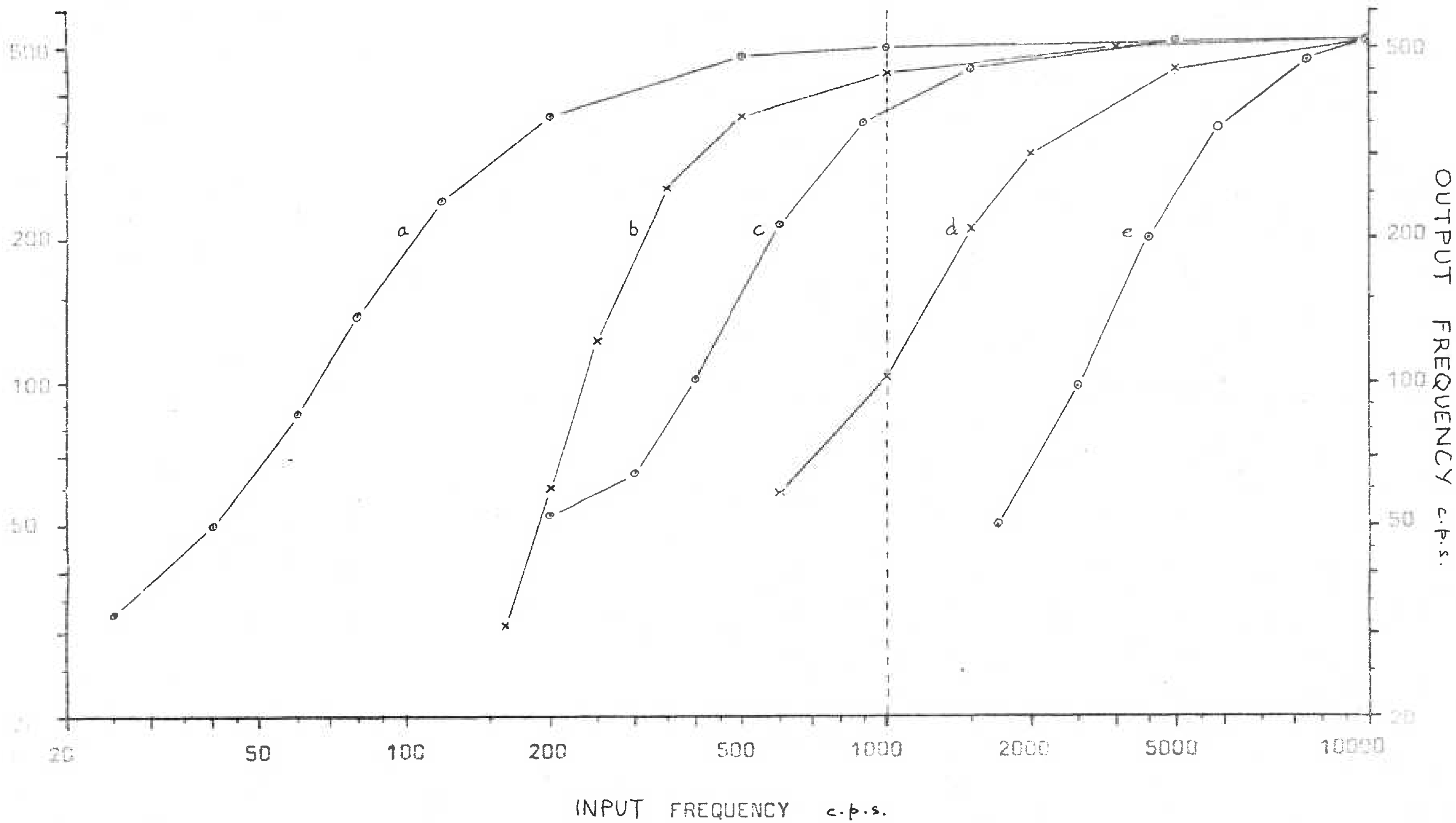


Fig. 5/8
(copy of figure 4/5)

This figure shows, as a function of the input frequency, the output frequencies of the five oscillators used to drive the transducers, 'a' – 'e'. The Input sinusoidal signal was maintained throughout at an intensity level on the panel meter of 60.

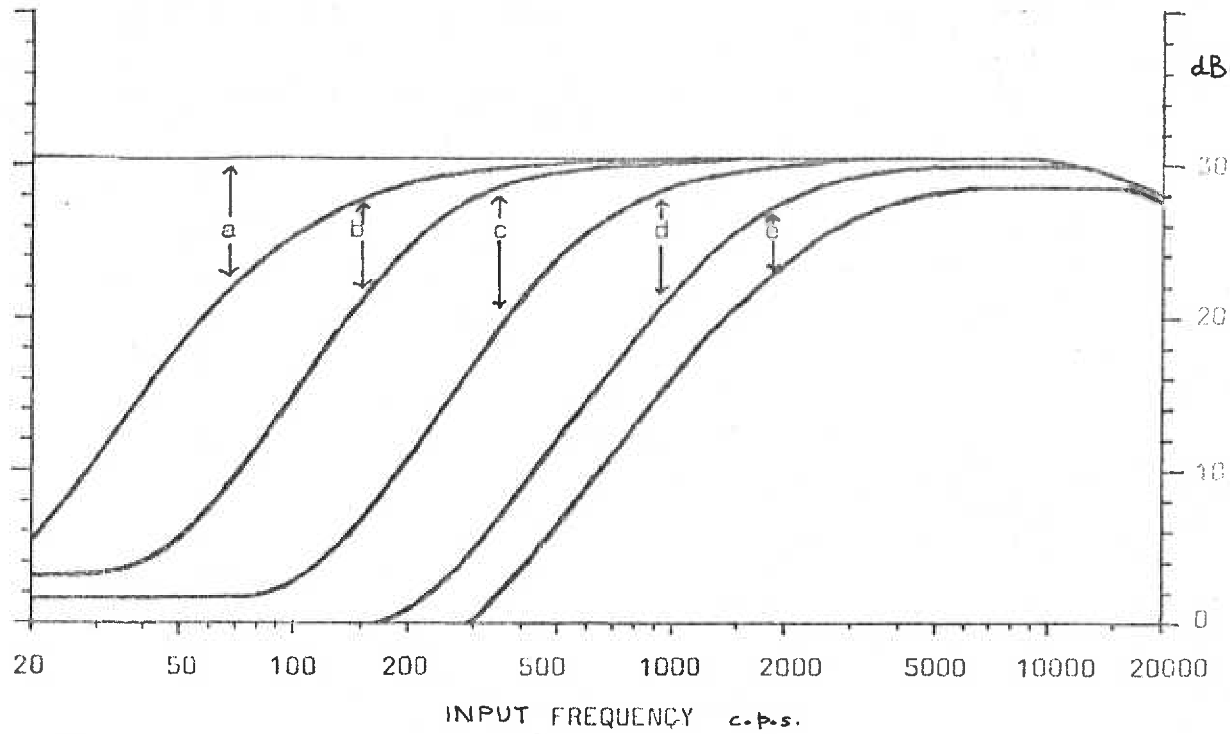


Fig 5/9

(copy of Figure 4/3)

The responses of the five filters used in the processor. Also shown are the filter sources from which the ratios are derived for controlling the stimulus oscillators 'a' — 'e'. (See also Figure 4/4a — 4e.) The ordinate scale is in dB., (the zero level is 10 millivolts). The input signal had an intensity of 80 on the panel meter.

The ratio circuits contribute their own effect which confuses the issue. At low amplitudes the frequency of the signal driving the vibrators tends towards a constant value. This adds to the inadequacies of the amplitude suppression function, but only at very low amplitudes.

Thus the three effects mentioned all blend in a complex manner to produce the curves shown in Figs 5/7a - 7e and 5/8 (ie. Figs 4/4a - 4e + 4/5). The solution, a practical one, is to greatly extend the dynamic range of the filters and to improve their shape, (the ratio measuring principle could be realised in a different manner which would help to increase the dynamic range available).

Initial adjustments must be made easier, (at the moment these are very time-consuming), and the dynamic range must be sufficient to cope with the signals provided by the improved first section of the equipment.

The positioning of the filters leaves much to be desired but was of necessity a fairly arbitrary procedure. For development of the equipment I would consider it necessary to justify the filter positions and to this end I would carry out a computer aided assessment/simulation of the equipment. This would be most easily accomplished by means of voltage controlled filters, controlled by the experimenter and coupled to the computer which could display the ratios, (as speech was supplied to the filters), variations in the ratios, and so on. The number of filters used, as well as their adjacency, could also be investigated. (The investigation would be an empirical one).

As with the amplitude measuring circuitry the energy measuring circuits coupled to the filters could have their dynamic range extended by means of compression and decompression circuits, (distortion might be a problem).

As regards pitch indication I now consider that further attempts at improving the positions of the filters so as to maximise the indications of pitch variation are unlikely to succeed. An altogether different approach, using a separate pitch extraction circuit and possibly a seventh vibrator, would, I consider, be more worthwhile. My reasons for making this suggestion are that a) the variations in pitch, (produced by any one person), are unlikely to be large enough to be well conveyed by the ratio signal controlling the thumb vibrator; b) the person to person variation may be too large, (men, women and children have different pitch levels), for the ratio circuit to operate reliably without providing a switch for altering the first filter response, (an inelegant solution).

Concerning the ratio measuring circuitry, it is the case that D.C. levels are critical and have caused considerable adjustment problems. The solution is probably the use of low pass filters and A.C. coupling, (as the frequencies of the controlling signals used are low). The circuits will also have to have an improved dynamic range to cope with the dynamic range to be provided elsewhere in the apparatus, (assuming the aid is developed). The ratio measuring circuits will also need to be better realised (using matched components), so as to avoid poor amplitude suppression, (attention has already been drawn to this effect in Fig. 5/7c). i.e. Fig 4/4c

iii) The processor-vibrator link.

In many respects this section of the equipment was the most disappointing, especially as a considerable amount of work was put into exploring some of the principles involved and matching the stimulus to the human's sensibility, (Chapter 3).

The fundamental problem encountered in this section of the apparatus, I consider, concerns the use of logarithmic and antilogarithmic responses in some of the circuits. Although these are very useful, and I consider it necessary to extend their use in order to increase the dynamic range of the equipment, there is associated with their use the need for great stability of D.C. voltage levels. This arises from the fact that a small change in the voltage level of the input signal to an antilogarithmic device causes a significant shift in the level of the output. Extending the use of such circuits will cause more problems, but it may be possible to overcome the problems already existing as well as the new ones by, (as suggested above), extensive use of low pass filters and A.C. coupling instead of the present extensive use of D.C. coupling.

Some frequency shift of the vibrator oscillators was caused by temperature changes, (ironically these were caused by local high power zener diodes serving to smooth and stabilise the supplies to different sections of the equipment, so as to prevent frequency anomalies). This is a compound problem involving power dissipation and temperature induced voltage changes which, though small, were important for the reason mentioned above. The general stability of the equipment was satisfactory.

The vibrator providing information about speech intensity needs to be controlled in a more sophisticated manner. A concept which could be explored is the following. A 'combination' of signals from two energy measuring circuits, (one with a long time constant and the other with a short time constant) using the ratio measuring principle, might provide the required insensitivity to gross long term variations in intensity whilst retaining and extending the vibrator's activity in response to rapid variations.

The vibrators, (identical to that employed in the psychophysical experiments), appeared to be reliable but they must be made smaller, or replaced by something smaller and more robust, if children are to use a developed version of the aid.

* * * *

The improvements and procedures suggested above should go some way toward improving the effectiveness of the vibrators' outputs in response to speech. It should become possible to build the aid so that the vibrators respond in a specified manner rather than, as was done, building a machine and examining its performance.

As regards the aid as an instrument, and not a collection of circuits, it seems that general constructional faults and faults arising from the use of cheap trimming resistors were the predominant causes of trouble.

The performance of the aid in a real acoustic environment has not been assessed and an extended dynamic range might cause problems here. Microphone positioning and delicate use of the volume control should overcome any problems introduced, (such as extraneous noises interfering, many people speaking at once, echo.....). Some of these problems can be overcome by the use of headworn microphones, for example, but difficulties could be experienced if the aid is developed for class-room use.

Specific conclusions concerning the technology are that:-

1) Inadequacies acceptable in a prototype have been recognised and must be remedied before a version of the aid suitable for trials in schools can be built.

2) Conceptually the aid is promising.

3) Conceptual improvements and concomitant technical areas for development can be identified.

Especially gratifying is the impression I have that, although not formally evaluated, (the realisation is too poor), the conceptual structure of the aid appears to be sound. The concept of high-pass filters with overlapping responses appears to have general application in speech work and the ratio measuring concept also has considerable potential. The isolation of information about the vocal tract configuration, (i.e. information about the speech gestures), from speech intensity information is important and allows the whole sensory range available to be used for tactile appreciation of the vocal gestures, (in fact it provides a remote 'hand on the throat' facility).

These remarks rest on the conclusions of section 5 a), namely that the aid appears promising in a training situation. Moreover the training situation has enabled the deficiencies to be recognised.

PART 3

'GENERAL CONCLUSIONS'

CHAPTER 6

These conclusions are presented in the order of presentation of the material to which they refer. (For more detailed conclusions see the relevant chapters).

The importance of the material of Part 1 is that it contains an up-to-date, although spotty, review of the controversy concerning the use of oral versus Total communication in the school and at home. I consider that the evidence presented supports the contention that Total communication is more appropriate, than is oralism, for the education of the deaf.

As part of Total communication and the concomitant recognition of deafness as a state of aural indifference, it is necessary for deaf children to be made aware of their effects in the aural world. This, and other aspects of aural culture should be 'taught' to deaf children, especially games involving rhythm which should if possible involve the group use of the voice. It may be necessary to make equipment for this, but this would not be difficult.

There is an educational problem associated with the advocacy of Total communication. Which children should be educated in this way? I would suggest that until audiometric techniques have become more sophisticated it would be better to educate too many this way than too few. The benefits of social confidence and the encouragement of natural communicativeness are too precious to withhold from those who would profit from the use of Total communication.

Also presented in Part 1 is a brief exposition of a model of perception and thought. This model is potentially powerful, (i.e. explanatory), and is thus more useful than merely description models. The presentation is not thorough, (in the sense exhaustive), and it is clearly necessary to formulate the model extensively and formally for presentation elsewhere.

Apart from the need for more detailed presentation of the theory there is the need to consider the predictions that it might make. This will only really be possible when the detailed formulation exists but one prediction is already possible. It should be possible to experimentally observe whether or not there is a preference for interpreting visual information environmentally and aural information socially.

The model does offer the possibility of describing relationships between perceptual activity, behavioural activity, and social activity.

It contains the potential for providing a system of character categorization, or personality assessment, and for linking this to educational history. This is politically a worrying possibility and any formal exposition of the model would have to contain a complete assessment of this possibility and a statement concerning the responsibility of scientists working in this field.

Assessment of a person's character or intelligence, whether it is postulated that heredity or race plays any part in the development of the person, raises the problem of what to do with the assessment. Should people be educated, for example, according to their 'measured' ability, or should attempts be made to alter their environment so as to increase their ability, (and if so how)? It cannot be denied that social environment and heredity both play a part in the development of personality, and my feeling is that although such an admission, when turned into fact, can be used unscrupulously, it can also be used beneficially. The possibility exists that genes affect one's intelligence and personality to some extent. Society, (including scientists), will not gain from a debate concerning the validity of this possibility. This idea has been used to justify atrocities, and is being used at the moment to remove from certain societies people who are a danger to the governments of those societies. Scientists, as reponsible and informed members of society, must be vigilant and must prevent the abuse of any addition to the psychiatrists' psychometric armamentarium, as well as the abuse of existing knowledge.

* * * *

In Part 2 I presented first the psychophysical work on the sensitivity of the fingers to vibration, and then the details of the construction, evaluation, and criticisms of a prototype speech training aid incorporating a novel stimulus system. This prototype aid, it is considered, provides the facility for mimicry and demonstration.

That the psychophysical experiments were inconclusive is not surprising, the possibility of this being the case was foreseen. The formulation of the purpose of the experiments shows the recognition of this and of the fact that generalisation from the results obtained in special circumstances is not a valid procedure. However, the trials with the aid were conducted with only one person, an adult deaf woman.

The conclusionstc be drawn from the experimental work with this one subject are weak but they are indicative. Indications are important and perhaps as much as can be expected from any limited study.

To indulge in the full rigorous procedures of experimental psychology may be commendable when testing experimental educational equipment on a large scale, with pupils and teachers willing to take part and with little or no possibility of detrimentally affecting the development of any individual. I would contend that the difficulties would be immense even so, because of the variability of the effectiveness of the teachers and the willingness of them and their pupils.

I would consider that a full experimental investigation of the oral/Total communication debate could be valid but again subject to large variability in commitment and skill. (I don't personally think it necessary but teachers of the deaf may well feel it is).

However, testing experimental teaching equipment in schools for the deaf, with teachers very emotionally involved in their work and pupils, and with great variability of style and effectiveness, would not, I consider, be worthwhile.

There is the extremely important ethical problem to consider. Is it right to deprive children of a possible benefit for the sake of establishing a control group? Unless two equivalent groups can be found with teachers personally committed for and against the particular equipment, (thus providing the control group by teacher preference), I would feel loath to inflict deprivation randomly on a group of children already struggling with enormous difficulties. This implies a prejudgement of the effectiveness of the equipment. This implication is made consciously, there is the important point to consider that experimental equipment will not be acceptable in schools, especially in deaf schools, unless there are reasonable grounds for assuming it to be valuable. If these reasonable grounds exist then why perform controlled experiments? Without controlled experiments, however, what can constitute reasonable grounds?

I consider that the work I have done, and the approach I have followed constitutes the establishment of 'reasonable grounds', that is, the recognition of the potential of a piece of equipment for use in a teaching situation.

I have called my approach the optimum route for optimisation. By this I mean to suggest that when developing and testing a piece of equipment a valid approach is to set up conditions to be as favourable as possible. If, under these conditions, certain flaws in design or conception become apparent then it can be valid, (with caution), to attribute them to the equipment and not to the people involved. In this way a prototype piece of equipment can be usefully evaluated and improved. Such a procedure does not of course guarantee the success of the developed version, but it does provide a means for continuous assessment and re-assessment, because, by selecting a suitable situation for trials with the developed version, the development of the equipment can continue.

The fundamental principle is that by inconveniencing as few people as possible, those people being selected for reasons of willingness and commitment, a process of development can continue which will result in an apparatus of known value in known conditions. This evolutionary approach depends on the assessments of the teachers and pupils involved but that is as it should be.

The work that I have done has been with this approach in mind. The stimulus devised was tried because it was considered likely to be at least no worse than previously tried stimuli, and very possibly better than such stimuli. The informal comparison with conventional stimulation, (i.e. vibration at a fixed frequency), does not prove anything, it suggests, however, that if tried on a larger scale the new stimulus would be judged by subjects to be more interesting and acceptable. (Both types of stimuli would be tried in any trials with a developed machine).

The prototype aid which has been developed to provide the facility for mimicry and demonstration has flaws in design and these have been identified as a result of using the system in a 'mock' teaching situation. There were no facilities for testing the equipment with children, (who are likely to be able to make better use of it for physiological reasons), and the equipment is too laboratory dependent in its present form for testing in schools.

An interesting observation was made by a visiting teacher of the deaf who observed that the subject, Lydia, was more confident, (in his opinion), when her hand was on the vibrators. The subject has also commented, spontaneously, that with her hand on the vibrators she feels as if she has a voice, without the vibrators she feels as if she has no voice.

I feel confident that a speech training aid of potential value has been devised which provides the facility for mimicry. I also consider that it can be developed and assessed without recourse to methods involving control groups. The success of this work has depended on an appreciation of deafness as being something more than a communication problem, and an awareness that any work done in this field must be guided by considerations of appropriateness.

APPENDIX 1

This Appendix is mainly concerned with tactual sensitivity to vibrations, although electrocutaneous stimulation will be briefly discussed.

A1 1). This section presents a brief review of the physiological work and theories as they are presented in a comprehensive set of papers written by Frank A. Geldard,(Geldard, 1940). I attempt to show where, in his efforts to disprove the existence of a separate vibratory sense, Geldard misses the significance of a substantial variety of evidence. With hindsight it is, of course, much easier to see the significance of the evidence.

My review is not intended to be exhaustive, but it is intended that it will provide an historical background without which the more modern work would seem to lack perspective and direction.

Al 1).

In 1940 Frank A. Geldard wrote a set of papers entitled "The Perception of Mechanical Vibration". The first paper deals with the history of the controversy, for that is what it was, over the process of perception of mechanical vibration. The second and third papers present some experimental work carried out by the author. The last paper analyses the arguments, as he sees them, of the proponents of a separate vibratory sense. The controversy concerns this one question... "Is there a specific separate vibratory sense?"

The history of this controversy will provide future students with a fascinating example of the scientific method and its development over the last one hundred years or so. It also provides a record of the part played by technology, and the development of instrumentation and electronics, in the solving of a consistently well defined problem.

I say future students because the answers put forward over the years, the quantities of experimental data, some good and some very poor, are only now beginning to coalesce. Unity of purpose per se does not produce unity of opinion, which is as it should be.

Geldard was of the opinion that there were no reasons for supposing the existence of a separate vibratory sense. He states:-

> "It is held to have been proven that pressure receptors, and presumably only pressure receptors, participate in the vibratory response." (p291. Unless otherwise indicated page numbers in this section refer to the set of articles already mentioned).

In his last article he puts forward ten arguments representing the 'separate vibratory sense faction' and deals with them one by one with a mixture of flippancy and disquiet. The disquiet manifests itself in the mention of the presumption in the above quotation, and in certain rebuttals where his arguments lose force. For example, in response to:-

> "Light pressure on the skin yields qualitatively different sensations from those produced by heavy pressure. No such division obtains in the vibratory sense." (Argument 5, p294,).

He proceeds:-

> "It is, of course, no longer fashionable to speak of qualitative differences in the feeling sense, but what is presumably meant is the distinction between 'contact' and 'pressure' sensations. If, in these days of feeling patterns, the distinction between the qualities of contact and pressure is to be retained, then one must

similiarly insist that weak vibration gives a 'tingling' or a 'whirring' whereas strong vibration produces a 'hammering'. But, obviously, all strengths of stimulation, whether pressure or vibration, yield patterns complex in nature. Some of these patterns, occurring familiarly, we come to endow with names, as Nafe has pointed out,(J.P. Nafe, Ch.20 Murchison's "Handbook of General Experimental Psychology", Clark Univ. Press, 1934,). Sufficient intimacy with various strengths and extents of vibration feelings leads to a whole family of designations".

He carefully avoids the qualitative differences in sensation by referring to 'strengths'. This is because he considers pressure and touch to be mediated by the same receptors, and light vibration to be only quantitatively different from strong vibration. However, he himself notes the assumption involved here, it is quoted above.

Argument 9 deals with some pathological findings,(p296).

"Clinical neurology teaches that vibratory sensations may be lost while touch is retained in certain nervous diseases. Furthermore, tactual sensitivity may disappear with full preservation of vibratory feelings".

He counters this with:-

"It is, of course, this set of arguments that has spoken most strongly from the beginning for a vibratory sense, and it is these considerations that perpetuate a separate sense of vibration in clinical neurology. It is perfectly easy to see how vibration can apparently be preserved with loss of touch. Spread of vibrations to sensitive tissues undoubtedly accounts for the result. And carelessness in manipulation of the fork has undoubtedly produced some exaggerations. After all, the neurological patient is merely required to report when he ceases to feel vibration or if he can feel it, depending upon the method of testing. He is not required to say where he feels it and it is doubtful if such an instruction could be followed with any fidelity. Localisations are of necessity diffuse".

He finds loss of vibration with preserved touch not so easy to dismiss. He continues:-

"Neurologists quite generally resort to the postulation of separate central pathways for vibration and touch. It seems highly improbable that such is the case in view of the many evidences pointing to an identification of the two. Perhaps it is at this point that it needs to be reasserted that we are dealing with a perceptual process. If vibration were to drop out as a perceptual thing, if discriminability of separate impacts were to disappear, before the basic tactual sensory process became disturbed in progressive infections such as tabes[consumption], we should not be too greatly surprised. In general, fine discriminations tend to disappear before cruder ones in any degenerative disease. But, on the whole, the evidences from pathology still need to be unscrambled. Taken as a group of empirical findings the facts concerning dissociation of vibration and touch are clinically valuable. But a separate vibratory sense is neither demanded nor implied by them."

What I think he fails to appreciate is that even if touch and pressure are mediated by different receptors, as the evidence suggests, his conclusion would not be invalidated.

In his treatment of argument 10,:-

"The evidences from local anaesthesia show that pressure sensitivity may be virtually abolished without seriously affecting vibratory thresholds,"

he reveals his presumption again. In his formulation of the argument he uses 'pressure' for two different sensations. This is revealed by his discussion of the work of Cummings,(1938), and Weitz,(1939). Both of these workers report on the effects of local anaesthesia on Tactile and Vibrational sensitivity. Both use a 256 c.p.s. sinusoidal stimulation, but differences in anaesthetisation lead to different results.

Cummings, who carefully anaesthetised only the surface of the skin, reported a consistent and slight decrease in sensitivity. He suggested that two sets of receptors are involved, one superficial, the other deep.

Weitz anaesthetised more extensively and found a marked decrease in sensitivity. Recovery of sensitivity was followed and it was found that "prick" returned first with no change in sensitivity. With the return of "contact" there was a "marked drop" in threshold, and thresholds "returned to normal with the recovery of pressure sensitivity."

Geldard does not notice the implications of this and fastens his attentions instead on the fact that Weitz found the skin to be locally insensitive.

"When, on raising the amplitude sufficiently, vibration is at length felt, it is found to be removed to an unaffected area. The tissue changes induced by cocaine and other anaesthetics (constriction of arrectores pilorium, etc.), merely serve to spread vibrations better. The apparently embarrassing facts of local anaesthesia are, like the pathological facts, simply the setting for a problem. But the problem is neither psychological nor neurological. It is one of skin mechanics, and the solution is absurdly simple.

It thus appears that to confront these various arguments with the available experimental facts is not only to make them appear less formidable but to render all of them totally invalid. There seems to be no ground whatever for the postulation of a separate vibratory sense."(p297,).

Apart from the omission of the absurdly simple solution he is contradicting his refutation of argument 9.

In the first of his papers Geldard discusses in more detail both the pathological findings and the experiments of Cummings and Weitz.

On page 249 he writes, "Certain writers, once having settled on an exclusion of bone reception and an acceptance of a cutaneous origin, attempted further to decide between superficial and deep receptors as the major contributors to vibratory feelings. Strumpell's 1904 paper,(A. Strumpell, Dtsch. med. Wschr., 1904, Vol. 30, p1411-1414, 1460-1463,), opened a major debate that <u>cannot yet be said to be settled satisfactorily</u> [my emphasis]. He reported extraordinary pathological cases in which, with contact sensitivity so well preserved that the lightest stroke of a hair was appreciated, strong pressure exerted on the skin by the finger went unnoticed. In fact, the pressure had to be increased to the point of yielding a painful quality to be perceived. Other pathological separations of the sensibilities of the skin and deeper lying structures were demonstrated. In accordance with this division Steinert,(H. Steinert, Dtsch. med. Wschr., Vol. 33, p637-639, 1907) concluded that vibratory feelings are mediated both by nerves for touch and those for deep pressure but that the latter are the better conductors for those feelings. The superficial component is too much determined by local conditions of stretching, elasticity, disposition of underlying structures, etc., to yield constancy of results. Practically, then the fork has value only as a test of deep sensibility and in pathological cases one might expect to find closer agreement between vibratory and tactual anaesthesias."

His last sentence in the above passage shows again his unease over the assumption of the equivalence of touch and pressure.

He notes that sensitivity to vibration, (applied via the base of a tuning fork), is used as a test for tabes in its early stages. Symns standardised the procedure, and this was extended by Wood and Ahrens. On page 255 he quotes Symns, as being representative of a group of workers using his technique, as saying, in 1917,

> "For the present, therefore, it seems best to content ourselves with the statement that the vibratory sensation appears to be a mixed sensation and closely allied to 'deep sensibility'....There is a possibility that the vibrations of the tuning fork give rise to cutaneous sensation of pressure."

Geldard continues:-

> "In the following year he was willing to hazard the guess that perhaps 80 per cent of the contribution was by 'deep', 20 per cent by 'superficial' sensibility."

On pages 263-264 he discusses the effects of local anaesthesia. He says on Cummings's work "This experiment, at first glance an entirely decisive one, has subsequently been repeated and extended by Weitz." He is clearly in error here. As already mentioned the Weitz experiments are not repetitions of those of Cummings. The anaesthetic differences are clear.

In the light of the evidence for two sets of receptors mediating sensitivity to vibration it is difficult to understand his concern to show that there is only one. He is obviously preoccupied with the main aim of his study, that is, to show that there is no separate sense for vibration, the sense of touch/pressure mediating sensitivity to vibration. However, this may have blinded him, because surely, to say that two sets of receptors are involved in the tactual perception process, one for touch and the other for pressure, does not in the slightest make his argument weaker. In fact it strengthens it! To say that touch and pressure are qualitatively different, and therefore different senses, implies that sensitivity to vibration is not specifically mediated and is not a 'separate sense'. Even worse, if pressure/touch was indeed one 'sense', then surely he is laying himself open to the argument that the vibration sense happens to mediate sensitivity to touch and pressure, instead of the other way around, as he would maintain.

This set of papers has had a marked effect on the subsequent work on the nature of sensitivity to vibration. His conclusion that there is no separate sense of vibration would not be questioned today, but his assumption that 'touch + pressure = tactual' and that only one sense is involved implies that only one set of receptors is involved. This has led to some dubious neurophysiological and psychophysical work. The results of some experiments, and the interpretations of them by their authors, and by others, need not be discarded but must be used with caution.

Al 2). This section deals with the more modern work on cutaneous sensitivity to mechanical stimuli. I have tried to capture the flavour of the main argument, which, I suggest is the inevitable outcome of the approach used. This centres on the use of 'laboratory' stimuli to settle the question of whether specific receptors responding to particular stimuli and connected,(in a manner analogous to a telephone cable with many parallel wires), to specific locations in the brain, mediate sensitivity to mechanical stimuli. The alternative proposal is that such specificity is unnecessary and that the mediation is by receptors, all the same,(located in different tissues), which respond with different patterns of neural impulses according to the nature and quality of stimulation. These patterns are,(after perhaps some transformations), decoded in the brain.

I suggest that the real situation is much less conveniently modelled. It would appear that the most thorough statement that can be made is the following:- 'The discharges from various receptor populations,(which may not be homogeneous), must to some extent be identifiable, (by the brain), with their source. The patterning of the discharges, which undoubtedly does take place, coupled with this 'knowledge' of the source, is all that is required for the different sensations,(qualities and intensities), to be felt as such.'

As regards sensitivity to vibrations it can safely be said that at least two populations of receptors are involved. The form of the 'threshold vs. frequency' plot is discussed and I suggest that its shape is the combination of the mechanical properties of the tissues concerned and the end-organs themselves.

Al 2).

Before passing on to the more recent work on the nature of sensitivity to vibration applied to the skin it is necessary to comment on the use of the word 'specific'. Hensel & Boman,(1960), note that two meanings can be given to this term. The term can be used to mean the sensation, or perception, aroused when a given receptor is stimulated, regardless of the nature of the stimulus. It also can be used to describe a receptor which arouses some sensation only when a specific stimulus is applied to it. The two meanings could be separated by using the terms 'sensation specific' and 'stimulus specific'.

Geldard, in the set of papers discussed, was probably influenced by Frey who considered that cutaneous sensations were stimulus specific and sensation specific, (Melzack & Wall, 1962, and Morgan, 1965). Geldard thus considered that the proposal for a separate vibratory sense implied a different stimulus specific set of receptors, (he was not concerned to challenge the psychological aspect of Frey's hypothesis). His view was that the pressure receptors, (a stimulus specific set of receptors), were quite adequate to mediate sensitivity to vibration as this was only pressure patterned in time.

Nowadays stimulus specificity must be considered too simple an idea because some receptors can be stimulated by different types of stimuli, for example Hensel & Boman,(op. cit.), report work on human subjects in which receptors were found which responded to pressure and cooling.

Stimulus specificity is unacceptable because of the findings of Békésy,(1958 & 1959), which show interactions, at the site of the stimulation, between various aspects of vibrotactile sensation. Melzack & Wall, (op. cit.), suggest that there might be interaction at a much higher level, (this is supported by the finding that contralateral masking can be detected using vibrotactile stimuli delivered to the hands,(Sherrick 1964, Gilson 1969)). These two locations for interaction between different aspects or qualities of the same sense are not mutually exclusive.

There is another objection to the idea of stimulus specificity. This centres on the definition of the stimulus. For example, the Ruffini ending is sensitive to stretching of the skin, (it is also sensitive to temperature),(Chambers, Andres, Duering & Iggo, 1972), but what exactly is stretching of the skin? The ending is most sensitive along its axis, and this is roughly parallel to the surface of the skin, but almost any mechanical stimulation will excite such an ending somewhere in the vicinity of its point of action, (assuming that the area of skin concerned contains such endings). The stimulus could equally be called 'displacement', and I would suggest that it would be more helpful to call it such and to say that the Ruffini ending is sensitive to stretching or extension of the tissue in which it is located. The difference I am emphasising is between 'that which is done to the skin', often called stimulation, and 'that to which the nerve ending is sensitive', also called stimulation by some. I will reserve the term 'stimulation' for the first sense instanced above. I will say more about the nature of many of the stimuli later in this section.

Stimulus specificity, (in the second sense in the above paragraph), has been challenged by the hypothesis of Kenshalo & Nafe,(1960), and Nafe,(1968). This concerns the modus operandi of all the cutaneous senses. Their view is that the cutaneous senses have a common element in their functioning, and that this is movement. Thermal and 'touch' receptors (i.e. touch and pressure), operate, they postulate, by causing movement of the nerve endings relative to the surrounding tissue. They suggest that this hypothesis is tenable for two reasons, a) no suitable chemical reactions can be found, and b) sufficient subtlety of sensation is available because of the different types of tissue on which the nerve endings are located. "Specificity lies in the nonnervous tissues which contain the peripheral nerve terminals."(Nafe, op. cit.).

The problem of specificity, (to which I shall return later), is linked to the problems of neural coding and transducer (in the receptor) mechanism. The controversy over the vibratory sense has been displaced in recent years by the controversy over 'specificity' versus 'spatio-temporal patterning of neural impulses'.

In a review article, Catton,(1970), discusses mechanoreceptor function and concludes:-

> "The most fundamental question, that of the transducer process converting mechanical energy into the initial electric change in the membrane, has not been answered....." (The membrane here is the end-organ membrane). He states, "It is established that excitation is initiated by an increased permeability to sodium ions, but it is not known how deformation leads to such an increase."

As regards the discharges in the nerve from a receptor, and their relation to the mechanical displacement of the skin, there has been much speculation,(Békésy, op. cit., (1957, & 1962,), and recently some evidence. Uttal,(1959), suggests on the basis of electrocutaneous stimulation that

> "...the magnitude of subjective experience is not simply coded by the interval between successive responses but, within the interval studied, by the sum of the response amplitudes."

Werner & Mountcastle,(1968), report on relations between mechanical stimuli to the skin and neural responses evoked by them. Chambers et al. (op. cit.), find a logarithmic relationship between interspike interval and the stimulus amplitude, and this for a dynamic stimulus.

Uttal & Krissoff,(1968), propose an approach to the problem which, simply, is to inject into the nervous system 'pseudoresponses'. By carefully manipulating the properties of the trains of pulses they insinuate into the sensory system they claim it is possible to discover which of many aspects of such nerve pulse trains are perceptually important, (and the correlated sensations experienced). However, the drawback with such an approach is that at some stage the real responses will be interpreted in terms of the 'nicely defined' neural pulses introduced into the system. The validity of this will rest on the careful measurement and mimicry of discharges in the nerve, at the site of injection, evoked by real stimuli. I would doubt the value of translation of the code which was not based on the use of copies of observed discharge patterns. This possibility of error in method and experimental technique is, in principle, the same as that which can occur in psychophysical experiments on the sense of touch. Investigation of sensitivity to vibration is very often carried out with unreal stimuli. This point relates to specificity and is taken up later in this section.

* * * *

With vibratory stimuli the situation becomes very complex. Békésy reports,(1957), that as the amplitude of such a stimulus was increased the pitch, (perceived frequency), decreased. His explanation is based on a modification of Wever's volley principle. This principle was put forward, (Wever, 1949), to explain how nerve fibres could, in groups, convey a signal synchronised with an oscillating stimulus, the frequency of which is above the maximum frequency which any individual fibre could transmit. The principle is that the firing in each fibre in a group will be synchronised to a submultiple of the stimulus frequency. As the intensity of stimulation increases the frequency of firing in any one fibre increases, (synchronised to a different submultiple), and in the group of fibres as a whole the intensity (i.e. the amount) of firing increases whilst the frequency of firing remains synchronised with the stimulus in a one-to-one manner. Such a scheme provides for the coding of both frequency and amplitude in the same group of fibres, by means of volleys.

Wever did not, however, postulate that this was the sole mechanism by which frequencies are discriminated in hearing. (The volley principle does require a centre somewhere in the auditory system where the frequency or period of trains of pulses could be measured although not necessarily in absolute terms). He suggests that the place theory of frequency discrimination is involved as well, (in this theory the location, along the basilar membrane, of the stimulated nerve endings is sufficient to code for the frequency).

Békésy, perhaps because of his concern to understand the mechanisms of cochlear functioning, carries his analogy between the basilar membrane and the skin of the forearm too far. (The forearm is the site he uses for many of his experiments). He at first postulates, (1957), (in violation of the volley principle he invokes), that at low intensities the high pitch is a sort of white noise perception.

> "Since there are several nerve tracks, the asynchronous discharges produce a sort of white noise, and the pitch of this white noise is higher than the pitch of the sinusoidal stimulus. With increasing amplitude of the vibrations, the discharges become more and more synchronous with the vibrations, and the somewhat smoother sensation produced by the white noise becomes increasingly rough, with a time pattern that produces the sensation of the lower pitch."

Later,(1962), he states;-

> "...the continuous rise in the vibratory pitch with increasing amplitude, which the electrophysiology would predict, holds only at low frequencies and at small sensation levels above threshold. The drop in pitch when the amplitude is increased to higher levels is unexpected."

The prediction is made on what appears to be a misunderstanding of the volley principle. On page 850 of the same article we find:-

> "The volley theory provides the best-known solution of how the two variables, frequency and amplitude, can be transmitted simultaneously in a single fiber. The question is, to what degree can our nervous system separate these two variables after they arrive at a higher level?"

On the problem of the drop in pitch with increased amplitude he cites an earlier paper of his,(1959), in which he states:-

> "...we may conclude that the pitch sensation on the skin is not given simply by the periodicity of the neural discharges produced by the end-organ. But it seems, rather, that the synchronous neural discharges of the end-organ merely trigger a center at a higher neural level, and its discharge rate depends not only on the discharge rhythm

of the end organ but also on other factors such as loudness, summation, and inhibition."

This latter is, I feel, the closest he comes to realising that it is wrong to expect of the mechanoreceptors in the skin the degree of fidelity with which frequency is perceived in hearing. However he persists, as the above quote from his later paper shows, in applying (incorrectly) the volley principle, the whole point of which is that it was proposed to account for the fidelity of pitch perception in hearing.

More recently, (Mountcastle, Talbot, Darian-Smith, & Kornhuber, 1967, and Talbot, Darian-Smith, Kornhuber, & Mountcastle, 1968,), work has been done on the 'Sense of Flutter-Vibration' using monkeys. Talbot et al., deal with the neurophysiology of sensitivity to vibrating stimuli and, (assuming the comparison to be valid), compare the results to some psychophysical data from experiments with humans.

They identify three types of receptors, in the hand, by the discharge patterns observed in the nerve fibres in the arm. These are 1) slowly adapting afferents, 2) quickly adapting cutaneous afferents, 3) Pacinian afferents. (Catton, op. cit., defines adaptation "as the rate of diminution of impulse frequency with time during a sustained deformation", and he goes on to comment "...(it) is probably the most important single functional property of a receptor, since it determines the nature of the sensory message delivered to the central nervous system."). They conclude that the slowly adapting afferents, which yield impulses in response to a steady indentation of the skin, do not play a part in sensitivity to vibration. At high frequencies, (around 100 c.p.s.), the intensity required to synchronise the discharges from these receptors is very much higher than the threshold (in humans) for these frequencies. At mid frequencies, (around 40 c.p.s.), synchronisation can be achieved at low amplitudes but the amplitude versus frequency response in this region is very unlike the psychophysical response in humans. This is because the synchronisation is the result of coincidental matching of discharge rate and stimulation rate. At low frequencies the discharge from this afferent is entrained at amplitudes much lower than those which elicit a sensation in man.

The cutaneous quickly adapting afferents are most sensitive in the region around 40 c.p.s. As the stimulus amplitude increases a fibre will quickly synchronise its discharge rate, (at 1 discharge/cycle), with the stimulus and will maintain this synchrony up to quite large amplitudes when it will begin to fire twice in some cycles. See Fig. A1/1.

The Pacinian afferents show the same type of behaviour but the region of greatest sensitivity is around 200-300 c.p.s. See Fig. A1/2.

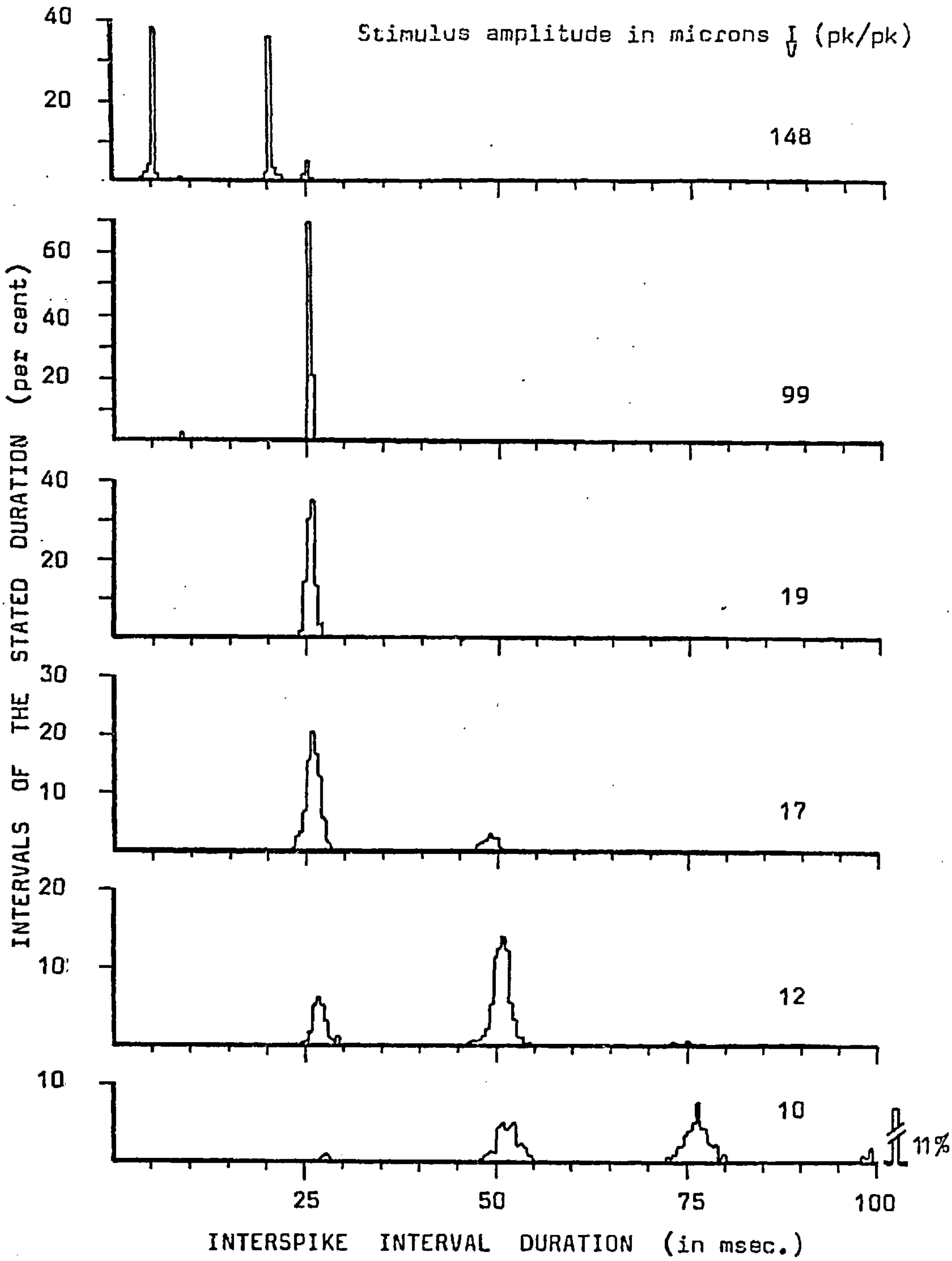


Fig. A1/1

Interspike interval histograms for a quickly adapting afferent in the hand of a monkey, (from Talbot et al, 1968). Stimuli at 40cps. delivered via a 2-mm. diameter probe tip oriented in the centre of the peripheral receptive field for this afferent. (Glabrous skin.)

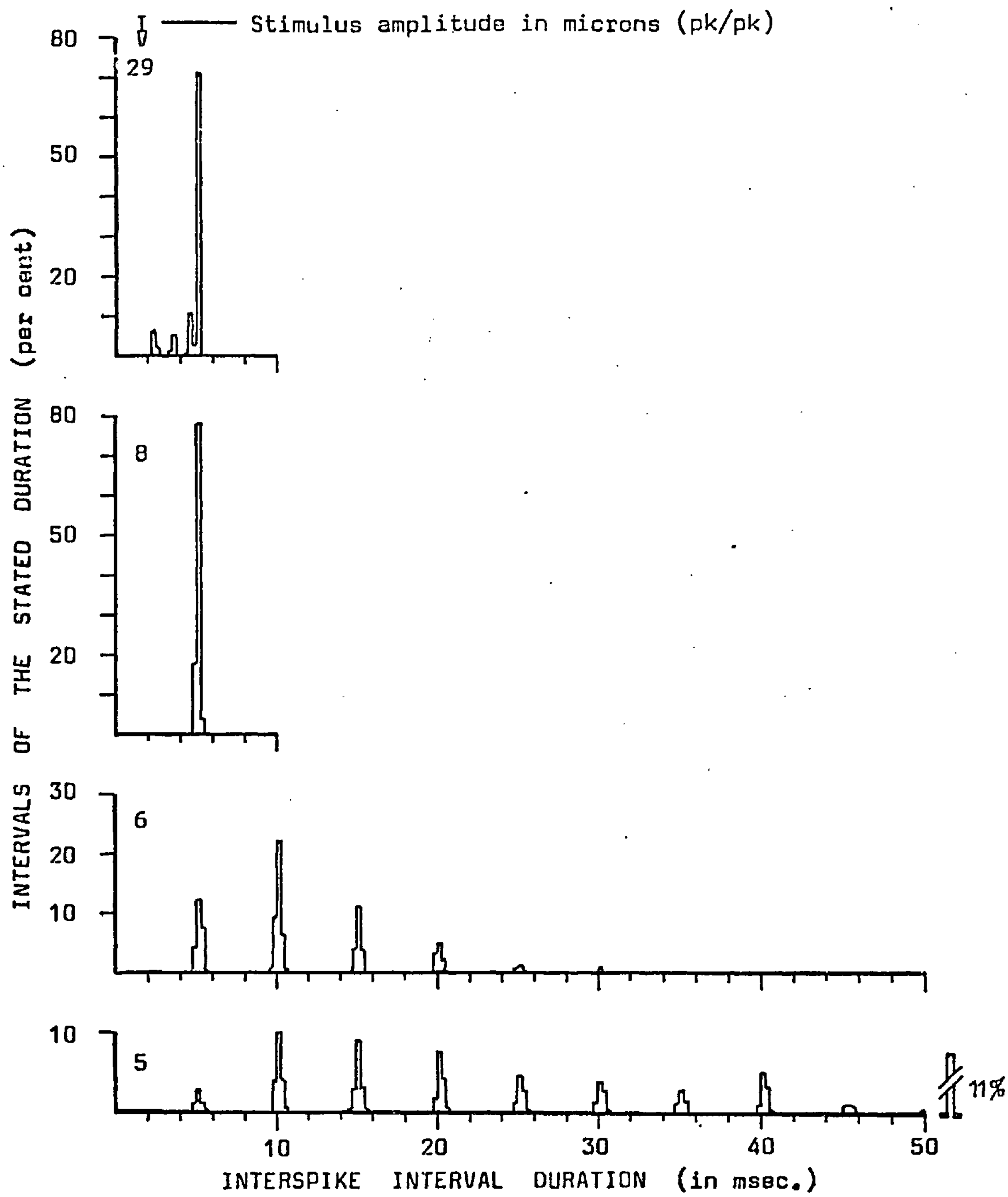


Fig. A1/2

Interspike interval histograms for a Pacinian afferent in the hand of a monkey, (from Talbot et al, 1968). Stimuli at 200cps. delivered via a 2-mm. diameter probe tip oriented at the 'best spot' on the skin for this afferent. (Glabrous skin.)

The amplitude required to ensure synchronisation of discharge with stimulus varies according to the frequency of the stimulus. This applies to the quickly adapting afferents and the Pacinian afferents.

For the quickly adapting afferents the amplitude necessary for synchronisation increases with frequency, (above the best frequency of around 40 c.p.s.), and at frequencies around 150 c.p.s. or more synchronisation is not possible. Similarly, at low frequencies, (2-10 c.p.s.), increases in intensity may produce doubling of discharge for some cycles without any synchronisation at all.

With the Pacinian afferents it is sometimes possible to obtain synchronisation at high amplitudes and at frequencies lower than the best frequency, (200-300 c.p.s.), but lack of synchronisation can occur in the same way as for the quickly adapting afferents.

Talbot et al. go on in their paper to demonstrate that the two types of afferent which they consider mediate sensitivity to vibration correspond to two aspects of the psychophysical data from humans. See Fig. A1/3. They also show that with cutaneous anaesthetisation only sensitivity to the lower frequencies is impaired. See Fig. A1/4.

Two aspects of their work are important. The first is that it shows that in monkeys and probably in humans at least two receptor populations are involved in the sensing of vibrotactile stimuli. This was not acceptable only two years previously. Calne & Pallis,(1966), state:-

> "The last few years have seen the emergence of the Pacinian corpuscle as the receptor most widely accepted as being responsible for the transduction of vibratory energy."

Later in the same paper they write:-

> "While both touch and hair receptors respond to low frequency stimuli, (below 60 c.p.s.), no skin receptor other than the Pacinian corpuscles could follow frequencies above 150 c.p.s." They go on "It is interesting that the frequency range to which the Pacinian corpuscles respond (90-600 c.p.s.) correlates with the range of stimulus frequencies perceived as 'vibration' in man."

On the next page they write:-

> "It is possible that the spatio-temporal pattern of activity in many different kinds of receptor may be involved in vibratory sensation, rather than excitation of a single type of end organ. In general, however, the Pacinian corpuscle may be accepted as the only

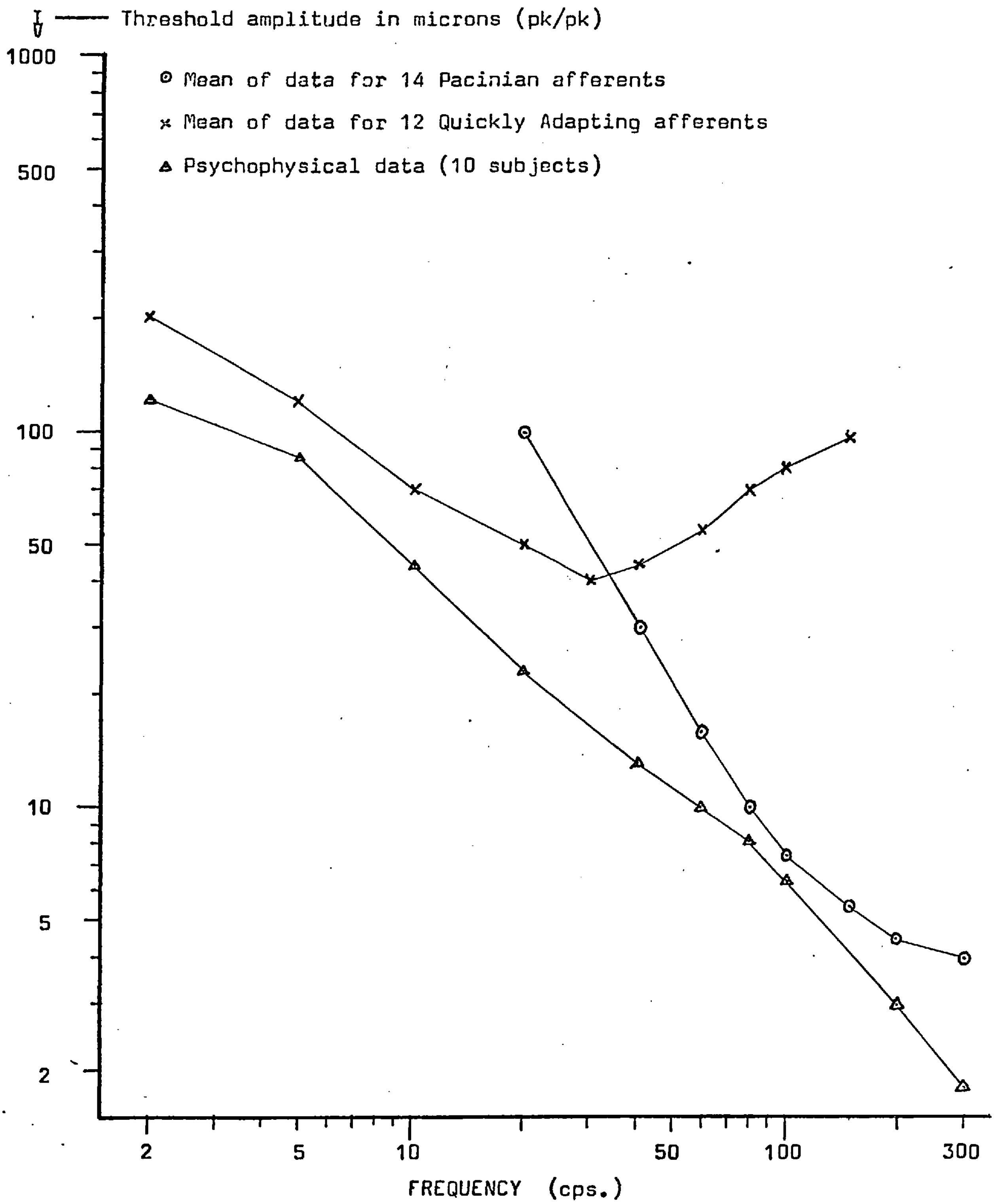


Fig. A1/3

Data, (redrawn from Talbot et al,1968), showing the Psychophysical threshold for the finger, and thresholds, for one-to-one synchrony of discharge with the stimulus, for the Pacinian afferents and the Quickly Adapting afferents.

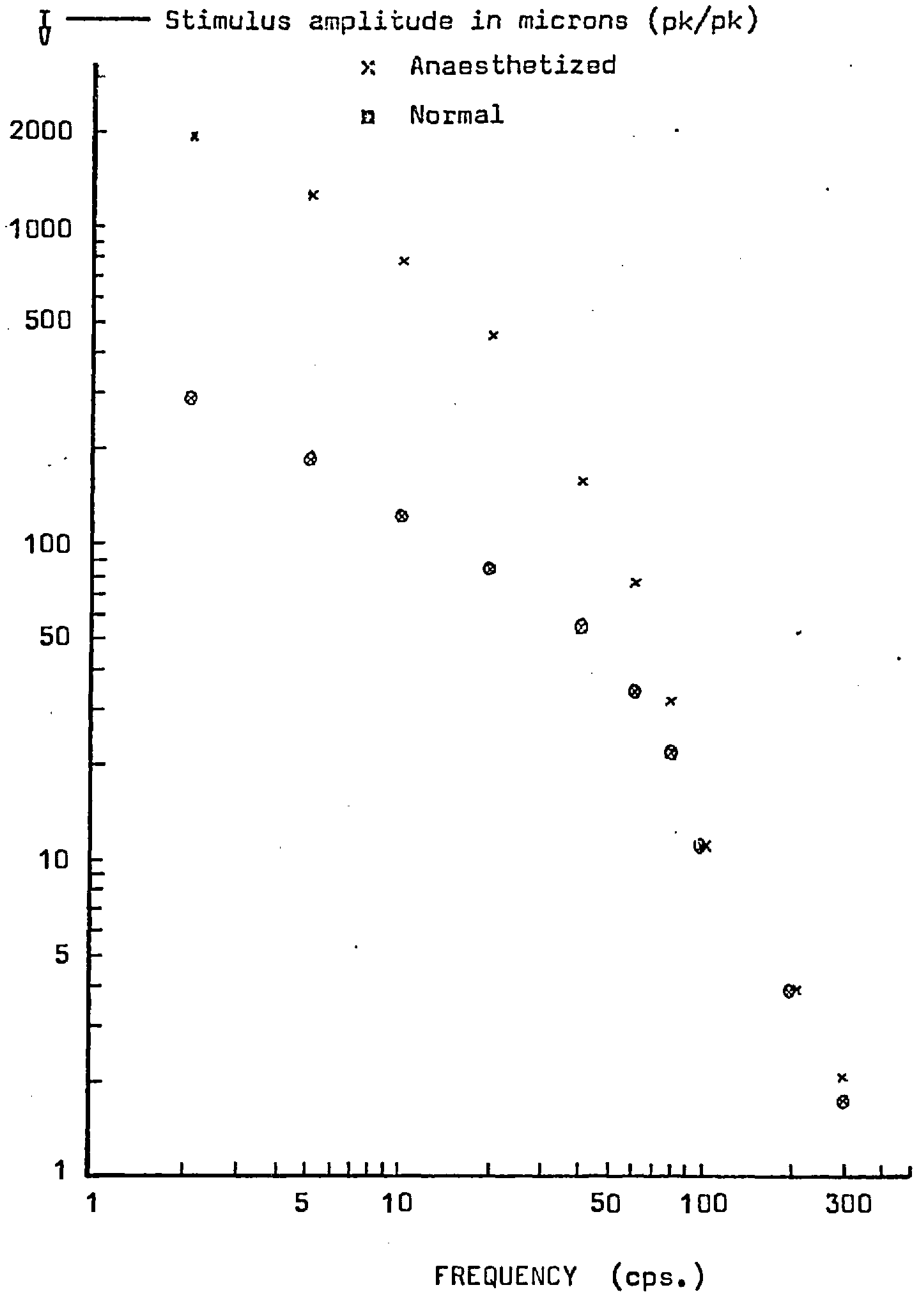


Fig. A1/4

Human thresholds, (from Talbot et al, 1968), for the Thenar Eminence. The data are averages for 10 subjects, before and after local skin anaesthetization.

> receptor whose properties and connections render it capable -- by itself -- of transducing vibratory energy."

The phrase "The definitive quality of a vibratory sensation," occurs earlier in their peper and probably explains their standpoint. Anything not mediated by the Pacinian corpuscles but aroused by a vibrating stimulus is not felt, somehow, as being truly vibratory. The title of the paper by Talbot et al. reflects this attitude as well, "The Sense of Flutter-Vibration: Comparison of the Human Capacity With Response Patterns of Mechanoreceptive Afferents From the Monkey Hand."

The second important aspect of their work is that neural coding of frequency is clearly demonstrated. In certain amplitude ranges the neural discharges are synchronised with the stimulus. Coding is discussed by Uttal & Krissoff,(op. cit.), but in general discharge interval or pulse rate are thought to reflect the intensity of the stimulu,(Chambers et al. op. cit. and Werner & Mountcastle op. cit.).

Further, psychophysical, evidence for the existence of more than one type of receptor sensitive to vibration as a stimulus is forthcoming from Verrillo,(1963), who concludes, from an investigation of the effect on the threshold of the area of contact between the vibrator and the skin, that there must be "more than 1 receptor system within glabrous skin responsive to mechanical deformation",(Verrillo, 1966). Hahn,(1968), provides direct psychophysical evidence for two different receptor populations. His experiments involve adaptation, (in the psychological sense, but see section 3a) 1).), and cross adaptation at two frequencies (10 c.p.s. & 200 c.p.s.). The experiments were designed to show that there were two different receptor populations. The recovery from adaptation followed the same course at the two frequencies, but when the 'adapting' stimulus was at one frequency and the threshold was afterwards measured at the other no adaptation was found to have taken place. He concludes:-

> "An interesting aspect of this conclusion, (i.e. that two different receptor populations were involved), stems from the fact that the psychophysical and electrophysiological evidence already cited, (i.e. Verrillo, 1963, op. cit. and Mountcastle et al. op. cit.), indicates that subcutaneous Pacinian corpuscles probably are mediating threshold at the higher frequency, while receptors in the skin are responsible for the lower frequency thresholds. The simplest interpretation of the present experiment is that tactile adaptation

to sinusoidal displacement was fairly independent of the mechanical properties of the tissues interposed between stimulus and receptors, as one might expect for sinusoids, so that the resulting adaptation functions primarily reflect sensory mechanisms; and that in spite of the differences in location and encapsulation of the different types of receptor, their adaptation and recovery are similar."

Catton,(op. cit.), says of adaptation:-

> "To account for the process of adaptation two kinds of theories must be considered: 1) those placing major emphasis on mechanical factors, i.e. theories of viscoelastic slip; and 2) those ascribing adaptation mainly to adaptive properties of the spike-generating mechanism of the receptor terminal itself. These theories are not mutually exclusive, and present evidence suggests that each may play a part in varying degree in different receptors."

The first of these two theoretical types is sometimes known as 'stimulus failure'. Concerning this theory Hahn wrote,(1968), in his discussion of some other experiments on adaptation,

> "... these data do not support the hypothesis that some form of stimulus failure is important in adaptation to vibrotactile stimuli."

In the same book A. Iggo,(1968), discusses evidence, which he feels is persuasive, for the existence of different types of nerve endings, as opposed to the hypothesis that the differences are to be found in the supporting tissues, and the mechanical linkages. He writes:-

> "... the slowly adapting endings showed a discharge for 30 mins. when the mechanical deformation was applied steadily all that time. This is to be compared with the Pacinian corpuscle where the discharge lasts for a few msec. or for the hair follicles where, when they are moved, the discharge disappears after 5-10 msec. There is an enormous difference in the time scales here. This is perhaps not very logical evidence for the difference, but it would imply you have got to have some kind of mechanical linkage that continues to slip over many hours in order to account for the existence of this persistent discharge with the maintained displacement if the mechanical linkage determines the rate of adaptation."

In support of the second type of theory Mashansky, Mirkin, and Vinnicheko,(1969), find changes in the ultrastructure of the Pacini corpuscle following "adequate vibration treatment". They suggest their findings might have significance in the process of adaptation. Catton discusses both types of theory, and the evidence for them, in detail in his paper.

Catton, and Iggo, use the term adaptation to mean the response of a receptor to sustained deformation of the skin. The use of the term for changes in threshold in response to vibrotactile stimulation is questionable, until lowering of threshold in response to such stimulation is demonstrated, (see 3a) 1).).

Returning to the receptors that might be involved in the perception of vibrotactile stimuli, Knibestol & Vallbo,(1970), report that four different types of mechanoreceptors are found in the glabrous skin of the human hand.

> "The four types seem to be present in the same skin area in sub-human primates."

They state:-

> "All available information indicates that each of the four types have, in broad, the same properties in man and monkey with regard to the organisation, size and shape of their receptive fields and their response to dynamic and static deformation. It seems therefore justified to infer that, in the human glabrous skin areas there are three types of intracutaneous mechanoreceptors with large diameter nerve fibres, one is rapidly adapting and two slowly adapting, and in addition there is one rapidly adapting type of ending which is very sensitive to skin deformation although it is located in the subcutaneous tissues. This receptor is very likely a Pacinian corpuscle. It should be noted that it is not claimed that the four groups of units are necessarily homogeneous. A deeper analysis might reveal that there are several types of receptors within the classes of endings suggested on the basis of the present analysis."

* * * *

Before drawing conclusions concerning specificity, neural coding and the number of receptors involved in human sensitivity to vibrotactile stimulation, it is necessary to comment on the 'frequency sensitivity' of the 'cutaneous' receptors. The data in Fig. A1/5, redrawn from some data presented by Talbot et al. are for the finger, with a small 'contactor' (3 mm. diameter, or less). Fig. A1/6 shows the data from some early papers, as presented in the third of the papers of Geldard, (A1 1).). More recently Verrillo, (1969, & 1970), reports on two ways of measuring the sensation magnitude as a function of the frequency of the stimulus. The values he gets using a contactor of 2.9 cm^2 stimulating the thenar eminence, are shown in Figs A1/7a and A1/7b.

The shape of the threshold response curve has been discussed by Sherrick, (1953), who concluded, on the basis of several experiments, that the

> "... selective sensitivity of the human observer to vibratory disturbances is a result of the mechanical characteristics of the skin and underlying tissues, assuming that the receptors do not themselves respond selectively to various frequencies of vibration ...".

He assumed that the shape of the threshold response curve is the result of resonance in the skin and underlying tissue. His work concerns sensitivity of the fingertip and tongue.

Gierke, (1957), reports a resonance frequency of about 35 c.p.s. for stimulation on soft body tissue. The resonance frequency appears to depend on the location and thus on tissue characteristics, (Moore, 1970). However, Sato, (1961), working on the Pacinian corpuscle in the mesentery in cats, finds the 'U'-shaped threshold versus frequency function in isolated corpuscles. He concludes his article by saying "The frequency-threshold relationship is explained on the basis of receptor potential behaviour." It can be concluded that the form of the 'frequency-function' is the result of the combination of resonances in the tissues stimulated and the properties of the end-organs concerned. This conclusion is, of course, by no means the 'last word' on the matter.

Another facet of the sensitivity to vibrations at different frequencies is that which led Verrillo to postulate that more than one receptor population is involved. (He was aware of the earlier work and of Geldard's papers, but does not, however, appear to have been aware of the mechanical explanation of the 'frequency-function'. (See the discussion of his paper (Verrillo, 1968).). This is the finding that the shape of the threshold response curve depends upon the area of 'contactor' used, and that at low frequencies the threshold is independent of contactor area. (See Fig. A1/8). Interestingly, Geldard in his third paper, (op. cit.) reports on an experiment in which he found that for a very small contactor area the 'frequency response' of cutaneous sensitivity is flat. Verrillo

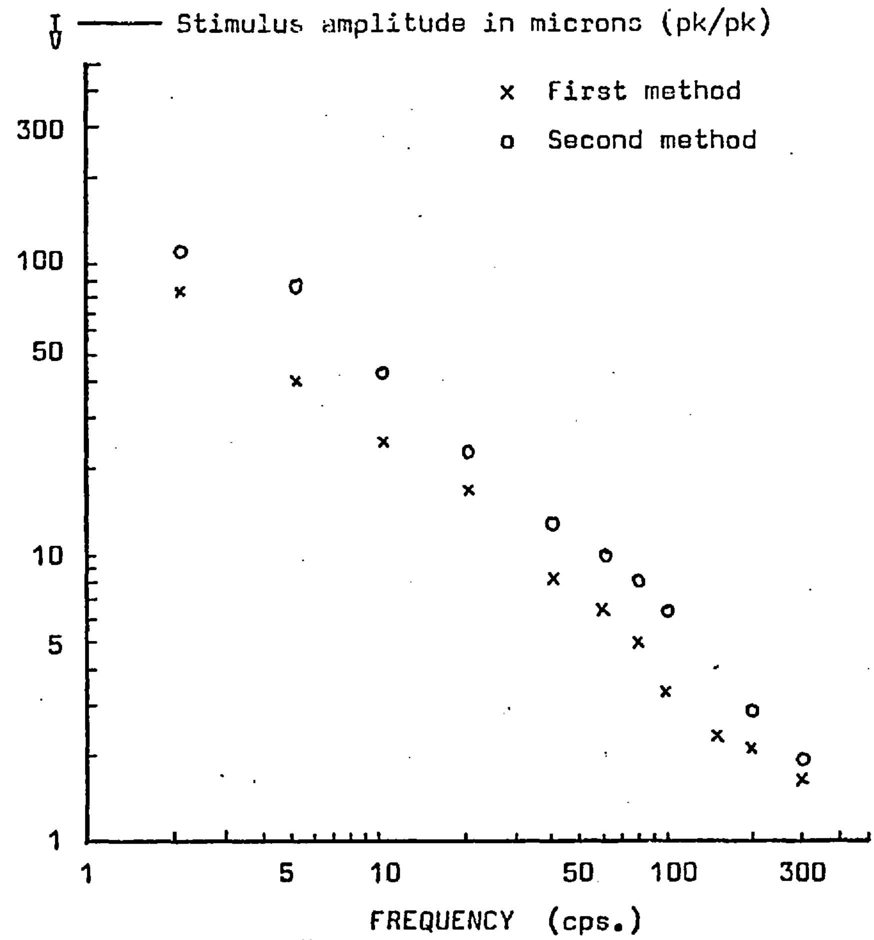


Fig. A1/5

Thresholds for the finger, (data from Talbot et al, 1968), obtained by two methods. The first method was one in which the subject indicated the threshold as the amplitude of the stimulus was continuously varied through it, (five times in each direction). The 'downgoing' values were lower and are shown. The second method required the subject to indicate, yes or no, whether he felt the stimulus, (different amplitudes of the vibration were presented every few seconds). No difference equivalent to that shown by method 1 was found. (10 subjects were used).

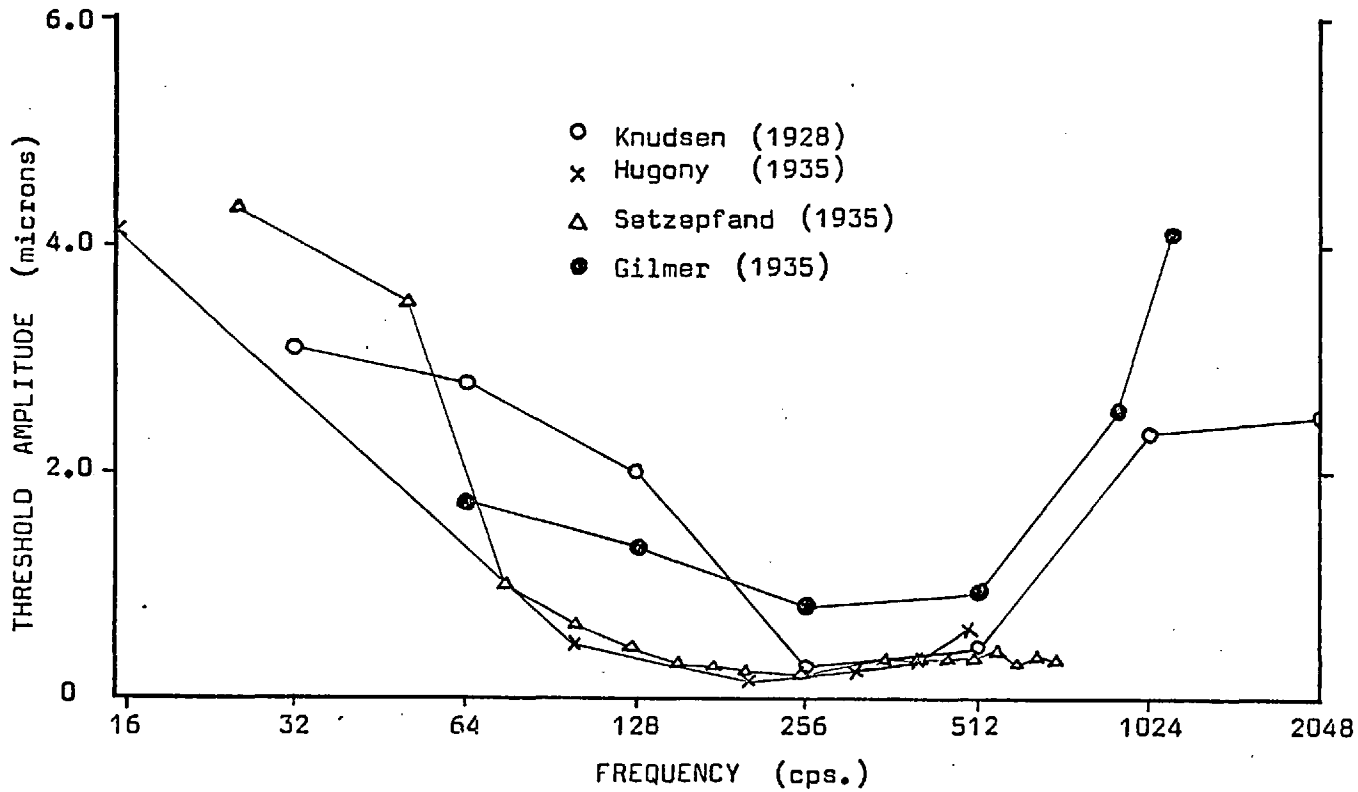


Fig. A1/6

Data, (as presented by Geldard, 1940), showing thesholds for the finger tip, as determined by four early workers. The stimuli were sinusoidal vibrations, and the contactors were 'relatively large'.

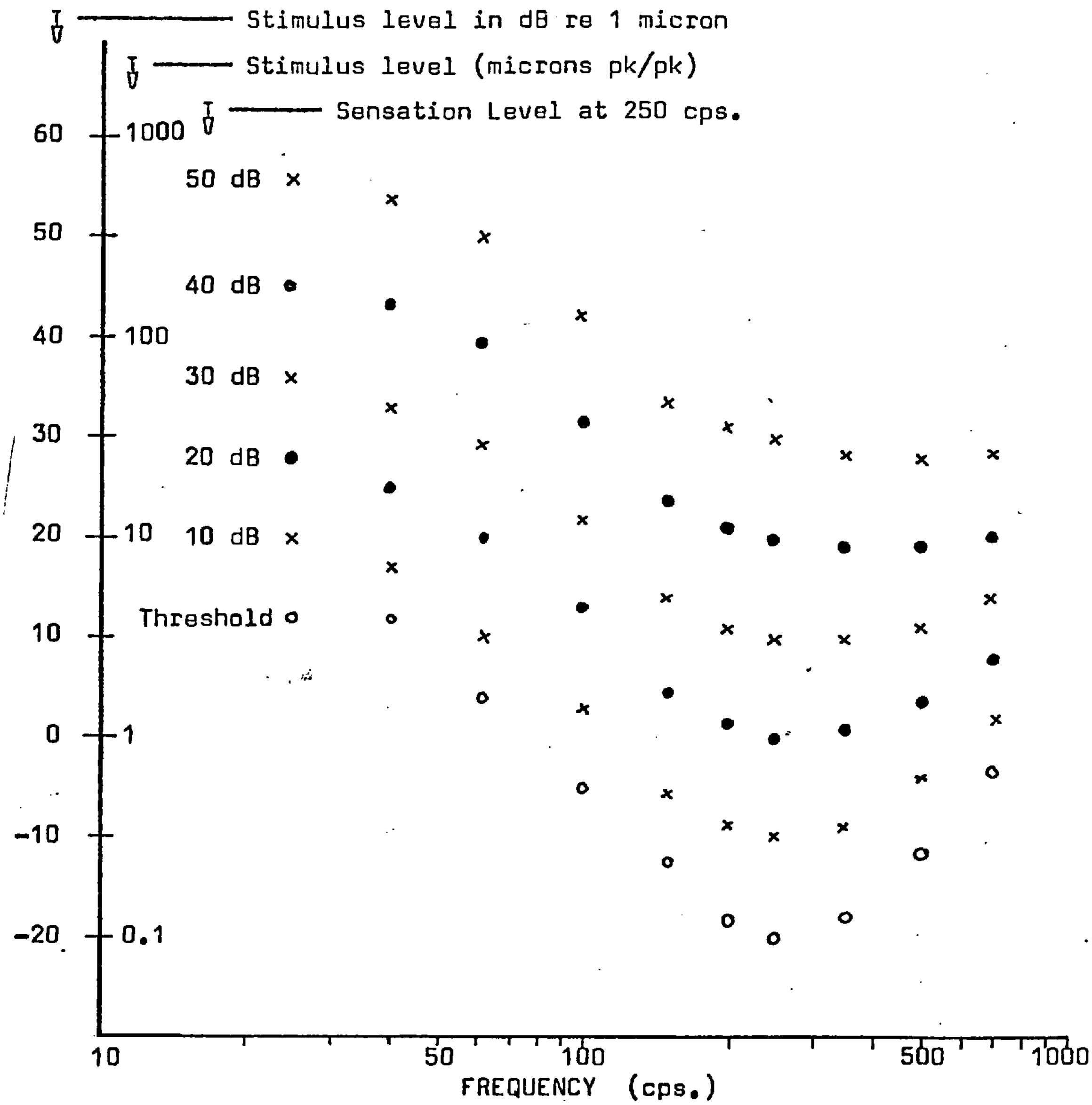


Fig. A1/7a

Contours of equal sensation magnitude derived from data obtained by the method of numerical magnitude balance. The geometric mean of the results of magnitude estimation (the assignment of numbers to presented stimuli), and magnitude production (adjustment of stimuli to match numbers presented), were used to produce the magnitude balance function. (From Verrillo, et al, 1969).

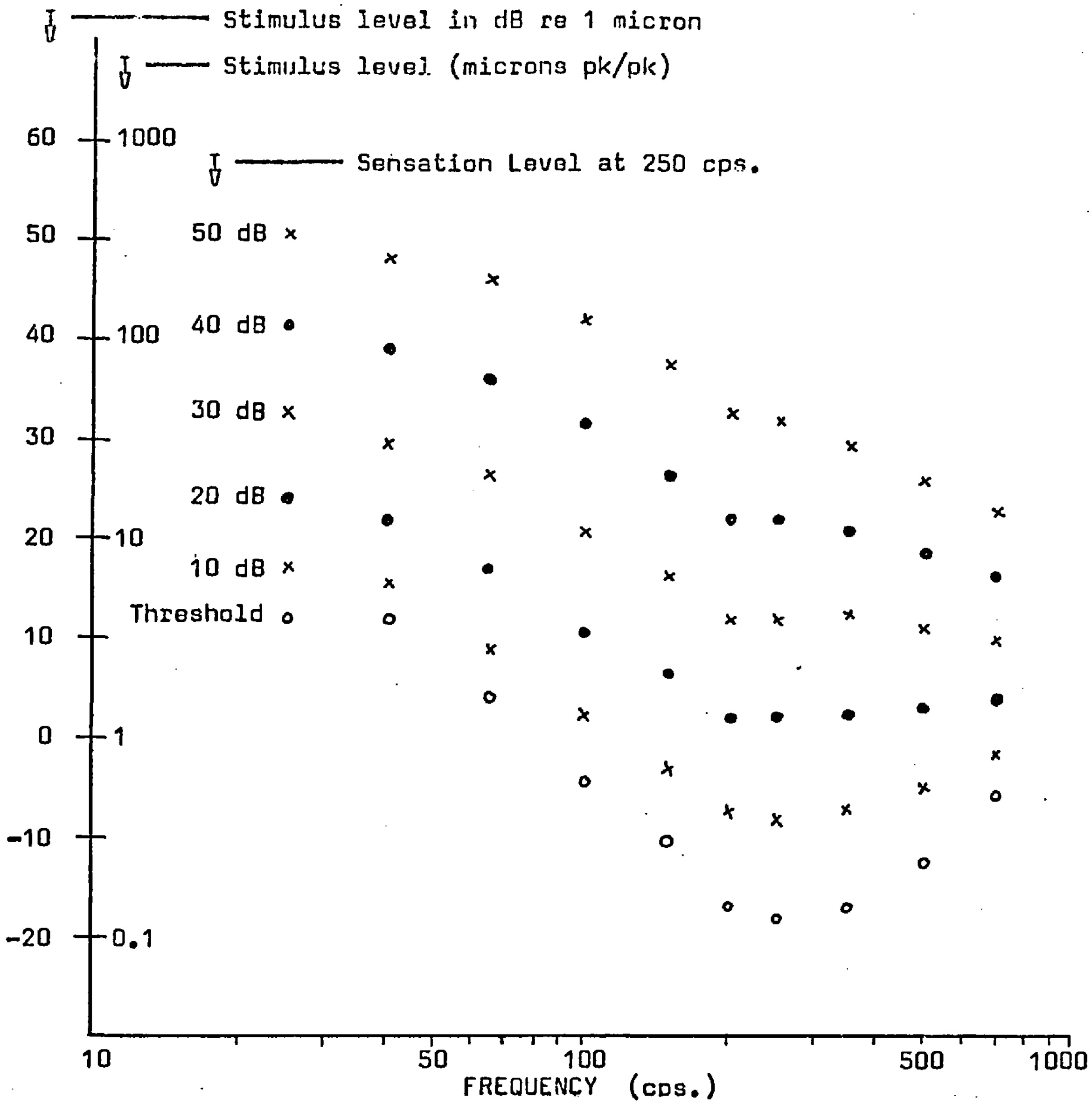


Fig. A1/7b

Contours of equal sensation magnitude derived from data obtained by the method of direct intensity matching. At two standard frequencies, (64 cps. & 250 cps.), 11 standard intensity levels above a subject's threshold were presented and then the subject asked to adjust the intensity of the stimulus, at a different frequency, to match that of the standard. (The procedure was then reversed and the intensity of the standard adjusted.) (From Verrillo, et al, 1969).

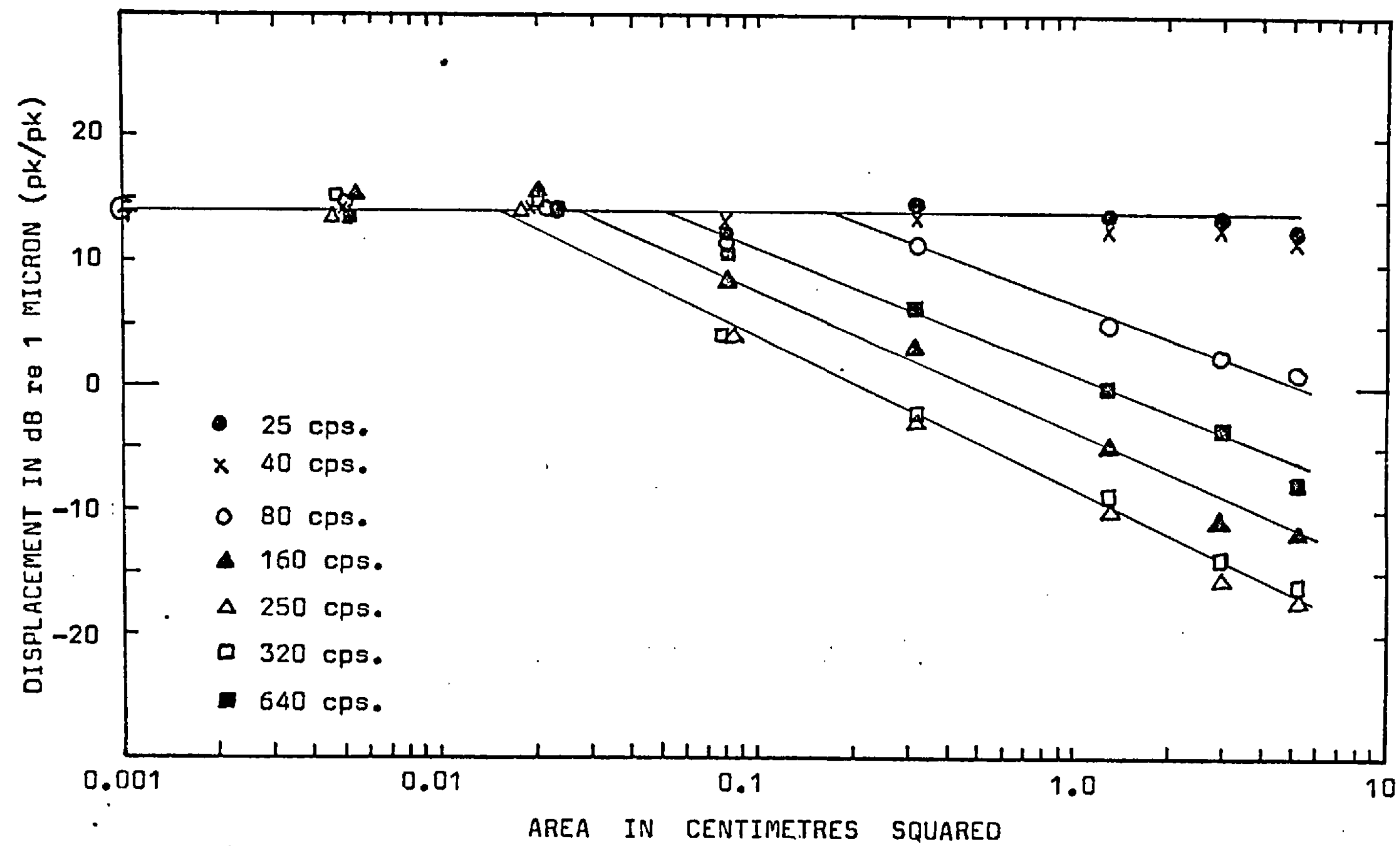


Fig. A1/B

Vibrotactile threshold as a function of contactor area. (From Verrillo, 1966). Data from Geldard, (1940), for the frequencies:- 64,128,256,512,&1024 cps., are shown, averaged, by G (the values lie between 20 & 30 microns). The area of Geldard's contactor is difficult to assess. A sharp needle with an area of about 0.02 sq.mms. was used. The data are shown as for an area of 0.001 sq.cms.

does not mention this. I have included Geldard's data in Fig. A1/8, (the data are for pressure spots on the volar side of the arm, near to the wrist).

A paper entitled "On the Tactile Perception of Vibration Frequency", by Joel,(1935,), is of historical interest. In this paper he argues that because amplitude of oscillation (mechanical) is not a measure of energy, (it requires more energy to move a given mass at x cms. peak-peak oscillation when the frequency is 450 c.p.s. than when it is 400 c.p.s.), it can not be possible to use as a separate parameter 'frequency' or 'amplitude'. He concludes that the experience of intensity depends on frequency, amplitude and energy, (see section 3a) 2) ,for a modern opinion concerning energy cues).

One other aspect of the 'frequency-function' of vibrotactile sensitivity is the 'upper frequency limit'. Sato,(op. cit.), finds that the Pacinian corpuscle responds with one spike per cycle over the range 40-1000 c.p.s. Collins,(1970), reports the 'flicker-fusion frequency' as greater than 400 c.p.s. (This is quite different from the frequency at which separate pulses become merged into a vibration. This is around 15 c.p.s., (Békésy, 1957).). He is therefore suggesting that at some frequency greater than 400 c.p.s. the sensation of 'vibration' will give way to something else, such as 'steady pressure'. This suggestion, with a figure somewhere between 350 and 450 c.p.s., depending on amplitude, is also made by Békésy,(1962). Other workers have put the 'upper frequency limit' much higher. Gault,(1927), mentions a figure of 2600 c.p.s. (See also the third paper of Geldard's 1940 set of 4). It is doubtful whether much meaning can be attached to many of these claims as the term used by Collins suggests. It appears that there are two frequencies at which 'flicker-fusion' takes place. The lower is not in doubt. Around 15 c.p.s. a vibratory stimulus becomes perceived as such and ceases to be perceived as a train of separate pulses, (the frequency depends slightly on the spot stimulated, see Békésy, op. cit.). At some higher, and as yet undetermined, frequency, there may be a change in the nature of the sensation from 'vibration' to 'steady pressure' or some other quality. This does not mean that the stimulus is not felt but merely that it would not be described as vibratory. (It should be pointed out the vibratory stimuli employed are almost exclusively sinusoidal in character).

It is instructive to look at the way in which the sensitivity to vibration is discussed in the literature. As already mentioned Békésy is concerned about the ability to discriminate frequencies vibrotactilely. Crawford & Copeland Barnes,(1966), discuss tactile sensitivity under such headings as:- Frequency, Intensity, Waveform, Amplitude, Duration, Interval, Threshold. They are concerned to describe 'discriminable dimensions'. Terms like 'frequency-function', 'frequency-response' and so on, occur sporadically throughout the literature, which isn't to say that they shouldn't but only to draw attention to reasons behind their precise form. They are useful because they are shorter than any more thorough alternative, but they are misleading for the unwary.

The fact that increasing amplitude results in a decrease in pitch, (perceived frequency), obviously worries Békésy, and Goff,(1967), in a paper called "Differential discrimination of frequency of cutaneous mechanical vibration" finds it necessary to allow for the changes in 'subjective intensity', (i.e. subjective amplitude, or intensity), which accompany changes in frequency, to remove the intensity 'cues'. Procedurally this is very thorough, but I consider that it has very little to do with real stimulation or sensitivity.

As recently as 1970, Lindblom,(1970), discusses vibrotactile sensitivity in terms of Frequency discrimination and Spatial discrimination. He also points out that the Quickly Adapting afferents fire at a rate proportional to the rate of deformation. It is not possible to relate the 'single-shot' stimuli used to determine this to the 'frequency-function' determined with vibrations. It is probably valid to refer to the frequency response as the 'frequency-function' and not as the 'rate of deformation function'.

* * * *

In conclusion, it is apparent that the mechanisms by which mechanoreceptors operate are little understood. A synthesis of Kenshalo & Nafe's hypothesis and of Frey's specificity hypothesis is probably the most acceptable conjecture concerning the modus operandi of the mechanoreceptors.

This is necessary because, contrary to Nafe's, and Kenshalo's reports, receptors do exist which respond to steady deformation of the skin. (See Knibestol & Vallbo, op. cit., Talbot et al. op. cit., and Chambers et al. op. cit.). I consider that deformation of the skin in the general sense (i.e. dynamic <u>and</u> static), is the necessary stimulus. The various nerve endings and the tissues surrounding them combine to provide an extraordinary sensitivity and subtlety of sensation. It is not necessary to try to answer the point made by Moore,(1965). This is that it was not possible, (in 1965), to say whether specific receptors exist for specific qualities or universal receptors exist with quality of sensation "dependent on spatio-temporal relations in discharge patterns." There is no reason, (and never was in my opinion), to suppose that tactile sensitivity is due to one <u>or</u> other of these possibilities.

The existence of different types of nerve endings has been known for a very long time. To propose that they play no part in tactile sensitivity is surely a little extreme. Similarly, there has been considerable evidence to suggest that more than one type of receptor is involved in vibrotactile sensitivity, available almost since the debate started. To take so long, as the research has done, to show that two or more receptor populations are involved is extraordinary.

Neither is it necessary to invoke spatio-temporal relations in the discharge patterns as the sole mechanism by which the qualities of sensation are perceived. (Uttal & Krissoff, op. cit., criticise the indeterminate nature of the much used 'spatio-temporal').

Melzack & Wall, in their paper,(op. cit.), take this view. In their summary they write:-

> "Although the 'specific-modality' and 'pattern' theories of somesthesis appear to be mutually exclusive, we believe that both contain valuable concepts that supplement one another. Recognition of receptor specialisation for the transduction of particular kinds and ranges of cutaneous stimulation does not preclude acceptance of the concept that the information generated by skin receptors is coded in the form of patterns of nerve impulses."

They continue:-

> "We believe that the assumption that each psychological dimension of somesthetic experience bears a one-to-one relation to a single stimulus dimension and to given type of skin receptor is the source of many of the controversies surrounding von Frey's theories."

The first part of this assumption is still made by many workers and, in my view has led to much wasted effort.

As regards tactile sensitivity in general I conclude that the discharges from various receptor populations, (which may not be homogeneous as Knibestol & Vallbo pointed out), must to some extent be identifiable, by the brain, with their source. The patterning of the discharges, which undoubtedly does take place, coupled with this 'knowledge' of the source is all that is required for the different sensations, (both qualitative and quantitative), to be perceived as such.

Concerning sensitivity to vibration, the above applies, but there remains the 'problem' of the drop in pitch with increased amplitude of stimulation. Talbot et al. conclude in their discussion of Békésy's finding that the drop in pitch is due to disorganised firing of the receptors at the higher amplitudes. They find it difficult to understand, especially, that it is a drop in pitch and not a rise.

It is not difficult to understand perceptually, however. Just as Geldard's set of papers set the theme of discussion about vibrotactile sensitivity, so has the physicist's and psychologist's classification of phenomena influenced the terms used, (as it influenced Frey).

Why should the cutaneous mechanoreceptors be capable of making, or mediating, fine frequency discriminations? Why should frequency be signalled to the higher centres with the fidelity we expect of the ear?, (although the latter is still not fully understood). Why should amplitude and frequency be separately discriminated cutaneously?, just because signal generators have two control knobs, one marked frequency and the other amplitude? Surely 'discriminable dimensions' are only going to exist where naturally occurring stimuli, of value to the animal concerned, have to be discriminated. The 'dimensions' will be dictated by the stimuli.

Thus to understand Békésy's finding we must turn to natural phenomena. What purpose can 'vibratory sensitivity' serve an animal?, and, seeing that we are mainly concerned with primate hands, (because apart from the tongue they are the most sensitive), we will constrain the discussion accordingly. The answer must be 'sensitivity to texture'. Although this can to some extent be appreciated visually, the staggering sensitivity of the fingers, and hand generally, indicates that sight is not good enough. In poor lighting conditions visual judgement will be inferior to tactual judgement,(Brown, 1960). (By poor is meant anything other than optimum).

Texture is a composite quality dependent on, at least, roughness, stickiness, dryness, hardness, sharpness, temperature, heat capacity and cues from vision and hearing. It is not, therefore, surprising that scientifically convenient labels for different aspects of stimulation of the skin prove unsuitable when the sensitivity of the skin is being discussed. I would suggest that the sensation of pitch decreases with amplitude of stimulation because that is, perceptually, what the human expects. This is simply because finer surface irregularities will be smaller in amplitude. It must be borne in mind that, outside the laboratory, vibrations will only be set up in the skin when the hand is moved across the surface of an object, (assuming the object is not vibrating).

Stimuli which 'defy the natural order of things' are a product, largely, of the 'industrial age of man'. In terms of evolutionary pressures it is difficult to conceive of a 'natural' stimulus which can be felt as a 'texture' by a stationary hand, (although this is perhaps possible such stimuli must be a small subset of all natural stimuli. I'm thinking here of the sensation experienced when tightly gripping a textured object). The temptation to discuss the quality of sensation has proved irresistable to others, (see for example Uttal & Krissoff, op. cit.), but, unlike many discussants I am concerned not with the 'specific' versus 'patterning' controversy, but with the general mechanisms of touch and pressure sensitivity.

Knibestol & Vallbo,(op. cit.), note that the receptor population in the monkey hand is very different from that found in the human hand. In the latter, slowly adapting receptors account for perhaps 75% of the receptor population, whereas in the monkey hand quickly adapting receptors account for about 80% of receptors. After discounting the possibility that this difference is an experimental artifact, they go on:-

> "The difference is remarkably large and it might be relevant to consider the possible implications of this finding. The higher proportion of slowly adapting receptors in man suggests that more accurate information of time invariant tactile stimuli is extracted by the human receptor population compared to that of the monkey. It is plausible that this implies an improvement of the hand, as an organ for tactile exploration, which has occurred during the phylogenetic development."

This finding does not invalidate the work of Talbot et al., but emphasises the fact that in order to solve the problems of sensitivity to vibration and other mechanical stimuli, more neurophysiological work must be done on humans. In addition, I cannot refrain from the comment that the problems are, in part at least, due to the lack of any consideration, on the part of experimenters, of the nature of natural stimuli. I consider that a great deal of speculation would have been avoided if effort had gone into defining 'natural stimuli' in laboratory terms, (making possible repeatable natural stimulation). Moreover, confusion would not have been so great if more work had been done with humans.

The goal of scientific enquiry would appear to be categorisation and description of the world in which we find ourselves, but we, as scientists, must ever be aware of the probability that we favour some descriptions over others because they fit our scheme of things. If Frey had asked himself why specificity was necessary, and what natural stimuli are, we might never have been presented with the foregoing mosaic of fact and speculation.

A1 3). This section concerns direct electrical stimulation of the skin.

The possibility of using electrocutaneous stimulation is assessed. The assessment shows that for coded information a set of conditions for successful stimulation does exist. It is suggested that electrocutaneous stimulation would be unsuitable for situations in which it is necessary to vary the stimulus continuously along a sensation scale.

Al 3).

There is some interest in electrocutaneous stimulation for communication aids, (~~this is discussed in Chapter 5~~), but most systems so far devised use codes.

The simple reason for this is illustrated by one of Békésy's findings,(1957). With electrocutaneous stimulation he describes three subjective qualities, (these occur simultaneously):-

> "1) a vibration sensation similar to the mechanical one,
> 2) a feeling of push or pressure, 3) a prickle."

The electrical stimulation was alternating current and as the frequency increased the 'vibration sensation' became more localised, the prickle sensation moved deeper beneath the skin, and the constant pressure sensation spread over the surface of the skin.

Electrocutaneous stimulation is indiscriminate. Any nerve in the locality of the electrode can be stimulated. Care is therefore required to avoid sensations of pain, whilst stimulating the skin. The simplest way of doing this is to use codes constructed from pulses the characteristics of which have been carefully selected so as to optimise the sensation intensity (the pulses are merely detected, their amplitudes do not vary), whilst minimising the likelihood of causing pain. (Electrode configuration is also important in this respect,(Gibson, 1968)). The conditions Gibson reports as being necessary are:-

> "Brief, (0.5 msec.), pulses of anodal direct current." They "can reliably arouse pain-free touch when delivered through sufficiently large electrodes by a constant current stimulator to ensure that current intensity does not vary with tissue impedence. Such pulses may effectively stimulate touch without pain when combined in short trains at low pulse repetition rates, because the tempral integration time is shorter for touch than for pain. Pulse repetition rates can be readily manipulated at low values, (5-50 c.p.s.), within the region of highest sensitivity to rate changes."

Collins,(op. cit.), reports on a vision substitution system employing electrical pulse trains delivered to a matrix, (20 x 20), of stimulators on a persons back. Stimulus currents of 5 to 10 ma. are used. Four pulses 20 microsecs. long and at 2 msecs. separation are used as a stimulus event, and these events occur at a rate of 25 per second.

An interesting electrotactile display is reported by Strong & Troxel,(1970). They report a texture sensation that is felt by subjects when they move one of their fingers over an array of closely spaced electrodes, (the 'indifferent' electrode being rested on by the heel of the hand). The electrodes were 0.07" in diameter and spaced on 0.1" centres. The maximum extent of the array was 1" x 1.8". The pulses driving this array came from a source with 200 kilo-ohm output impedance, and the pulse was symmetrical with a half-width of 0.06 msec and the repition rate was 200 p.p.s.

If the subject's hand was not very dry the sensation was uncomfortable and deep within the fingers. They report:-

> "This sensation apparently has a mechanism more directly related to the peak stimulus current than to the voltage applied between the electrodes." They go on, "A completely different sensation was experienced by most subjects when their fingers were dry and had, therefore, a high skin resistance. If the subject brushed his finger lightly over the surface the surface appeared to acquire a texture, which could be varied by varying the stimulus parameters. This sensation bears many of the properties of an ordinary texture sensation, the most important being that it is a relatively small amplitude effect, and that it disappears in the absence of finger motion. The sensation does not seem to be at all unpleasant, and its qualities can be varied quite a bit by changing either the pulse repetition rate or the peak voltage. The mechanism for this 'texture' appears to be directly related to the peak stimulus voltage, not to the stimulus current."

They do note two disadvantages. The first is that some sort of 'warm-up' period is required by subjects before they can reliably feel the texture. They attribute this to 1) skin resistance increasing as the skin dries, and 2) subject's adjustment of contact pressure. The second drawback is that the sensation tends to fail or become sporadic. They suggest that this "appears to occur on days when the subject is, for unknown reasons, unable to increase his skin resistance sufficiently."

This sensation appears to be that felt when one moves ones fingers lightly over a 'live' piece of equipment, (i.e. one where exposed metal is connected to the live terminal of the mains supply). The high resistance of the return path to earth is presumably sufficient protection, (of course one must not touch anything connected directly to earth), and one must have dry hands.

A third form of electrical stimulation is discussed by Moore, (1968), in a paper entitled:- "Vibratory stimulation of the skin by electrostatic field: Effects of size of electrode and site of stimulation on thresholds.". He writes:-

> "... a force can be applied between the body surface and an electrode not in direct contact with the body, the body surface acting as one plate of a parallel-plate capacitor and the electrode as the second plate. In this situation the force is generated by the application of a high D.C. biasing voltage across the two 'capacitor plates', with the result that the skin is attracted or displaced towards the electrode. The D.C. biasing voltage may then be modulated by imposing an A.C. voltage whose frequency and amplitude may be varied by the experimenter."

This method has the advantage over the previously discussed methods, in that the modulating voltage can be at different frequencies. Moore measured the threshold at three different locations, with three different electrode areas, and at several frequencies. His values are shown in Fig. A1/9. He did not make measurements above the threshold level.

The possibility of replacing conventional vibrotactile stimulators with electrostatic ones is remote, however, because, the voltages required are so large. The D.C. biasing voltage referred to above was 1000 - 1600 volts for the finger, and higher still for the other locations, and the modulating voltage was large, ("At no time was the peak-to-peak value of the modulating voltage allowed to exceed half that of the biassing voltage.").

Apart from the above case, which is not electrocutaneous stimulation in the strict sense, electrocutaneous stimulation is likely only to be of value where coded information is being received via the integument. It has one advantage over mechanical stimulation, and that is the fact that it can easily be made light-weight and portable.

It suffers, in my opinion, from the serious drawback of being almost completely unknown in nature. Thus investigation of the sensations evoked, although attempted,(Uttal, 1958, Vernon, 1953, Anderson & Munson, 1951, and more recently,Gibson & Tomko, 1972, Hawkes, 1960, and Hawkes & Warm, 1960), does not show electrocutaneous stimulation to be of use where a sensation scale is required.

For example, Hawkes,(op. cit.), discusses identification of intensity level. He determines the effect of current intensity on estimated magnitude and finds, as have others, a very steep increase in

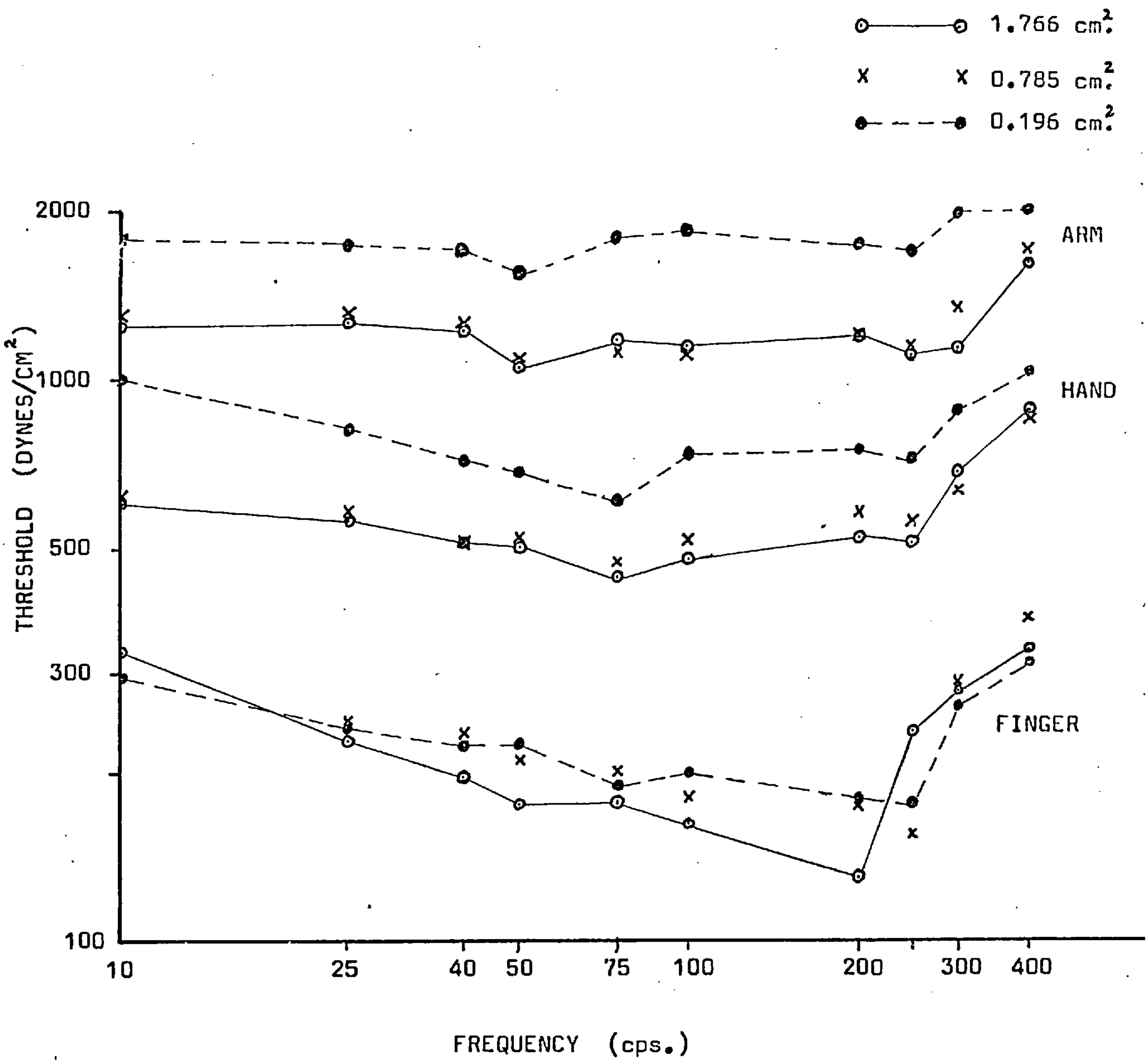


Fig. A1/9

Effects of the size of the electrode, and the site of stimulation, on the Threshold-Frequency function; using electrostatic fields to stimulate the skin. (From T.J.Moore, 1968).

estimated magnitude for a doubling of current. He concludes that "two intensity levels would be useful, (and would give 100% accuracy), in a communication system using a code based on different levels. Three levels would "maximise transmission of information" but would introduce some error.

Hawkes & Warm,(op. cit.), report on the current levels which elicit pain and find that at 1000 c.p.s. A.C. stimulus for example, threshold for 'tingle' is at about 0.8 ma., threshold for pain is at about 2 ma., and the tolerance limit is at about 3 ma. They note "... that the energy ranges determined are still considerably less than comparable ranges for vision, audition, or mechanical vibration of the skin." This in itself is not a drawback but it makes the stimulus less useful than it might otherwise be by necessitating determination of thresholds every time the stimulus is to be used, and then adjusting the stimulus levels accordingly. This would be for a situation where electrocutaneous stimulation was being employed as a stimulus along a scale of sensation.

A set of conditions for using electrocutaneous stimulation in an 'analogue' manner, rather than a 'binary code' (stimulus present or not present) manner, may well be discovered, but my inclination away from such stimulation, and towards mechanical stimulation, is strengthened by the additional complexities concerning administration and long term side effects. Electrode creams have to be applied, discomfort might be difficult to avoid and there is the possibility of undesirable side effects and changes in the skin condition. In addition, there is the possibility of psychological reaction against such stimulation, by the subjects employed for experiments.

Altogether I consider such stimulation unnatural and unsuitable for use in a system which relies on a sensation scale, or 'analogue' presentation of information. I would be very hesitant before using any sort of electrocutaneous system, (coded or otherwise), in any long-term experiment.

SUPPLEMENT TO APPENDIX 1

I consider that, despite the lack of data for changes in pitch caused by changes in amplitude, (i.e. equivalent to the families of graphs shown in Fig. A1/7), it seems likely that increasing amplitude and frequency together could provide a useful scale of sensation.

Although the effect Békésy reports is not surprising for the reason I outlined in the Appendix, there does exist a simple physiological explanation as well.

Fig. A1/3 shows the frequency function for the two types of receptor thought by Talbot et al. to mediate sensitivity to vibration. These functions are averages for several fibres for one-to-one synchrony between firing and stimulus. Qualitatively, it is not difficult to accept that up to a frequency of about 40 c.p.s. increasing the amplitude of stimulation will result in a rise in pitch.

This will come about because the threshold for synchrony for the Pacinian corpuscles is higher than the threshold for the other receptors. In an informal way therefore, this will result in the increase, proportionately, in the signalling of 'high pitch', (i.e. the proportion of signals from Quickly Adapting Afferents will increase). I am assuming the simplest possible description of events, namely, that pitch is categorised at the receptor level into two levels, 'high' and 'low', and that interaction combines these signals. The receptors have the required non-linearity for such mixing to produce the desired effect.

As the frequency increases I would suggest that the sensation of pitch drops with increasing amplitude, the effect becoming less pronounced above perhaps 100 c.p.s. and perhaps disappearing above 200 c.p.s. Moreover, it could be predicted on the basis of this very crude model that as the amplitude became large the effect would diminish, (provided an isolated stimulus location was chosen, perhaps the finger or a site on the forearm with a surround to prevent travelling waves).

Bearing in mind the finding of Knibestol & Vallbo, that the human hand has a very much higher proportion of Quickly Adapting Afferents than Pacinian afferents, (thus giving more 'weight' to the 'low' pitch signals, comparatively), it might be expected that the influence of the 'high' pitch signals would be less than half the total signal. (The above model assumed equal weighting of the two signals). Thus the frequency of onset of the drop in pitch with increased amplitude would still be 40 c.p.s. but the effect would probably not become large until some higher frequency, perhaps 100 c.p.s.

Earlier, (in the Appendix), I quoted Békésy as saying "... the continuous rise in the vibratory pitch with increasing amplitude, which the electrophysiology would predict, holds only at low frequencies and at small sensation levels above threshold. The drop in pitch when the amplitude is increased at high levels is unexpected." To put figures to this, he reports that up to about 100 c.p.s. an increase in amplitude above threshold will produce a slight rise in pitch. The amplitude range over which this happens is larger for the lower frequencies, as my 'model' would suggest.

He reports a drop in pitch with increased amplitude at all frequencies (10, 40 and 100 c.p.s.), (with the above qualification for the lower frequencies). Thus at 10 c.p.s., after the initial rise in pitch, increasing the amplitude still further produces a drop in pitch. The rate at which the pitch drops decreases a little at higher amplitudes, and is less for the lower frequencies.

My 'model' is not able to cope with the decrease in pitch at lower frequencies except by invoking stimulation of increased numbers of 'low' pitch receptors, resulting from increased levels of stimulation. Also, as Talbot et al. point out, at higher levels of stimulation the frequency signalling of the Pacinian corpuscle becomes disorganised, (at amplitudes lower than those required for this effect to be seen in the Quickly Adapting Afferents).

The situation is obviously more complicated than the model suggests, but nevertheless it does offer a general framework of explanation.

I must state here that the model was conceived after the psychophysical experiments had been performed, and therefore it does not play any part in my decision concerning the stimulus. I refer to it in subsection 3 a) 1). to show that an explanation for the effect can be offered.

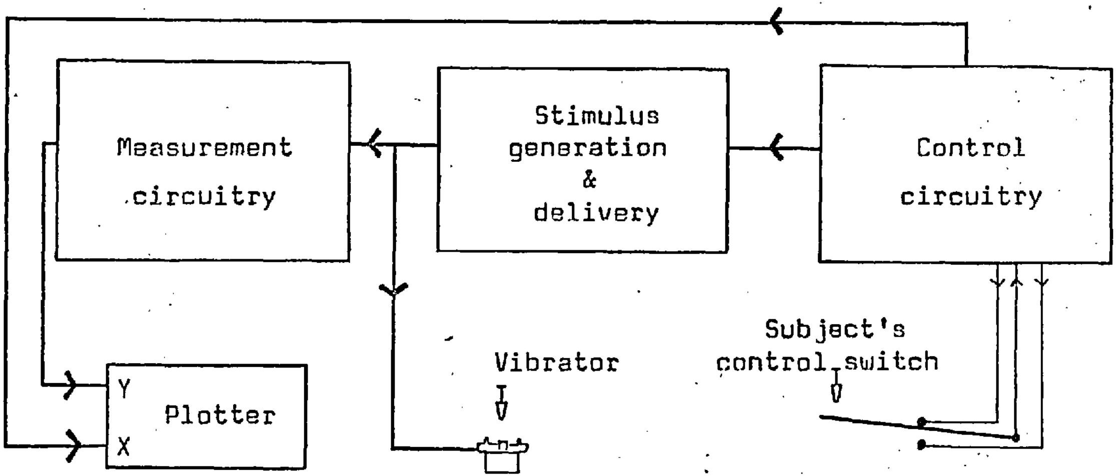

Measurement
circuitry
Stimulus
generation
&
delivery
Control
circuitry
Y
X
Plotter
Vibrator
Subject's
control switch

Fig. A2/A

APPENDIX 2

This Appendix contains details of the apparatus used to make the measurements reported in Part 3 of the thesis. The apparatus is conveniently described in four sections.

1) Generation of the stimulus.
2) Generation of signals for controlling the stimulus.
3) Measurement of the stimulus.
4) Delivery of the stimulus.

The drawing below shows, schematically, the relations between these four functions and the subject.

1).

The circuit used for generating the stimulus is shown in Fig. A 2/1. No attempt was made to find the best possible circuit, if such a thing can be said to exist. The circuit was designed to deliver to the vibrator a signal of variable frequency and amplitude. It was necessary to control both the frequency and amplitude by means of voltages. The amplitude and frequency had to be independently controlled, and it was necessary to use a circuit which permitted instantaneous change in frequency and/or amplitude.

In addition, it was decided to attempt to match, in some sense, the nature of the 'intensity' scale and the nature of the physical amplitude, (and in some cases frequency), range. This was made possible by the discovery that the output of the oscillator was 'logarithmically' related to the control voltage. This relationship is shown in Fig. A 2/2, the data for two widely spaced days being shown to demonstate the consistency of the relation. The cause of the difference between the slopes of the two sets of data is not known, (it is possibly due to differences between the two F.E.T.s). No attempt was made to reduce the difference.

The circuit is based, (as are all the oscillators used in the circuits of the control voltage generator and the speech training aid itself), on a simple and versatile bi-stable oscillator built around two operational amplifiers. The principle of operation is illustrated in Fig. A 2/3 below.

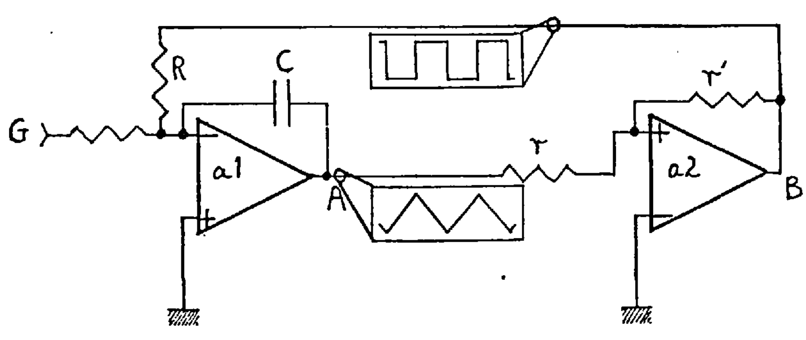


Fig. A2/3

A steady voltage x (negative, say) at point B will result in a 'positive going' ramp appearing at A. The voltage at A will increase linearly with time as the capacitor C charges, until it reaches a value y given by

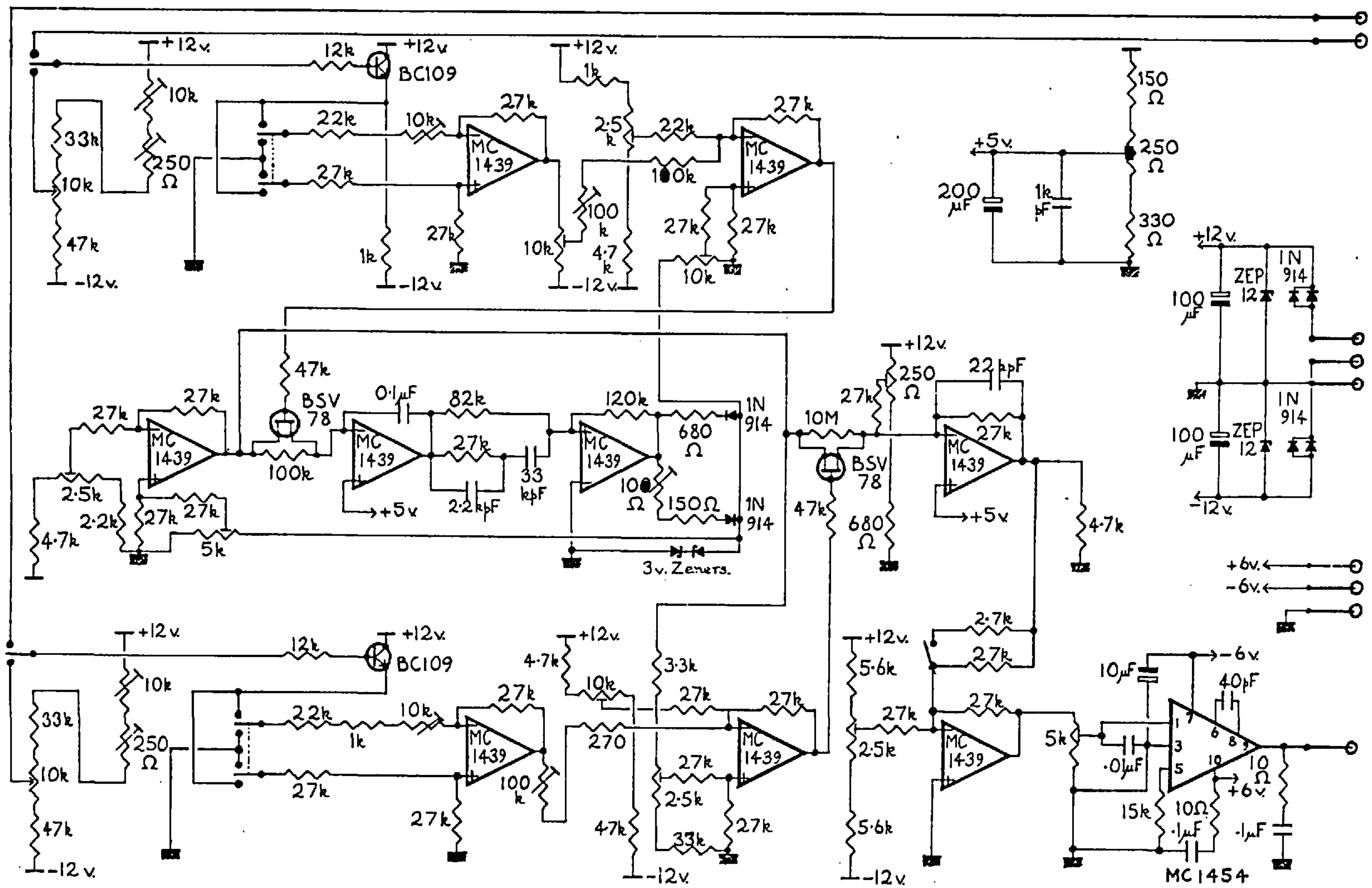

+12v.
12k
BC109
10k
250 Ω
33k
47k
-12v.
22k
27k
MC 1439
1k
2·5 k
100k
4·7 k
+5v.
200 μF
1k pF
150 Ω
330 Ω
ZEP 12
1N 914
100 μF
47k
BSV 78
0·1μF
82k
120k
680 Ω
2·2kpF
33 kpF
150Ω
3v. Zeners.
2·5k
2·2k
4·7k
5k
10M
22kpF
+6v.
-6v.
3·3k
270
2·7k
5·6k
10μF
40pF
·01μF
15k
10Ω
·1μF
MC 1454

Fig. A 2/1.

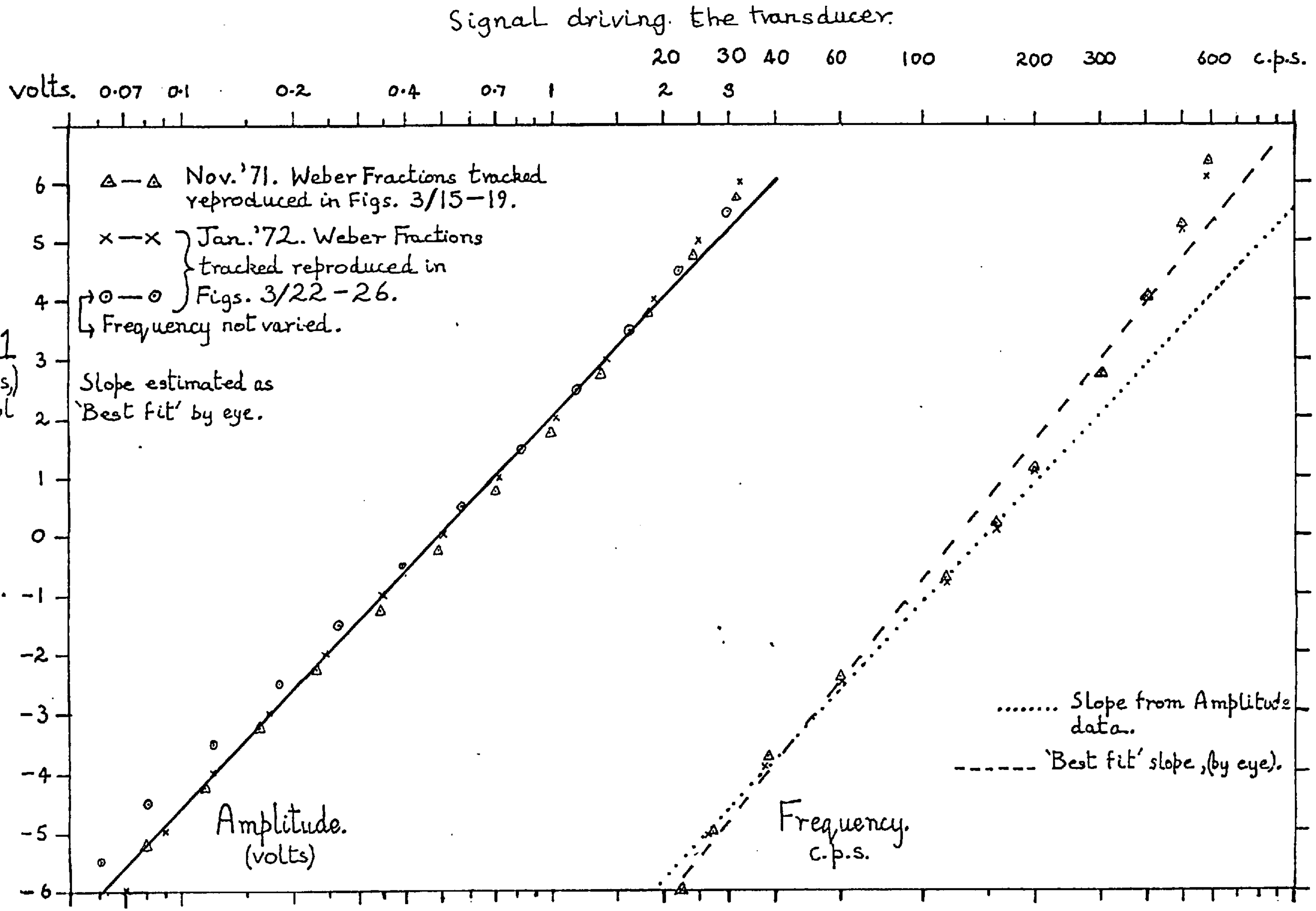

Signal driving the transducer.
20 30 40 60 100 200 300 600 c.p.s.
volts. 0·07 0·1 0·2 0·4 0·7 1 2 3
6 5 4 3 2 1 0 -1 -2 -3 -4 -5 -6
Oscillator 1 output, (volts) used to control X of plotter.
△—△ Nov.'71. Weber Fractions tracked reproduced in Figs. 3/15–19.
×—× Jan.'72. Weber Fractions tracked reproduced in Figs. 3/22–26.
⊙—⊙
Frequency not varied.
Slope estimated as 'Best fit' by eye.
Amplitude. (volts)
Frequency. c.p.s.
.... Slope from Amplitude data.
------ 'Best fit' slope, (by eye).

Fig. A2/2.

y = -rx/r' + dy, (where dy is some very small voltage dependent on the open loop gain, and other characteristics, of the amplifier).

The voltage at B will then rapidly change to become ~~negative,~~ positive, causing the voltage at A to decrease linearly with time. The circuit can thus be seen to be an oscillator, and the period T in seconds is given by

T = 2RC (with R in Ohms and C in farads).

The Frequency = 1/2RC in kilocycles per second, if R is in kiloohms and C is in microfarads.

The output can be taken from A if a 'triangle-wave' is required, or from B if a square-wave is required.

As the oscillator is a bi-stable state oscillator it does not have a resonance frequency and its period can be changed almost instantaneously if necessary. A varying voltage at G will change the 'mark-to-space' ratio and changing R or C will change the frequency. A change in r or r' will change the amplitude of the voltage at A required to change the state of a2, and hence the frequency. The voltages at B are governed by the operational amplifier's characteristics, but they will be approximately equal to the supply voltages to the amplifier. For this reason r must be less than or equal to r'.

To make the oscillator voltage controlled R is replaced by a Field Effect Transistor. With a suitable circuit environment and appropriate biasing conditions the F.E.T. will act as a voltage controlled resistor and the characteristic of the transistor is such that the effective resistance varies 'logarithmically', (in fact antilogarithmically), with the voltage applied to the gate. Thus the frequency is 'logarithmically' related to the controlling voltage. (The relationship is good for the limited range for which it is employed, see Fig. A 2/2).

The voltage control of amplitude was achieved by using a F.E.T. as the input resistance for a simple amplifier, (using an operational amplifier). The gain is directly varied, again with a 'logarithmic' response, by the voltage applied to the gate.

The voltage applied to the gates of the F.E.T.s are supplied by amplifiers which allow adjustment of the D.C. levels of the control voltages, addition of suitable fractions of the signal voltage, (to compensate for what would otherwise be changes in the biasing conditions), etc.

The D.C. level of the signal voltage, (i.e. the oscillation), is also important.

*　　*　　*　　*

2).

The controlling signals are generated by three oscillators of the type described in 1). They are provided and combined as follows:-

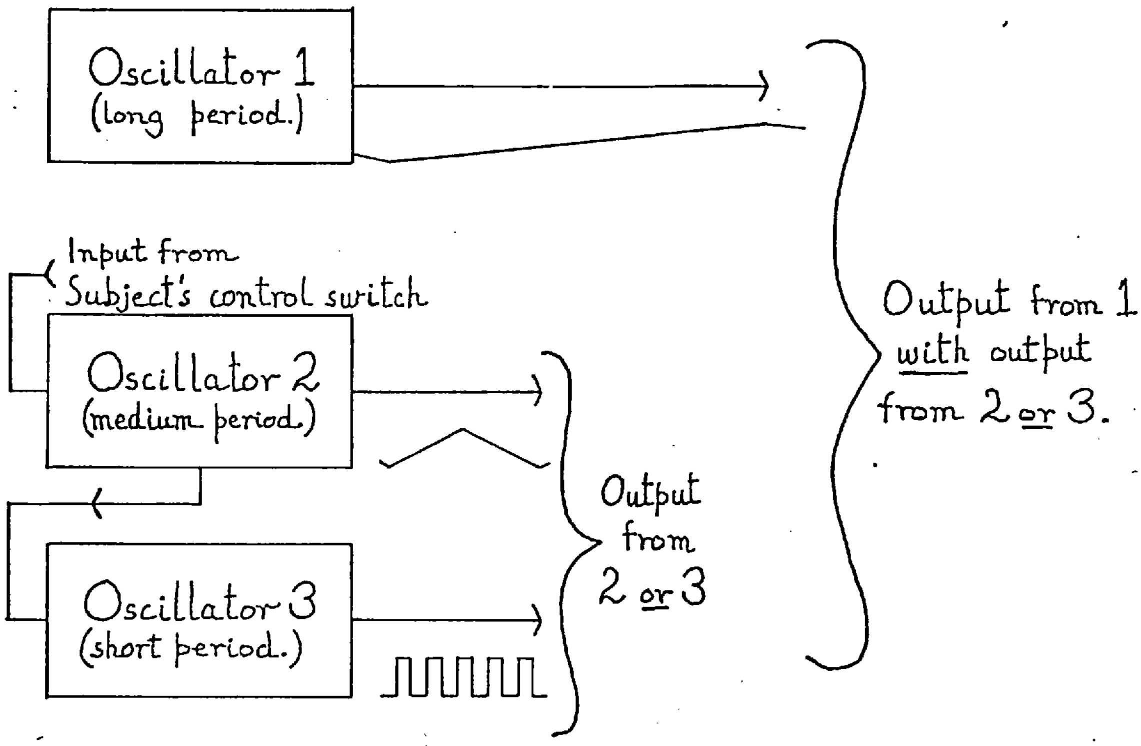


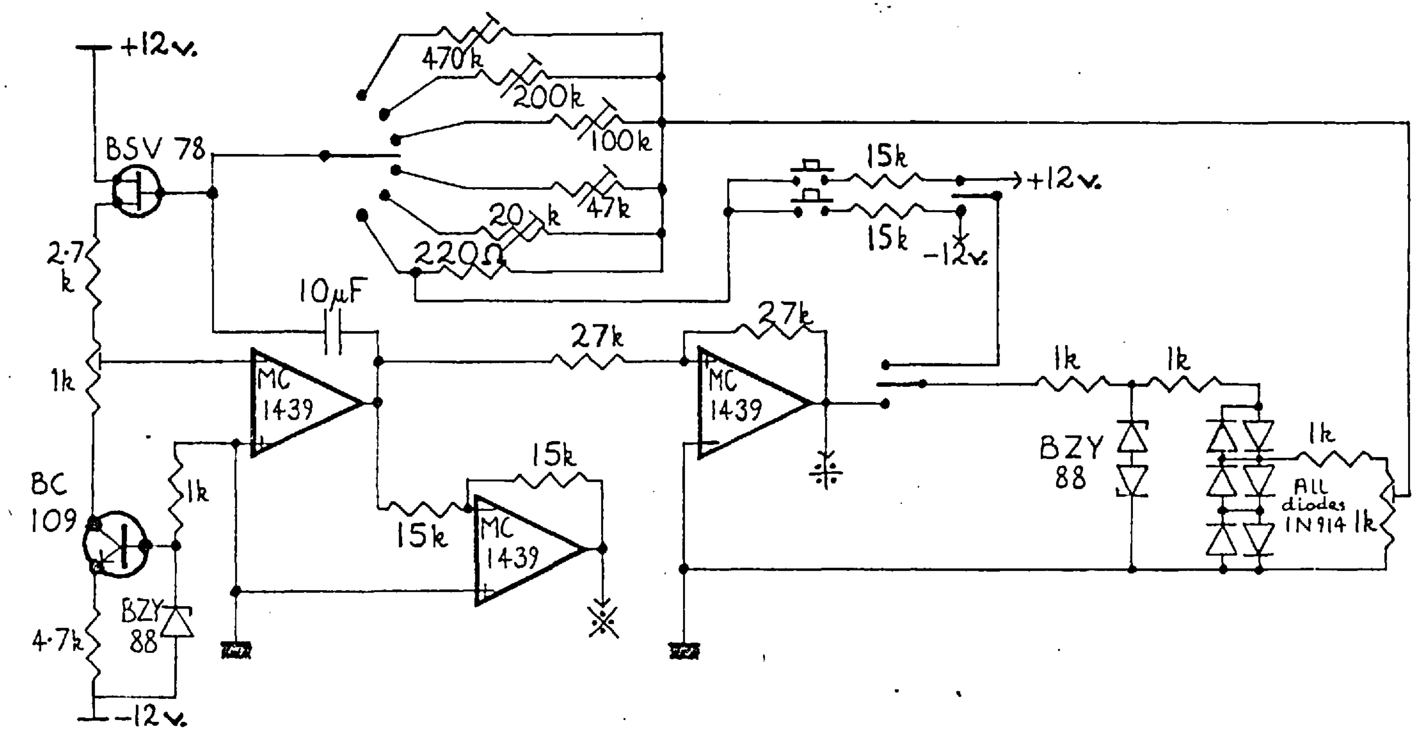
Fig A2/B.

The circuit is shown in full in Figs. A 2/4(a), A 2/4(b) & A 2/5.

The output from Oscillator 1 provides a signal for the X-Y plotter, and causes the amplitude and/or frequency of the transducer driving circuit to vary slowly.

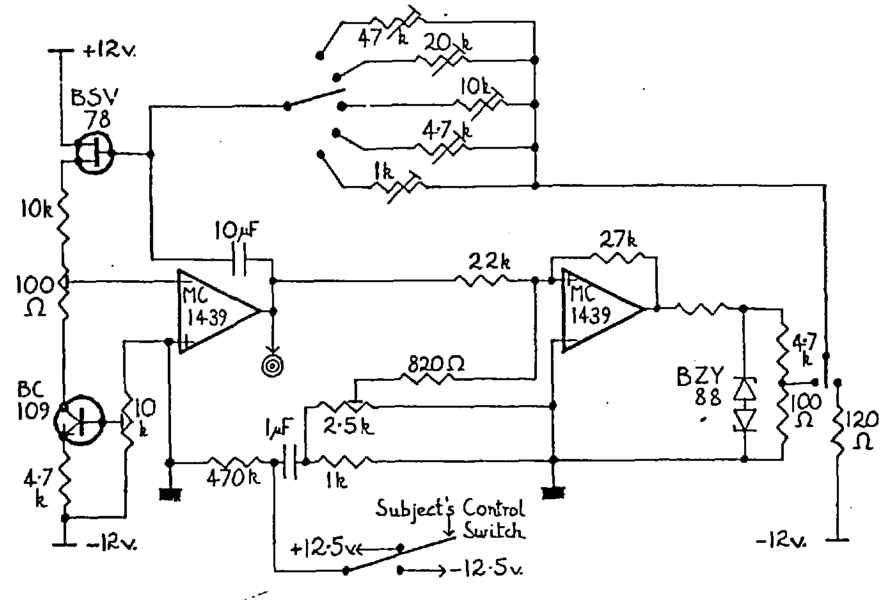
The output from Oscillator 2 or Oscillator 3 can be combined with the above signal or used to control the other parameter, (i.e. the parameter not controlled by Oscillator 1). For example, for measurement of threshold the output from Oscillator 1 changes the frequency of the stimulus whilst the output from Oscillator 2 changes the amplitude. The subject's control switch triggers Oscillator 2 into its alternate state, causing the signal level to decrease or increase as the subject wishes. The actual frequency at which Oscillator 2 'oscillates' is thus always higher than its 'natural' frequency.

If the subject neglects to change the state of the oscillator this is done automatically, of course, and such misses remind the subject to react or inform her that the range of response has exceeded the range of the equipment. Adjustments were made to reduce misses to a very low rate,

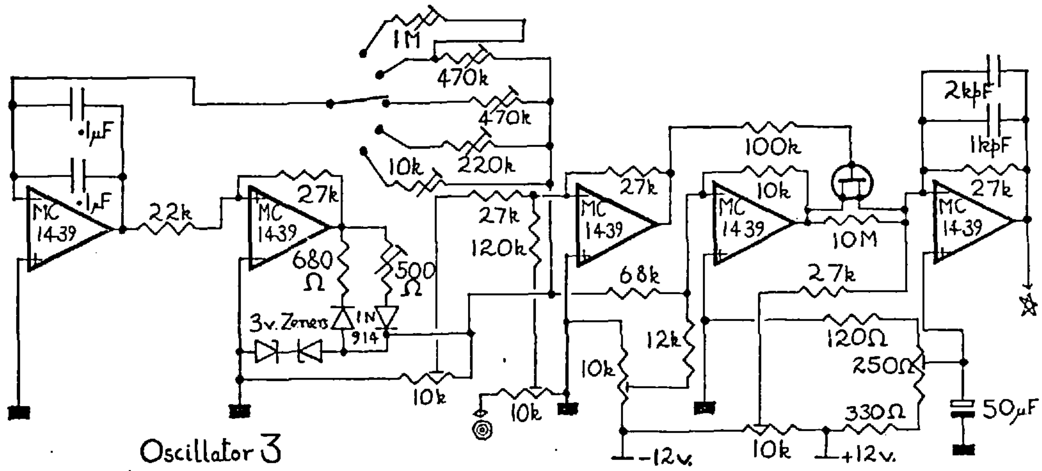


Oscillator 1

Fig. A 2/4a.



Oscillator 2



Oscillator 3

Fig. A2/4b.

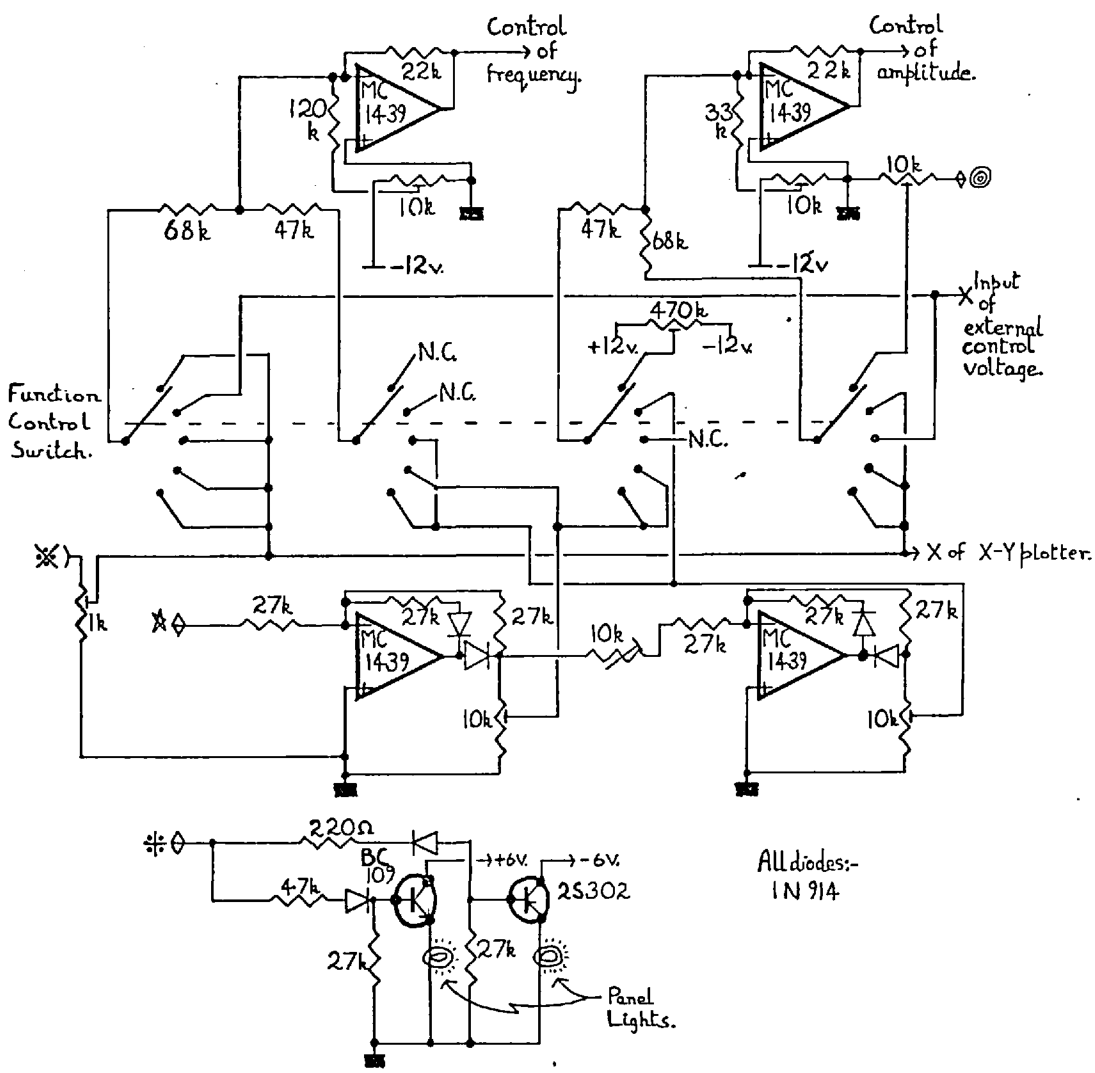

Control
of
frequency.
22k
MC
1439
120
k
10k
-12v.
68k
47k
Control
of
amplitude.
22k
MC
1439
33
k
10k
10k
-12v
47k
68k
Input
of
external
control
voltage.
470k
+12v.
-12v.
N.C.
N.C.
N.C.
Function
Control
Switch.
X of X-Y plotter.
1k
27k
27k
27k
MC
1439
10k
10k
27k
27k
27k
MC
1439
10k
220Ω
BC
109
47k
+6V.
-6V.
2S302
27k
27k
Panel
Lights.
All diodes:-
IN 914

Fig. A2/5.

(they only occurred on the threshold tracks as a result of insufficient range, this had to be guessed at to start with, and any misses on Weber Fraction tracks were the result of negligence).

For the measurement of Weber Fractions, Oscillator 2 is used to control Oscillator 3. However, as this oscillator is of the same type as the stimulus generating oscillator, its output bears a 'logarithmic' relation to the output of Oscillator 2. The square-wave output of Oscillator 3, at 5 c.p.s., modulates the stimulus, and the subject, via control of Oscillator 2, controls the amount of modulation. (The stimulus generation oscillator responds 'logarithmically' to the modulation). The modulation is arranged to increment the stimulus positively or negatively every half cycle, (i.e. 100 msecs.). Every other half cycle the stimulus reverts to the unmodulated value.

The use of the 'double logarithmic' relation in the control loop for the Weber Fraction tracking experiments is discussed in Chapter 3 of the thesis, (in subsection 3 b) 1).).

* * * *

3).

The measurement circuitry, shown in Fig. A 2/6, below, is used to convert the stimulus signal, or the output of Oscillator 3, into a D.C. voltage.

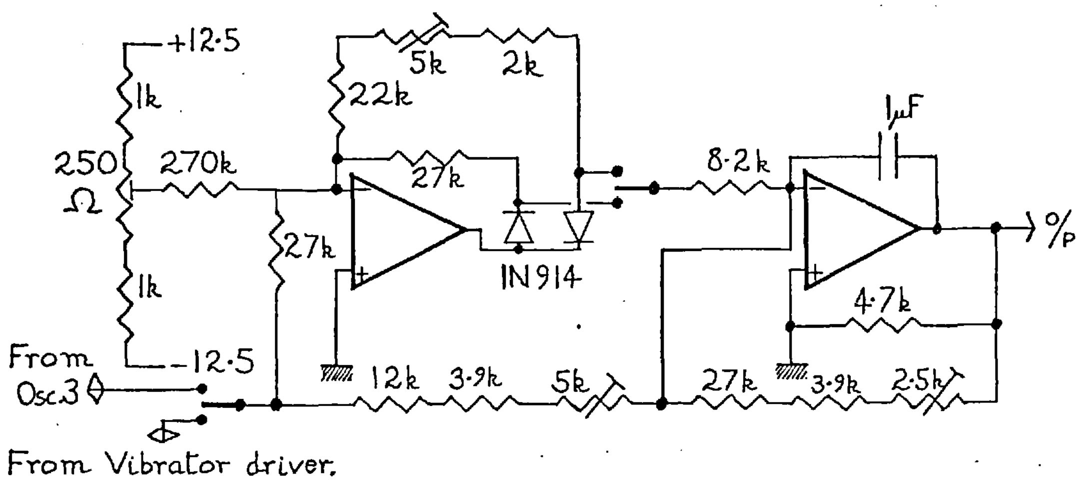


Fig. A2/6

For threshold measurements the voltage is supplied to the ordinate of the plotter, and with the output from Oscillator 1 supplied to the abscissa a threshold track can be plotted. Originally it was intended to track the threshold in both directions, (i.e. as frequency increased and decreased), and a few measurements were made in this fashion, (see 3 b) 2).), but after a short time a small D.C. error appeared in the stimulus signal whenever Oscillator 1 changed state. This fault, in the stimulus generating circuit, Oscillator 1 circuit, or measurement circuit, was difficult to track down, and the solution adopted was to adjust the D.C. level of the stimulus to be zero when the frequency was being tracked from low values to high values. This does not appear to effect the threshold results. The error, a few millivolts, will not have effected the Weber Fraction measurements, nor indeed the larger values obtained for the threshold, because of its small magnitude. Nevertheless, small discontinuities can be observed in some of the tracks of threshold, at around 550 c.p.s., and if one was observed the track was stopped, (if not, the track was continued to the limit), or not, depending on how important comparison between two such traces was going to be. It is not known whether the subject could feel these discontinuities, or whether they were in part or whole artifacts of the measurement circuit.

For measurement of the Weber Fraction the output of Oscillator 3 is supplied, to the measurement circuit, and at carefully adjusted amplitude, to the stimulus generating circuit. The D.C. voltage from the measuring circuitry, (obtained by full wave rectification of the squarewave), is arranged to be such that, (half the peak-to-peak value of the full oscillation), if the output of Oscillator 1 were to be increased by the same value the stimulus would change by the same amount. The tracks recording Weber Fraction as a function of time are made by supplying the output of the measuring circuit (itself supplied with the signal from Oscillator 3), to the 'Y' of the plotter. Because of the adjustment of the amplitude of the Oscillator 3 output the ordinate scales of the Weber Fraction tracks can be calibrated in percentage change.

Typical sets of Weber Fraction tracks are shown, with the ordinate labelled, in 3 b) 3). These are photographically reproduced, not copied. The 'noise' on the abscissae is the result of some intermittent dirt or oxide on the 'slide-wire' potentiometer of the plotter. Attempts were made to clean it, they were not entirely successful. (Such noise is also present on some of the tracks reproduced in subsection 3 b) 2).

* * * *

Measurements of the physical displacement of the transducer were made in an indirect manner. These measurements are necessary for the conversion, from threshold expressed in millivolts of driving signal, to microns of movement.

diam = 6·9 mms &

The method used was as follows. A small accelerometer, (about 2 grams weight), was glued to the plastic peg. The transducer was driven by either a sinusoidal signal or the normal square-wave signal. The output of the accelerometer was fed to the preamplifier (supplied with it), where it could be amplified and integrated to provide a voltage corresponding to velocity of movement, and integrated again to provide a voltage corresponding to the distance moved. Unfortunately only one output at a time was available from the equipment, 'acceleration', 'velocity', or 'distance'.

The voltage output corresponding to distance moved by the accelerometer had to be calibrated at each frequency of the vibration at which it was intended to make measurements. This was done using the sinusoidal signal. With this signal the displacement can be calculated from a record of the acceleration, as well as observed on an oscilloscope. Photographs of the displacement waveform, and the acceleration waveform, (with sinusoidal driving signal), enable peak-to-peak excursions of the accelerometer to be measured in microns. Effectively the face of the oscilloscope is being calibrated, and displaying upon it the signals corresponding to the displacement of the accelerometer when the vibrator is driven with a square-wave signal enables the peak-to-peak displacement to be measured. Photographs of all the relevant waveforms, coupled with records of the settings of the equipment, enable the comparisons, and hence the measurements, to be made at leisure. This is done using an enlarger, upon the baseboard of which the waveforms can be displayed and measured.

The availability of only one output from the preamplifier means that the record of acceleration does not correspond exactly to the acceleration present when the displacement is being recorded. It was assumed that the correspondence was exact.

The finger of the subject, or more extensively, myself, was not continuously on the transducer. (It was thought that there might be a need for a record of the transducer's performance without the finger in position, but no such need arose).

The assumption of the correspondence between the displacement recorded and the acceleration recorded is the source of unknown errors. It is, however, unavoidable. Assuming the exactness of the correspondence leaves the comparison made between measurements taken on the baseboard of the enlarger as the source of errors in the procedure. These include the errors in the range switches on the oscilloscope.

The measurements were taken at many amplitudes and frequencies, and except for the smallest amplitudes measured I would consider the overall error to be of the order of + or - 10%. The errors introduced by assuming that the results obtained with myself as the subject, apply to the subject herself, are unknowable.

The final source of errors in the calibration of the transducer is the accelerometer's effect on the transducer. Once again this is unknowable, but allowances can be made. With the accelerometer in position, but without a finger on it, the system resonated at about 86 c.p.s. With a finger (my index finger) on the accelerometer the resonance appeared to disappear. Without my finger on the transducer and without the accelerometer attached the resonance was at about 240 c.p.s. With my finger in position, but again without the accelerometer, the resonance was at about 300 c.p.s.

Allowing for the above effects and assumptions (one of which is that the accelerometer affects the threshold), the calibration curves shown in Fig. A 2/7 are produced from the data. The overall accuracy of the process which produced them may be as good as + or - 10% but is probably worse. A figure of + or - 50% is, in my view, a probable limit for the error. (My view is based on an examination of the data, obtained over several days, and all of it duplicated or triplicated. The scanty data obtained with the subject's finger on the accelerometer is not so reliable but nevertheless agrees in general with the much more extensive data obtained using myself as subject).

It should be borne in mind that comparison between threshold tracks, (the purpose of the experiments), is not subject to the above uncertainties.

*   *   *   *

4).

The stimulus delivery circuitry is a continuation of the generation circuitry and is thus included in Fig. A 2/1.

The output power amplifier is required to drive an 8 ohm impedance loudspeaker movement. The integrated circuit used, (MC 1454), is quite capable of delivering 'square-wave' voltages of the order of 6 volts peak-to-peak into a load of 8 ohms. It is designated to operate over the range 50 c.p.s. - 500 kc.p.s. with an output level of ½ amp. peak current, but with suitable components it can be operated as a D.C. amplifier.

The signal delivered to the loudspeaker movement is a 'square-wave', with equal excursions, positive and negative, about zero. The

The calibration curves used to produce Figs. 3/4 & 3/5.

Fig. A2/7

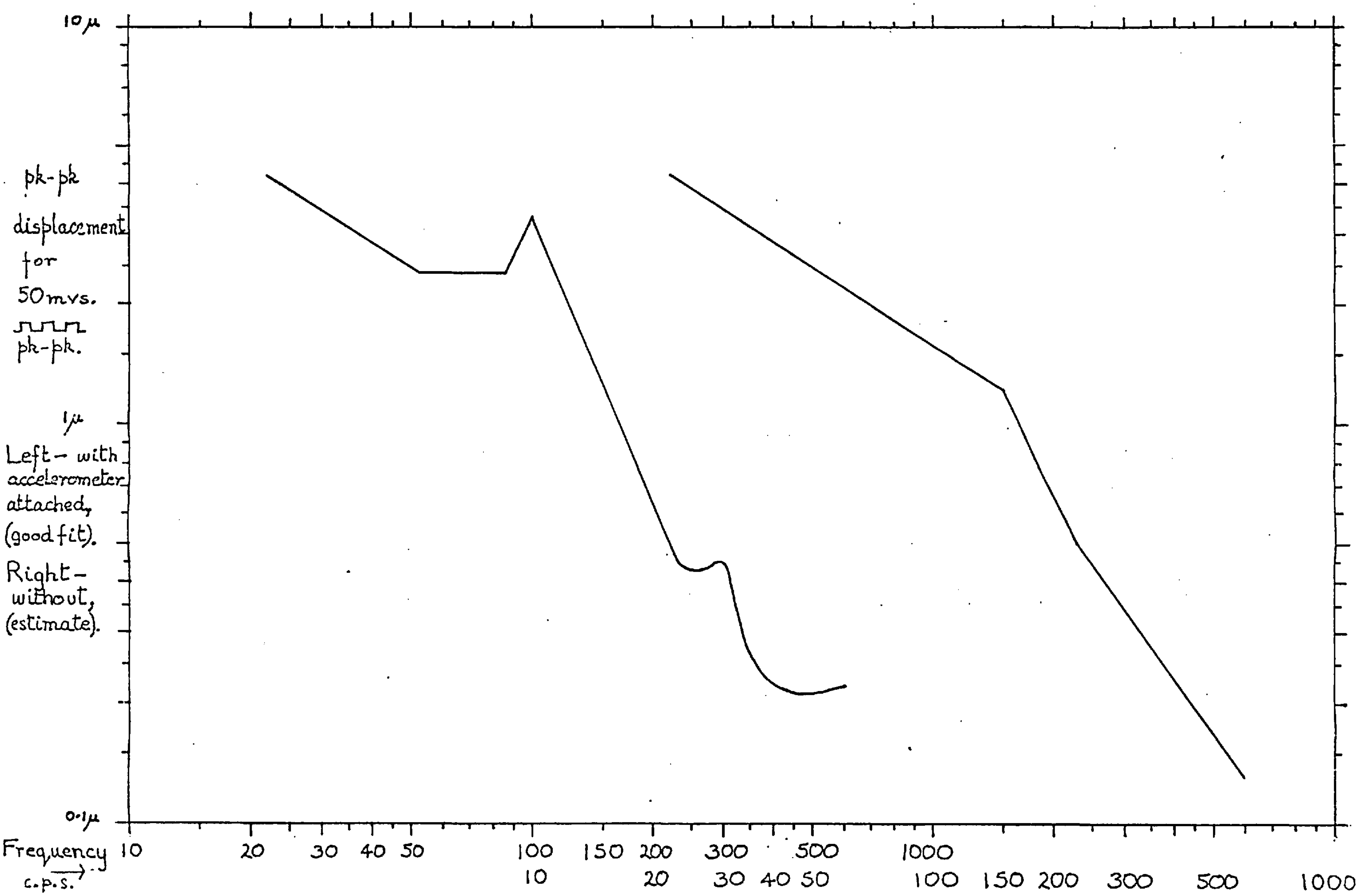


Fig A2/7

movement is rated at 200 milliwatts R.M.S., and will operate without electrical failure if the peak-to-peak 'square-wave' voltage is as high as 6 volts. Mechanical reliability is higher if the voltage is kept below 4 volts peak-to-peak, (2 watts!).

The Japanese loudspeaker movement is imported as a 2½" loudspeaker. The cone is removed, as is part of the metal support for it, and a plastic surround, with a suitable diameter hole in it, is held in place by the remaining metal from the cone support. The small plastic peg is glued to the centre of the diaphragm. The vibrator is shown in various stages of construction in Figs. A 2/8 & 9.

Pegs of diameters 3 mm. and 6 mm. were used for the contactors, and the latter value was quickly adopted as suitable for permanent use. It was likely that this would be the case, as Verrillo's work on contactor area indicated. (See Fig. 3/1 and Fig. 3/4). This vibrator would also be quite suitable for work on the forearm, although a slightly bigger area would give a lower threshold. If the area of the contactor were too large, however, the power limitations of the transducer could become significant. This would have to be determined experimentally.

Fig. A 2/10 shows the driving waveform, as supplied to the vibrator, at two frequencies. Fig. A 2/11 shows the displacement waveform, (with the accelerometer attached), and the acceleration waveform, at several frequencies.

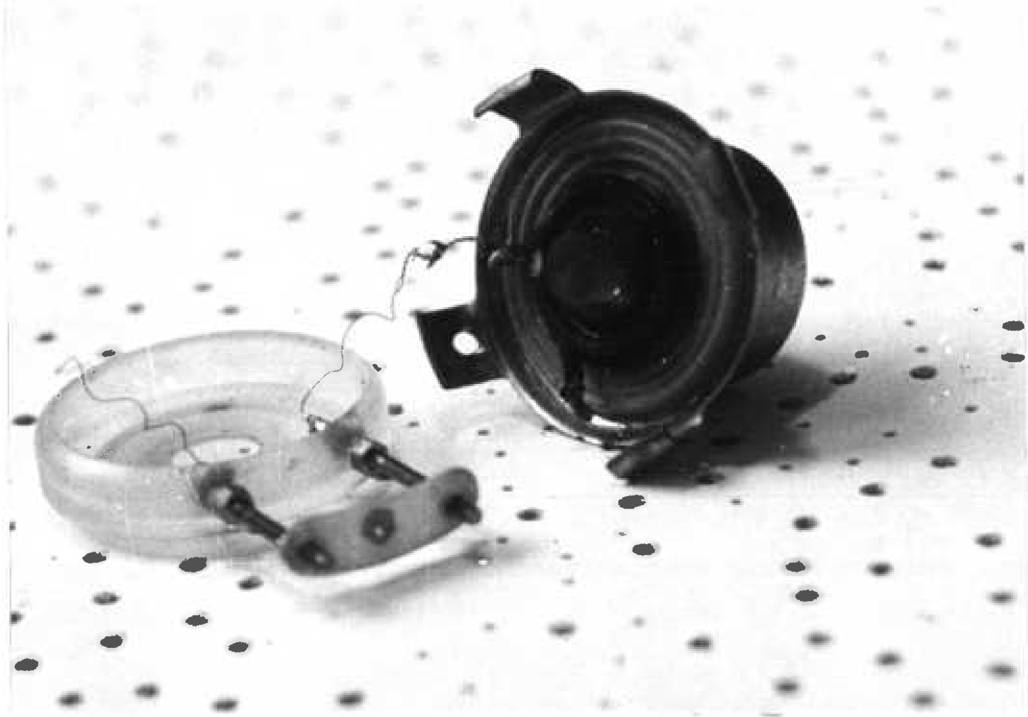


Fig. A2/8

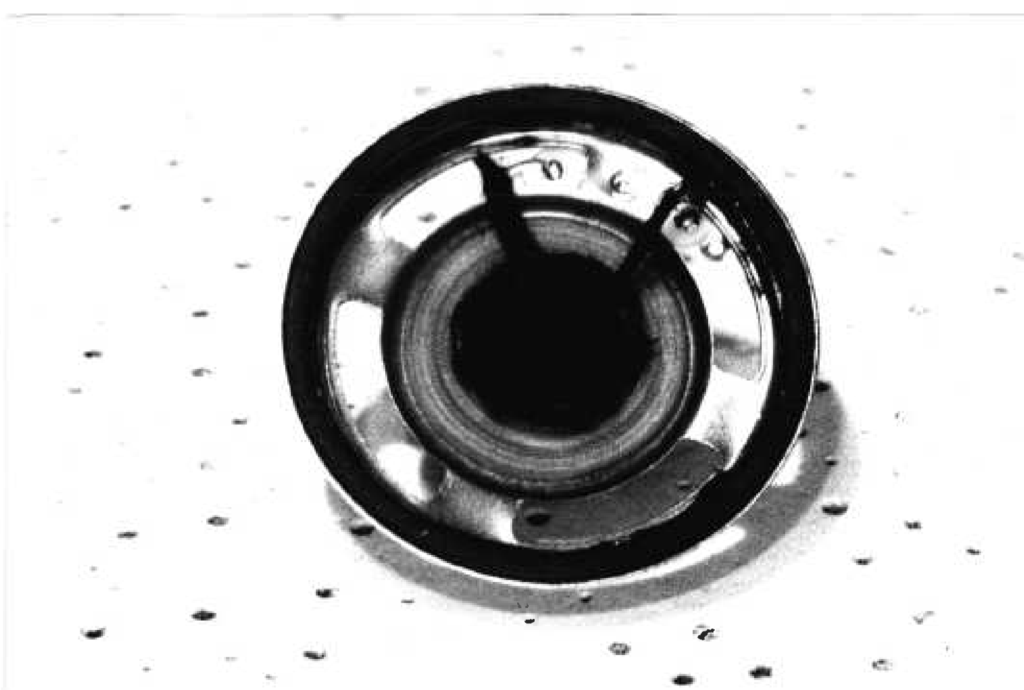
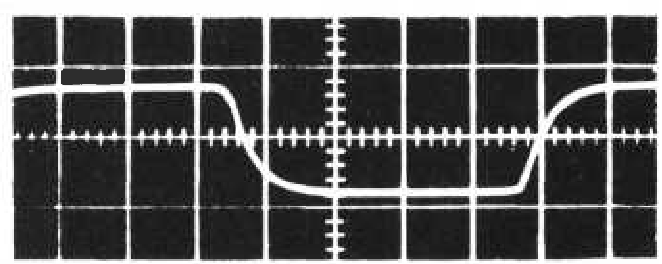
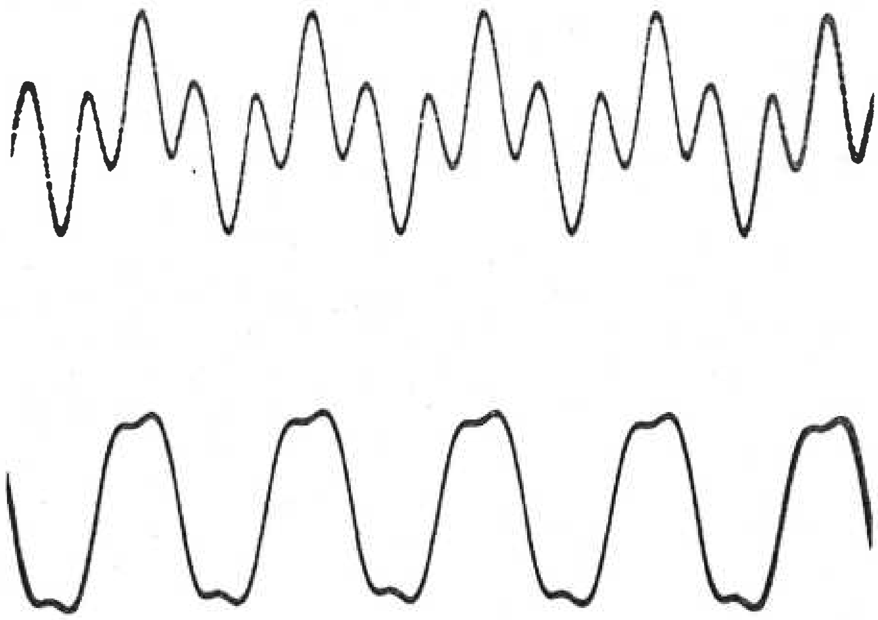


Fig. A2/9

Fig. A2/10

The driving waveforms are shown for, (above), 24 c.p.s. and, (below), 580 c.p.s. In both cases the trace is moving horizontally at 5 centimeters per millisecond, (the graticule indicates centimeters).





The above waveforms show Acceleration, (top), and Displacement, with a driving signal amplitude of 100 mv., at 300 c.p.s. A finger-tip is in position on the vibrator.

Fig. A2/11

The waveforms below show Acceleration and Displacement, (bottom), with a driving signal amplitude of 1 volt, at 30 c.p.s.

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