A STUDY IN EMPIRICAL KNOWLEDGE: THE PRECONDITIONS AND STRUCTURE OF MEASUREMENT

by

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ABSTRACT

This is an epistemological study, in which measurement is taken as a paradigm of perceptual recognition - a notion in which perception is joined with judgment as a factor in understanding. Hence it has proved necessary to give an analysis of such recognition in general, with metric contexts as a special case. This has been done in terms of a very weak fundamental form of 'theory', as a form of basic comprehension, in which language (as part of the theories analysed) is not essentially involved, but treated as a special development.

One type of theory is given thorough formal analysis: those 'recognitive theories' whose elements are taken, in the theory itself, to be recognized directly from perception, or extrapolated as in principle recognizable. Another type consists of 'substantive theories', seen as constructed to provide deeper understanding of the reality underlying recognized structures, but essentially involving elements not taken to be recognizable: this type receives only informal treatment, in terms of its associations with the first (especially in measurement).

Special consideration is (unusually) given to attention and neglect,not in psychological terms, but as theory-guided selection from total experience. Neglect is seen not merely as negation of attention, but often a positive strategy (in measurement, strictly determined).

Part I introduces the basic concepts, distinguishing the general approach from other relevant traditions:

foundational studies in measurement (Suppes et al.); linguistic analysis; some epistemologies (e.g., Goodman); philosophy of science. Part II sets up the formal analysis. Part III applies this analysis to contexts of measurement, with examples (only distance is fully treated, others only in synopsis). Probability assessment is analysed as distinct from measurement. Part IV examines consequences for wider philosophical questions: language-based problems of knowledge and meaning; Wittgenstein's 'private language': and theory-based considerations of ontology; identity; truth, falsity and error; and observation in science.

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Warm thanks are also due to L. Jonathan Cohen, Wilfrid Hodges, Anthony Savile and David Wiggins, each of whom helped me through a stage of the unlikely journey that led to a point of departure for the project.

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A STUDY IN EMPIRICAL KNOWLEDGE: THE PRECONDITIONS AND STRUCTURE OF MEASUREMENT

I. INTRODUCTION

A. The Field of Study

This study looks at measurement as a specialised form of perceptual recognition, or observation, with the aim of discovering reasons for its powerful status as a basic component of many branches of human understanding. The search involves consideration of the role of perceptual recognition in the structure of understanding in general. My use of the term 'recognition' (whose definition will become progressively sharper) reflects these senses, given in the Shorter Oxford Dictionary under 'recognize': "to acknowledge by special notice, to treat as valid, as having existence or as entitled to consideration, to take notice of in some way; to know again, to perceive to be identical with something previously known; to know by means of some distinguishing feature, to identify from knowledge of appearance or character; to perceive clearly, to realize." Thus it carries strong connotations of attention, identification, and characterization, going beyond the bare notion of perception. (The word is, of course, also used in non-perceptual senses; the qualification 'perceptual' is to be understood here, unless explicitly cancelled.) It is very close to 'observation', as used in expressions like 'observation sentence' (e.g. by Quine) or 'observation theory' (e.g. by Lakatos). But 'observation' tends to be restricted to cases falling within a framework of explicit, often previously given, rules, whereas some forms of recognition tacitly make up their own rules as they go along. Some kinds of measurement are improvised in this way, though most qualify strictly as cases of observation, in these senses.

The ground covered - though not large in itself - overlaps the territories of four established traditions of academic enquiry (using a broad classification), three entirely philosophical, and the fourth with strong philosophical implications: epistemology, philosophy of science, linguistic analysis, and the mathematical foundations of measurement. The perspective induced by my own interest turns out to differ fairly widely from any I have found in these traditions, though I hope to draw support, at least as often as I find myself in conflict, at points of mutual concern. Precise statements about these points of support and conflict must await the construction of a language and a framework of theory in which they can be formed. They will therefore appear at the end of this study. But brief suggestions must be made at the outset, to forestall misunderstandings from confusions of perspective. I shall discuss the fourth tradition first.

Mathematical Foundations of Measurement

Current developments in this field employ a variety of hypothetical structures, assumed to be given by observation, consisting of systems of empirical entities exhibiting relations with respect to some chosen attribute (which may or may not be physical). Functional correspondence between these systems and appropriate structures consisting of numbers and known mathematical relations is claimed to be plausibly assertible, with the aid of sophisticated formal argument. (These developments, using a terminology and theoretical framework mainly traceable to Suppes (1962), have their fullest expression to date in Foundations of Measurement, by Krantz, Luce, Suppes and Tversky, Academic Press, 1971, to which I shall have reason to refer quite frequently, and will therefore abbreviate as 'KLST'. Only vol. 1 of this work is at present to hand.) They proceed by increasingly restricted axiomatisations of these assumed 'empirical relational structures' (ers), from each of which arrays of theorems are derived, whose main thrust is to demonstrate how the supposed functional correspondences between these and the associated 'numerical relational structures' (nrs) are preserved under various systematic mathematical operations; in such a way as to facilitate the use of numerical structures and mathematical logic in theories of the more intractable empirical structures (mostly in the human sciences).

. KLST starts from a demonstration that the axiomatic basis of the weakest (most general) of their hypothetical ers - the 'weak order' - yields, as a simply-proved theorem, that a homomorphism exists from this weak order into the real numbers, as ordered by the familar relation '$\geqslant$' (i.e., a functional correspondence with a subset of these numbers); and so onto any chosen nrs which is just a chosen set of numbers so determined and ordered (Theorem 1, p. 15). This fundamental "result", however, merely formalises their intuitive characterization (ib., p. xxiii), of the ordering relation '$\succsim$' for the 'weak order' itself in terms precisely of the numerical relation '$\geqslant$'.

Similarly, and crucially for this study, the most fundamental of all types of hypothetical empirical <u>operation</u>, that by which two elements of any ers are 'concatenated' - or taken together to form a single element - is equally frankly modelled on the operation of arithmetical addition; with only quite cursory consideration of how such an operation might be realised in actual ers (ib., p. 2, 82-91, 155). The discovery, or construction, of actual cases of ers exhibiting these features (and all others considered by this means) is generally left for the practical investigator: for whom what is provided is a variety of axiomatic systems to guide his/her <u>testing</u> of whether a particular numerical structure is appropriate or not for a theory of a particular empirical context. The methods or conditions of testing (other than mathematical) are outside the scope of this kind of analysis, and receive scant mention in KLST. The boundaries of shared interest with this study lie, therefore, in the question of how and where empirical relational structures which might satisfy axiom-systems of all, or some, of the types offered may be found, or constructed; and what restrictions, if any, are placed on the variety of available structures by the nature of the world, and our experience of it.

The result of this enquiry will, in fact, be to put in question the notion of an 'empirical relational structure' as an a priori system whose own theoretical basis is not subjected to independent scrutiny. It will attempt to clarify some aspects of the theoretical status of these analyses of mathematical foundations - not their validity as logical structures, but their epistemological status as theories, in a larger framework. It will be argued that they are not so much parts of the theory of measurement per se, as formal schemata for the formulation in numerical language of theories about empirical structures, so that these theories can be tested by measurement. This testing turns out to require a distinct theoretical basis of its own.

Hölder (1901) adopted, for physical measurement, the basic assumption of an empirically-determined relational structure <u>isomorphic</u> to a numerical structure, on which so much has since been built; N.R. Campbell (1928) being perhaps the last major writer in that field not to be directly influenced by it. By many, indeed, the problems of the foundations of physical measurement came to be considered solved for ever by Helmholz and Hölder - ignoring the uncertainties introduced by relativity and quantum theories (see, e.g., Pfanzagl (1968) p. 11). But Hölder's assumption (which will be questioned here) was too strong, and its demands on the investigator too rigorous, to accommodate more recent pressures for the authentication of measurement procedures in non-physical disciplines. The project of retaining the intuitive commitment underlying Hölder's formulation, while weakening its formal structure from isomorphism to homomorphism, probably began with the psychologist S.S. Stevens at least as early as his (1936). But the further development of the mathematical formalism, up to and including KLST, has been so continuously associated with the name of Suppes that I shall call it, for short, the Suppes tradition. A paper by Kanger (1972) shows how much of this earlier work can be roughly reinterpreted as somewhat naïve versions of restricted parts of modern foundational analysis. But insights can also be found in these earlier writers which are relevant to this study, and which do not fall within any such reinterpretation.

My concern here with recognition will lead also, especially in the context of non-physical measurement (Section III.R), to an interest in the parallel work on scaling theory, especially by J.C. Nunnally, C.H. Coombs - and S.S. Stevens, who has made important contributions here too. The principal topics in this context are the validation and reliability of particular procedural systems. One aim will be to offer a firm general rationale for this field, which seems so far to be lacking. This will involve a rather deep analysis of the theory of probability, especially as it is used in measurement contexts.

Epistemology

The theory of knowledge is a vast region which for many is still the primary concern of philosophy (see, e.g., Quine in Guttenplan (1975) p. 67). But (apart from its use in contexts of linguistic analysis) the label 'epistemology' seems to have become specifically attached recently to a group of theories which look for the grounds of knowledge in some proposed analysis of the fundamental constituents of experience. Following roughly the system of J.J. Ross (1967), recent theories of this kind can be ordered in a number of ways on different criteria, of which one is the size or complexity of what are taken to be "given" as the fundamental constituents, and another - which does not yield the same ordering of the theories - the level of size or complexity at which our understanding of what we perceive achieves the greatest clarity, certainty or justifiability (the degree of supposed corrigibility being yet another ordering criterion). The size-or-complexity scale (to use a measurement metaphor) ranges from the whole interrelated field of experience, through the "natural" objects, classifications and relations of a common-sense reading of perception, to some minimal, irreducible system of units of sensation which can be loosely classed as 'sense data'. These last have in practice proved elusive in themselves, and hard to organize into coherent accounts of ordinary experience.

All these epistemologies are suspicious of 'inference' as a prevalent source of error with respect to the chosen structure of superior certainty - though there are differences about where inference operates in the system, and how conscious or verbally explicit it need be. Ross himself takes the view - also that of R.M. Chisholm - that many kinds of structure of different levels of size and complexity are given in perception without inference - or, at least, conscious or explicit inference: though he refuses to follow Chisholm in assigning special certainty to any particular level.

Another kind of multi-level account of experience may be taken to underlie analyses in terms of structures of parts and wholes, as in the 'mereology' of Lesniewski (Luschei, 1962), or the essentially set-theoretical 'Structure of Appearance' of Goodman (1951), which do not assign epistemological priority to any particular level in the conceptual organization. These have proved more amenable to interpretations in terms of actual recognizable structures than have sense data theories. But the search for universal primitive components from which whole entities may be built up (either in appearance or in reality) has turned out not to yield, of itself, any principle of composition by which the part-structure of any particular whole is determined. Indeed, Lesniewski's mereology (a theory of whole/part composition) is a distinct structure, using a different form of "class" or "set", from his ontology (a logical theory of predication). Luschei and Kotarbinski insist that this last is a "true ontology" or "theory of being"; but it has no commitments of its own to the existence of any particular kinds of entity, composed of parts or not. Leonard and Goodman's "calculus of individuals", designed as a simplified version of Lesniewski's theories (but rejected as such by Luschei), also lacks a principle of composition. Lesniewski's systems will be briefly considered, in more detail, in Part IV.

The perspective of this study compels a multi-level account, which accommodates at least all the scales at which observation is found possible in measurement contexts. It seeks to impose a manageable order on this account, not by according priority to any level of conceptual organization, but by attempting to provide a theoretical framework for the structure of attention, by which, it will be argued, we actually organize our understandings of the many levels and aspects of experience. The task turns out to be less problematic than it may sound. Understanding, according to this view, is made up of an indefinite number of more or less interdependent conceptual frameworks, each constituting a partial theory of some aspect of the whole, selected by the faculty of attention in accordance with interest from time to time, shifting from one theory to another, maintaining their coherence through shifts of attention with the help of memory.

No psychological theory of attention, or memory, is involved. Using a simple primitive analysis of the concept of a 'theory', it has been found possible to construct a highly generalized account of the structure of attention, readily interpretable for ordinary contexts of perceptual recognition, which I shall call '<u>concrete contexts</u>'. Measurement is then analysed as a special class of such contexts. <u>Inference</u> is seen as taking place not in perception itself, but within one or other theory of the context in which it occurs - however vague and tacit, or explicit and sophisticated, the theory may be. <u>Certainty</u>, as a property of empirical theories, is regarded as a purely contingent matter - which does not mean that no certainty is available.

<u>Philosophy of Science</u>

Karl Popper was clear from the outset that the enterprise he launched belonged to the theory of knowledge, or 'epistemology', though current academic usage seems, as I said, to keep this term mainly for the class of theories just described. He took the central epistemological problem to be that of the '<u>growth</u>' of knowledge, and scientific theories as the best - though not the only - exemplars of this growth (see especially the Logic of Scientific

Discovery, introduction to the English edition, 1959, p. 15). With the field of enquiry chosen so high in the 'size-or-complexity' scale, all the then traditional notions of certainty or ultimate justification were not merely abandoned, but explicitly rejected as misconceived. Scientific theories, each with its own specific system of inference (the 'hypothetico-deductive' principle) were boldly classed as competitive 'conjectures', the expert jury of current scientific opinion being accepted as judges of the resulting competitions - with some guidance, or interpretation, offered by the philosopher.

There is much common ground between this approach and that of this study,* the most fundamental difference being the attempt, here, to formulate a primitive analysis of theoretical structure (just mentioned) in such a way that it can be generalized to include the simplest possible conceptual frameworks within which experience is understood. Philosophers in the Popperian tradition do not seem to have thought it desirable, or perhaps even possible, to set up a general metatheoretical analysis of empirical theories. But by showing how this can be done at a basic level for measurement systems, as special cases of contexts of perceptual recognition, this study may hope to

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* There turns out to be much common ground, also, with a quite different approach to scientific theory-building recently presented by van Fraassen (1980), and called by him 'constructive empiricism'. This ground will be briefly explored at the end of the study (Section IV.8).

reveal a common underlying structure. Lakatos (especially in his (1970), pp. 98-9, 109) stressed the importance for philosophy of science of 'observational' theories (the cautionary quotes are his), but without attempting a general analysis in which they can be distinguished from other theories. His remarks on this subject will be further considered at the end of this study.

Popper himself always insisted that the principles of his analysis - including a somewhat loosely conceived notion of 'theory' - informed all levels of human understanding, and even that of some animals. Other followers of his tradition do not seem to have pursued this line of thought. The more rigorous analysis of theory used here is not explicitly considered in relation to animals. But it leads to the view that, as the size and sophistication of the group of persons involved in a theoretical context decreases, from Popper's "scientific jury" to a single individual organizing his/her understanding of some everyday context of experience, the importance of language as a vehicle for comprehension or communication declines, and that of unverbalised perceptual recognition increases.

Linguistic Analysis

The last remark suggests a radical difference of perspective on language from that of the development in this century of linguistic analysis as a philosophical method. At the end of this study, I shall argue that these perspectives are complementary rather than conflicting; and suggest ways in which the present analysis is relevant to the actual structures of language. But little use will be made of the standard concepts of linguistic analysis in the main exposition. These concepts have mainly been developed within the study of particular languages, 'naturally' developed or formally constructed. The aspects of these languages which have attracted most attention have been their grammatical or syntactic structures; in terms of their relevance to the logical or truth-functional properties of sentences, or structures defined in terms of the truth of sentences, such as truth-values, truth conditions, propositions or possible worlds. In demonstrating that truth is to be understood, not as a metaphysical principle, but rather as a property of sentences, Tarski emphasized that its definition must be made relative to a particular language in each case (recursive procedures being carried out within each language for which the definition is specified*). In the tradition of

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* A recent new departure in model theoretic semantics, due to Hans Kamp, in which truth is defined for a fragment of discourse not only in terms of the language of that discourse, but of a functional embedding of a representation of that discourse in a real-world model, is explicitly distinguished in Part IV. In its present form, it remains specific to discourse in a given language.

semantics developed from this seminal insight and those of Davidson, theories of meaning have equally been relativized to particular forms of natural or formal language, for which meanings are supposed given - in the case of natural languages by the shared knowledge of a speech-community, and in formal languages by definition. In general, the development of the method owes its persuasiveness to an underlying intuition, most explicit in the work of Wittgenstein, that problems of philosophical analysis can only be coherently stated, let alone solved, in terms of the structures of language in which they are expressed: that any attempt to discuss the nature of our experience of the world otherwise than in terms of its conventional linguistic expression is doomed to circularity or meaninglessness. The effect is to exclude from analysis all non-linguistic structures involved in meaning.

This study seeks to escape the fundamental restriction on the scope of philosophical analysis, imposed by acceptance of this intuition. A technical device in the realm of metatheoretical analysis is introduced below, which is claimed to permit a distinction, in the language of metatheory, between elements of an instantiating theory which are elements of a language of that theory, and others which are not. Using this device, it has been found possible to produce a more precise account of the role of language in concrete contexts. It turns out to be less central than has often been thought.

Operationalism

A fifth philosophical interest - now hardly discernible in the field of study - must be briefly distinguished as having special relevance to measurement. Operationalism, associated primarily with Bridgman, is the doctrine that the ultimate significance of observation statements, and of theories constructed on them, is given by the (physical) operations carried out in determining their truth. It can be seen as a special application of the general doctrine of verificationism, implicit in all positivism and explicit in the early work of Wittgenstein: that the meaning of any statement is given by its actual or potential means of verification, and that no other ground of meaning is ultimately available.

I shall certainly pay more attention to the structure of measurement operations (in the sense of concrete procedures) than do, say, the followers of the Suppes tradition of foundational analysis - and so risk being mistaken for an operationalist. This would be unfortunate, since the position has properly become discredited in recent times, as being far too rigid and particularised to permit fertile theoretical development. But I hope it is already clear, and will become clearer, that from the perspective of this study the meaning of operational procedures is to be sought in the theory under whose commitments they are carried out, and not vice versa. Examples will be given where more than one theory can be applied to an operation (in this sense) so as to assign different, mutually exclusive, meanings; and, even more crucially, where a theory will assign equivalent meanings to two or more recognizably different operations.

In view of these marked differences of perspective from those of many previous lines of enquiry, it seems wise to provide a few preliminary notes on certain key concepts which are shared, to a greater or less extent, between all perspectives: namely theory, context, property, set, entity and relation.

B. Notes on Key Concepts

Theory and theoretical status

Before specifying the content of these notions for the present analysis, I want to reject what I take to be excessive restrictions which are often tacitly, sometimes explicitly, placed on their use. The first is that theoretical commitments, at least in empirical contexts, are confined to those which are speculative or doubtful, in some way that prevents them forming parts of _knowledge_ in any valid sense. I claim that we _know_ that the earth rotates once a day and orbits the sun once a year, and that our commitments to these beliefs as knowledge remain theoretical in just the way they were four centuries ago, when they were not merely thought speculative, but spiritually and politically dangerous. Alternative theories remain available - no doubt there are still flat-earthers living somewhere - but they are no longer seriously entertained. If I see that old table in front of me, I don't think of this recognition of involving a theoretical commitment, unless and until somethings happens to create conflict in my reading of current perception; for instance, if someone appears to walk straight through the space occupied by the table. I then cast around for alternatives to the theory that I see the table and the person because they are where they appear to be; I may think of mirrors, hypnotism, incipient lunacy or some form of Berkeleianism. These alternatives would have been present, even without the anomalous event that made me think of them. It is always one of several possible theories that we know things are where we see them, when we see them and as we see them, just by seeing them. The prejudice that theory is only present (rather than only attended to) when things go wrong, is merely the converse of the prejudice that theory is always doubtful.

Within the field of empirical theories I find, intuitively, a continuous spectrum of certitude, from the belief that there is a sheet of typed paper in front of me, to theories of the British economy, and possibly even beyond. The commitment that some empirical proposition is known is itself metatheoretical - involving the proposal that any empirical theory on which it is founded forms part of what is accepted as knowledge (by whatever individual, society or group, at whatever historical juncture).

The second, and more difficult, prejudice I wish to reject, is that theoretical commitments necessarily form parts of some relatively sophisticated structure, which must at least be framed in a properly constituted and fully conscious system of language.

The constituents of a theory, as the notion is used here, are:

A. A system of sets of supposed elements which are held to exist for the theory, and constitute its subject-matter (or its ontology).

B. A system of primitive structural relationships understood, in the theory, to hold between items of A.

C. A system of commitments of the theory, constituting a regular structure of dependence understood to obtain between items of B, such that some such relationships are understood to be dependent on others (essentially, a system of inferential commitments).

D. A system of logical connections and operations governing the understanding of items of B and C in combination.

The elements (A) of the theories which will be the main subject of this study will be those elements in experience taken to be perceptually recognized, or in principle recognizable. The broad general assumption will be made that non-verbal recognition of such elements is prior to any use of language to draw attention to them, describe them, or make statements about them; and, to a large extent, independent of such use of language. (The word 'language' will be restricted in this study to structures using words, or conventional symbols such as numerals or algebras combined with words in a single system of discourse in any context. Full discussion of the distinction between linguistic and non-linguistic structures, and the relationships between them, must be left to the end of the study.)

The notion of a theory constituting some understanding of non-verbal recognition involves that of a non-verbal logic governing this understanding (under head D above). This notion will be explored in some depth. To recognize that chair x is in room y, and room y in house z, is to recognize that chair x is in house z. Though I must use words to invoke it here, it will be argued that such an inference may be (and frequently is) reached independently of any use of language (in thought or otherwise). To discuss a logic governing wordless inferences of this kind (which may take quite sophisticated forms) we cannot use the language of the inference itself, where there is none. I shall therefore introduce a formal language for the theory of this study, which I shall call a general theory of concrete contexts, or C-theory. This language of C-theory will contain distinctive expressions for linguistic and non-linguistic elements of recognitive theories, (or R-theories), in terms of which the general structure of these theories will be analysed. So C-theory is a metatheory of R-theories: part of its language will constitute a metalanguage, in Tarski's sense, for the linguistic components of R-theories, but the major part will be used to describe their non-linguistic structures.

Though this formal construction is necessary to articulate fully and rigorously the reasoning leading to certain conclusions about the theoretical structure of recognition in general, and measurement contexts in particular, both reasoning and conclusions are also discussed and illustrated in informal language: most of the reasoning in Part II, applications to measurement in Part III, and general philosophical considerations in Part IV.

The everyday situations in which recognitive theories are used (and there are very few in which they are not) frequently involve the concurrent use of more abstract forms of understanding, in more or less close interaction with R-theories. These will be discussed, where relevant, in informal language. Though they will be assumed to have at least the structure broadly analysed in heads A to D above, no attempt will be made at a general or comprehensive analysis. Such abstract theories are relevant here to the extent that they share certain elements (A) with R-theories, and can be seen as attempts to organize understanding of the reality underlying the recognized structure, in ways which go beyond what is (even in principle) recognizable in perception. I shall therefore refer to them as _substantive theories_ (or _S-theories_).

Although we shall find some general characteristic differences between R- and S-theoretical structures, no universal rules can be laid down for distinguishing them in all contexts, as if there were separate bodies of R- and S-theory, universally understood as such. Plainly, different ways of understanding a given situation may differ over just what, or how much, is recognizable in perception: in more sophisticated situations this is the question what may count as _evidence_ for any particular theory (on the ground that it is evident from _observation_). If I see a man, at night, working with a screwdriver at a rear window of a darkened house, I may form the (substantive) theory that he is a burglar. Challenged, he may offer an alternative: that he is the owner, whose wife is away at her mother's, and who has carelessly locked himself out. He may draw attention to various aspects of the evidence which, he claims, support his version. As the story develops, it is easy to see how different features of the situation may be claimed as "evident", depending on what theory is being promoted.

The question of the borderline between R-theory and S-theory, and how sharply that line is drawn, will be taken to be a matter for autonomous decision by the individual or group adopting the theoretical strategy in each case - subject to conditions of rationality and consistency, many of which will emerge during the main exposition. At this point it must be said that the logical relationships between the two types of theory depend on a variety of considerations in different contexts. For example, although S-theoretical commitments may be thought of as in some sense deeper and more general, they are not necessarily either more permanent or wider in application than R-theoretical commitments with which they may be associated. Our common recognitive commitments about sunrise, sunset and the alternation of day and night have surely proved more stable, and are involved in more different kinds of context, than the S-theoretical explanations of astronomers. Again, where real or apparent conflict develops between applications of R- and S-theories in particular contexts, there is no simple rule of priority: Popper's first intuition that, in science, a single contrary 'observation' may falsify a scientific theory (in my terms, a particular type of well-developed S-theory), has become the subject of extensive reinterpretation and modification. This aspect will be discussed in Part IV (Section 8).

One way in which the borderline between R- and S-theories may shift from one context to another is specially important in the analysis of measurement systems. I shall show how aspects of theory which in some contexts may be taken to go beyond the recognitive evidence, may in others be incorporated in R-theories. For example, where geometrical or trigonometrical theory is used in measuring distances by rangefinder, or the theory of expansion of mercury under heat in measuring temperature, these substantive theories are not attended to in context, the perceived readings of the instruments being taken as objective evidence. This principle of incorporation is not restricted to contexts of measurement or sophisticated theory; we may recognize a fruit as ripe by its colour, without attending to the theoretical commitment involved in thus recognizing what cannot be seen.

It may help to think of the simplest kinds of theoretical construction considered here as roughly equivalent to what many philosophers have discussed under the heading of belief. The new aspect of the concept introduced here could be called systematic belief - not necessarily involving a sophisticated structure which would ordinarily be called 'a theory', merely a collection of beliefs which are in some way mutually dependent by virtue of the beliefs themselves and the logical connections and operations incorporated in them. Both the words 'theory' and 'belief' risk connoting too much in contexts of perceptual recognition, but no others - not even 'recognition' itself - seem immune from this tendency: the basic notion is a system of commitments on which we rely.

I shall take it that in empirical contexts generally, and contexts of perceptual recognition in particular, the evidence on which existent elements under A are recognized is always partial, in that our perception can never be assumed to give us complete information about any perceived entity. There may always be (and usually are) unperceived entities and properties in any state of affairs which is an object of perception. Some, but not all, may be recognized by theoretical extrapolation. Only in more sophisticated theories are some recognitions distinguished as evidence for the extrapolation of others. In a vast number of ordinary cases, extrapolation is not merely a tacit but an unconscious factor in recognition. As illustrated above, it is only in special circumstances that we attend to its theoretical character. When we do so, it is frequently quite easy to interpret.

Context

The word 'context' is used in a variety of senses. In linguistic analysis, apart from Frege's insight that the sense of a word is in general determinate only in the context of a sentence, the initial concern was to find aspects of the theory of language which are independent of context; and this remains an important motive. But the effect of context with respect to particular utterances, especially in determining the precise references of referring terms, and hence the meanings and truth-values of the uttered sentences, became increasingly acknowledged. Amongst possible aspects of a context, most attention has been paid to the time of an utterance, and the identity of the speaker (or writer); it will be argued, in Part IV, that these factors are frequently insufficient to determine meaning.

More formally, a context has been presented in terms of an 'interpretation' or 'model' in which the truth-values of a particular set of uttered sentences are taken to be determined (usually by unspecified means); on the basis that more than one such interpretation is typically possible for any given set or sequence of sentences, considered independently of a context of utterance, and that such a context ideally determines a restriction on the system of possible interpretations so as to disambiguate meanings. Following Geach's concern with the subtle ways in which conversational practice may place interpretations on words or sentences not obvious from conventional analysis of lexicon or syntax, students of pragmatics have paid special attention to contexts of utterance. These have been analysed, for example, with reference to sets of propositions, or possible worlds, defined in terms of possible interpretations of specified utterances: in such a way that successive utterances may be understood to modify 'a context' by progressively restricting alternative interpretations of earlier utterances in a sequence (esp. by Gazdar, 1979). But in none of the accounts so far considered is the concept of 'context' itself specified or analysed except in terms of a supposedly given structure of sentences or sentential forms employed in a set or sequence of utterances; or of otherwise unspecified factors supposed to restrict their interpretations.

By contrast, the term will be given a strict sense in this study, at least for the special class of contexts being investigated. Broadly, a context will be analysed as a set of theories which share enough items under heads A to C above (p. 19), in the terms of the theories themselves, to be combined under a common logic (under head D) into a total structure of understanding of these items: the set of theories being adopted either by a group of persons (using language to coordinate their theories), or by one person (using language or not). Greater or less inconsistency between the commitments of different theories of the same context will affect the degree of comprehensibility of the theories, used together, usually with the idea of reconciling them or deciding between them. This account is vague, as it should be to match living experience of attempts to reconcile or adjudicate rival theories: this is the problem of 'commensurability' familiar to post-Popperian philosophers of science. But I hope to show that the notion of a single recognitive theory of a concrete context can be made relatively sharp, so as to make the notion of commensurability more precise in such contexts.

People are, from a logical point of view, entitled to believe what they like about what they perceive: so this analysis accepts a fundamental <u>autonomy of theory</u>, especially under heads A to C above. I shall however restrict myself to a very simple set of logical terms under head D, taken to reflect the structure of the tacit logic inherent in native intelligence. The autonomy of theory is seen as limited only by the demands of internal consistency; which, in the case of recognitive theories includes consistency between their readings of the perceptual evidence - which are parts of the theories - and between these readings and findings derived from them under the theories. In practice, concrete contexts yield a high degree of consistency in many cases between the readings of theories operated in a context by users of a common language: measurement contexts are constructed to maximise such consistency. Hence the concentration in this analysis on the notion of context.

Properties, Sets and Entities

A mathematical property of numbers can be identified with the set of numbers possessing that property: we say, the set 'determined by' the property. This also applies to relational properties of number; any n-ary relation (relating n numbers) being identified with the specific set of n-tuples of numbers exhibiting the relation, determined by the relational properties of the numbers with respect to each other. These identifications are unproblematic, because it is a simple basic commitment of the theory of numbers that the membership of the set of numbers or n-tuples determined by any particular property or relation is fixed for all contexts by the rules of mathematical logic, even if - as often happens - the set is infinite, so that we cannot identify all its individual members by name or in any other way. If two properties, or two relations, determine the same set, they are logically identical.

In principle, something similar may also be logically true of actual sets of non-numerical entities, such as we recognize in our daily experience. Considering, for the time being, only unary properties (those taken to be possessed by single individual entities), it may be possible to say that if any two such properties, A and B, are to be distinguished, there must in principle be some entity which belongs to the set determined by A and not that determined by B, or vice versa; and that, if A and B determine the same set of entities, there are not two distinct properties but one, giving A = B. Whether or not this is so for a universe of logically possible recognized entities, such a principle is useless for the interpretation of ordinary concrete contexts, where our information is restricted to the field of immediate attention and memory, and we need to discover which relevant sets are distinguishable on the evidence of their perceptible properties. The fact that two or more properties determine the same set in this restricted field will not, of itself, induce us to identify these properties. More often it is the contingent coincidence of recognizably different properties in the same set of entities which is the centre of interest. Indeed, it is difficult to see what sense can be given to the identification of recognizably different properties.

This important effect of contextual restriction on the universe of entities and properties comes to a head in contexts where we wish to assign numbers to elements of the context and need to decide, in the light of some theory of the context, just which numbers to assign to which elements (entities or sets). Measurement is a relatively sophisticated class of such contexts. The simplest form in which this requirement arises is in the counting of sets. Two fundamental conditions are needed for the counting of sets:

(1) A criterion by which it is decidable whether or not an entity belongs to the set;

(2) A means of discrimination between members of the set, such that each is counted, and none is counted twice.

Further conditions are needed for the counting of sets in concrete contexts, but these can be neglected at this stage. In simple set-theoretical terms, these basic conditions are the availability of criteria, with respect to any set $\mathcal{S}$ and any entities x, y recognized in context, for:

(1) $x \in S$;

(2) $x, y \in S \wedge x \neq y$.

The second condition requires us to find criteria distinguishing each member of S from every other. If we do so by recognizing each as a member of some set, we need to recognize as many distinct, additional sets as there are members of S: that is to say, we have to recognize that no two of these sets contain all and only the same members. Plainly, the most direct and simple way of doing this is to distinguish just the members of S itself, by distinguishing properties which determine their membership of different sets. Having done this, there is no point whatever (merely in the context of counting S) in setting out to determine the membership of these distinguishing sets outside the membership of S (their intersects with S). How much simpler and more direct, therefore, simply to distinguish the properties, and ignore the sets they determine, unless we have some reason in context to recognize these sets, as such, as well as S .

This is, unquestionably, the way we do it in practice. Most of the sets we ordinarily count are determined by a multiplicity of properties (realizing condition (1) above, the criterion for $x \in S$). We have no need to attend to the collection of all sets, determined by every such property, of which the set to be counted forms the intersect. If we are counting swans, it is not generally enough for membership of the set of all swans in context that the candidate should be a bird which is white, has a long neck and webbed feet. If we wish to tell swans from white geese we shall have to attend to other, more subtle perceptual properties. Often the only way we can tell the swans themselves apart, on the other hand, is by recognizing relational properties, to do with position in space, of which more shortly. All I want to say at this point is that such properties are not readily associated, in practical contexts, with membership of sets.

Since the aim of contextual theory is to reflect the structure of attention, it will follow the indications above, by containing in its language specified algebraic symbols for *properties* recognized in context, in addition to specified symbols for the entities to which properties are attributed or '*assigned*', and for sets of entities or properties. To distinguish them from properties in general, they will be called '*characters*'. Although, logically, assignment of a character to an entity may be equivalent to the assignment of that entity to some set, the assignment of characters will be treated as prior. Sets of entities, generally labelled with the characters which determine their membership, or sets of characters themselves, will only be introduced where their recognition *as* sets has some specified role in the theoretical structure.

The resulting emphasis on characters has certain other advantages for a theory of recognition. The *entities* of the theory, those taken to be recognized in context, are to be called '<u>idents</u>', since their primary significance for the theory lies in their identity (no preexisting word, unfortunately, seems to capture this unequivocally). They are understood to be distinguished from one another by their <u>boundaries</u> in space, at any one time - i.e., they are conceived as spatially bounded entities. These boundaries are taken to be recognized in terms of <u>differences in character</u>. (They need be no more, and must be no less, sharply distinguished than the context requires. A more detailed account of this aspect is given on p. 60).

By dispensing with the identification of properties with determinate sets, contextual theory is left with no general criterion for the identity of characters. This, like many other aspects of the structure of recognition, becomes a matter for the autonomous determinations of theory in context, the only check on the validity of such determinations being the consistency of the theory as a framework for the understanding of the context. The decision that two objects are the same colour, for instance, only holds so long as the same standards of "sameness" are maintained for the whole of the context. It is claimed that this analysis reflects the actual structures of recognized similarities.

<u>Properties and Relations</u>

As has been said, there is no difficulty in

identifying relations between numbers with determinate sets, any n-ary relation (or n-place relation, or relation of order n) being identified with the set of n-tuples of numbers exhibiting the relation (ordered or not, as the definition requires). This relation can be called a property of any n-tuple which is a member of the set: for each member of such an n-tuple we can identify a group of relational properties with respect to some or all of the other members of the n-tuple. It has therefore been natural to identify properties of numbers in general with relations, treating a unary property (such, perhaps, as primeness) as a 'one-place relation'. All talk of properties in numerical structures can therefore be replaced by formulae in an algebra of numbers and relations.
In foundational analysis in the Suppes tradition empirical relational systems are treated in the same way, as systems of sets and relations, with no mention of properties other than as "attributes" identified with complete systems of sets and relations.

Again, something similar can be done in the abstract in linguistic analysis, associating properties with n-place 'predicates', whose extensions are sets or classes identified with the corresponding relations, a unary property, in the above sense, being the extension of a one-place predicate. However, bearing in mind the problems raised above for linguistic analysis in concrete contexts, nothing more can usefully be said on this point at this stage.

The general difficulties just illustrated, for the identification of unary properties with sets in concrete contexts, increase beyond natural comprehension for properties involving pairs or larger n-tuples of recognized entities - and natural comprehension, we must remember, is what we have to do with in concrete contexts. If we are to recognize the relation exhibited by the pair consisting of a book on a table, are we to do so in terms of a restriction on the set of all pairs in a "supportive" relation given by 'x is on y'? The hat on the hook, the nose on your face, the fly on the ceiling? If not, what more reasonable account is feasible?

From the perspective of this study, it has proved imperative to pursue the policy of priority for the recognized 'character' over the set of entities determined by it, into the field of properties involving two or more 'idents'. Standard analyses of such properties have tended to exhibit them in terms of relations, by means of sentences or formulae interpretable as making statements about the related entities, which may or may not be true or applicable under particular interpretations. Such relational statements are always, in principle, analysable as logically equivalent to one or more different statements about the same related entities. For example, "a is to the left of b", "b is to the right of a"; "a is louder than b", "b is softer than a" (of sounds); "b is between a and c", "b is between c and a". When considering how to render the structures involved in recognizing, perceptually, the conditions for truth or applicability of such collections of logically equivalent statements, no general principle suggests itself for preferring one to another. To recognize the conditions for one is necessarily to recognize those for any of its equivalents.

To capture the intuitions suggested by these considerations, use is made of a concept of 'figure', similar to that employed in Gestalt psychology (though without committing us to any of its doctrines). It will be assumed that in concrete contexts a large class of relational structures are recognized in a simple unsophisticated way as configurations, understood as assemblies of two or more related idents. Different configurations are taken to be recognizable, in many cases, as similar in structure - in much the way that different idents are recognized as similar in shape or form. So a figure will be defined as a set of similar configurations, represented as a set of pairs of idents (or n-tuples - assemblies of n idents) to which are assigned a common (binary or n-ary) character, on the same principle as that by which (unary) characters are assigned to individual idents. Figures are typically, but not always, recognized visually. (But a triple musical chord, for example, could plausibly be analysed as a triple configuration in sound, a set of similarly related chords then forming a figure.)

The conditions for two or more relations, in the standard sense, are thus in principle derivable from the recognition of any one figure. Which of these relations, if any, are attended to in context, is again a matter for autonomous decision by the Reader in each case. Such decisions may be assumed made in the light of commitments of the Reader's theory of the context; and we shall find that such commitments are, indeed, frequently associated with particular relations.

To give some simple examples: we may recognize a cupboard as *on* the floor but merely *touching* the wall; while a curtain beside it is *on* the wall but merely *touching* the floor. This judgment involves *both* recognizing the configurations cupboard-wall-floor and curtain-wall-floor, *and* making use of a theory about the means of support of cupboard and curtain. (Note that the recognition of configurations does not require that they correspond with any neat, generalised linguistic expression, since many of them are unique structures, though readily recognized as similar to others, especially in terms of spatial organisation.) Measurement involves recognized configurations of comparison, together with specific theories about the entities compared. We shall find that axiomatic theories, about *sets* of such comparisons, such as those of the Suppes tradition, go beyond the evidence of recognition, which requires its own distinct theoretical foundation. There are, therefore, at least two levels of theory to be analysed in measurement.

Enclosure and contiguity

Special treatment is given to two specified figures which are taken as fundamental for all recognitive theories, as part of their metatheoretical definition. These are the figures of '<u>enclosure</u>' (of one ident in another), and '<u>contiguity</u>' (or '<u>touch</u>'), understood to be recognized solely by the distribution of boundary-determining characters in space, without any further theoretical commitment with respect to the recognized configurative characters. (No additional commitment was, for example, needed to recognize <u>both</u> cupboard and curtain as touching <u>both</u> wall and floor; but only to judge the means of support justifying the use of 'on' in each case.) Each of these is a binary figure, to be analysed, like all binary figures, in terms of a partial function associating chosen pairs of idents with the relevant binary character in each case. (It will not, in practice, be necessary to deal in this study, formally or in any depth, with n-ary figures of higher order than the binary.)

C. Attention and Neglect

Lastly, before launching a more formal exposition of my analysis, I must try to clarify further the notion of attention, which, with its complementary notion of neglect, is to form the intuitive principle underlying its organization. So far as I know, the attempt to make such central use of this rather familiar pair of concepts - linked with that of <u>relevance</u> in context - is unique to this study. Philosophers may, I think, have fought shy of placing too much reliance on it, because of its threat to raise the spectre of motivation, or other unruly psychological factors. The claim here is, however, that whatever the psychological origins of the structure of attention and neglect, the form this structure takes is expressible in terms of the elements of the resulting conceptual framework as a selection from all those which we may imagine to be present in the situation under study: if other elements <u>are</u> present, they are neglected in the subject's own understanding of the context, as discriminated from the total situation, and should therefore also be neglected in our metatheoretical account of that understanding.

This approach to the structure of attention in understanding does not involve a crudely "behaviourist" claim that psychological factors do not exist apart from this structure, or that no psychological theories of particular structures of attention can be validly constructed, or that I am wrong in thinking that I usually know more or less what my motives are for attending to some aspects of my experience and neglecting others. The claim is only that, although each person's understanding of any context is necessarily subjective, their selection of features for attention can be studied independently of psychological theories; and, when so studied, exhibits certain general structural principles. Groups of people, in reaching an understanding of what they take to be the same context, can use evidence of each others' selection of items for attention - from their use of language, or other indicative action - to work towards a common understanding, without necessarily sharing psychological motivation. Similarly, we can construct a metatheoretical analysis of such a common understanding (or 'group recognitive theory') without attending to psychological factors. (We shall see that a similar dispensation of neglect, to that applied to the selection of elements for attention, extends to the structures of theoretical commitment adopted by members of a group, governing the coherence or otherwise of the total understanding of the context.)

Without knowing why anyone would want to count the number of swans, say, on the Thames, on a particular June day, we can predict that, in the context of his counting, he will neglect just the following features of his experience on that day:

(a) all features but rivers and birds;

(b) all rivers but the Thames;

(c) all characters of birds except those which distinguish swans from other birds.

'Neglect' does not, of course, mean an absolute denial of existence; it merely absolves us from including in the counter's theory of the context any positive suppositions of the existence of neglected features (entities or properties). Some of his decisions - such as what he counts as swans (say, cygnets or not), or as being 'on the Thames' (in terms of boundaries on the banks, at junctions with tributaries or the sea, or in the air-space above) - and how precise he tries to be, will depend on his motivation, and perhaps also on his theoretical commitments about the behaviour of swans. We can neglect the first in our metatheory, but not the second, if his commitments are relevant to the coherence of his understanding of his task. If his decisions appear inconsistent, we can criticise his theory of the context without necessarily impugning his motives.

Contexts of measurement, as we shall see, are constructed so as to restrict and define the field of attention, and regulate precisely the degree of tolerance of neglect, to a point determined by motivations in each case. The rigour of definition is, amongst other things, a means of minimizing discrepancies due to vagaries of motivation, so that they can be neglected in the result.

Attention and neglect are also highly relevant to the intuitive basis for the temporal structure of contexts, as understood here. Our attention to any particular context is typically intermittent. It would be highly unrealistic, therefore, to assume continuity of recognition through time for the general case. If we are to achieve a thorough understanding of the role of measurement - not only of time, but of any quantity which changes over time - this aspect must be brought into our account. Take, for example, a research physicist leaving home for work. At home, he may operate a number of contexts, of which some may be shared with wife or children, built around one aspect or another of the maintenance of life or the pleasures of living, special interests in politics, art, religion, sport, and so on. His journey to work will involve his attention to one or more different contexts, of which some may be shared with random selections of bystanders or fellow travellers, and others private to himself. At work, he may enter a context of sustained attention needing a rigorous framework of common understanding within a small group of trained colleagues, where boundaries of phenomenal discrimination in space and time are sharply drawn for all members of the group, so that readings and findings may be strictly binding on them all.

During the day, his attention shifts from one context to another. There may be interruptions, when demands related to a different context break into a period of sustained attention. <u>Conceptually</u>, however, the time-structures of different contexts are typically understood as <u>running continuously in parallel with one another</u>, more or less independently. Some may be constructed with minimal time-intervals which are vaguely bounded and may be days, years, or centuries long; in others, time-marking events may be sharply distinguished, with minimal measured intervals down to fractions of a second which are only recognizable by reference to recording devices whose discrimination is finer than that of unaided perception. Continuous space or time intervals for each context are typically constructed hypothetically, within the relevant theories, on the partial, discontinuous evidence available within the space-time limits of actual attention to that context. (This is generally so even where there are no interruptions from other contexts.)

The strongest claim that can plausibly be made is that to the extent that anyone's understanding of experience at any time *is* organized, it can be analysed as organized according to some scheme expressible as a coherent theory, however simple. Imagine our physicist kidnapped as he leaves the lab late one night. He is knocked out, and comes to in a small, featureless room, which is almost completely dark. His experience at that moment is very poor in recognizable elements on which he can construct a context. He may search memory or fantasy for contexts to which these few elements (including those of self-perception) can be related. But during this period of confusion he cannot be said to be operating any context. In the absence of a coherent context, we may say that rational thought or action is impossible. Contexts of measurement belong near the opposite end of a spectrum of relative coherence in construction.

To reflect this intuitive structure, with as little idealisation as is consistent with forming a coherent metatheory, the time-structure of Recognition Theories is to be analysed as a '<u>sequence</u>' of '<u>frames</u>', whose duration (if any) is left to autonomous decision under the instantiating theory for each context. A recognitive frame (or '<u>R-frame</u>') is defined as a structure of characterization within which <u>no change</u> of assignments of characters to idents is <u>recognized</u>. Since we are only concerned with changes which are attended to, the duration of the R-frame may be as long or as short as the attention/neglect policy of the instantiating theory requires - or, if so required, it may be treated for the theory as an 'instant', whose duration is neglected. Unobserved intervals between R-frames - again, if any - must be filled in with the aid of commitments of the theory.

Some problems of analysis are connected with the successive appearance in different contexts of what may or may not be "the same" entities, properties or concepts, especially where the same verbal forms are associated with them in each context. For example, our physicist may use the word "heat" in both domestic and scientific contexts: but the way he recognizes it, the types of phenomenal effect he connects with it, the account he would give of its nature, or of the meaning or reference of the word, may differ widely as between the two contexts. The question may be asked whether he is speaking of the same entity, or property, in each case. Problems of this kind present themselves in a somewhat different perspective in this study from that, for example, of linguistic analysis, since the contexts with which I am dealing are primarily perceptual, and identity a matter of perceptual recognition, for which questions of verbal meaning or reference are secondary. Nor would I wish to say, with the operationalist, that the recognition of identity resides in the means by which it is recognized. Identity will be located in the framework of the theory within which recognition takes place.

II. A GENERAL THEORY OF CONCRETE CONTEXTS

Theoretical Structure

Three levels of theory will be distinguished in this study:

(1) Recognitive Theory (R-theory): the structure of understanding in terms of which the individual human perceiver (to be called the Reader) interprets those elements in perception to which he attends in any context. A structure of Group-R-theory (GR-theory) will be defined, intended to capture the form of understanding of perception by any group of persons associated by perception (including that of language) with a common concrete context. The term "concrete" is used to distinguish contexts dominated by a concern with the understanding of perception in this way: it will be omitted except for emphasis or the avoidance of ambiguity, since we shall be dealing almost exclusively with such contexts.

A context for this study is defined as a set of R-theories and a GR-theory adopted as a coherent whole by any group of persons. A degree of idealisation is, of course, involved, and will be discussed in the analysis.

(2) Contextual Theory (C-theory): the fundamental theory proposed in this study for the analysis of contexts. C-theory is therefore presented as a metatheory of R-theories (including GR-theories); which are regarded as instances of C-theoretical structures. The language of C-theory is thus the main part of the language of this study: the

languages of particular R-theories will be discussed in the analysis, and will be as clearly as possible distinguished from non-linguistic elements and aspects of R-theories. These last are taken to be the primary constituents of R-theories, epistemologically prior to their linguistic elements and aspects.

The term 'C-theory' will often be used to distinguish the special approaches or commitments of this study from those attributed to other sources.

(3) Substantive Theory (S-theory): empirical theory, at least partly concerning elements of R-theories, adopted by Readers in association with these theories, and giving rise to commitments going beyond the immediate evidence of perception. C-theory itself adopts no general commitments regarding the borderlines between S- and R-theory, which are taken to be matters for autonomous decisions by Readers themselves in particular contexts, as to what counts as perceptual evidence on which S-theoretical commitments may be raised. It is, however, taken to be typical of concrete contexts in general that well-established systems of S-theory become, in course of experience, incorporated into R-theories by Readers for whom they have become matters of background knowledge. For example, in the light of experience of the changing colours of fruits as they ripen, we come to recognize particular kinds simply as ripe, or not: though this cannot strictly be seen, and the theoretical status of the commitment is attested by the possible

conflict between prediction and fresh experience. An important part of the analysis will concern examples of the use of this principle of incorporation in contexts of measurement (Part III).

The structures of R-theories and GR-theories

In accordance with the intuitive analysis of the structure of recognition given above (introduction, pp. 19-42) an R-theory will be analysed as a sequence of recognitive frames, perceived by a Reader of the context as succeeding one another in time. Each such frame will contain, as elements, two distinct but closely associated fundamental sets: a set of spatially bounded entities, the idents; and the set of properties, the characters, assigned to the idents (or pairs of idents) by autonomous decision of the Reader in the context. I shall also define a set of entities called composites formed from subsets of the set of idents, each of which is recognized, in any one frame, as a whole of which the members of the subset are parts.

The frames are distinguished in that no recognized change of character-assignment occurs within them, while each frame is distinguished from its predecessor by at least one change of character-assignment.

Commitments of R-theory are analysed as of two main kinds: structural commitments according to which certain characters are recognized, within any frame, as determining particular relationships between idents; and implicative commitments according to which assignments of some characters are assumed dependent on assignments of others. The latter are shown to be intimately bound up with structures of assignment of the relevant characters over successive frames of a sequence.

A GR-theory will be analysed as having a form similar to that of an individual R-theory, being structurally associated with a specific set of such theories, with an important role for the language of the particular context.

The steps in the formal exposition of C-theory, as a metatheory of particular instantiating R- and GR-theories for all concrete contexts, are as follows:

A. The Recognitive Frame (R-frame).

B. Characteristic sets and composites (Γ-sets).

C. Some typical structural commitments for unary characters.

D. Some typical structural commitments for binary characters (including position and ordering).

E. The Recognitive Sequence (R-sequence).

F. Sequential (implicative) commitments (including extrapolation and induction).

G. Group R-theory (GR-theory); and the role of language in concrete contexts.

H. Some linguistic and semantic consequences.

J. Some abstract concepts in C-theory: non-affirmative assignments, objectivity and empirical truth, error and falsity.

Summaries appear at pp. 144 (ss. A - D) and 172 (ss. E - F). A general theory of measurement, as a special class of R-theoretical contexts described by C-theory, follows in Part III.

Notes on symbolism used

Although the elements of R-theories, interpreted as values of variables in C-theoretical formulae, are non-linguistic (unless otherwise specified), the ordinary system of logical symbols will be used throughout. It will be claimed that these symbols, so used, can be given their normal meaning, as restricted to this particular domain of application, which concerns structures of non-linguistic perceptual recognition, and includes commitments of R-theory (such as those of implication or existence) governing such structures. Some justifications of this claim will be offered in relation to specified forms of recognitive structure, as the C-theoretical language for their specification is developed. Some deeper aspects of the claim, and its consequences for the application of logic to empirical contexts, will be considered in Part IV.

The logical symbols used will be: $\rightarrow$, implication; $\leftrightarrow$, double implication or logical equivalence; $\neg$, negation; $=$, identity of instantiation in all R-theories; $\neq$, in no R-theory will the connected formulae be instantiated by the identical structure; $=_{df}$, the preceding expression is introduced as a typographical abbreviation of the succeeding one. I shall use normal quantification by $\forall$, $\exists$; the connectives $\wedge$ and; $\vee$, (non-exclusive) or; $\vdash$, the succeeding formula follows logically from previous formulations (specified where necessary); and the set-membership symbol, ε. The symbols $\subset$, $\supset$, $\cup$, $\cap$, will be used solely for set inclusions, union and intersect, and $\emptyset$ for the empty set; $\wp S$, the power set of subsets of S.

I must emphasize that all logical symbols are to be understood, unless otherwise stated, strictly as symbols of C-theory denoting elements in the logical structures of instantiating R-theories. C-theory itself is purely descriptive. In particular, all implications described by the use of '$\rightarrow$' or '$\leftrightarrow$' are to be read solely as constituting commitments adopted by particular Readers under their R-theories of the context in each case: not as overriding commitments of C-theory or as independent logical truths of whatever sort. A special note on the effect of this stipulation on the use of the expression '$\neg(\exists\ldots)$' appears on pp. 66 ff.

A. The R-frame

The R-frame is constituted of (1) a structure of assignments of characters to idents and pairs of idents, *the assignment structure*; and (2) a whole/part structure of idents defined under a concept of *composition*, the *composite structure*.

(1) The Assignment Structure

An assignment structure F^* is a septuple $\langle S, C, C_2, f, f_2, ⋒, ⋔ \rangle$ such that:

A1. $S = \{a, b, \ldots, x, y, \ldots\}$: a finite non-empty set of idents recognized spatially-bounded entities);

A2. $C = \{A, B, \ldots, P, Q, \ldots\}$: a finite non-empty set of (unary) characters (recognized properties of idents);

A3. $C_2 = \{\alpha, \beta, \ldots, \eta, \tau, \ldots\}$: a finite non-empty set of binary characters (or, 2-characters: recognized properties assigned to pairs of idents in S^2 by f_2);

A4. f is a function from S into the power set $\mathcal{P}C$. We may say that f assigns to each x in S a unique subset $f(x) = \{P, Q, \ldots\}$ of characters in C, and that each member or subset of f(x), as well as f(x) itself is assigned to x; conversely, that x is assigned f(x), and each of its members or subsets.

A5. f_2 is a partial function from S^2 into $\mathcal{P}C_2$ so that $(\forall x, y)(x, y \in S \rightarrow f_2(x, y) \subseteq C_2)$.

(Abbreviations: the following are introduced here to simplify the typography of succeeding definitions and expositions: $P/x =_{df} P \in f(x)$; $PQ/x =_{df} P \in f(x) \wedge Q \in f(x)$; $\alpha/x,y =_{df} \alpha \in f_2(x,y)$; $C'/x =_{df} C' \subseteq f(x)$; $C'C''/x =_{df} C' \subseteq f(x) \wedge C'' \subseteq f(x)$.

Note that these formulae have the logical form of conditions of membership or inclusion of subsets of the sets of characters C, C_2, and not of predicative propositions whose subject is the ident x. Hence the use of a distinctive abbreviation rather than the usual propositional forms Px or P(x), etc. Their relationships to such propositions will be discussed in due course.

Terminology: The ordered pair $\langle x, y\rangle$ such that $\alpha/x,y$ is called an α-configuration; and the set $\hat{\alpha} \subset S^2$ of all α-configurations is called the α-figure.) (see note (c)).

A6. $(\forall P)(\exists x)(P/x)$

In all cases C_2 contains the fundamental 2-characters η, τ, with the following properties.

A7. $\hat{\eta}$ is the enclosure figure, defined on $\eta \in C_2$, such that for all a, b, c:

(i) $\eta/a,b \wedge \eta/b,a \leftrightarrow a = b$;

(ii) $\eta/a,b \wedge \eta/b,c \rightarrow \eta/a,c$;

and, writing $-\eta/a,b =_{df} \neg(\exists c)(\eta/c,a \wedge \eta/c,b)$ ...AD 1

(iii) $\eta/a,b \vee \eta/b,a \vee -\eta/a,b \vee (\exists c)(\eta/c,a \wedge \eta/c,b \wedge (\forall d)(\eta/d,a \wedge \eta/d,b \rightarrow \eta/d,c)$

(iv) There is a maximal ident $m^\eta \in S$ such that $(\forall x)(x \in S \leftrightarrow \eta/x,m^\eta)$. N.B. By (i), $\vdash (\forall a)(a \in S \rightarrow \eta/a,a)$; and by AD1, $\vdash -\eta/a,b \leftrightarrow -\eta/b,a$, i.e., a and b exclude one another.

Terminology: Each instance of $\eta/a,b$ is understood to mean that a is recognized as spatially enclosed in b: read, 'a is enclosed in b', 'a is in b', or 'b encloses a'. We may also say that a is an enclosure of b. (see notes (c) and (d)).

A8. $\hat{\tau}$ is the contiguity figure, defined on $\tau \in C_2$, such

(i) $\tau/a,a$

(ii) $\tau/a,b \leftrightarrow \tau/b,a$

(iii) $\tau/a,b \rightarrow (\exists c)(\eta/a,c \wedge \eta/b,c \wedge (\tau/a,c \vee \tau/b,c)$

(iv) $\eta/a,b \wedge -\eta/b,c \wedge \tau/a,b \wedge \tau/b,c$

Terminology: Each instance of $\tau/a,b$ is understood to mean that a is recognized as sharing some part of its spatial boundary with b: read, 'a touches b'. (See note (d)).

(2) The Composite Structure

The C-theoretical account of whole/part composition involves the definition, with respect to the set S of idents, of a set $S^{()}$ of composites of sets of idents: by means of an extension of the domain of assignment under f_2, for the 2-characters η, τ, from pairs in S^2 to pairs in $S \times S^{()}$, $S^{()} \times S$, and $S^{()} \times S^{()}$, as follows:

A9. The membership of the composite set $S^{()}$ is given by:

$(U) \in S^{()}$ iff (U) is the composite of the non-empty set $U \subseteq S$, such that:

(i) $(\forall x)(\tau/x,(U) \leftrightarrow (\exists a)(a \in U \wedge \tau/x,a))$

(ii) $(\forall x)(\eta/x,(U) \leftrightarrow (\exists a)(a \in U \wedge \eta/x,a))$

(iii) $(\forall x)(\eta/(U),x \leftrightarrow (\forall a)(\eta/a,(U) \rightarrow \eta/a,x))$

(iv) $(\exists x)(\eta/x,(U) \wedge \eta/(U),x)$.

We also define exclusions for composites by:

$-\eta/x,(U) \leftrightarrow_{df} (\forall a)(a \in U \leftrightarrow -\eta/a,x)$.

N.B. $\vdash \eta/x,(U) \wedge \eta/(U),x \rightarrow (\exists a)(a \in U \wedge \eta/x,a \wedge \eta/a,x)$; $\vdash x = a$. So x satisfying condition (iv) is a member of the component set U of (U). It will be called the bound-ident of U or of (U): written $x = \textcircled{U}$. We say that (U) is composed of the members of U, which are its components. Bearing in mind that for all a, $\tau/a,a$ - which is to be read as saying that the boundary of each ident is determined by this contiguity-figure at every point of its recognition - condition (i) is taken to determine that a composite shares all recognized boundaries of its components, in that whatever touches a component touches the composite, and conversely. This is taken to justify an assumption that to recognize a composite is in principle to recognize its bound-ident. A full note on composite structure follows in note (f).

(3) The R-frame

A10. A recognitive frame (R-frame) is a pair $F = (F^*, S^{()})$ such that the composite set $S^{()}$ consists entirely of composites of sets of idents recognized in the assignment structure F^*.

We shall see that in most instances the structure of an R-frame is restricted by one or more commitments of the associated R-theory, for which typical forms will be defined. The frame (generally so called, in the absence of ambiguity) may also be enriched, especially in contexts of measurement, by the recognition of a positional function Π, to be defined below (Section C(3)): yielding specific positional characters in C with respect to recognized figures as defined in A5.

Notes on the R-frame

(a) Language of illustrations and examples. In order to bring out the intuitive meanings of various aspects of the analytical treatment throughout the study, illustrative examples will be given in ordinary language. Double quotation markes "..." will be used to signal the use of terms to evoke types of idents, characters, etc., in illustration of typical R-theories, as elements of non-linguistic recognition, rather than as terms either of C-theory or of the language of the context.

(b) Uniqueness of f(x) for each x (A4) is to be understood as the condition that idents are only distinguishable as such where character-differences are recognized. This has one consequence which may appear at first counter-intuitive: that where a number of spatially-bounded entities are recognized which are not distinguishable from one another by unique characters (say, a "pile" of "bricks"), such entities are not to be analysed as so many distinct idents, but as constituting, all together, a single spatially-disconnected ident. Such disconnected idents also occur as the bound-idents of certain composites, a case which will be considered below.

In many cases, where the majority of disconnected enclosures of such an ident are indistinguishable as distinct idents enclosed in it, any one may exhibit a special character which we may call a mark (such as a crack or a patch of untypical colour), by which it acquires unique characterization and recognition as an ident. Such a mark may, indeed, be put on it for the purpose. Marks may not only distinguish particular idents, but also configurations or composites, made up of several idents which thereby acquire unique characterizations (such as a "course of bricks", one or more of which may be marked). Counting, from a particular recognized boundary or mark, may be used to pick out otherwise unmarked enclosures uniquely. This involves the notion of a series of configurations in a serial figure - to be defined below (C13). Pointing may act as a mark by setting up a configuration consisting of a finger and a particular enclosure to be picked out. A series of marks may be constructed along a countable series of enclosures determined by a serial figure. No general account is likely to cover all possible cases of recognitions of disconnected idents or the uses of marks: but certain particular types of structure of these kinds will be analysed in more detail for measurement contexts.

(c) Figure and Relations. Only binary figures, based on the recognition of 2-characters, have been defined above for the R-frame; although, as indicated in the introduction (p. 36f), it is supposed that n-ary figures of higher cardinality are commonly recognized. For example, we may regard the "human-shaped" figure as a set of 6-ary configurations of trunk, head and limbs, including non-human members like ginseng or mandrake roots. Any formal reduction of such figures to complexes of binary figures, even if possible, fails to capture the essential intuition in such a case. For the purposes of the present study, however, the definition of a number of different types of binary figures will be found adequate for the analysis of a wide range of contexts, including all those of measurement.

The set $\mathcal{C}_2$ of binary characters is by implication non-empty, since it always contains η and τ; like $\mathcal{C}$, $\mathcal{S}$, and consequently $\mathcal{S}^{()}$, it is defined finite since no one can recognize (though they may conceive) an infinite number of characters (or idents) in a concrete context. The figures $\hat{\eta}$ and $\hat{\tau}$ have been taken as fundamental to all recognition in concrete contexts, since they are claimed to capture the essential properties of enclosure and contiguity inherent in the recognition of ident-<u>boundaries</u>; these being determined by differences in recognized <u>unary</u> characters (in $\mathcal{C}$) as distributed in space, which are thereby assigned to idents on either side of each boundary so determined. A full account of this aspect is given in note (d) below. The most obvious cases of figures are those recognized visually, in terms of the spatial disposition of their components, just as we may recognize relatively complex unary characters like "ring-" or "pear-shaped". But, as was indicated in the introduction, figures may be recognized in other sensory modes: configurations may be recognized, for example, in terms of the perceived weights, warmths, or colours of their components. Examples of what will be called <u>comparison</u>-figures in these modes will be discussed in depth below.

It was also pointed out that the recognition of a configuration creates the conditions for the recognition of more than one <u>relation</u>, in the standard sense; but that the question <u>which</u>, if any, of the relations generated by a particular configuration recognized in a particular context is <u>attended</u> to, is determined by the motivation of the Reader in each case. This will be analysed only in terms of the logical <u>consequences</u>, in the relevant R-theory, of the particular recognition; and these will be shown as analysable in terms of specifiable types of <u>commitment</u> of the theory, making some recognitions dependent on others. The commitments defined for the figures of enclosure and contiguity by the axioms A7, 8 are fundamental examples. These figures are also typically recognized visually; but they can also be recognized, for example, by touch; someone or something may be heard as inside a room or building; something may be even smelt as inside a box or cupboard, which itself may be only felt (in the dark, or by a blind person).

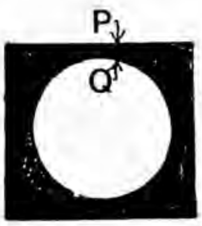
The anti-symmetry of $\widehat{\eta}$, and the symmetry of $\widehat{\tau}$, reflect their intimate involvement in the system of determination of the boundaries of idents. Both are also reflexive: all other figures with which we shall be concerned are irreflexive, i.e., they relate only pairs of

<u>different</u> idents. No ident can be recognized as "to the left of", "larger, warmer, or louder than", itself. Indeed, no sense can be made of comparing an ident with itself in terms of size, warmth, loudness, or any other comparative character. Binary characters may have a number of other properties, in addition to asymmetry, restricting their assignment to pairs of idents. These aspects will be discussed below, following definitions of the relevant types of structural commitments. Apart from η and τ, no binary character is fundamental to the recognition of idents in general; though others may help, as we shall see, in fixing the identities of particular idents.

<u>(d) Enclosure, contiguity ($\hat{\eta}$, $\hat{\tau}$) and boundaries.</u>

A simple diagram, below, will be used to illustrate some of the more fundamental aspects of the analysis of figures and relations of enclosure and contiguity, in a context of the characterizations of the relevant idents.

Three alternative descriptions of the diagram will be given, using different types of character-recognition. The diagram is <u>not</u> offered as an illustration of typical configurations in particular R-theories, but as a set of characterized idents to be immediately recognized: the descriptions in the table belong, in each case, to the language of an R-theory of the diagram.



<u>Diagram 1</u>

Each of the three descriptions are taken as independently adequate for the recognition of three idents from the diagram, so that each could form an independent R-theoretical reading of it. But the three descriptions are reconcilable, as determining what, under a fourth, comprehensive R-theory, would be recognized as the same set of three idents, a, b, and c. (The idents are not so marked on the diagram, or distinguished other than by their descriptions.) The comprehensive R-theory could either be a Group-R-theory operated by three different Readers of the context, each providing one of the descriptions; or it could be operated by a single Reader assigning different characterizations successively (in three R-frames following one another in time) or contemporaneously (in a single R-frame).

Reading by	Ident a	Ident b	Ident c
1. Shape	The square area (enclosing the round area)	The round area	The area with one square and one round boundary
2. Colour	The black area with a white centre	The white area	The black area surrounding the white area
3. Marked boundaries	The area bounded by P (not Q)	The area bounded by Q (not P)	The area bounded by P and Q

Attention is now drawn to the following points:

(i) Note the distinction between surrounding (description

2(c)) and enclosing (1(a)).

(ii) We have already discussed the use of marks to distinguish idents (above, p.56, note (b)). The 'marked boundary' method of description (3) introduces a variation, where the mark is used to distinguish one or more idents by characterizing their boundaries. In such a case the mark is not to be analysed either as part of any ident whose boundary it marks, or of the boundary itself; the space it occupies, whether enclosed in the ident (3(b)) or not (3(c)) is neglected. Such a use is common in measurement in which they may or may not be enclosed. In any case, a mark is always to be analysed as a character, whether or not a boundary-character (i.e., a character used to distinguish a boundary).

(iii) More generally, a character which, in some sense, is assignable only to part of an ident, is frequently used to characterize the whole ident without, necessarily, involving the recognition of the part exhibiting the character as a distinct ident. Description 2a would describe a, even if there were no theory of the context recognizing b or c as distinct idents.

A more naturalistic example (in both senses) is the common description of the robin as "red-breasted": it plainly characterizes the whole robin, and not the breast, which is not normally thought of as a separate entity. If the breast were to be picked out in this way, it would be called not "red-breasted" but "red". (Before leaving the linguistic aspect, I suggest that the locution, "the robin has a red breast", would not affect the matter by introducing the apparently referring noun, "breast". The information given and received by both locutions would normally be the same.)

It may be instructive to mention a possible context in which a relevant theory <u>would</u> pick out the breast in this way: an ethological experiment in which a male robin, in breeding condition and in his breeding territory, is presented with

(1) a normal stuffed robin

(2) a stuffed robin with the breast discoloured

(3) a red object of roughly the right size but attached to a shapeless lump of material.

The expected result is that the robin will attack (1) and (3) but not (2), supporting the (non-recognitive) theory that the robin reacts to the red breast and not to the bird or bird-shaped object. So the breast, here, is a distinct entity to which the experimenter's recognitive theory of the context attends as such. The fact that it forms part of the <u>boundary</u> of the bird, or other ident, is not specifically relevant to its role as distinguishing, or identifying, character. Generally, identifying characters may be of many kinds, not necessarily recognized in every part of the ident, nor especially in its boundary. The idea that we may distinguish a property in part of an entity (or, indeed, part of more than one entity) without distinguishing that part as a different entity, seems intuitively reasonable, and will prove useful in our analysis, especially of marks and boundaries. But such an idea will be unfamiliar to readers of many earlier analyses. For example, in Lesniewsky's mereological analysis (Luschei 1962), every part of a whole is represented in the theory, as a distinct element, whether or not it is perceptually distinguishable, or actually distinguished. Goodman, in his Structure of Appearance (1951), distinguishes as a different entity whatever appears differently. Again, topological analysis makes a point of distinguishing boundaries from the entities they bound, and entities which do from those which do not include their own boundaries (as closed or open sets). It may be claimed equally plausible, at least for the analysis of perceptual recognition, that we identify the boundaries of an "open" space, such as a "room", with those of the entities such as "walls" which bound it. In any case, an analysis which distinguishes boundaries as entities which are themselves bounded is in danger of vicious regress.

Note that descriptions 2(b) and 2(c) provide an example of a boundary recognized as a locus of discrimination between colour-characters uniform over the whole of each ident, and not by any specific boundary-character. The essential point, for C-theory, is that, for each pair of distinct idents, there must in principle exist at least one character of the R-theory in each frame which is assigned to one and not to the other, in such a way as to determine the boundary between them. We have seen, also, that this boundary need not even be continuous; an ident may consist of any finite number of disconnected spatial enclosures, without these enclosures being recognized as distinct idents.

Contiguity, defined in terms of the sharing of boundaries, can only be understood in the context of the idea that idents may share parts. Axiom A7(iii) requires that, if any shared part encloses space, it is recognized as an ident: but, however vague a boundary at which two idents are distinguished, any space it may enclose is neglected. It has already been indicated (p.33) that the boundaries at which idents are distinguished need be no sharper - though they must be no less sharp - than the instantiating R-theory requires. The fundamental criterion for adequate sharpness is just that axioms A1, 2, 4 are satisfied, so that characters are unequivocally assigned to distinct idents on either side.

However, in the R-frame as defined, not all boundaries necessarily divide one ident from another. The space determined for the context itself, by recognition of the boundaries of its maximal ident m (A7(iv)), is inherently limited. Only elements within these boundaries are attended to, the remainder of space being neglected. Similarly, within these boundaries, only the space occupied by recognized idents, as characterized, is attended to; typically, large parts of the space enclosed by m are neglected (see next note). The boundary between an ident and neglected space is subject only to the same criteria for adequate sharpness as that between two idents; namely, that recognized characters are unequivocally assigned to the ident so bounded. Recall, incidentally, that any such ident, or the maximal ident itself, may be spatially disconnected.

(e) Neglect and complementarity

The effect of the normal restriction of the scope of quantifiers to elements of the theory under analysis (in this case the idents and characters of any instantiating R-theory) is to be understood as strictly governed by the limits of attention of the Reader in context. So, where the negation sign $\neg$ appears before the existential quantifier $\exists$, (as in the abbreviative definition AD1, p.53 , what is being denied is not necessarily the presence of any entity or property which might possibly be perceived or supposed to satisfy the stated conditions; but merely that any such entity or property is attended to as an element of the R-theory. In other words, it may be read as saying that either no such element is present, or, if such an element is present, it is neglected. Obviously the decision of any Reader to neglect the possible presence of any element satisfying any given condition lays his theory open to failure as an understanding of the context; nevertheless, it is an autonomous decision in the light of a rational consideration of the situation, and not subject to any general logical restriction (other than that on open self-contradiction). In measurement contexts, as is well known, such decisions are made systematically under precise mathematical rules, which will be analysed in due course.

The formula $\neg(\exists x)(\phi(x))$ - where $\phi(x)$ is any condition on x - is logically equivalent to $(\forall x)\neg(\phi(x))$. But the truth of an instance of such a formula for any particular ϕ in any particular context is determined, in the first form, by a single decision of the Reader; and in the second, only by reference to every ident of the context. The first form will therefore be used in every case of the occurrence of such formulae in this study. Such cases, which aim to formulate the main types of commitment to neglect in R-theories, are few; but, as in AD1, important. Attention will be drawn to other cases as they occur (see pp. 85, 108, 145, 154).

Justifications for decisions to neglect possible elements of a context are of two kinds: (i) force majeure - that such elements, if present, cannot be unequivocally recognized; (ii) irrelevance - that the presence or otherwise of such elements has no logical consequences under the theory of the context, a matter which is intimately bound up with the motives for the construction of that theory. In the first case, commitments of S-theory (going beyond the perceptible evidence) are often used to complete the relevant theoretical structure.

The structure of neglect is reflected not only in explicit negations of existential commitments, but also in the lack of commitment to the existence of certain possible elements of the R-theoretical structure. This shows itself most pervasively for possible spatial complements of enclosed idents under η, contrasting with the automatic commitment to complementarity in the memberships of sets under ε. Specifically, we are not committed to recognizing:

$$(\forall a,b)(\eta/a,b \wedge a \neq b \rightarrow (\exists c)(\eta/c,b \wedge -\eta/c,a))$$

whether or not we add the condition:

$$(\forall d)(\eta/d,b \wedge -\eta/d,a \leftrightarrow \eta/d,c);$$

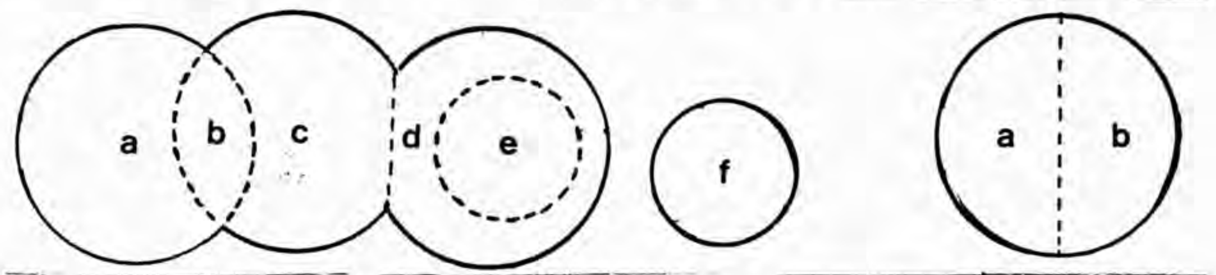
which would make c into the counterpart, for η, of the normal set-theoretical complement of a in b. This aspect of neglect has profound consequences for our analysis, as indicated in the introduction (pp. 38 ff). Commitments to spatial complementarity will be shown to depend on relatively sophisticated constructions of R-theory, involving special characterizations of composites.

(f) Composition

The composite is not defined for every subset of idents in *S*, only for those whose composites are recognized as such. This condition is implicit in the existential quantifier on the last bracket in the definitions A9(i), (ii); only those idents, sets and contiguity relations exist for the theory which are recognized, i.e., attended to, in context. All such recognitions depend on the associated recognitions of characters assigned to the relevant idents. A number of ways in which composition (i.e. the structure of composites) is determined by characterization will be schematically defined in the following Sections. These will not be claimed as exhaustive, but will be found extremely versatile as frameworks for the analysis of a variety of contexts.

Many forms of composition are possible, since the definition does not restrict in any way the structures of enclosure or contiguity between the components. Two main types of structure are of special interest. Where the component set consists only of discrete, mutually excluded idents, the outer boundary of the composite is the totality of those of all the components. If we recognize the composite of "all the swans on the pond", its boundary is just that of all the individual swans. In the contrasting case where all the components are enclosed in one member of the set, the boundary structure of the composite may be extremely complex. In a square diagram ruled into smaller squares, we may recognize the diagram itself as a member of the component set of the composite of "all the squares in the diagram"; but the set may include not only the smallest squares but many others which overlap or enclose one another. Similarly complex, and less regularly ordered, composites of this type will be considered later.

The key property of the bound-ident $\boxed{U}$ (A9(iv)) is that whatever touches the composite either touches $\boxed{U}$, or a part of a boundary of another member of U not shared with $\textcircled{U}$, i.e., an internal boundary of the composite. We shall say therefore that the boundary of x is the external boundary of the composite; and any part of any boundary of any member a ε U which is shared with x is an external boundary of a (with respect to U). If the members of U are disconnected, in the sense that none touches, overlaps or encloses another, the external boundary of the composite consists just of those of the members of the component set, and no internal boundaries are recognized. An ident, unlike a composite, may not have internal boundaries. Some elementary aspects of these structures are illustrated in diagram 2(a), below. Heavy lines indicate external boundaries, dotted lines internal ones, of the composite ({a,b,c,d,e,f,x}), where x is the (unmarked) bound-ident whose boundaries are just the heavy lines.



(a) (b)

Diagram 2

(The diagram is ambiguous as to whether b is enclosed in a or c, or not; and whether d encloses or surrounds e: autonomous decisions for any Reader of the context, and immaterial to the present purpose of the diagram.)

As regards the identity of composites, note that it does not follow from the axioms that $(U) = (V) \rightarrow U = V$. Diagram 2(b) gives a simple counterexample; $\{x,a\} \neq \{x,b\}$ - where x is again the unmarked bound-ident - but their composites are identical, under the axioms. Such cases are, however, rare, since few natural characterizations will yield structures like this. On the other hand, it is not uncommon for the same ident to bound two or more different composites; a "human body" may be seen as bounding a composite of "cells" or of "organs", or a "man-shaped" configuration of "head, trunk and limbs" - a figure which it may share with a mandrake root. However, these failures of identity are no embarassment to the theory of this study. As with idents, composites are not primarily recognized by their spatial structure: rather their spatial structure is recognized from the dispositions of recognized characters in space. If two different character sets determine the identical set of idents in any context, they determine the same composite (if any). The theoretical interest of such cases was mentioned above (introduction, p. 30), where it was cited against the policy of identifying properties with sets of entities.

These considerations give a central role to uniformly characterized sets of idents, and their composites, broadly corresponding to a concept of recognized classes. Since they do not necessarily occur in all R-frames, they are not analysed as elements in the basic structure of the frame; they form our first major construction on the basis of the frame.

B. Characteristic Sets and Composites (Γ-sets)

Although all idents, configurations and composites are, as we have said, recognized by virtue of their characterizations, the term characteristic composite will be restricted to the case where recognition of a composite is determined by a set $C' \subseteq C$ of characters assigned to its component idents. Defining $S^x_{C'} \subseteq S$ by:

$$S^x_{C'} =_{df} \{a : \eta/a, x \wedge C'/a\} \qquad \ldots \text{BD1}$$

then, if and only if $(S^x_{C'}) \in S^{()}$ (i.e., $S^x_{C'}$ is composed), we write:

$$\Gamma_{C'}(x) = (S^x_{C'}) \qquad \ldots \text{BD2}$$

N.B. $\vdash \Gamma_{C'}(x) \quad S$; $(\Gamma_{C'}(x)) \in S^{()}$; and $(\Gamma_{C'}(x)) = (S^x_{C'})$.
Initial comment on the rationale of the form of BD2, on which much will depend, is in note (c) below, p. 76.
Terminology: $S^x_{C'}$ is called the base set of $\Gamma_{C'}(x)$; $\Gamma_{C'}(x)$ is called a Γ-set, being the C'-set on x; $(\Gamma_{C'}(x))$ is a Γ-composite, or the C'-composite on x; the members of $\Gamma_{C'}(x)$ are its Γ-components; the members of $S^x_{C'}$ are the C'-components of $(\Gamma_{C'}(x))$; and $\boxed{S^x_{C'}}$ or $\boxed{\Gamma_{C'}(x)}$ is called a Γ-bound, or the C'-bound on x. C' is a Γ-determinant set,

<u>the determinant set of</u> $\Gamma_{C'}(x)$; and where $C' = \{P_1, \ldots, P_n\}$, or $\{P\}$, we may write $\Gamma_{P_1}, \ldots, P_n(x)$ or $\Gamma_P(x)$; in any case, $P \varepsilon C'$ is a Γ-determinant character for $\Gamma_{C'}(x)$.

The following <u>special cases</u> are of interest:

(1) The Γ-set has been defined 'on' a variable ident x, which may be put = m^η (the maximal ident). $S^{m^\eta}_{C'}$ is then the set of all idents recognized in context as characterized by C'; it will be written just '$S_{C'}$', wherever appropriate. $\Gamma_{C'}$, Γ_P are therefore the most general forms of characteristic sets, those of all C'- or P-characterized idents in context (and their Γ-bounds, whether or not so characterized: see below).

But the <u>x-restricted</u> form (defined <u>on</u> x) is at least as important in the analysis of concrete contexts. It reflects, for example, the situation mentioned in a context of the counting of particular sets of idents (above, p.31), where many of the Γ-determinant characters of a set to be counted (such, e.g., as "whiteness" for "swans") may or may not be Γ-determinant, or otherwise attended to, outside the boundaries either of the composite itself (that of swans), or of some larger composite (say, that of "birds"). We shall call x the <u>space-restrictive ident</u> in such a case.

(2) If $\eta/x,y$, $\Gamma_{f(x)}(y) = \{x\}$; otherwise $\Gamma_{f(x)}(y) = \emptyset$. Necessarily $\Gamma_{f(x)} = \{x\}$ (on m^η). So, as we add members to a Γ-determinant set, we tend to reduce the membership of the Γ-component set, until we reach the '<u>identifying</u>' set f(x) for some x.

(3) But cases often occur when a characteristic set has only one member, x, although its I-determinant set does not include every member of f(x). A very general case is given by the result:

$$(\forall P,x)((\exists y)(\forall z)(\eta/z,x \wedge P/z \rightarrow z = y) \rightarrow \Gamma_P(x) = \{y\}).$$

This is just the case where y is the only ident in x characterized by P, but may be assigned any number of other characters, attended to on account of theoretical commitments attaching to them: even if a swan is the only "white" thing in a particular R-frame, it will not only be its whiteness which identifies it for us, if we are counting swans.

<u>Notes on Γ-composites and Γ-sets</u>

(a) Every subset $S' \subseteq S$ of idents in an R-frame is, in principle, recognized; <u>a fortiori</u>, every subset $S^x_{C'}$, which may happen to be determined by the common assignment of any subset $C' \subseteq C$ of characters. By definition, however, <u>nothing follows</u> under an R-theory from the recognition of any such set, unless the theory adopts some <u>commitment</u> for the set by virtue of its means of recognition: i.e., its characterization. We shall find therefore that all commitments of R-theories are described in this study in terms of particular <u>sets of characters</u> for which they are adopted (including relational commitments defined on configurative characters). All commitments defined for a given set C' are uniquely adopted just for the set of idents so determined (where relevant, with respect to a chosen space-restrictive ident). It will consequently be assumed that the recognitions of sets of idents as Γ-sets are restricted to those whose Γ-determinant sets carry commitments of the R-theory of the context.

Nelson Goodman, in his Structure of Appearance (1951), showed some concern over the existence of what he called 'anomalous sums' amongst the permissible collections of the 'qualia' which formed the elements of his theoretical structure - such, perhaps, as the set of all swans and empty cigarette packets. (I shall generally use single quotes, '...', to mark technical terms used by other analysts.) These supposed anomalies gave him, in fact, no serious trouble; and the present analysis allows us to observe with confidence that they need not be expected to do so - they are generally, and properly, ignored. They break no rules, they are just boring: their structure as sets is neglected.

(b) A more intuitive way of describing the recognition of a set of idents as a Γ-set is to say that it is recognized as a whole (to which a meaning is attributed, in terms of theoretical commitments applying to all of it). But to recognize a set as a whole is not the same as perceiving the whole of it. In this respect it does not differ from the recognitions of single idents, which, as has been said (e.g., p. 25), are almost without exception done on partial evidence. Just as a house is recognized as a whole from a view of its street frontage, so a characteristic set is typically recognized from the perception of one or more of its members. The project of counting swans on the Thames starts with the recognition of one swan as a member of the set, whether or not the remainder are immediately perceived.

(c) Γ-bounds, Γ-sets, and base sets. Recall that, if a character-set $\mathcal{C}'$ determines a set of idents, it determines a set of boundaries at which these idents are discriminated from all others in context. These are just the boundaries (external and internal) of a composite which, if recognized, has a component set consisting of this $\mathcal{C}'$-set itself, including the $\mathcal{C}'$-bound (whether or not this is a member of the base set $S^x_{\mathcal{C}'}$). The question whether the $\mathcal{C}'$-bound is a $\mathcal{C}'$-component or not is, as we shall shortly see, a matter for decision under the commitments of the R-theory for each $\mathcal{C}' \subseteq \mathcal{C}$ whose Γ-composite is recognized. But, in either case, every part of the boundary of the $\mathcal{C}'$-bound necessarily coincides with part or all of the external boundary of at least one $\mathcal{C}'$-component. So to recognize a $\mathcal{C}'$-set is inevitably to recognize every part of its external boundary, i.e. of the boundary of its $\mathcal{C}'$-bound, by virtue of just that characterization by which each of its members is recognized, and which carries any relevant commitment of the R-theory. This consideration completes the reasoning by which the definition of a Γ-set in terms of its composite, and so including its Γ-bound in every case, is justified.

The Γ-bound, so recognized, can easily be shown to be unique. Considering the case shown in diagram 2(a) (p. 70), we see that once the ident x, bounded by the external boundaries of the members of the set {a, ..., f} (heavy lines), is recognized, no other bound-ident will satisfy the axioms of A9 for that set. Either it will fail to enclose {a, ..., f}; or it will enclose x - so failing to satisfy A9(ii), since x is not enclosed in any member of {a, ..., f}. The argument is completely generalisable, whether the relevant Γ-set is two-dimensional, as in the diagram, or one- or three-dimensional (it is assumed that no spaces of more than three dimensions are perceptually recognizable).

(d) It was pointed out that the diagram is ambiguous with regard to the mutual enclosures of the members of the set {a, ..., f}. Three alternative characterizations of the space bounded by x, which illustrate several points which have been raised, may be considered: (i) the set of <u>round areas</u>, in which a, c both enclose b, and d encloses e; (ii) the set of <u>unbroken areas</u> (not intersected by recognized boundaries), in which a, c exclude b, and d surrounds, but does not enclose, e; (iii) the set of all <u>outlined areas</u>, which will include every member of set (ii) plus every composite of a subset of that set. (In set (i), c, for example, is intersected by part of the boundary of b.

This part-boundary is an internal boundary of the composite, but not an internal boundary of c: it is not a boundary of c at all.) Each description, by its characterization, generates a different Γ-set. We recognize these three sets, as described, as alternative decompositions of x. Note, first, that the Γ-bound x is a member only of (iii), not being either round or unbroken. Secondly, under all three descriptions, the sets {a,c,d,f} and {b,e} are complementary in the set {a, ..., f}: but only under description (ii) can we form the spatial complements ({a,c,d,f}) and ({b,e}), in x as bound of the whole set. Fuller discussions of the relevance of Γ-bound membership of the base set, in relation to semantic analyses of 'mass' and 'count' terms, and to recognitions of the "same" character as assigned to different idents, is given at the end of the next Section (pp. 90 ff, (h), (j)); which introduces certain types of commitment involved in the recognition of ident-boundaries.

C. Some Typical Structural Commitments for Characters in

I have emphasized throughout (esp. pp. 55 , 71) that the structure of the frame is determined by the recognized distribution of characters in space. This primary role for character-recognition is realised in each particular frame by the Reader's autonomous choice of characters for attention; and his recognitions of particular sets of characters (alone or in combination) as having specific structural properties in context. It is claimed that, in spite of the evident variety of perceptual experience, schematic definitions of a few types of commitment with respect to sets of characters can be presented, in terms of which the vast majority, if not all, such recognized structural properties can be analysed. Those given here will at least be found adequate for the analysis of all contexts of measurement discussed below (in Part III).

In the present Section I present those for characters in C (unary characters), which are most directly involved in boundary-recognition. In the next Section I shall present types of commitments for characters in C_2 (2-characters), which are involved in the recognitions of countable sets of idents, position, comparison and valuation.

The five types of commitment schematized below are (1) separativity and (2) atomicity, for single character-sets; and (3) distinctivity, (4) exclusivity and (5) scalarity for pairs of sets. The last is used to generate a concept of 'C-scale', potentially involving any finite number of sets. (All definitions given in terms of sets C', C", ... of characters in C are, of course, interpretable for unit sets of characters {P}, {Q}, ...)

(i) One-place commitments: for all $C' \subseteq C$,

C1. $+[C']x =_{df} (\forall a,b)(\eta/a,x \wedge \eta/b,x \wedge C'/a \wedge C'/b \wedge a \neq b \rightarrow -\eta/a,b)$

(read, 'C' is separative on x').

C2. Writing: $\bar{\eta}/a,b =_{df} \eta/a,b \wedge a \neq b$ (read, 'a is <u>a proper enclosure of</u>, or <u>properly enclosed in</u>, b'; or, 'b properly encloses a'):

$K[C']x =_{df} (\forall a)(\eta/a,x \wedge C'/a \rightarrow \neg(\exists b)(\bar{\eta}/b,a)$ (read, 'C' is <u>atomizing</u> on x'. a is called an R-atom of x).

<u>(ii) Two-place commitments</u>: for all $C', C'' \subseteq C$,

C3. $\neq[C',C'']x =_{df} (\forall a,b)(\eta/a,x \wedge \eta/b,x \wedge C'/a \wedge C''/b \rightarrow a \neq b)$ (read, '(C',C") is <u>a distinctive pair</u> on x').

C4. $/\!/[C',C'']x =_{df} (\forall a,b)(\eta/a,x \wedge \eta/b,x \wedge C'/a \wedge C''/b \rightarrow -\eta/a,b)$

(read, '(C',C") is <u>an exclusive pair</u> on x').

C5. $\downarrow[C',C'']x =_{df} (\exists a,b)(\eta/a,x \wedge \eta/b,x \wedge C'/a \wedge C''/b \wedge \bar{\eta}/b,a) \wedge (\forall a,b)(\eta/a,x \wedge \eta/b,x \wedge C'/a \wedge C''/b \rightarrow (\bar{\eta}/b,a \vee -\eta/b,a)$

(read, '(C',C") is <u>a scalar pair</u> on x'. In this case, therefore, (C',C") is an <u>ordered</u> pair).

C6. $\updownarrow[C',C'']x =_{df} \downarrow[C',C'']x \wedge +[C']x \wedge +[C'']x$

(read, '(C',C") is <u>a separative scalar pair</u> on x').

<u>(iii) The C-scale:</u>

C7. $C^S \subseteq \wp C$ is <u>an n-ary C-scale</u> on x (written '$\downarrow[C^S,n]x$') iff C^S has just n members $C^1, \ldots, C^n \subset C$ such that $(\forall i,k)(n \geq i > k \geq 1 \rightarrow \updownarrow[C^i,C^k]x)$.

We also say that C^S is ordered by the relation $\lambda C_i \lambda C_k(\updownarrow[C_i,C_k]x)$, which by C7 is transitive. (Note that simple scalarity is not transitive; given C',C",C'", and a,b,c in x, such that $|[C', C'']x \wedge |[C'', C''']x \wedge C'/a \wedge C''/b \wedge C'''/c$, we have $(\bar{\eta}/b,a \vee -\eta/b,a) \wedge (\bar{\eta}/c,b \vee -\eta/c,b)$; $-\eta/c,b \wedge \bar{\eta}/b,a$ permits that c shall overlap, or even (counterintuitively) enclose a. Nor does separativity for each of the two scalar pairs prevent this result. I do not think that any weaker form of C-scale is useful or interesting.)

Limits of scalarity. The upper limit of scalarity is set only by the recognition of $\Gamma_C n(x)$ for any C-scale $\{C^1, \ldots, C^n\}$ on x in context, which is of course autonomous. But the limiting case, for the maximal ident m , given by $\Gamma_C 1 = \{m^\eta\}$ implies a highly structured and simplified context of little theoretical interest. The lower limit is set by R-atomicity, for which the following further schemata are relevant:

C8. $K\!\downarrow\![C',C'']x =_{df} \updownarrow\![C',C'']x \wedge K[C'']x$

(read, '(C',C'') is an atomizing scalar pair on x')

C9. $K\!\downarrow\![C^S,n]x =_{df} \downarrow\![C^S,n]x \wedge K[C_n]x$

(read, 'C is an atomizing C-scale on x').

Notes on Structural Commitments for Unary Characters

(a) Each schema has been presented as a condition on assignments of the relevant set or sets of characters, which carries a commitment attributing a structural property; for which an equivalent abbreviative notation has been given in each case. Formally, each condition can be read either (i) as imposing a restriction on the assignments of the relevant character-set(s), according to how the characterized idents are spatially related; or (ii) as imposing a restriction on how idents, assigned one or more of the relevant character-sets, may be recognized as spatially related. In view of the stated primacy of character-recognitions (as governed by actual perceptual experience) it is the second reading which is intended to capture the structural role of these commitments.

Structural commitments are not defined as constituents of the R-frame (nor of the R-sequence of frames). It is assumed rather that the conditions defined for R-frame and -sequence are only satisfiable, in the circumstances of human perception, in a context of the adoption of one or more such commitments, according to which ident-boundaries are determined by recognition of characters. Such commitments are, therefore, taken to be necessary constituents of the R-theory of each Reader of a context, though many are adopted tacitly or even unconsciously.

(b) Space-restrictions and Γ-closure of commitments.
The considerations calling for the above schemata to be defined on a space-restrictive ident x (chosen for each commitment by Reader in context) are similar to those stated for the space-restriction of Γ-composites (above, p. 73). It has been argued that we do not, in general, recognize a character-set as Γ-determinant, unless some

theoretical commitment is adopted for the set, as a whole, in context (above, ib.). The converse principle does not hold with the same generality, at least with respect to structural commitments. Counterexamples will be given below in relation to particular types of commitment. Broadly, the point is that structural commitments are necessary for the unequivocal recognitions of idents and composites in the first place; and only if further commitments, important for the motives of the context, attach to particular character-sets, do we go on to recognize these as Γ-determinant.

However, the recognition of a character-set as Γ-determinant can be expected to add to the theoretical significance of any structural commitments adopted for the same set. Where a structural commitment is adopted for a character-set which is also Γ-determinant, we shall say that the commitment is $\underline{\Gamma\text{-closed}}$. Again, the effects of Γ-closure on particular types of commitment will be discussed, with examples, below.

Plainly, if any commitment is adopted for a given $\mathcal{C}'$ on a given x, it holds for $\mathcal{C}'$ on any proper enclosure of x - including the $\mathcal{C}'$-bound on x, or the $\mathcal{C}'$-bound on any proper enclosure of x on which the composite of any subset of the base set is recognized. Now, a space-restrictive ident x must be characterized like any other. It may be of interest that, while f(x) for space-restrictive x on any $\mathcal{C}'$-determined condition need have no character in common with $\mathcal{C}'$ ("the Thames" shares no characters with the Γ-determinant set for "swans" on the Thames), it may share some characters with $\mathcal{C}'$; but it may not share all the same characters without ceasing to be space-restrictive. In such a case, x is identical with the $\mathcal{C}'$-bound on any space-restrictor in which x may be enclosed, including m^{η}; since no $\mathcal{C}'$-characterized ident recognized in context may lie outside it. In other words, if x is to be space-restrictive, f(x) must contain at least one character which restricts the scope of any commitment for $\mathcal{C}'$, or the recognition of $\mathcal{C}'$-composites, by virtue of some structural commitment adopted for f(x). Some implications of this observation will be discussed below, but no attempt will be made to give a fuller analysis.

(c) Separativity and partitivity. Intuitively, $+[\mathcal{C}']x$ means, "any two $\mathcal{C}'$-characterized enclosures of x exclude one another". It would apply to any adequate Γ-determinant set for "swans", since no two swans enclose or overlap one another. One can easily imagine a context in which the visible outlines of two or more swans become cnfused, say in poor light, but the recognition of two or more necks and heads carries a commitment of separativity whereby the number present can be counted. (In mammals, a fetus may be recognized as a distinct ident - Γ-component - surrounded, but not enclosed, by the mother). Separativity could not be adopted for "whiteness" in swans, in any context where white parts of their bodies were recognized as distinct, enclosed idents; say, their "necks". Separativity does not, on the other hand, prohibit contiguity: e.g., "bricks" in a "pile" might be separative, since no brick overlaps or encloses another, even if, neglecting any spaces between them, they were recognized as touching (a normal case).

Any Γ-determinant set $\mathcal{C}'$ for which separativity is Γ-closed necessarily determines a partition on the $\mathcal{C}'$-bound, in the usual sense: i.e., the $\mathcal{C}'$-bound is completely composed of separate $\mathcal{C}'$-characterized idents. In such a case we say $\mathcal{C}'$ is partitive on the $\mathcal{C}'$-bound (written '$\oplus[\mathcal{C}']x$').

Partitivity could easily be adopted for either "swans" or "bricks" in suitable contexts; and is common in measurement. In counting "swans on the Thames" we might adopt separativity for "birds" in general as well as for swans, but recognize only swans as a Γ-set; separativity would then be Γ-closed only for swans, being thus partitive on that set, and so determining a strict count, not required for birds in general. Either "the Thames" or "birds on the Thames" would be a natural choice of space-restrictor.

Separativity plays an important role in the analyses of Γ-bound membership of the $\mathcal{C}'$-set , and of spatial complementarity, both of which will be discussed fully later.

(d) Atomicity and neglect. $\mathcal{K}[\mathcal{C}']x$ means, "any entity enclosed in, or overlapping, any $\mathcal{C}'$-characterized enclosure of x is neglected in context". Atomicity thus represents one of the few major types of systematic neglect in recognition, as mentioned above (p. 66). For instance, we may choose not to attend to any "brick" part of a "wall" which is smaller than "a brick", or involves proper enclosures of bricks. It follows obviously that if $\mathcal{C}'$ is atomizing, it is also separative, on any x; but it is not necessarily Γ-closed, nor partitive. Some recognized enclosures of our wall may not be made of bricks. Hence the need to distinguish a further condition, to be written '$\mathring{K}[\mathcal{C}']x$', for the case where atomicity is Γ-closed for $\mathcal{C}'$ on x: $\mathcal{C}'$ is then called completely atomizing on x.

In principle, since $\mathcal{S}$ is finite, and η is transitive, every ident which is not R-atomic encloses at least one R-atom; otherwise there is nothing to stop an infinite series of enclosures. But it does not follow that every part of the space of the context is atomized. If a is an R-atom and $\overline{\eta}$/a,b all potential idents in the space enclosed by b and not by a may be neglected; the space itself is not neglected, being part of the space recognized as characterized by f(b). But, if all idents in it are neglected, it is neither itself R-atomic (not being a distinct ident) nor encloses R-atoms. So, only Γ-closed atomizing character-sets are unequivocally completely atomizing within their Γ-bounds; hence the chosen term.

The special case where f(x) is uniquely atomizing on m^{η} for some x, so that x is a singleton R-atom, is plainly quite common. In counting swans, a pond on which there is only one swan may yield such a singleton, if we neglect its proper enclosures as distinct idents. Some simple contexts may be wholly analysable in terms of such singletons. But more interest attaches to richer contexts. In counting swans on the Thames, "birds", including swans, may be R-atomic, but only "swans" completely atomized. "The Thames" may have to be quite richly characterized, to decide just what waters are to be included in the count; but it is plainly not R-atomic, nor are R-atoms in the parts not occupied by birds likely to attract attention in context. More subtly, if we recognize a particular marked "brick" as R-atomic in a "wall" which contains many other bricks not separately recognized, the whole wall may indeed be characterized by many of the characters assigned to the marked brick; such characters could not then form an atomizing Γ-determinant set, but the mark-character would determine recognition of the brick as a singleton R-atom on the wall as space-restrictor.

In contexts of measurement, certain specified elements of the standard apparatus will be found invariably to be recognized as completely atomized; while data are commonly recognized as having a much less regular R-atomic structure. This, indeed, will emerge as one of the important distinguishing characteristics of such contexts.

Recalling that atomicity entails separativity, a further broad observation is of interest. The recognitions of particular characterizations, such as those for "swans" or "bricks", as separative, relies typically (but not always) on memories of previous recognitions, spread over many contexts having only the most tenuous R-theoretical connections. Decisions to treat them as atomizing, except under force majeure, are invariably determined by more or less strict regard to the motives of the context. This reflects the importance of the chosen strategy of neglect to the structure of atomicity, as of other aspects of particular instances of R-theories. In measurement contexts, this strategy is precisely specified.

These considerations bring into sharper focus the question, implicit in a great deal else of what has so far been said, whether more precision can be given to my notion of a character; as a property which is perceptually recognized prior to verbal description, if any. Some clues were given in my discussion of diagram 1, p.60, ff; more will be offered shortly under the head of "sameness of characters" - i.e., what is involved in recognizing a character as the same as one recognized previously, or elsewhere in a frame. It will be found that neglect is again a factor.

(e) Distinctivity. The intuitive meaning here is obvious, and may be reflected in a number of different forms of verbal expression. For example, "≠ ["swan","black"] ("this lake")" could be read, "There are no black swans on this lake", "None of the black birds on this lake are swans", or "No swan on this lake is black". A common basis for commitments to distinctivity is typical patterns of proper enclosure; no "leg" is a "body", no "brick" is a "wall", in the ordinary senses of these words. Such cases will appear again under scalarity, which entails distinctivity.

No general conclusions will be offered regarding the effects of Γ-closure on structural commitments for two or more character-sets. By definition, these concern the structures of <u>intersects</u> of the relevant Γ-sets, if recognized, a matter which will be discussed fully later (pp. 155 ff.). What is to be borne in mind with a view to that discussion is the point already made, that a one-place commitment adopted as Γ-closed on a given space-restrictor is <u>ipso facto</u> adopted for any subset of the relevant Γ-set whose composite is separately recognized for any reason; and therefore, in particular, for any subset defined as its intersect with another Γ-set. In this way, separativity or atomicity adopted for one characterization, say "bricks", may partly impose this structure on another, say "walls", only parts of which are so characterized (made of brick).

(f) <u>Exclusivity</u>. Again, we can illustrate the meaning by alternative expressions. //["oil","water"]("this vessel") could be read, "Oil and water are distinguished as separate volumes in this vessel", or, "There is no water in the oil here, and no oil in the water". (Another vessel in the context may contain emulsion, for which this need not be recognized.) The definition neither requires nor prohibits either of the exclusive pair to be separative on its own; it effectively rules out $C' = C''$, which would be consistent with separativity only where a $\neq$ b. It would rule out, as distinctivity would not, any separately recognized proper enclosure of a "swan" being "black", under // ["swan","black"]("this lake") - such as its "feet". "Black" in that case must be read as "predominantly black".

(g) Scalarity. C5 expresses a very weak notion of scale, not involving any metric concepts. Such a commitment is most commonly instantiated by cases where one sort of phenomenal feature is habitually recognized as enclosed in, or part of, another sort, and no examples of the reverse relationship are known: e.g. "man" and "house", "human leg" and "human body", "table leg" and "table". While not ruling out vacant houses, homeless men, unassembled table legs or amputated human ones, it commits us to the assumption that enclosure of one by the other, if and when it occurs (and it must occur at least once in the context) will be proper enclosure of the familiar kind.

However, C5 would permit various structures, such as overlaps, or the enclosure of one C''-characterized ident in more than one C'-characterized one, which would be anomalous for the quoted examples - though they can be imagined, for instance, with wave-forms.

(h) Γ-bound inclusion in base set: and 'mass' or 'count' terms in semantics. It will be illuminating to discuss these two topics together, since it will bring out some of the ways in which the approach of the present study leads to a different analytical structure from that generated by more language-centred approaches. Semantic analysts have noted that some terms of ordinary language (most simply, certain nouns or adjectives) have the property that, if such a term is applicable to each of the members of any set of entities, it is applicable to any collection of these members, or the set itself, considered as a whole. Other terms have the contrary property, that they are not applicable as a whole to any collection of more than one member of any set to which they are individually applicable. These classes of terms are known as 'mass' or 'count' terms, respectively, on the ground that a term of the second class determines a unique count on any set of entities which it picks out in a given context; while applications of a term of the first class is consistent with many different ways of discriminating and counting particular entities making up a totality to which it is applied. Examples of mass terms are "snow", "white"; and of count terms "swan", "house". Any collection of patches or bodies of snow is e qually "snow", and equally "white"; no collection of "swans" is "a swan", nor of "houses" "a house" (the syntactic role of the indefinite article thus assumes importance in these contexts). Though the role of language in concrete contexts cannot be dealt with fully until later (Section G), we can at this point already note suggestive analogies between my analyses of Γ-bounds which are, or are not, members of their base-sets: and the semantic distinction between mass and count terms. Before the precise nature of these analogies can be understood, two largely terminological sources of possible confusion must be cleared up.

Early semantic treatments of this subject (e.g. by Quine, Word and Object, p. 95; and Moravcsik in Approaches to Natural Language, Stanford U., 1970, pp.264ff.) ran into an awkward problem in the analysis of mass terms, which was presented in the form that, if such a term applies to any entity, it applies to every part of it. The problem then was that most, if not all, such terms were found in practice to exhibit logically arbitrary cut-off points, in the progressive subdivision of entities into parts (e.g., snow "flakes", or at most "molecules") where the principle broke down. This has been overcome, in a sense, by later writers (e.g., Lauri Carlson, 1979), by restating the principle in terms not of subdivisions of wholes, but of collections of parts, roughly in the form I have given it at the start of this note. The relevant property of mass terms was 'additivity'; but additivity, as we shall see, is associated in contexts of measurement with a concept having a quite different structure. The concept of additivity for empirical structures in measurement is analogous to the additive property of numbers, and is more naturally applicable to sets of elements picked out by count terms; in C-theory it will be closely associated with separative Γ-sets, whose bounds are not members of their base sets. The use of 'additivity' in semantics is better understood here in terms of unions than of sums. We may say that a mass term is one which, if it applies to the members of any set, applies to any subset, or any union of subsets of that set, including the whole set.

In this form it comes very close to an analysis in terms of Γ-sets, in form of a commitment that if a $\mathcal{C}'$-bound is a member of its base set, so is the $\mathcal{C}'$-bound of every subset of that set whose composite is recognized. Two points of distinction emerge. The smallest elements of our Γ-structure (not necessarily R-atoms) are those determined by the Γ-determinant set in each case, whose place in the structure of theoretical commitment in context can be made explicit: not just those which happen to be picked out by a particular term of language. Nor are we necessarily concerned with every logically possible collection of these elements, only those whose composites are recognized by virtue of commitments of the context - which may prove an important restriction. But we note that if Γ-bounds are members of more than one subset of the total base set, they may overlap or enclose one another: which is just the condition which is ruled out by separativity of the Γ-determinant set. So a mass term cannot apply to a separative characterization; but we cannot be sure that a non-separative one can be described by a mass term.

If we now wish to associate count terms with separative characterizations, we come up against our second threat of terminological confusion. In any particular

R-theoretical context any recognized characterization determines a unique count of idents and composites (if any), whether or not these may overlap or enclose one another. What separativity ensures for us is not merely a determinate count, but the absence of mutual enclosures or overlaps by members of the base set: so prohibiting base set membership for any Γ-bound of a subset of more than one member. So the count of any subset of the base set is just that of its minimal enclosures; a property which will turn out to have great importance in measurement. A characterization which determines readily recognizable sets of minimal (and maximal) enclosures in this way is one which typically (but not always) leads to the assignment of simple, unitary terms of language: the count terms. But we cannot be sure that such terms draw attention only to separative characters.

We have seen that structural and other properties of Γ-sets are intimately bound up with theoretical commitments of all kinds; these are typically (but not always) carried over from one context to another by the use of language. The main fundamental difference between my approach here and that of semantics is that I set out to distinguish between those <u>specific characters</u> which are shared by a component-set and its Γ-bound and those which are not: the associated verbal expressions do not, typically, do so unequivocally. Nor, indeed, does Γ-set analysis: but it allows the distinction to be precisely stated, and located within a wider framework of theory. It is true that if C' is separative for any Γ-set in context, it remains separative whatever other characters may consistently be assigned to all components: i.e., whether or not there is C'' such that $S^x_{C'} \subseteq S^y_{C''}$ for some x,y and C'' is not separative, and whether or not $C'' \subset C'$. The question just which minimal subset of is separative may have great importance, deriving from any further commitments which may be adopted in context for C', C'', or any subset of either.

The form this distinction may take in any context varies according to the motives of the context and the particular structures of characterization for which commitments are adopted; and I can do no more here than offer an illustration. A Γ-determinant set for "bricks" composing a "wall" is plainly separative, in that it determines the set of individual bricks which exclude one another. But only those characters in that set which determine shape are responsible for the separativity of the set: all other characters of "bricks" are shared by the wall as a whole (such as "red", "baked-clay"). "Bricks" is distinguishable as a count term by its plural form (as is "a brick" by its attached article): "red", "baked-clay" operate here, at least, as mass terms, since they characterize all collections of separate bricks. "Brick-shaped" is separative and not shared by the wall as Γ-bound; "red" and "baked-clay" are so shared, and non-separative.

It is the shape of bricks that leads to the expectation that they can be readily arranged in the form of a wall of any suitable dimensions. Redness may have some aesthetic implications; but the baked-clay fabric is what generates expectations that the final "brick wall" will have the necessary properties of strength and insulation. These are matters of empirical commitment, based on past experience. Different particular sets of characters, all associated with the term "brick" and its grammatical variants, give rise to different commitments. It is seldom necessary, and may be quite difficult, to use linguistic distinctions to analyse out which characters are involved in which commitments, this being left to common background knowledge. We can simply say, "bricks are easily assembled, strong and weatherproof", allowing the relevant associations of characters with commitments to be tacitly understood.

In technical contexts, however, it may become crucial to distinguish the sets of recognizable characters implicated in various particular commitments - especially those characters susceptible of measurement as quantities. Here the precise recognitive basis for structural commitments of separativity will be found to have special theoretical importance. Language must, if necessary, be constructed to make the necessary distinctions. It is the recognitive structure required in context which elicits the linguistic forms: not the converse. At this point the analysis of separativity as a property of character-sets appears to present the recognitive requirements with some precision.

Linguistic considerations of this kind are outside the main concerns of this study. But this discussion may serve to illustrate the way in which commitments to the inclusion, or not, of $\mathcal{C}'$-bounds in $\mathcal{C}'$-sets are associated, inversely, with commitments to the separativity, or not, of $\mathcal{C}'$ in context. Such commitments rest in practice on past experience of the recognition of particular $\mathcal{C}'$-sets, and their survival in R-theory on their consistency with successive recognitions of $\mathcal{C}'$-sets over time: in short, they are _inductive_. The form of such inductive commitments will be considered below, after an exposition of the structure of a sequence of R-frames over time. Meanwhile, questions arise over our understanding of what is meant by recognizing the _same_ characters from time to time, or in different entities at the same time.

(j) _Sameness of characters and similarity of idents._

The salient aspect of perceptible properties is their vast range of difference and variability. So it is hard to say what exactly is meant by 'a character'. One approach is to ask what is involved in recognizing a character, twice or more, as "the same" - not that this question, either, is easy.

The answer must rest ultimately on our intuitions about the experience of such recognitions. But our intuitions of sameness apply differently to recognition of those perceptible properties I have called 'characters' and of those bounded entities I have called 'idents', according to the different ways these elements are involved in the structure of R-theories. I shall therefore keep the use of the term 'identity' for idents, using 'sameness' for characters: idents assigned the same character will be called 'similar' with respect to that character, avoiding any reference to characters as similar to one another. Characters are those elements of an R-theory by which idents are distinguished in space, and identified through time. An ident is identified as the locus of particular sets of characters at different times: its identification is thus dependent on the recognitions of assigned characters. The profound philosophical problems associated with the notion of identity involved, where characterizations change, must be postponed to the end of the study (Section IV.6). A character may be recognized as the same when contributing to the characterizations either of (a) the same ident at different times, or (b) different idents at any time (and assigned accordingly).

It is with respect to the commitments of the relevant R-theory that the difference in the roles of sameness of character and identity of ident emerge most clearly. All commitments are exhibited here as attaching to sets of idents by virtues of the characters assigned to them (both these commitments already schematized and those to be introduced later). Thus, recognitions of two or more characterizations as sharing a common character typically carry commitments of the theory associated with that character. Generally, these commitments are shown as specifying relations of enclosure, overlap or exclusion between Γ-sets of idents (any of which may be unit sets), determined by (sets of) characters recognized as "the same" over time or across space. The only check we ultimately have on the validity of such recognitions of sameness is that these associated commitments prove consistent with experience.

One type of commitment commonly and importantly (though not universally) associated with such recognitions is that to the use or understanding of the same word or expression, at each occurrence, to describe each character (or a syntactically appropriate demonstrative or anaphoric construction). Can we therefore appeal to the language of a context to arbitrate questions over the sameness of characters? In the last note it was pointed out that words typically draw attention to more or less complex combinations of characters, and that the sets of characters associated with any particular word may vary with context, within a reasonably stable range. Words may also evoke more or less complex changes of characterization, especially in the case of verbs or their derivatives. Thus, they are often used to think or speak of sets of characters commonly recognized together, rather than to recall or specify single characters: whose presence or absence, nevertheless, often distinguishes one ident from another, or whose appearance or disappearance in an ident is recognized as change. A large class of contexts which come near to providing numbers of apposite examples in precise form, is found in books describing biological species (from which the term 'character' has been borrowed). Minute perceptible distinctions between one species and another, or between different phases of development in the same species, are given in the text; but, if the book is to be used for recognitions in the field, verbal descriptions must be supported by visual illustrations, which exhibit visual characters for comparison with actual specimens. Even technical terms specially constructed for the purpose fail, by themselves, to determine recognitions of the characters concerned; combined with illustration, however, they come close to determining unequivocally the critical single characters assigned to different species or phases. When we move from technical contexts like these to questions of what characters determine our daily recognitions of well-known faces or voices, we have passed beyond the point at which words or illustrations can hope to yield unequivocal determinations; yet the discriminations are made, the characters are recognized. We may say we recognize George on the phone, without being able, or wanting, to specify verbally just what character(s) distinguish the voice from all others.

It seems, then, that linguistic forms fail, in many cases, to capture the essential criteria by which we recognize characters as the same, or different, when contributing to different characterizations. The response of linguistic analysis to this situation must, it seems, be either to claim that no true distinctions or similarities exist where none can be specified in appropriate language; or, more modestly, to say that the best we can do is to study those which can be so specified, and assume that no serious principle is thereby neglected. These strategies of neglect essentially originate with Wittgenstein, and some of the questions involved arise in a different form in the arguments he started about the notion of a "private language". That notion will be discussed in Section IV.3. But such neglect is not acceptable in the context of this study, for reasons which will soon emerge. I must fall back on the resources of my analysis of the role of characters in the structure of R-theories.

It was proposed in the introduction that the principle of identifying a property with a set of entities possessing it, cannot be extended from properties of numbers to recognizable characters in context (p.29); and that consequently C-theory has no general criterion for the identity of characters (p.33). In the absence of such a criterion, we must invoke our principle of autonomous decision by the Reader, who may or may not attend to any particular perceptible difference in context. For example, one R-theory may treat all recognizably "red" characters as the same, while another may distinguish many different shades of red, even neglecting the collective characterization "red" altogether.

However, it is plainly unsatisfactory to leave this declaration of contextual autonomy open to arbitrary choice, unsupported by any ground of theory. We shall therefore say that, rationally, a character is the same, as assigned to different idents or in different frames, iff the logical consequences of these assignments are the same under the commitments of the theory of the context for the character. The following loose definition suggests itself: a character is a recognized property which, when assigned (alone or with others) to any ident, contributes essentially to the determination of at least one commitment adopted for that ident. In many cases the fulfilment of these conditions may be unproblematic. The patterns of commitment associated with recognizing traffic-lights as "red" or "green" call for no subtle discriminations of colour, such as those which may be involved in recognizing a fungus as poisonous or not. If, in context, nothing important turns on a question of similarity or distinction, we can ignore it; but, if we are concerned about the consequences, we must determine it if we can. If we cannot do it with words alone, we must use illustrations; or, failing these, rely on our personal powers of perception, comparison and memory.

The common principle in all cases is that the extent to which we can (or must) neglect differences in recognized character is exactly the extent to which we can (or must) tolerate differences in their expected consequences. This (often large) tolerance is determined by the motivation of the context, and the structure of theoretical commitments adopted in its pursuit: consistency of recognitions under these commitments being the sole test of the validity of different assignments of the same character. Such "tolerance" (strategy of neglect of difference) is systematically specified, in numerical terms, in contexts of measurement.

Further consideration of the kinds of commitment that determine the relevant logical consequences must be left until after giving an account of time-successive aspects of R-theories in Section E; and a fuller account of the role of language awaits the analysis of Group-R-theories in Section G. Meanwhile the schematic treatment of structural commitments of the R-frame continues with those for binary characters.

D. Some Typical Structural Commitments for Characters in C_2

Commitments of R-theories for 2-characters always include the conditions laid down above for η and τ; they may also include conditions adopted for particular 2-characters in context, constructed from one or more of the forms of commitment now to be schematized. Four of these are given immediately: asymmetry, transitivity, progressivity and seriality - on which a schema for a counting series is then constructed. Before presenting a fifth, comparativity, we define a notion of position with respect to a given binary figure; and a structure of values for a given unary character, which are in some cases compared. We shall find a close association between the ideas of position and comparative value.

D1. $\neq[\alpha] =_{df} (\forall a,b)\neg(\alpha/a,b \wedge \alpha/b,a)$

(read, 'α is asymmetric'. $\vdash \alpha/a,b \rightarrow a \neq b$).

D2. $\rangle[\alpha] =_{df} (\forall a,b,c)(\alpha/a,b \wedge \alpha/b,c \rightarrow \alpha/a,c)$

(read, 'α is transitive').

D3. $\gg[\alpha] =_{df} \neq[\alpha] \wedge \rangle[\alpha]$

(read, 'α is progressive'. $\vdash \neg(\alpha/a,b \wedge \alpha/b,c \wedge \alpha/c,a)$).

Before defining seriality, we give an abbreviative definition for the base set S_α of α: $S_\alpha \subset S$ is called base set of α if $S_\alpha = \{a: (\exists b)(\alpha/a,b \vee \alpha/b,a)\}$. ...DD1

D4. With each $\alpha \in C_2$ governed only by the conditions so far defined may be associated a distinct character $\dot{\alpha} \in C_2$ governed by the further condition:

$$(\forall a,b)(\dot{\alpha}/a,b \leftrightarrow \alpha/a,b \wedge \neg(\exists c)(a \neq c \neq b \wedge \alpha/a,c \wedge \alpha/c,b)).$$

('$\dot{\alpha}$' may be read "α-next", or "α-dot".

$\vdash S_{\dot{\alpha}} = S_\alpha$, and $\hat{\dot{\alpha}} \subseteq \hat{\alpha}$.

The case $\hat{\dot{\alpha}} = \hat{\alpha}$ is just the case where $\hat{\alpha}$ has only one member, and is of no further interest).

$(\alpha,\dot{\alpha})$ is a <u>serial pair</u> iff $\gg[\alpha]$.

(It is understood that $\dot{\alpha}$ is in principle recognizable for all α, but is only an element of the R-theory if recognized. In general, it is only attended to in association with at least one α-series, as now defined.)

D5. $S^s \subseteq S$ is a <u>series on α</u>, (or <u>α-series</u>), iff

$\gg[\alpha]$, $S^s \subseteq S_{\dot{\alpha}}$, S^s has at least three members, and:

(i) $(\forall a,b)(\dot{\alpha}/a,b \rightarrow (a \in S^s \leftrightarrow b \in S^s))$.

(ii) $(\forall a)(a \in S^s \rightarrow (\forall b,c)(((\dot{\alpha}/a,b \wedge \dot{\alpha}/a,c) \vee (\dot{\alpha}/b,a \wedge \dot{\alpha}/c,a)) \rightarrow b = c))$.

N.B. It follows that if S^s is a series under this definition, its ordering is homomorphic to that of any subseries of the normal series of integers; it is in this sense that I may in some cases call it a <u>counting series</u>.

If series are defined on more than one 2-character in any context, they may be distinguished by subscripts, e.g. S^s_α, S^s_β, ... If more than one series is defined on the same 2-character α, they may be distinguished by numbers, e.g., S^{s1}_α, S^{s2}_α, ... This may easily occur: for example, lines of type at the top and bottom of a printed page may each present a clearly recognized "left-right series of letters", and separately used for counting, without any left-right series being recognized in context for sets of letters drawn from different, widely separated lines.

The principal importance of the counting property of series is seen as its use in the recognitions of distinct identities of idents which are otherwise indistinguishable in context - e.g., in the counting of swans on a pond. This aspect of the property must, in this analysis, be expressed in terms of character-assignment; and it will be convenient to deal with it in the context of the notion of position, now to be defined.

Position

For each $\alpha \in C_2$, and each $x \in S$, we have two determinate (but not necessarily unique) subsets of S, which will be abbreviated as:

$$\overline{\alpha x} =_{df} \{y:\alpha/y,x\} \ ; \qquad \overline{x\alpha} =_{df} \{y:\alpha/x,y\} \qquad \ldots \text{ DD2}$$

Either or both may be empty for any particular α, x.

D6. We now define a partial function Π from $(\wp S)^2$ into C such that:

(i) $\neg(\overline{\alpha x} = \emptyset = \overline{x\alpha}) \rightarrow (\exists P)(P \in C \wedge P/x \wedge P = \Pi(\overline{\alpha x}\ , \overline{x\alpha}))$

(ii) $(\overline{\alpha x} = \emptyset = \overline{x\alpha}) \rightarrow (\Pi(\overline{\alpha x},\overline{x\alpha}) = \emptyset)$.

We shall write $P^x_\alpha = P$ such that $\Pi(\overline{\alpha x},\overline{x\alpha}) = P$. $\overline{\alpha x},\overline{x\alpha}$ are called the <u>α-position determinants</u> of x, or PDs; <u>Π the positional function</u>; P^x_α the <u>α-positional character</u>, or PC, of x in $\mathfrak{a}$. Intuitively, where, for example, $\mathfrak{a}$ is a "left-right" figure, $P^x_\alpha = \Pi(\overline{\alpha x},\overline{x\alpha})$ is just the property of "being to the right of" all members of $\overline{\alpha x}$, and "to the left of" all members of $\overline{x\alpha}$. It is not held necessary or typical that a Reader should be constantly or specifically aware of the identities of all members of both PDs of every ident involved in every figure of the R-frame; merely that the question whether any particular y is a member of $\overline{\alpha x},\overline{x\alpha}$, or neither for any particular x, should, if it arises, have an answer determined by the recognition (or not) of a configuration in $\hat{\alpha}$. Where $\overline{\alpha x} = \emptyset = \overline{x\alpha}$, x has no position in $\hat{\alpha}$, but if either is non-empty, its position is determined. PDs and PCs are in principle determined for all binary figures, including $\hat{n}$ and $\hat{t}$, and all idents have a position in these last.

Notes on position, counting and identity.

(a) All the commitments in this Section have been defined without reference to any space-restrictive ident; i.e., as if they were adopted for all pairs of idents in the R-frame (see pp. 73, 82 above). This has been done for simplicity of exposition. Space-restrictions for such commitments could easily be constructed if required, in the same manner as has been done in C1 - 9. Cases do certainly occur; for example, we may attend to series of "bricks", satisfying D5, in some parts of "walls" and not in others.

(b) The main purpose of this sequence of definitions has been to analyse the structures of commitment involved in a strategy of uniquely characterizing idents by counting; a strategy which has many applications. The principal structure for counting is, of course, D5; which defines an α-series as a base set (of at least three members) each of whose members has a unique predecessor and successor (if any) in the series, in whichever direction we choose to count. The common use of the term 'base set' for both binary figures and Γ-sets (p.72) reflects the fact that counting strategies are frequently used on such sets, whose Γ-determinant characterizations, by definition, consist of those characters with respect to which they are similar, and by means of which they cannot, therefore, be uniquely characterized (if the set has more than one number).

But the analysis reflects the general case in which a serial figure potentially capable of generating counting-series (such as "next right", "next larger", satisfying D4) does not automatically generate uniquely determined series. Formally, the negative condition in D4, which excludes intermediate idents, does not ential D5(ii), which determines unique succession. Two or more idents may be "to the left of", or "larger than" a given ident, with none intervening, and so form parts of different series. Moreover, there is nothing in the structure of D5 itself which decisively distinguishes one possible series from another.

(c) Note, further, that this negative condition in D4 is another major fundamental case of a strategy of neglect (p.66); what is denied is not that no such intermediate could ever be supposed to exist, but that its existence is recognized in context - either because it would be practically unrecognizable in the given conditions; or because its existence would be inconsequential in the

R-theory; or because the members of the base set have been specially constructed, like "bricks in a wall", to form series with this property. Where we are concerned with figures determining comparisons between idents over a range of values of such characters as those of size, warmth, or loudness, the possible existence of intermediate cannot be ruled out in any absolute sense without doing violence to normal intuitions about such characters. Such cases will be fully considered below in the context of comparative structures, valuation, and their developments for purposes of measurement. We shall see that they make no appeal to any notion of a "least perceptible difference": it will be argued that no such notion is theoretically sustainable.

(d) The condition $a \neq b$ in D4, which defines $\dot{\alpha}$ as a basis for the construction of α-series under D5, is introduced only to provide for series in $\hat{\eta}$ and $\hat{\tau}$, which are <u>reflexive</u>. All other figures in which we shall be concerned to construct series are <u>asymmetric</u> and therefore irreflexive; so that all their configurations automatically satisfy the condition. Again, where a figure $\hat{\alpha}$ is <u>transitive</u>, it will include as α-configurations all pairs of distinct idents drawn from any one recognized α-series, and not only α-next pairs. Thus, any pair of distinct members of a "left-right" series is in principle recognizable as a left-right configuration. If counting is carried out in such a series, the direction of counting is determined by the choice for attention (in any part of the series) of one or other of the two distinct relations which are in principle derivable from any figure which is not symmetric. The only figure with which we are concerned which is not transitive is $\hat{\tau}$; it is also symmetric. τ-series can nevertheless be constructed; but call for special treatment, to be given in the context of measurement in Part III. $\hat{\eta}$ is transitive and anti-symmetric, and the C-scale (C7, p.80) exhibits a characteristic structure which readily generates η-series. (Incidentally, systems such as sets of Russian dolls, or the skins of an onion, are regarded in this analysis not as potential η-series, but as sets of idents which successively surround, but do not enclose, one another. η-series formed of bound-idents of composites of such sets, or series in an appropriate surround-figure, can be defined; but the definitions are complex, and will be omitted as of no further interest here.)

The figures in which series will principally concern us will be both asymmetric and transitive; so that series are readily formed in them, in suitable conditions. A direction of counting is associated with a choice of a dominant counting relation, so as to generate a determinate order for that relation in the series, corresponding homomorphically with that of the series of numerals used in the counting. This is relatively unproblematic, in that these figures have a <u>progressive</u>

structure, and so (as noted under D3, p. 104) the cyclic case is eliminated. Cyclic series can, of course, be formed, notably in $\hat{\tau}$; and easily generate ambiguities in ordering. As we count right-handed round a circle of dancers holding hands, each one touching the next, people we passed to our left turn up again on our right. When we come to consider τ-series, this case will have to be eliminated by definition, where we wish to determine a unique order for counting. In all the wide variety of contexts to be discussed, however, this is the only such problem we shall meet.

(e) We have said that idents are distinguished as such (we shall say, accorded distinct '<u>identities</u>', or '<u>identified</u>') by the assignment of distinct sets of characters, unique to each one in context. In general, they are identified by one or more <u>non</u>-positional characters, not (one or all) assigned to any other. Indeed, no ident can be recognized as such by positional characters alone. Each ident must first (in a sense of logical priority) be recognized as spatially bounded, in the manner explored above (pp. 60ff.). The recognition of a positional character depends on that of at least one configuration of a 2-character to a pair of distinct idents (the case of a reflexive configuration in $\hat{\eta}$ or $\hat{\tau}$ can here be neglected). Thus the recognition of a boundary-character, which must be unary, is necessarily prior to the assignment of a 2-character to any pair with respect to which a figure may yield positional characters to its members. However, such boundary-characters need not themselves be unique to the idents they bound; and in such cases we may rely on a positional character for identification. Positional characters are, of course, identifying if and only if they are uniquely assigned to the ident to be identified; which may be the case in favourable circumstances.

Thus, a particular letter "a" in the immediately following pair aa can readily be identified as "the one on the left", although none of the recognized non-positional characters of the members of the pair, nor in particular their boundary-characters ("a-shaped, black touching white") is to be distinguished in a normal reading context. (I am, of course, assuming such a context, including for example the normal orientation of the page to the reader; few of the structures of common experience on which we build our contexts of recognition can be quite so uniform and reliable.) It is distinguished by its positional character in $\widehat{\dot{\alpha}}$, where α is the "left-right" figure in context; since its PD set in this figure consists uniquely of a letter "a" next to its right; the other of the pair having uniquely an "a" next to its left. We are able, in this context, and for this purpose, to neglect all other configurations of $\widehat{\alpha}$ or $\widehat{\dot{\alpha}}$, since they cannot disturb this uniqueness of positional character, obtained by a particular application of the Π-function for all possibly relevant R-frames. Indeed we may go on to say that our recognition that there are just two "a"s, and not one, immediately following the word 'pair' above, rests on our recognition not only of the unary non-positional characters of the members of the pair, but, simultaneously, of their distinct positional characters in the relevant "left-right" configuration.

This form of identification seems to infringe at least the spirit of the Leibnizian prohibition against the use of relational properties to determine identity, since although the PCs are unary characters, their determinations require logically prior determinations of the memberships of PDs, in turn dependent on the assignments of 2-characters which are in a Leibnizian sense relational. But the more general philosophical problems of identity will be postponed to Part IV (Section 6). I want now only to consider how the principle of identification just used can be extended to counted positions in series.

(f) If an ident, indistinguishable from others by non-positional characters, is to be so distinguished by position, its PDs must be unique: they must contain at least one ident not in the PDs of any other ident. This requires only the condition given by $(\exists\alpha)\ (\forall y)(((\overline{\alpha x},\overline{x\alpha}) = (\overline{\alpha y},\overline{y\alpha})) \rightarrow x = y)$. In some instances this can be determined for particular idents without regard to series: to give simple examples, the only "a" in the array $a\overset{a}{a}\underset{a}{a}$ which is "to the left of" five "a"s, or the only one "to the right of" just three or "to the left of" just two.

Once a particular member of an unambiguously recognized series has been identified, by this or any other means, all other members of that series can be identified from their positions in that series, by counting from it. This is simple enough where only one series in a particular figure is recognized in context. But in the more general case, in which more than one series containing otherwise indistinguishable idents is recognizable in a given figure, the situation is likely to be more complex. Indeed, the variety of possible cases is so great that I shall only consider a few relatively simple examples of typical situations in which counting series are commonly used in this way, and which illustrate some of the aspects of this strategy which are specially relevant to the understanding of measurement.

In the examples given above, I have made use of a kind of space-restrictive strategy (such as that analysed in Section C above) by directing attention to a particular pair and a particular set of six "a"s, among all those appearing in these pages. An effect similar to that of space-restriction can in some cases be obtained in the recognitions of figures, and positions in them, just by restricting attention to the intersection of the base sets generated by a given Γ-determinant set of unary characters $\mathcal{C}'$ and the figure concerned. In favourable circumstances the intersect $S_{\mathcal{C}} \cap S_{\alpha}^{x}$ of the base sets of $\Gamma_{\mathcal{C}'}$ and $\hat{\alpha}$ will contain one or more unambiguously recognized α-series, which we may call $S^{s1,\ldots,sn}$: e.g., a number of "left-right" series of letters on this page can be recognized, and unequivocally distinguished by locating their initial and final letters by position in an "up-down" figure which can, in turn, be unambiguously recognized. For each S^{sk} in this series we may recognize a restriction Π^k on the positional function Π, which will operate on the PDs for each member x of S^{sk} consisting of subsets of that series which we may write $(\overline{\alpha x},\overline{x\alpha})^k$, unique to each x in S^{sk}; so as to determine a unique PC for x in the series which we may write $(P^x_\alpha)^k$. Plainly, if $(P^x_\alpha)^k$ is unique to x in context, so is the unrestricted PC, P^x_α, since the members unique to $(\overline{\alpha x},\overline{x\alpha})^k$ will also be unique to $(\overline{\alpha x},\overline{x\alpha})$. Once a counting direction on S^{sk} has been determined by choice of a dominant relation, any arbitrarily chosen sub-sequence of the natural numbers can be made to correspond, each with each of the members of the series as identified by their positions in it. Treating thses as ordinals, we can identify each member as the nth member of the series for the value of n so determined; this value corresponding uniquely with its positional character in the figure as a whole.

But the circumstances do indeed have to be specially favourable (either by accident or design) if one or more series are to be unambiguously recognized in any particular pair (C',α) in $\wp C \times C_2$, so as to provide the conditions for identification by position in this way. Two kinds of interesting case which may arise, even where series are readily recognizable, are illustrated in a simplified form below:

(1) $a_a \;\; a \;\; {}^{a}a \;\; a^{a}$ (2) $a \;\; {}^{a}_{a} \;\; a \;\; a$

(1') $b_c \;\; d \;\; {}^{e}f \;\; g^{h}$ (2') $b \;\; {}^{c}_{d} \;\; e \;\; f$

(The letters in (1') and (2') have been disposed in such a way that they can be used to refer in discussion to corresponding "a"s in (1) and (2) - which we may think of as "swans on a pond", or anything else we might want to identify by position.)

Considering the "left-right" figure in (1) and (1'), it seems reasonable to point to at least five distinct series of "a"s which might naturally be distinguished: those corresponding to b,c,d,e,f,g,h; b,d,f,g; c,d,e,h; b,d,e,h; and c,d,f,g. (From now on, I shall just use the letters in (1') and (2') as names for the corresponding "a"s.) However, the identifications of particular "a"s by counting from left might already be somewhat ambiguous; and would become more so in conditions where the "left-right" configurations formed by the pairs (b,c), (e,f), (g,h) were less clear than I have made them here. Again, if we think of less easily recognized figures, such as those suggested by recognitions of pairs of idents as "warmer-cooler" or "heavier-lighter", unassisted by measurement techniques, we can see that the unambiguous recognition of series yielding a determinate order would be highly problematic in an analogous case. (For such figures, see pp. 35, 57, and fuller discussion under 'comparison figure' below, pp.124 f.) These are, of course, typical of just the kinds of situation in which we may turn to measurement techniques to improve conditions for recognition. The theoretical basis of these will be fully explored in Part III; but a first step towards an understanding of this basis can be taken at once.

Looking at the case (2), (2'), we may again point to more than one possible left-right series: b,c,d,e,f; b,c,e,f; b,d,e,f. But considering a situation in which the "left-right" configuration of the pair (c,d) is unrecognized in context, we are faced with the simple choice between the last two. In such a case, I shall speak of 'alternative series'. No unambiguous counting strategy for all "a"s is now available, but some things are already clear. (1) b has a unique position to the left of four "a"s, three in each alternative series; f an analogous position on the right; and e as being to the left of f and right of b,c, and d. These unique positions are recognizable independently of the recognition of any series. (2) c and d have identical PDs and so identical PCs; we can say they are similar in "left-right" position to one another, or in similar positions in that figure, just as they are similar in those characters by which they are recognized as "a"s. It is colloquially natural to say they are roughly, or approximately, the same left-right position. But the word "approximate" carries at least some connotation that some exactly determined position (or whatever) exists, to which the position so qualified approximates; and I shall now say how I think this notion can be most simply understood.

c and d are indistinguishable in terms of "left-right" position; but can be distinguished from one another in terms of an "up-down" figure. In this way we can unambiguously fix the memberships of the two alternative series b,c,e,f and b,d,e,f, in each of which either c or d has a uniquely determined position, while the other has not. It makes precise sense, therefore, to say that d has approximately the same position as c with respect to the series b,c,e,f; and similarly for c with respect to the series b,d,e,f. In each case we are taking one series as a "standard", with respect to which the odd-letter-out is assigned an approximate position. The choice of standard, in this case, is evidently arbitrary. Equally, in the case of the array (1), (1') we could arbitrarily choose, say, the "lowest" series c,d,f,g as standard, assigning approximate positions to b, e and f with respect to it; and similarly for all the other four-member series mentioned. The use of a second figure ("up-down") to assist in unambiguous recognitions among multiple alternative series has, of course, its most sophisticated expression in the Cartesian system of coordinates, which may be analysed as a structure designed to permit exact assignments of positions with respect to any number of series parallel to the axes, taken as equivalently standard. But this calls for specially constructed metric properties which we shall not reach in analysis until Part III. The examples given above are themselves constructed to yield relatively easy determinations of "left-right" and "up-down" positions, which we should not expect even from such naturally occurring arrays as those of "swans on a pond" or "stars in the sky"; still less from arrays in more problematic figures such as those mentioned, in which simple spatial configurations do not play a straightforward determining role.

Factors affecting the choice of standard series from amongst possible alternatives vary enormously with the figure involved and the conditions of the context. They demand, of course, particularly rigorous treatment in contexts of measurement. It will be argued that philosophical problems associated with the recognitive aspects of such choices have been neglected in studies of the foundations of measurement like those of the Suppes tradition, which have concentrated on the mathematical properties desired; and that this neglect has resulted in a failure to reveal important factors affecting the empirical validity of the systems analysed. In many non-metric contexts, however, arbitrary choices of standard series, provided these are unambiguously and uniquely distinguished, may be perfectly adequate for the relevant purposes. But, in order to give this proposal clear interpretations over a wide range of contexts, I must first give an account of two quite separate concepts:

that of a set of values associated with a particular unary character; and that of the comparison of idents with respect to such values.

D7. The valuative function Δ is a partial function from C into C such that for each $Q \in C$ either $\Delta(Q) = \emptyset$ or there is a subset $C' \subset C$ having at least two members such that $\Delta(Q) = C'$, and for all $P \in C'$, $P \neq Q$ and

(i) writing $C' = \{P_1, \ldots, P_n\}$, $S_Q = \bigcup(S_{P_1}, \ldots, S_{P_n})$; and

(ii) $(\forall P_i, P_k)(P_i, P_k \in C' \wedge P_i \neq P_k \rightarrow S_{P_i} \neq S_{P_k})$

(for definition of the base sets S_P, S_Q, etc., see BD1, p. 72).

Terminology: where $\Delta(Q) \neq \emptyset$, Q is called a valuative character (or V-character), whose values are the members of the value set C' of Q. Note that, by (i), $\vdash (\forall x)(Q/x \rightarrow (P_1/x \vee \ldots \vee P_n/x)$, and $(\forall P, x)(P \in C' \wedge P/x \rightarrow Q/x)$.

Abbreviation: Where Q is a valuative character and C' its value set we write $V[C', Q]$

Notes:

(a) As with the definitions of binary commitments, the notion of space-restrictions on valuation has been neglected for simplicity, though it could readily be carried out in terms of restrictions on Δ defined only for the enclosures of any given ident.

(b) The notion of valuation captured here is a very broad one, within which comparative value ranges, and ultimately metric values, will be distinguished as special cases. The aim is to bring such notions as 'quality', 'attribute' (KLST), 'aspect' (Kanger), within a unified analysis encompassing all recognitive classification, as restricted by various contexts. Thus a valuative character (as Q in D7) has a status corresponding, in most cases, with a linguistic 'general term' which collects less general terms as its values - in the way that the term "animal", for instance, collects "man" and "insect" as well as "horse" in some contexts, but in others specifically excludes the first two. At this stage, of course, we are not concerned with the assignment of verbal lables except as a means of evoking examples; but with the recognition of a set of phenomenal characters as alternative values of a general character - the general character being recognized in terms of (not by inference from) recognitions of one or other of its values. Where such recognitions are associated with linguistic terms, it seems not too misleading to interpret base sets of this kind as the extensions of these terms.

(c) Values are not, in the general case, mutually exclusive (in the sense defined by C4, p.80), though their base sets are distinct. For instance, "equines", "horses", "mules" and "cows" are all "animals". Recognition of any of these would justify an assignment of the characters understood by the term "animal". Further distinctive character-recognition would be needed to justify discrimination between "horse" and "mule" as different values of "equine".

(d) In most ordinary, simple contexts the number of valuative characters, and of their recognized values, is small; but in others the numbers may be indefinitely large - though not merely finite but recognitively manageable. Large numbers may occur in contexts involving many accepted classification-systems, as in chemistry or biology. In metric contexts, where numerical values are strictly associated with certain values of V-characters, they will be called quantities; and Δ will emerge as the function which determines recognitions of distinct parameters or dimensions in a context.

Certain V-characters have the special property that their values are comparable in such a way that it becomes natural to consider idents so characterized as having <u>more</u> or <u>less</u> of a property associated with such a V-character; or, alternatively (and more perspicuously) to consider that one assigned value is <u>greater</u> or <u>less</u> than another with respect to the valuative structure as a whole. However, a scheme of comparison in which each ident is assigned just one value (if any) of a given V-character excludes the possibility of a comparison of values, except in the form of a comparison between idents to which these values are assigned; and any such scheme is analysable in terms of a particular type of <u>figure</u>.

(If one part of an apple, say, is recognized as redder than another, then these parts are recognized as distinct idents - however vaguely bounded - enclosed in the apple; and no sense can be made of comparing such an apple, as a whole, as redder or less red than some other apple, except perhaps in terms of the spatial extents of their parts recognized as redder or less red. This restriction of value-comparisons to cases where distinct idents are compared is even more obvious for V-characters such as length, weight (mass), or warmth (temperature).) So it is claimed that the notion of value-comparison is correctly captured in terms of a combination of a mutually-distinctive value structure as defined in D7 above (essentially a form of classification) with an associated comparison figure, now to be defined. It will be found that this leads to the view that the notion of a continuously variable range of values of a property associated with a V-character, of which at most one may be assigned to each ident at any one time in any context, belongs strictly not to recognitive theory (as expressed in particular R-theories) but to S-theory: in that it goes beyond what can in principle be recognized. We shall see in due course, however, that such a notion can in some sense be incorporated in R-theories, at least in contexts of measurement.

D8. (i) Q is a comparative character iff:

$$(\exists C')(V[C',Q] \wedge (\forall P,x)(P \in C' \wedge P/x \rightarrow$$
$$(\exists \delta)(\delta \in C_2 \wedge \gg[\delta] \wedge P = P^{x}_{\delta})$$

(ii) δ is a <u>comparison figure</u> for Q iff Q is comparative, $V[C',Q]$, $\gg[\delta]$, and $(\forall x)(x \in S_\delta \rightarrow P^x_\delta \in C')$.

(i.e., all values of Q are determined by position in at least one progressive figure, called a comparison figure for Q. In practice, it is rare for more than comparison figure to be used for any one comparative character in ordinary contexts; but in some measurement contexts a manageable number of distinct comparison figures are so used. It follows that $S_Q = (S_\delta, S_{\delta'}, \ldots)$, where $\{\delta, \delta', \ldots\}$ is the set of all comparison figures for Q in context).

We now, for the sake of broader comprehension, define a <u>non-recognitive</u> property $\overset{\delta}{\underset{Q}{>}}$ for a comparative property Q, assigned to pairs of values of Q, by:

D9. The ordered pair P,P' of values of a comparative character Q has the property $\overset{\delta}{\underset{Q}{>}}$ (so that $\overset{\delta}{\underset{Q}{>}}(P,P')$ is read, "P is Q-greater than P' under δ") iff δ is a comparison figure for Q and $(\exists x,y)(\delta/x,y \wedge P = P^x_\delta \wedge P' = P^y_\delta)$.

<u>Notes</u>:

(a) The relation $\overset{\delta}{\underset{Q}{>}}(P^x_\delta, P^y_\delta)$ derivable from a recognized configuration given by $\delta/x,y$ is hard to distinguish intuitively from the recognitive configuration itself; but for the purposes of this study it is important that we should attempt to grasp the distinction in "real" terms, and not only formally. The intuitive difficulty almost certainly arises from a form of unconscious inference, ingrained by habit (if not innate), which goes from perceptual recognitions to judgments about the recognized entities and their inherent properties; this judgment being almost as automatically embedded in our total system of beliefs about these properties, as associated with physical entities in general. This structure of unconscious judgment is reflected in common speech: we say that something "looks redder", "looks longer", "feels heavier", "feels warmer" than something else. In the terms of this study these represent cases of (normally) unconscious <u>incorporation</u> of substantive (S-theoretical) judgments about the relevant idents into the R-theory of the context in each case. I shall postpone discussion of colour recognition till we come to consider it in a context of measurement; in spite of the eagerness with which some philosophers rush to call it in support of their views, it is one of the most difficult modes of perception to analyse (the substantive judgments commonly formed are generally false, or at least profoundly misleading). Judgments of relative length, however, are plainly enough typically based on recognitions of apparent spatial extension in the visual field, and are notoriously fallible in any but specially favourable conditions. Relative weight is judged from muscular tensions or pressure on the flesh: quite clearly mass, as such, cannot be felt, and 'felt weight' is dependent on many factors, other than the inherent properties of the bodies felt. 'Felt warmth' is similarly judged from familiar kinds of sensation, mainly in the skin; it is notoriously unreliable as a guide to the inherent properties of the bodies felt, depending almost as much on their conductivity as their temperature, between which properties we cannot feel the difference. Thus judgments like these are clearly substantive, in that they go beyond the recognitive evidence.

The theoretical basis of this distinction, in various modes, will be fully dealt with in contexts of measurement. They are raised here to reinforce the point that the distinction is important, and that we must continue throughout this study to attend to the precise role of substantive theory in judgments of this kind.

(b) Recall that a progressive figure does not necessarily or typically generate a determinate order on its base set, since it is not necessarily connected (not all pairs in S_{δ}^{2} are necessarily recognized as δ-configurations; still less all pairs in S_{Q}^{2}, if these are not the same). Consequently, nor does $\underset{Q}{\overset{\delta}{>}}$ necessarily generate a determinate order on the value-set C'. This difference from the fundamental 'weak order' of KLST is deliberate, and intended to capture an intuitive structure associated with contexts of non-metric value-comparison. The order properties of the system are more fully considered at the end of this Section, and in Part III (pp.273ff.)

By the comparative structure for Q (a comparative character) we shall understand the (n + 1)-tuple $(Q, \delta_1, \ldots, \delta_n)$ comprising all comparison figures for Q in context; which determines all positional characters yielding values of Q in that context. I shall consider, now, factors affecting the choice of standard series with respect to which a system of determinations of such values can be made more coherent, in contexts where more than one series may be recognized. It will be convenient to confine attention, initially, to the restriction of this concept to the base set S_δ of a particular comparison figure δ for Q, the Q-values of whose members are determined by positions in $\hat{\delta}$: we shall call this a comparative pair on Q, namely the pair (Q,δ). In cases like the "left-right" array of "a"s above, which are typical of many naturally occurring contexts of value-comparison, nothing can usefully be added to the observation that the choice is logically arbitrary, depending only on practical matters such as stability, familiarity and ease of unambiguous recognition. In many quite ordinary contexts, however, familiar objects may be chosen so as to form a system of series which can be recognized as equivalently standard with respect to some V-character, in a way now to be formally analysed. Such a system will be called a standard set for a given comparative pair (Q,δ); and will be shown to have formal properties which prove relevant, in due course, to the analysis of measurement contexts. We shall see in those contexts that questions concerned with possible equivalences between values determined in standard sets generated by different comparison figures for the same V-character (or quantity, in measurement) are fundamentally S-theoretical; though the relevant S-theoretical commitments may readily become incorporated in R-theories, and will in any case be subject to the ultimate test of consistency with perceptual recognitions.

We shall first define the following 2-character of δ-equivalence, on any progressive 2-character δ:

D10. For all pairs $(x,y) \varepsilon\ S^2_\delta$, where $\gg[\delta]$, the associated 2-character $\overset{\approx}{\delta}$ (<u>δ-equivalence</u>) is given by:

$$\overset{\approx}{\delta}/x,y \leftrightarrow x \neq y \wedge (\forall z)((\delta/x,z \leftrightarrow \delta/y,z) \wedge (\delta/z,x \leftrightarrow \delta/z,y))$$

Note: It follows that $P^x_\delta = P^y_\delta$, since the principal condition ensures that the memberships of the PDs of x and y for δ are identical. It is important to realise, however, that recognition of the condition as stated for $\overset{\approx}{\delta}/x,y$ is logically prior to the recognition of the identity of position; although it may appear as if the converse derivation is formally possible. In the first place, the definition of δ as progressive requires that at least one more distinct member of the base set must be recognized if the condition is to be recognized as satisfied; since δ is asymmetric, we must have at least one pair of distinct idents characterized by δ: and our condition gives $\delta/x,y \rightarrow \delta/y,y$ and $\delta/y,x \rightarrow \delta/x,x$, an obvious contradiction. Recognition that the condition of D10 holds for a pair x,y requires that each δ-configuration involving x or y is determined and found consistent with that condition, either by immediate recognition of that configuration, or under a prior commitment of the R-theory. Thus, in principle, recognition of the complete structure of $\hat{\delta}$ is prior to that of satisfaction of D10, and so to that of the identity of position of x and y. The condition D10 is, of course, to be read as a commitment of the R-theory of the context in each case, holding that just those δ-configurations involving x or y which satisfy the condition are recognized, and none which contradict it: recalling that all implications invoked in this study are to be read strictly as confined to the universe of the relevant theory, either as commitments of the theory or derivations from these.

The form of the standard set, now to be defined, makes use of the prior commitments determined by the recognized progressivity of δ, and by the structure of its associated $\dot{\delta}$-figure, to construct a strongly coherent system within which recognitions of $\approx$-δ-configurations can be articulated. Some useful properties of this construction, and the kinds of empirical conditions in which its rather strong requirements may be satisfied, will be analysed following the formal treatment.

D11. <u>A standard set</u> $S^{\approx}_{\delta} \subseteq S$ for the comparative pair (Q,δ) is given by:

(i) $(\forall x \varepsilon S^{\approx}_{\delta})(\exists y)(y \varepsilon S^{\approx}_{\delta} \wedge (\dot{\delta}/x,y \vee \dot{\delta}/y,x \vee (\exists S^{s}_{\delta})(S^{s}_{\delta} \subseteq S^{\approx}_{\delta} \wedge (x,y \varepsilon S^{s}_{\delta}))$

where $(\exists S^s_\delta)$ is to be read, 'there is a δ-series S^s_δ satisfying D5, p.105).

(ii) For all x, y, $\varepsilon\ S^{\approx}_\delta$,

$\tilde{\tilde{\delta}}/x,y \leftrightarrow (\exists z)(z\varepsilon S^{\approx}_\delta \wedge ((\dot{\delta}/x,z \wedge \dot{\delta}/y,z) \vee (\dot{\delta}/z,x \wedge \dot{\delta}/z,y)))$.

i.e., all members of $S^{\approx}_\delta$ belong to at least one δ-next pair, which may belong to a δ-series; and we shall see that in any such series (each of whose members must, by D5, belong to a δ-next pair) either member of a δ-next pair may be replaced by any δ-equivalent ident in $S^{\approx}_\delta$ so as to satisfy the conditions for a δ-next pair forming part of an alternative series.

We may associate with each member $x\ \varepsilon\ S^{\approx}_\delta$ an equivalence set (or <u>$\tilde{\tilde{\delta}}$-set</u>) which we shall <u>abbreviate</u> $S^x_{\tilde{\tilde{\delta}}} = \{y: \tilde{\tilde{\delta}}/x,y\}$: any such set may, however, be empty.

<u>Theorem (D11)</u>. Where $C^{\approx} \subseteq C'$ is the set of all δ-positional characters assigned to the members of a standard set $S^{\approx}_\delta$ for the comparative pair (Q,δ) such that $V[C',Q]$, (Q,δ) determines a <u>simple order</u> on $C^{\approx}$, in terms of the non-recognitive relation $\overset{\delta}{\underset{Q}{>}}$. (A 'simple order' is one which is transitive and antisymmetric.)

The conditions for $S^{\approx}_\delta$ are satisfied by a single δ-next pair, or by just two $\tilde{\tilde{\delta}}$-sets whose product consists of δ-next pairs: in which case either choice of dominant relation in $\hat{\delta}$ immediately determines the same simple order on every such pair. In the more general case, where one or more δ-series are recognized in $S^{\approx}_\delta$, there is a sense in which these can be called <u>equivalently standard</u>

series. Using these as counting-series, and assigning the same number to the identical positional characters of δ-equivalent idents in different series, we can write the value set $C^{\approx} = \{P_1, \ldots, P_n\}$, for suitable n, such that for all $x \in S_\delta^{\approx}$ we have:

$(\exists k)(1 \leq k \leq n \wedge P_\delta^x = P_k \wedge P_k/x \wedge (\forall y)(\tilde{\tilde{\delta}}/x,y \leftrightarrow P_\delta^y = P_k))$.

(The possibility of a gap in the count is ruled out by the final bracket in D11(i), which determines that any pair in $(S_\delta^{\approx})^2$ belongs to at least one δ-series; including the extreme cases of δ-sets of members having no predecessor, or no successor, in any series. These we naturally choose to assign P_1 and P_n, where n is the total number of δ-equivalent sets, and members with no δ-equivalents, in $S_\delta^{\approx}$.) Clearly, this system of assignments determines a simple order on $C^{\approx}$, homomorphic with that on the natural numbers 1 to n.

Proof: The cases where $S_\delta^{\approx}$ contains no series, and only one or more δ-next pairs, have been shown to meet the condition self-evidently. To show that the counting-system set out above can be carried out in all other cases, it is necessary and sufficient to show that we can choose at least one maximal series $S^s \subseteq S_\delta^{\approx}$ with first and last members x,y such that P_1/x and P_n/y, and $(\forall a \in S_\delta^{\approx})(\exists k)(k \in S^s \wedge (\overset{\approx}{\delta}/a,k \vee a = k))$. We shall first prove:

$$(\forall x,y,z)(x,y,z \in S_\delta^{\approx} \rightarrow (\tilde{\tilde{\delta}}/x,y \wedge \dot{\delta}/x,z \rightarrow \dot{\delta}/y,z)) \quad \ldots (1)$$

$$\overset{\approx}{\delta}/x,y \wedge \delta/x,z \rightarrow \delta/y,z \quad \ldots \text{by D10}$$

Suppose there is $w \in S_\delta^{\approx}$ such that $\delta/y,w \wedge \delta/w,z$;

$\tilde{\tilde{\delta}}/x,y \wedge \delta/y,w \rightarrow \delta/x,w$ ... by D10

whence $\delta/x,w \wedge \delta/w,z$; contra $\dot{\delta}/x,z$

and $\neg(\exists w)(\delta/y,w \wedge \delta/w,z) \rightarrow \dot{\delta}/y,z$ ... by D4

Now let k be any chosen member of $S_{\delta}^{\approx}$ as defined by D11. Then either there is some $j \in S_{\delta}^{\approx}$ such that $\dot{\delta}/j,k$, which we shall call its 'predecessor' or there is not. If there is not, we shall take k as our first member of the maximal series S^s. If there is, then j either has a predecessor distinct from j or k (since δ is progressive), or not. If not, we take j as the first member of S^s; if so, this predecessor either has its own predecessor or not. Since S is finite, $S_{\delta}^{\approx} \subseteq S$ is finite, and so in this manner we shall eventually reach some x which has no predecessor, and which is either identical with k, or its predecessor, or connected with k by a recognized δ-series consisting of members of $S_{\delta}^{\approx}$. We shall take such an x as our first member and assign it the position given by P_1/x (we have tacitly taken the "left-to-right" relation in any pair (x,k) such that $\delta/x,k$ is our dominant relation for counting). Similarly we shall find a last member y, with no successor, to be assigned the PC P_n, where n is the number of members of the series S^s containing x, k and y, which we take as our maximal series. (Note that $\neg(\exists a)(\delta/a,x)$, since such an a is either the predecessor of x, or there is a series connecting a and x containing such a predecessor; and similarly $\neg(\exists a)(\delta/y,a)$.)

Consider now any chosen $a \in S_{\delta}^{\approx}$ such that $a \notin S^{s}$ (where $S^{s} \neq S_{\delta}^{\approx}$); then either $\overset{\approx}{\delta}/x,a$ or $\dot{\delta}/x,a$, or there is a series in $S_{\delta}^{\approx}$ connecting x and a. In any case there is an alternative series $S^{s'} \subset S_{\delta}^{\approx}$ containing x and a, such that (writing $b,c \in S^{s}$ such that $\dot{\delta}/x,b \wedge \dot{\delta}/b,c$ - including the case $c = y$), <u>either</u>

(i) $\overset{\approx}{\delta}/x,a \wedge \dot{\delta}/a,b$ (by (1) above), $S^{s'}$ consists just of a and all members of S^{s} but x, and P_1/a;

(ii) $\dot{\delta}/x,a$, whence $\overset{\approx}{\delta}/a,b \wedge \dot{\delta}/a,c$ (again, by (1)), $S^{s'}$ consists of the members of S^{s} replacing b by a, and P_2/a; <u>or</u>

(iii) there are members $d,e \in S^{s'}$ such that $\dot{\delta}/x,d \wedge \dot{\delta}/d,e$ (including the case $e = a$). Now
$\dot{\delta}/x,b \wedge \dot{\delta}/x,d$ gives <u>either</u> $b = d$ <u>or</u> $\overset{\approx}{\delta}/b,d$, and in either case $\dot{\delta}/b,c \rightarrow \dot{\delta}/d,c$;
$\dot{\delta}/d,c \wedge \dot{\delta}/d,e$ gives <u>either</u> $c = e$ <u>or</u> $\overset{\approx}{\delta}/c,e$.
Then <u>either</u> $e = a$ and $\overset{\approx}{\delta}/c,a$, $S^{s'}$ consists of x,d,a and all members of S^{s} after c (if any); <u>or</u>, proceeding in this manner, showing that each successive member of $S^{s'}$ is either identical with, or δ-equivalent to, a corresponding member of S^{s}, we shall eventually reach some member $k \in S^{s}$ such $\overset{\approx}{\delta}/a,k$, the membership of $S^{s'}$ being determined accordingly. The case $\delta/y,a$ being ruled out, either $k = y$ and P_n/a, or a is assigned a value P_k of Q with $2 < k < n$ such that P_k/k.

All this may seem to be labouring the obvious, since we are familiar with so many structures of this sort. However, the empirical conditions for the recognition of such a structure are actually quite severe, and will be explored shortly after the definition, below, of an <u>approximate</u> set associated with a standard set as just defined.

D12. The <u>approximate set</u> $S_\delta^{\sim}$ with respect to a standard set $S_\delta^{\approx}$ is given by:

$$(\forall w)(w \in S_\delta^{\sim}) \leftrightarrow_{df} w \notin S_\delta^{\approx} \wedge (\exists x,y,z)(x,y,z \in S_\delta^{\approx} \wedge \dot{\delta}/x,y \wedge \dot{\delta}/y,z \wedge \delta/x,w \wedge \delta/w,z)$$

In such a case we recognize an associated $\tilde{\delta}$-figure, such that $\tilde{\delta}/w,y$.

<u>Note</u> that if the same condition were fulfilled by w such that $w \in S_\delta^{\approx}$, it would follow that $w = y$ or $\tilde{\tilde{\delta}}/w,y$, and $\dot{\delta}/x,w \wedge \dot{\delta}/w,z$; the proof is just a restriction of that in para. (iii) above to a standard series of just three members. The empirical interpretation of the difference between these two cases will be considered shortly. First I wish to draw two useful conclusions from the definition of an approximate set in D12.

<u>Corollaries</u>

(1) $(\forall v)(\tilde{\tilde{\delta}}/y,v \wedge \tilde{\delta}/w,y \rightarrow \tilde{\delta}/w,v)$. This follows easily, substituting v for y in D12.

(2) $\neg(x,y \in S_{\delta}^{\approx} \wedge \delta/x,y \wedge \tilde{\delta}/z,x \wedge \tilde{\delta}/z,y)$. By D11 x,y belong to at least one series in $S_{\delta}^{\approx}$, so there exist $a,b \in S_{\delta}^{\approx}$ such that $\dot{\delta}/x,a \wedge \dot{\delta}/b,y$. Then if $\tilde{\delta}/z,x \wedge \tilde{\delta}/z,y$, we have $\delta/z,a \wedge \delta/b,z$ (by D12); whence $\delta/b,a$.

$\dot{\delta}/b,y \rightarrow \neg(\delta/b,a \wedge \delta/a,y)$, whence $\neg(\delta/a,y)$;
$\dot{\delta}/z,a \rightarrow \neg(\delta/x,y \wedge \delta/y,a)$, whence $\neg(\delta/y,a)$;
whence $\tilde{\tilde{\delta}}/a,y$; and similarly $\tilde{\tilde{\delta}}/b,x$; so that $\dot{\delta}/x,y \wedge \dot{\delta}/b,a$. But we have $\delta/b,z \wedge \delta/z,a$; which contradicts $\dot{\delta}/b,a$ by D4.

Terminology

Thus for each (x,k) such that $x \in S_{\delta}^{\approx} \wedge P_k/x$ there is a potential subset $S_{\delta}^{\sim k} \subset S_{\delta}^{\sim}$ which we will call the k-cluster, given by $S_{\delta}^{\sim k} = \{y\colon\ \tilde{\delta}/y,x \wedge P_k/x\}$, which may be either empty or indefinitely large. Plainly, if $\tilde{\tilde{\delta}}/x,y$ then x and y determine the same k-cluster. The k-cluster is not an equivalence set, since the $\tilde{\delta}$-figure is not recognized for pairs in that cluster, but only for pairs drawn from $S_{\delta}^{\sim} \times S_{\delta}^{\approx}$. Indeed, the definitions do not exclude $\tilde{\delta}/a,x \wedge \tilde{\delta}/b,x \wedge \delta/a,b$; we shall see that this condition may have a plausible empirical interpretation.

We may thus define a non-recognitive relation on the value sets $(C'-C^{\approx}) \times C^{\approx}$, called the approximation relation $\underset{Q}{\overset{\delta}{\sim}}$, given by: $\underset{Q}{\overset{\delta}{\sim}}(P_{\delta}^{y},P_k) \leftrightarrow_{df} (\exists x)(P_k/x \wedge \tilde{\delta}/y,x)$, and read 'the value of y approximates P_k'; noting that if the values of a and b approximate P_k, their values are not necessarily or typically identical.

Notes

(a) The definitions permit the recognition of standard sets for more than one comparison figure with respect to one V-character Q; and if so, the members of one may be assigned to the approximate set of the other. This case will be considered in a context of measurement, but neglected at this stage. We may also have more than one standard set for the same comparison pair (for example, where they are widely separated in Q-values); but this does not make for a coherent R-theory, or interesting analysis, and will not be considered again. Where only one comparison figure is under consideration, the δ symbol may be omitted from $\overset{\delta}{\underset{Q}{>}}, \overset{\delta}{\underset{Q}{\sim}}$: and if only one V-character is concerned, and no ambiguity can result, Q also may be omitted.

(b) The references in D11 to the comparative character Q and its associated value-set C' (within which the subset $C^{\approx}$ is distinguished) do not occur essentially in that definition; it is at least formally plausible to suppose the recognition of a δ-figure, with all its associated structural properties including standard and approximate sets, without associating with it any assignments of values of a comparative character. But this case holds no interest for us here, which is why I have introduced the association with Q-values from the start. Of great interest, however, is the case where more than one comparative character is associated with a single δ-figure;

as where values of "ripeness" are derived from the same recognitive figure as those of "redness". These, as we have seen, are most commonly cases of the incorporation of S-theory into R-theory specific to the context; and will prove of the first importance in contexts of measurement.

More trivially, for any comparative pair (Q,δ), alternative choices of dominant counting relation will yield opposing orders for what are, at least verbally, different V-characters (so that we might wish to call one Q, and the other -Q): e.g., "distance from the left, ... from the right", or "warmth, coolness".

(c) To illustrate the empirical interpretation of D11, D12, consider first the arrays $\begin{smallmatrix} a & b & c & d \\ e & f & g & \end{smallmatrix}$; $\begin{smallmatrix} & q & r \\ p & st & \end{smallmatrix}$. We shall use δ = the "left-right" figure as comparison figure for Q = "distance (of an ident) from the left side of the page" (again taking the "left-to-right" as the dominant counting direction, reading $\delta/x,y$ as "y is <u>to the right of</u> x", and deriving "the <u>distance of</u> y <u>from the left</u> of the page <u>is greater than</u> that of x", or $\underset{Q}{>}(P^x_\delta, P^y_\delta)$. The S-theoretical aspects of this derivation will emerge during the treatment of distance, or length, measurement, in Part III).

I have made use of the given metric properties of the type-spacing system so that it is easy to take the series a,b,c,d; e,f,g; p,q,r as standard series whose union can be taken as the standard set in context. Taking the spacing between members of these series as the least recognized (*not* least perceptible) standard difference of distance in context, we can see that s,t do not belong to this set. We can also see, for example, that b is Q-equal to f, and that a,f,c,d or e,b,g,(d) are equivalently standard with the series already mentioned; a, ..., r can be taken as maximal series, choosing any suitable intermediates from the standard set. Putting $\dot{\delta}/p,q$ and $\dot{\delta}/q,r$, we exclude by definition $\delta/p,z \wedge \delta/s,q$ or $\delta/q,t \wedge \delta/t,r$. We may, consistently with the definitions, take $\dot{\delta}/p,s \wedge \dot{\delta}/s,t \wedge \dot{\delta}/t,r$: but, if so, no δ-configuration involving the pairs (s,q) and (q,t) can be recognized consistently with taking p,q,r as a series in the standard set. The series p,s,t,r is non-standard, and we can take $\tilde{\delta}/s,q$ and $\tilde{\delta}/t,q$.

(d) Although I have made use of metric properties in the above illustration, I have not appealed to their theoretical structure as such, relying rather on intuitive perceptions of apparently regular intervals of distance. A degree of arbitrariness is inherent in such a context, which may nevertheless be quite adequate for the purpose in hand. This aspect of the matter becomes clearer when we recall the early use of parts of the human body as standards of length: the thumb-joint, hand-width, hand-span, forearm, adult male stride, etc. Though these later become conventionalised as more or less precisely defined metric standards in many cultures, they can still be used in a purely intuitive, non-quantitative way today. To take an example even more clearly independent of metric theory, consider how we might use the sizes of familiar animals to describe those of unfamiliar exotic species, saying that one is "about the size of a rat", while others might be "more the size of a mouse, rabbit, cat, sheep, donkey, horse, ... etc." If we were to pick just one "about the size of a rat", it might be understood rather vaguely as indicating that this one familiar animal was chosen from a reasonably closely-spaced (but unspecified) series as that to which the unknown exotic most closely approximates in size. Or, asked, "Well, just how exact is that?", we might say, "Bigger than a mouse and smaller than a rabbit", or "Bigger than a hamster and smaller than a guinea-pig". To get closer than that, we might have to look to measurement.

The simplest case is the standard set consisting of a single δ-next pair, with no approximate set: a simple comparison, with no implications to other valuations, such as, "That basket is too small for that dog".

The essential foundation for any empirical interpretation of the structures set out in D10, 11, 12, if it is to offer a prospect of overall consistency and coherence of recognition, lies in the specific interpretations for the particular context of the notion of a 'δ-next pair' as previously defined (D4). This amounts to a <u>strategy of neglect</u> adopted for the R-theory according to which, under the condition $\neg(\exists c)(a \neq c \neq b \wedge \delta/a,c \wedge \delta/c,b))$ in D4, certain particular δ-configurations are recognized in a standard set and all others excluded by fiat in such a way that the strong commitments adopted for that set may rationally be expected to be met. The sole criteria for the validity of such a stragegy are that both clearness of separations between values, and closeness of determination of individual values, shall be adequate for the purposes of the context and yield consistent results. Thus the choice of strategy is both logically arbitrary and strongly context-dependent; the internal logical consistency of the R-theory being the main aim of the construction.

In some cases this aim can be secured by simply selecting members of a standard set in context so that (a) δ-next configurations are recognized in the set so as to form at least one maximal series, and (b) every member of the set is recognized as either identical or δ-equivalent to a member of at least one such maximal series. More often in practice, however, not all relevant δ-next configurations or δ-equivalences are immediately recognized at once, some or all being, as it were, reconstituted from memory or what is sometimes called 'background knowledge'. This is seen in this study as a matter of inductive theory: a notion which will be more clearly specified after the full treatment of recognitive sequence (time-sequence in the R-theory) in the next Section; and explored as a fundamental philosophical concept in Part IV. At this stage it need only be said that the inductive nature of such commitments requires that they be attached to sets of idents by virtue of certain relatively stable and familiar character-assignments, other than assignments of relative value, associated in memory or general knowledge with previous δ-next configurations involving pairs of these idents or relevantly similar pairs. To make sense of our animal-sizing example above, both utterer and interpreters must once have seen, or known by illustration or description, how "mice, rats, and rabbits" would look if appropriately compared.

All these considerations apply equally to standard sets used in measurement. The main difference is that non-metric standards rely on regularities and familiarities occurring naturally or "by accident", and chosen from what is suitable and available; while metric standards (with few but important exceptions) are specially constructed for the purpose, and must fulfil additional conditions. "By accident" here means without human contrivance <u>for the purpose</u>: it does not exclude the use of building bricks, for instance, as intuitive standards for length, weight, or even colour. However, the nature of these demands makes it less than surprising that living things, because their genetic organization is the main source of human-scale regularities in the world, figure prominently in non-metric contexts of value-comparison. Dogs, however, because of the way we have manipulated their breeding habits, will not do unless a well-known breed or a particular dog is specified.

Finally, before analysing the notions of recognitive time-structure in terms of an R-sequence, I want to point to certain aspects of the type of order structure on values of a comparative character which emerges from the above analysis. Although other methods of formal description may be possible, this analysis has been constructed to reflect certain intuitions about perceptual comparison, especially:

(1) The relation of exact equality is not recognizable, since elements which appear equal for some comparative character may always be unequal at some finer level of discrimination, whether perceptible or not.

(2) Ambiguous relations of the form '$\leq$' are also unrecognizable (except, perhaps, in special cases of no theoretical importance); since such a recognition can always be read as a disjunction of the form '..$<$...or ...=...', which is consistent with '=' and therefore with '$\geq$'. The form '$\neg(\succ((P^{x}_{\delta},P^{y}_{\delta})$', in C-theoretical terms, which may be read as formally equivalent, is to be read strictly as saying that there is no <u>recognized</u> figure δ/x,y from which such a relation is to be derived; taking account of the strategy of neglect adopted for the relevant R-theory. It need have nothing to do with a "least perceptible" inequality, if such a notion has meaning. In measurement contexts, where arithmetical properties are attributed to values in the standard structure, we shall see that the notion of least <u>recognized</u> inequality can, by contrast, be given precise meaning, in arithmetical terms.

(3) The present analysis restricts the existence of $\overset{\delta}{\underset{Q}{>}}$-relations to cases where the relevant comparative pair (Q,δ) is recognized, and either the appropriate δ-configuration is immediately recognized or it can be derived from other such recognitions, by extrapolation, using commitments of the R-theory for which schemata have been, or will shortly be, provided. One of the most fruitful of these is the commitment to the progressivity of δ, and therefore of $\overset{\delta}{\underset{Q}{>}}$, built into our definition of the comparative pair. The intuition this claims to capture is that the kinds of recognitive comparison, and associated valuation, we are analysing here can only be clearly understood if it is assumed that this property holds for all configurations and derived valuations determined for the same time (the same R-frame, whatever its duration, during which assignments do not change), under a particular R-theory: i.e., for any one Reader of the context. No strictly recognitive commitment to progressivity outside such limits seems to me plausible; commitments to constancy or universality of any particular system of value-relations belong to the realm of substantive theory (S-theory). The ways in which such commitments may be associated with, or incorporated in, R-theories, will be considered in contexts of measurement (Part III).

Summary of Sections A - D: R-frame and Structural Commitments

The R-frame has been defined (A10) as a pair consisting of an assignment structure $\mathcal{F}^*$ (A1-8) and a composite structure $\mathcal{S}^{()}$ (A9). $\mathcal{F}^*$ is a septuple consisting of the set $\mathcal{S}$ of idents, the sets $\mathcal{C}, \mathcal{C}_2$ of unary and binary characters, the corresponding assignment functions f, f_2, and the figures $\hat{\eta}, \hat{\tau}$ of enclosure and contiguity. This frame structure is intended to reflect the form of a context of recognition over a period of time (however short or long) during which no recognized changes of characterization occur: in terms of the characterizations of idents, or configurations of pairs of idents, and their recognized compositions as wholes consisting of distinct parts.

The structure of characterization consists of assignments of perceptually recognized (or recognizable) properties (the characters) to spatially-bounded entities (the idents): each boundary being recognized in terms of differentiating boundary-characters assigned to the idents either side of the boundary. These boundary-characters are not a special kind of characters, but any characters recognized as determining boundaries; a boundary need be no more, but must be no less, sharply determined than the purposes of the context require. The conditions for boundary-discrimination, and consequent distinctions between idents, are determined for each R-frame by the relevant structural commitments of the R-theory in which the R-frame occurs. Typical forms of structural commitment have been schematized as restrictions on the determination of the assignment functions f, f_2 for chosen sets of unary or binary characters respectively. The restrictions on f (C1-9) include one-place commitments of separativity and atomicity (C1,2), and two-place commitments of distinctivity, exclusivity and scalarity (C3-5), on which a weak notion of scale is defined (C7). The restrictions on f_2 (D1-5) are defined on commitments of asymmetry and transitivity (D1,2); from which a series structure is derived for any suitable binary figure $\hat{\alpha}$ (D5). This involves the use of a strategy of neglect in determining a secondary 'α-next' figure associated with $\hat{\alpha}$ (D4). Full notes on the concept of neglect in C-theory are at pp. 66 ff.

The definition of a positional function Π is used to derive a notion of position in any suitable figure (D6). This may be associated in some cases with a quite distinct notion, defined in terms of a valuative function Δ, of a set of characters, recognized with respect to a single, distinct character, as values of that character (D8); in such a way that configurations of a particular figure can be recognized as comparisons of values for the associated valuative character, and a position in the figure corresponds with a value of the character. This pair of notions is specially fruitful where the comparison figure can be used to generate series; in which case positions in a series are naturally associated with an ordered range of values. This last notion is strictly substantive and non-recognitive; i.e., it belongs strictly not to any instantiating R-theory, but to its associated S-theory. But in particular cases such a structure may become incorporated in the R-theory. For preliminary discussion of R-theory, S-theory and the concept of incorporation, see notes on theoretical structure at p.48 : these aspects are fully developed later. The structures of Π and Δ are, however, strictly R-theoretical.

The notion of whole/part composition is fully discussed in note (f) to Section A (pp.68 ff): it has its most important development for this study in the notion of characteristic sets or Γ-sets (Section B, pp. 72 ff). These are sets of idents recognized as composing distinct wholes, whose members are their parts, by virtue of a shared set of characters: each such Γ-set contains a bound-ident which may or may not share the Γ-determinant set of characters, according to the structural commitments of the R-theory for that set. It has been said that the full importance of these Γ-sets, and the main basis for their recognition, depends on the concurrent adoption of specific commitments (other than those of composition itself) for their determinant sets of characters: this applies specially (though not only) to what will be called sequential commitments: i.e., those adopted for more than one frame in a temporal sequence of frames of an R-theory. They include commitments of constancy, implication and extrapolation: the importance of their association with Γ-sets will emerge clearly in the following Sections E, F, on R-sequence and sequential commitments, summarised on p.172 f.

E. The Recognitive Sequence (R-sequence)

The R-sequence will be defined with respect to a sequence of R-frames $\langle F_0, \ldots, F_k, \ldots, F_n \rangle$ adopted by a particular Reader R (an individual perceiving subject); such that each frame F_k has the form defined for the R-frame in Sections A, B above. We shall write:

$$F_k = F_k^*, S_k^{()} \text{ , where } F_k^* = \langle S_k, C_k, C_{k2}, f_k, f_{k2}, \hat{\eta}_k, \hat{\tau}_k \rangle,$$

satisfying the definitions A1-8; and $S_k^{()}$ satisfies A9 with respect to F_k^* (see preceding summary). The conditions now to be defined for the R-sequence are those under which the Reader is taken to recognize a particular sequence of R-frames as governed by a single R-theory (and hence belonging to a single context). Since this will involve assignments (by the Reader) of characters to idents and configurations for frames other than those at (the time of) which these assignments are made (i.e., assignments relating to times other than "the present" at any frame), we introduce the following abbreviations:

ED1. $i \xrightarrow{C'/x} k$, $i \xrightarrow{P/x} k =_{df}$: at R-frame F_i, the set of characters C', or the character P, is assigned to the ident x for the R-frame F_k.

ED2. $i \xrightarrow{C'2/x,y} k$, $i \xrightarrow{\alpha/x,y} k =_{df}$: at R-frame F_i, the set C_2' of binary characters, or the binary character α, is assigned to the pair (x,y) for the R-frame F_k. (Cases may occur under FD1,2 where F_k is earlier or later than F_i, or identical with it.)

We now define:

E1. F is a R-sequence with respect to the ordered (n+1)-tuple $\langle F_0, \ldots, F_k, \ldots, F_n \rangle$ of R-frames for a Reader R iff:

$F = \langle S, C, C_2, S^{()}, \Theta, \Theta_2 \rangle$ such that:

$S = \bigcup\{S_{0-n}\}$, $C = \bigcup\{C_{0-n}\}$, $C_2 = \bigcup\{C_{02-n2}\}$,

$S^{()} = \bigcup\{S^{()}_{0-n}\}$, and:

(i)(a) Θ is a sequence of functions θ_{0-n} from S into $\wp C$, associated one-to-one with frames F_{0-n}, whose values are given by:

$\theta_i(x,k) =_{df} \{P : P \in C \wedge i \xrightarrow{P/x} k\}$: such that:

(b) $(\forall x,i.j.k)(0 \leq i < j \leq n \rightarrow \theta_i(x,k) \subseteq \theta_j(x,k))$; and:

(c) $(\forall P)(P \in C_k \rightarrow (\exists x,i)(x \in S_k \wedge P \in \theta_i(x,k))$.

(d) Θ_2 is a sequence of functions θ_{02-n2} from S^2 into $\wp C_2$, associated one-to-one with frames F_{0-n}, whose values are given by:

$\theta_{i2}(x,y,k) =_{df} \{\alpha : \alpha \in C_2 \wedge i \xrightarrow{\alpha/x,y} k\}$; such that:

(e) $(\forall x,y,i,j,k)(0 \leq i < j \leq n \rightarrow \theta_{i2}(x,y,k) \subseteq \theta_{j2}(x,y,k))$; and:

(f) $(\forall \alpha)(\alpha \in C_{k2} \rightarrow (\exists x,y,i)(x,y \in S_k \wedge \alpha \in \theta_{i2}(x,y,k))$.

(ii) $(\forall x,j)(x \in S_i \wedge x \in S_k \wedge i < j < k \rightarrow x \in S_j)$.

(iii) $(\forall k)(1 < k < n \rightarrow (f_k \neq f_{(k-1)} \vee f_{k2} \neq f_{(k-1)2}))$

Notes

(a) It follows from (i)(a) that, for all x,k, $\theta_k(x,k) = f_k(x)$;

and from (i)(d) that for all x,y,k, $\theta_{k2}(x,y,k) = f_{k2}(x,y)$. (The value of $f_{k2}(x,y)$ for any particular x,y,k may, of course, be the empty set $\emptyset$.) Within this framework, the expression $i \xrightarrow{P/x} k$ emerges as an abbreviated form of $P \varepsilon \theta_i(x,k)$, in the same way that, for the single fram, P/x abbreviates $P \varepsilon f(x)$ - and similarly for $i \xrightarrow{\alpha/x,y} k$. The reason for introducing the θ-functions is to reflect an important conceptual difference between recognitions in successive "presents", dominated by the evidence of immediate perception, and (typically partial) characterizations of idents over a succession of frames, "past", "present" and "future", in a manner which is dependent on commitments going beyond the perceptual evidence. The forms of the relevant <u>sequential commitments of the R-theory</u> are the subject of the next Section. Just as structural commitments restrict assignments under Θ and Θ_2 in the R-sequence. Formally, each function θ_i assigns a sequence of characterizations to each ident, reflecting its "life-history" as understood at a particular time, that of the frame F_i; taken together, all such characterizations form a major contribution to what we shall call the <u>state-of-the-theory</u> at that frame. Clearly, the structure of an R-theory changes over time, both in terms of assignments and associated commitments.

(b) Since the basic sets S, C, C_2, $S^{()}$ of the R-sequence are defined only as unions of the corresponding sets in each frame of the sequence, it is consistent with this account that there should be no members in common between (any pair of) different frames. Such a situation would be very untypical, bearing in mind that the analysis presupposes a Reader's adoption of the structure as falling under a single R-theory throughout. But it may perhaps not be unexampled. Consider the case of a population of "caterpillars" which are recognized as turning, simultaneously, into a population of "butterflies". No characters are recognized as common to caterpillars and butterflies; nor can any one butterfly be strictly identified with a corresponding caterpillar. We may have some substantive theory by which the transformation is explained, involving constant elements which survive it. But, whatever these may be, they are not recognizable; they may indeed be neglected without endangering the continuity of the R-theory. The philosophical problems of identity raised by such a case are more deeply considered in Part IV. Meanwhile, the best we can say is that for some reason (perhaps because the transformation takes place inside a glass case) we are committed to strict identity for a Γ-bound ident determined as bounding "caterpillars" in one frame and "butterflies" in the next.

More generally, however, it is not uncommon for either characters or idents to occur in some frames of a sequence and not in others. For more discussion, see note (d) below on frame-changes.

(c) Only two forms of continuity are defined into the R-sequence as such. One is the weak form of continuity for idents under condition (ii), which says effectively that nothing can be recognized in two successive frames without being supposed to exist in intervening frames, if any. (Its characterizations in intervening frames may, perhaps, be almost completely neglected.) This is taken to be the weakest possible form of commitment consistent with our intuitive notions about the persistence of objects. It is, for example, perfectly consistent with instances of theories involving "quantum jumps" which preserve identity over discontinuities of time (or space).

The second form of continuity is that for characterization under conditions (i)(b) and (e): that all assignments to idents or configurations, once determined for a particular frame, remain constant for that frame. This is an undoubted idealization: we typically change our minds about details of characterization during the course of a context, without feeling that our whole understanding of that context is destroyed. No general rule can, I think, be proposed under which it can be determined whether a particular change in characterization (for a particular ident or configuration at a particular time) is to be regarded merely as a <u>revision</u> of the relevant theory, as the replacement of that theory by another, or as a total disruption of understanding. What the present analysis proposes is that the least that can properly be said is that such a change demands <u>either</u> revision <u>or</u> abandonment of the theory. It will be assumed that revision of R-theories (however simple and intuitive or sophisticated and rigorous) is a common feature, and philsophically unproblematic. This is another, and more crucial aspect of theory-change underlying the importance of the notion of the <u>state-of-the-theory</u> at a given frame, mentioned above (p. 149 note (a)).

(d) Condition (iii) of E1 provides the structure of <u>frame-change</u>. Recalling that the R-frame is defined so that no recognized change of characterization takes place within it (see also note (e) below), this condition says in effect that each frame is distinguished from the next by at least one change of characterization. Since all characterization for F_k is determined by f_k and f_{k2}, the condition as framed is necessary and sufficient for a change as between F_{k-1} and F_k. Thus, $f_k \neq f_{k-1}$ entails <u>either</u> $(\exists x)(x \in S_k \wedge x \in S_{k-1} \wedge f_k(x) \neq f_{k-1}(x))$ <u>or</u> $(\exists x)(x \in S_k \wedge x \notin S_{k-1})$ <u>or</u> $(\exists x)(x \notin S_k \wedge x \in S_{k-1})$. In each case, the range of f is different in the two frames; in the last two cases, the domain also differs (the domain cannot change without the range also changing, by A4). Alternatively, $f_{k2} \neq f_{(k-1)2}$ entails that <u>either</u> there is at least one pair x,y whose binary characterization is different in the two frames, <u>or</u> there is at least one pair in one of the frames, one or both of whose members is absent from the other frame. Defining an <u>event</u> for C-theory as <u>either</u> a pair (x,k) <u>or</u> a triple (x,y,k) for which one of these conditions holds, E1(iii) can be read as saying that at least one such event determines a frame-change identifiable with the pair $(\mathcal{F}_{k-1}, \mathcal{F}_k)$. Such frame-changes are often over-determined, in the sense that more than one event determines the same change; it is common for some kinds of event to be picked out as mark-events in association with which other contemporaneous events are situated in time. This role is analogous to that described for characters used as marks distinguishing idents or their boundaries in space (p.56 note (b), and p.62 (d)(ii)).

It has been said that in all cases $\theta_k(x,k) = f_k(x)$, and similarly for θ_{k2} (note (a) above). Characterizations for $\mathcal{F}_k$ under θ_i, θ_{i2} where $i \neq k$ may of course be incomplete: indeed, new characterizations for $\mathcal{F}_k$ may be added at a later frame in some cases. Similar considerations apply to the recognition of particular idents as members of $\mathcal{S}_k$ at different times. This incompleteness of recognition or characterization need not mean, for instance, that a particular frame-change is not determined until it occurs. But this can obviously happen; the conditions mean only that, once a frame-change has been recognized, it remains a feature of the R-theory.

Where such a change involves the occurrence of an ident in one frame but not in the preceding or succeeding one, it is not necessary to interpret this intuitively as a case of "creation" or "annihilation"; the recognition of what I shall call new or ceased idents in particular contexts is generally interpretable, in principle at some level of theory, as a rearrangement of persistent elements so as to yield different structures of recognizable idents. The "birth" or "death" of an "animal" (to sidestep the question of souls) are only among the more dramatic examples; bound-idents of new or ceased composites - a very large class indeed, as we shall see - are the principal candidates. Among these, configurations are prominent; all recognitions of motion involve successive changes in structures of enclosure, and so of enclosure configurations which may be recognized as composed.

(e) Not all perceived changes constitute recognized character-changes determining changes of R-frame. Aspects of idents perceived as forms of continual change - steady, intermittent or random movement, variation of colour, shape, size or temperature, for example - may be recognized as persistent characters, a frame-change being recognized only when some particular kind of change occurs which is attended to, as relevant to the particular R-theory. That is, recognition of time-boundaries between frames, like that of space-boundaries between idents within a frame, are subject to the principle of <u>attention and neglect</u>. Most of us will be content to characterize a goldfish as "swimming about" in its tank, even if it is sometmes still; paying attention only, perhaps, if it stops and floats to the surface, or jumps out. A biologist, on the other hand, may be concerned with changes of speed or direction associated with the presence of certain chemicals or nutrients, measuring these effects on a chosen plan in which particular variations are seen as significant changes of behaviour, marking frame-changes. But even the biologist will neglect variations between chosen limits, related to his theory. They need not be the utmost limits of perception.

F. Sequential Commitments; and the Logic of Recognitive Implication

Just as the realisation of any particular instance of the structure defined for a single R-frame is dependent on the adoption by a Reader of specific structural commitments for chosen sets of characters (Sections C,D), so the realisation of an instance of an R-sequence is dependent on the adoption of forms of commitment, which will be called sequential: specific to sets of characters chosen for attention in context. As for structural commitments, I can offer no more than schematic analyses of typical forms of sequential commitments. These cannot be shown to be exhaustive of all possible forms, but, as before, are claimed to cover a wide range of commitments found in practice, including all those with which we shall be concerned in this study - and by no means confined to contexts of measurement.

(1) Γ-sets and the logic of assignment

All commitments of R-theory are assumed expressible in terms of relations of implication governing conjunctions or disjunctions of assignments of the general form $i \xrightarrow{C'/x} k$, as defined for the R-sequence. In the state-of-the-theory at a given frame F_i, the totality of such assignments determines a frame-structure for F_k. At any frame F_i, the state-of-the-theory with respect to any C',x,k for which $i \xrightarrow{C'/x} k$ may be taken to incorporate all commitments involving the assignment of members of C' for F_k, subject to all revisions up to F_i. Thus the membership of the set $\{x: C' \subseteq f_k(x)\}$ may be regarded as fixed for F_k and any set $C' \subseteq C$ at any state of the theory; though not necessarily identical at all such states, consistence of any earlier state with a later state is secured if necessary by revisions at the later state, if the theory survives. The further assumption is then made that the adoption for the R-theory of any general commitment at any frame with respect to assignments of particular sets of characters for any sequence of frames (including, especially, the dependence of any such assignments on those of other sets of characters) involves the prior recognition, in principle, of these sets as <u>composed</u>; i.e., to their recognition as <u>Γ-sets</u> for the appropriate frames (Section B, pp.72 ff.). It therefore becomes of interest to analyse the structure of Γ-sets, and in particular their structures of union, intersection and complement, as these are related to disjunctions and conjunctions of the relevant particular assignments. Modifying our notation for the I-set to take account of sequential structure, we now write:

$$i \xrightarrow[\Gamma_{C'}]{} k(x) =_{df} (S^{x}_{i \to k C'}); \quad \text{where}$$

$$S^{x}_{i \to k C'} =_{df} \{y: \eta_k/y,x \wedge i \xrightarrow{C'/x} k\} \qquad \ldots \text{ FD1}$$

Under the above assumptions, any such set is either empty or fixed in its composition in the state of the theory at any frame F_i.

It will be convenient to analyse the structures of unions, intersects and complements for composites in general, before examining these structures in the special case of Γ-sets (for the definition of composites, see Subsection A(2), p.54).

Consider now the ident-sets $U, U', U'' \subset S$ such that $(U), (U'), (U'') \in S^{()}$:

(a) Putting $U'' = U \cup U'$:

(i) $(\forall x)(\tau/x,(U'') \leftrightarrow (\exists a)(a \in U \vee a \in U') \wedge \tau/x,a)$

(ii) $(\forall x)(\eta/x,(U'') \leftrightarrow (\exists a)((a \in U \vee a \in U') \wedge \eta/x,a)$

(iii) $(\forall x)(\eta/(U''),x \leftrightarrow (\forall a)((\eta/a,(U) \vee \eta/a,(U')) \rightarrow \eta/a,x)$

(all by virtue of the corresponding conditions for composites in A9, p.54).

Thus the logical equivalence $(U'') = (U \cup U')$, where $U'' = U \cup U'$, generates no contradictions or ambiguities, and may be freely used in further construction. Moreover, the bound-idents $(U),(U')$ are members of their respective component sets (whether or not, in the case of Γ-sets, they belong to the respective base sets); and satisfy all conditions as components of (U''). So we may recognize unproblematically the further composite (U , U') such that $U'' = \{U, U'\}$.

(b) Putting $U'' = U \cap U'$, a similar construction is available by putting $(a \varepsilon U \,\underline{\wedge}\, a \varepsilon U')$ in the appropriate brackets in (i) and (ii), and $(\eta/a,(U) \wedge \eta/a,(U'))$ in (iii) above. This gives the acceptable equivalence $(U'') = (U \cap U')$; and $U'' = \{U, U'\}$ applies in this case also.

(c) Putting $U'' = U - U'$, where $\eta/U', U$, the relevant formulae are $(a \varepsilon U \wedge a \not\varepsilon U')$ in (i) and (ii), and $(\eta/a,(U) \wedge -\eta/a,(U'))$ in (iii) above; yielding $(U'') = (U - U')$ and $U'' = U - U'$, which (if recognized) constitutes the <u>spatial complement</u> of U' in U. (Note that such a spatial component is not recognized unless it is distinctly characterized.) This last construction will prove specially significant in the context of metric structures.

Going on to apply these results to Γ-sets, as a special case of composites, we note that their unions, intersects and complements have interesting properties in terms of characterization, which can then be interpreted in the context of their spatial organization. Since the memberships of I-sets for a given determinant set of characters may vary from frame to frame, their unions, intersects, etc., are only determinate within a single frame. But the principles now to be presented plainly hold for all frames; they also hold independently of space-restriction, provided that all Γ-sets involved in a construction are subject to the same overall restriction. I shall therefore omit these factors from this part of the analysis, for simplicity, putting $U = \Gamma_{C'}$, $U' = \Gamma_{C''}$, etc.

(a') $\Gamma_{C'} \cup \Gamma_{C''} = (\{x: C'/x \vee C''/x\})$; so that a composite formed by the union of two Γ-sets is characterized by a disjunction of their Γ-determinant sets. This principle can plainly be extended to unions of more than two Γ-sets; so that, for example, the union of all Γ-sets determined by the values in context of a given valuative character is identical (in each frame of its recognition) with the Γ-set determined by that V-character (D7, p.120).

(b') $\Gamma_{C'} \cap \Gamma_{C''} = (\{x: C'/x \wedge C''/x\}) = \Gamma_{(C' \cup C'')}$; whence $(\Gamma_{(C' \cup C'')}) = (\Gamma_{C'} \cap \Gamma_{C''})$. This equivalence will be important in the context of the analysis of implication and extrapolation, below.

(c') The construction of a spatial complement given by $\Gamma_{C'} - \Gamma_{C''}$ is dependent on the recognition of a base set $\{x: C'/x \wedge \neg(C''/x)\}$: but our analysis reveals problems associated with the idea of a negative assignment such as $\neg(C''/x)$. Not to recognize a particular assignment C''/x does not amount to recognizing that no such assignment holds. The problems will be more fully discussed below (pp. 212 ff.); at this point it is enough to say that we shall only be concerned with cases where the negation of an assignment is derived from a relevant positive assignment together with a commitment prohibiting the assignment, to any one ident in any one frame, of both the positively-assigned characters and those which are thereby negated. (Prominent among such commitments is that which says that an assignment of one value of a V-character rules out an assignment of any other value to the same ident for the same frame.)

Thus, given a commitment of distinctivity $\neq [C',C'']$ (C3, p.80) together with a recognition of the set $\Gamma_{C'} = \Gamma_{C''} \cup \Gamma_{C'''}$, we have $\{x: C'/x \wedge \neg(C''/x)\} = \{x: C'/x \wedge C'''/x\}$ in context. This is plainly consonant with the point made above, that the spatial complement of one composite in another is recognized only if distinctly characterized.

So, given the above conditions, we can write $\Gamma_{C'} - \Gamma_{C''} = \Gamma_{C'} \cap \Gamma_{C'''} = \Gamma_{(C'\ C''')}$: we may also write it as $\Gamma_{C',\overline{C''}}$, leaving the existence of C''' such that $\neq [C'',C''']$ and determining the Γ-set structure, to be understood.

Note, finally, that for the space-restricted case we have $\Gamma_{C'}(x) = \Gamma_{C'} \cap \Gamma_{f(x)}$ in any frame; so that in the following analysis, exhibiting the correspondences between Γ-set structures and those of certain forms of theoretical commitment, space-restricted structures do not require special treatment. It is understood that in many contexts, for particular C',x, the complement $\Gamma_{C',\overline{f(x)}}$ is neglected as uncharacterized; but the principles of the analysis are not thereby flouted.

The above equivalences demonstrate that an ordinary Boolean algebra on Γ-sets is strictly associated with an equivalent spatial structure recognized in context in terms of the unions, intersects and complements determined by the perceived spatial boundaries of their Γ-bounds (a concrete analogy with the system of Venn diagrams in set-teoretical analysis). It is this which enables us to feel some confidence in the availability of sound <u>logical</u> relationships between assignments recognized on perceptual evidence only, <u>before</u> introducing as part of our metatheoretical account any defined structure for propositions in a <u>language</u> of a context; and to argue, therefore, that authoritative sense can be given to the use of logical symbolism for recognitive structures <u>independently</u> of any associated linguistic structure (except that of the metatheory, C-theory).

Thus, to provide a simple basic "translation" (within the language of C-theory) from forms of implication into Γ-set structures, we write:

(a") $(\forall x)(C'/x \vee C''/x \rightarrow C'''/x)$ iff $\Gamma_{C'''} = \Gamma_{C'} \cup \Gamma_{C''}$
iff $\boxed{\Gamma_{C'''}} = \boxed{\boxed{\Gamma_{C'}}, \boxed{\Gamma_{C''}}}$

(b") $(\forall x)(C'/x \wedge C''/x \rightarrow C'''/x)$ iff $\Gamma_{C'''} = \Gamma_{C'} \cap \Gamma_{C''}$
iff $\boxed{\Gamma_{C'''}}$ is the intersect or overlap of $\boxed{\Gamma_{C'}}$, and $\boxed{\Gamma_{C''}}$

(c") $(\forall x)(C'/x \wedge \neg(C''/x) \rightarrow C'''/x)$ iff $\Gamma_{C'''} = \Gamma_{C'\overline{C''}}$
iff $\boxed{\Gamma_{C'''}}$ is the spatial complement of $\boxed{\Gamma_{C''}}$ in $\boxed{\Gamma_{C'}}$.

It should be recalled that, although not everything enclosed in $\boxed{\Gamma_{C'}}$ is necessarily C'-characterized - in particular, the bound-ident itself may not be so characterized

- its boundary is recognized precisely by virtue of the character-differences which mark the external boundaries of its C'-characterized components. It is this which guarantees the converse inferences from the last formulation in each case to those which precede it. Accordingly a frequent motive for the adoption of theoretical commitments is to reflect the interdependence of characterizations of external boundaries with other characterizations of the same idents.

The most important feature, however, of the above set of equivalences is that formulae using the logical connective '$\rightarrow$' are shown to be equivalent in every case to formulae defining a relationship of union, intersection or complement of Γ-sets - recognized composites of idents. These formulae do not appeal to a phenomenal form of implication, but only to structures of enclosure or exclusion of characterized spaces which are in principle directly accessible to perception.

We may now ask, in the light of this aspect of the analysis, in what sense the recognitions of such structures are to be understood as <u>theoretical commitments</u>, equivalent to acknowledgements of implications. The answer appears to lie in the extent to which such structures are confidently recognized on the basis of <u>partial evidence</u>; for example, by the recognition of C''/x (x as a member of $\Gamma_{C''}$) on phenomenal evidence only of C'/x (x as a member of $\Gamma_{C'}$). The source of such confidence can only be the readings of <u>present</u> experience in the light of past experience; a structure of inductive theory relying on supposed regularities in relationships between elements of the context over time. The concept of "induction" here appealed to will be more fully considered in Part IV; at this point, I will only say that it is not seen as resting on any general principle of regularity in nature. It will rest rather on the consistency with experience of specific commitments adopted in the theory of the context: whose general forms are analysed in the next subsection.

(2) Sequential commitments: constancy, implication and extrapolation

Once again we shall find that a small number of basic structures are adequate to found an analysis of most (if not all) of the more sophisticated systems of commitment commonly used in concrete contexts - now looking beyond the single R-frame to the R-sequence. Sequential commitments are generally based on just two such structures, those of constancy and implication. Each of these is to be defined with respect to a particular adoption frame F_i, with which is associated a specific state of the theory at that frame; under which commitments are defined as adopted for a sequence of frames $F_g,\dots,F_m$, a sub-sequence of the total R-sequence $F_0,\dots,F_n$ (in many instances identical with the whole sequence). Thus we have the conventional numerical relation $0 \le g \le m \le n$, commitments being defined for frames $F_k, F_{k'}$, such that $g \le k \le m$ and $g \le k' \le m$.

(i) Constancy

F1. C' is a constant set of characters for the sequence $\{F_g,\ldots,F_m\}$ at the frame F_i in the R-sequence formed on $\{F_0,\ldots,F_n\}$ (the condition will be written: $\perp_g^m[C']_i$) iff:

$(\forall k,k')(g \le k \le m \wedge g \le k' \le m \rightarrow {}^{i\rightarrow k}\Gamma_{C'} = {}^{i\rightarrow k'}\Gamma_{C'})$.

The intuitive interpretation of this definition is that certain sets of characters are recognized as picking out idents which retain this element in their characterization unchanged over a specific period of time (sequence of frames). Typically, such a set consists of two distinct subsets. For example, we may have $C' = C'' \cup \{P\}$ and $C'' \cap \{P\} = \emptyset$, such that P is a value in a range of values, and $\Gamma_{C''}$ a set of idents for which the commitment adopts P as a constant value (it may, of course, be a unit set). It may either be a case where P is assigned constantly to all C''-characterized idents, while other idents may vary in value from frame to frame; or where all C''-characterized idents maintain this constant value while their characterizations vary in other respects. A set of measuring rods may be maintained in such conditions that their lengths and temperatures vary. Note that the recognition of constancy in the first case requires that the constant-valued idents are distinguished from variable ones not only by their constant value for the relevant V-character, but by at least one other character (in C'') which determines that the commitment to constancy of P applies exclusively to C'-characterized idents. Thus, here as in the general case, the constancy commitment applies to C' as a whole and not merely to the constant value P.

Plainly such constancy commitments are specially important for the recognitions of sets of idents as standards for the comparison of V-characters, whether metric or not. This necessarily involves constancy of positional characters in the associated comparison-figures (above, D6 - 8, and summary pp. 144 ff.). Since such PCs are determined with respect to other idents in the figure, the relevant constancy commitment may have to be space-restricted so as to apply only to position within a series or pair whose other members are also subject to constancy commitments. The relevant space-restrictor will be the bound-ident of a suitably constructed composite; and a restriction must also be placed on the positional function Π so as to determine just positions within the series or pair. The formulation might be rather complex, but clearly raises no question of principle, and I shall therefore not carry it out in detail.

(ii) Implication and Extrapolation

Having said that all theoretical commitments involving the idea of recognitive implication (or its equivalents in terms of Γ-set structure) rest on readings of experience over time, we now need to provide a simple notation by which to indicate aspects of temporal ordering among the frames of the R-sequence - however this may be recognized in context. Let $\mathcal{T}$ be a set of binary numerical relations such that for each $T \in \mathcal{T}$, and each pair (i,k) such that $0 \leq i \leq n$ and $0 \leq k \leq n$, the expression T(i,k) denotes a temporal relation in the terms of the context between frames F_i and F_k of the sequence of frames F_{0-n} of an R-sequence; we shall call $\mathcal{T}$ a <u>temporal ordering</u> of the sequence.

F2. The triple (C',C'',T), where $T \in \mathcal{T}$ and $C',C'' \subset C$, is an <u>implicative triple</u> for the sequence $\{F_g,\ldots,F_m\}$ at the frame F_i in the R-sequence formed on $\{F_0,\ldots,F_m\}$ (the condition will be written: $\sqsubset_g^m[C',C'',T]_i$) iff:

$$(\forall k,k')(g \leq k \leq m \wedge g \leq k' \leq m \wedge T(k,k') \rightarrow {}^{i\rightarrow k}\Gamma_{C'} \subseteq {}^{i\rightarrow k'}\Gamma_{C''})$$

<u>Notes</u>:

(a) The membership of the temporal ordering $\mathcal{T}$ is chosen to reflect the temporal structure of the commitment in each case. In simple, intuitive instances it will be chosen from <,>, and = ("before", "after", "at the same time as"). Where metric values of time are involved - at the simplest level, dates and clock times - i,k and k' can be assigned such values and the members of $\mathcal{T}$ given appropriate numerical expression. This aspect will be considered more fully in the context of time measurement systems.

(b) Where $k \neq k'$, the formula ${}^{i\rightarrow k}\Gamma_{C'} \subseteq {}^{i\rightarrow k'}\Gamma_{C''}$ cannot be read simply as equivalent to a recognition of I-bound simply as equivalent to a recognition of I-bound enclosure, since such a recognition can only take place within a single frame. It should rather be read as a commitment to a relation of set-inclusion between sets of idents recognized at F_i for $F_{k,k'}$. This is interpretable only on the basis that the same idents in S_k which are characterized at F_i for F_k by C' are also members of $S_{k'}$ in $F_{k'}$; for which frame they are among those recognized at F_i as characterized by C'' (whether or not they are also characterized by C' for that frame). This involves the further tacit commitment that the members of ${}^{i}\overrightarrow{\Gamma}^{k}_{C'}$ will be (or were) exhaustively identifiable with the members of one or more Γ-sets recognizable, in principle, at $F_{k'}$: either ${}^{i}\overrightarrow{\Gamma}^{k'}_{C''}$ itself, or a number of its proper subsets. All idents, however characterized, belonging to any such subset must, necessarily, be properly enclosed in the bound-ident of ${}^{i}\overrightarrow{\Gamma}^{k'}_{C''}$, and so also must the bound-idents of any Γ-sets to which they are supposed to belong. So, to form such a commitment, it is unnecessary to specify or attend to any characterizations of the members of ${}^{i}\overrightarrow{\Gamma}^{k}_{C'}$ for $F_{k'}$, other than C'': the condition is in any case expressible in the form of one or more relations of Γ-bound enclosure, in the manner given by example (a") above (p. 161). While sequential commitments of this kind can, obviously, be empirically problematic, their expression in this form makes no assumptions not made by the relevant theory of the context in each case.

Different forms of commitment will emerge from this formulation in particular instances, according to whether or not $C' \cap C'' = \emptyset$; if so, the commitment is to a total change of characterization in repsect of C', C'' as between F_k and $F_{k'}$ (whether $k < k'$ or $k' < k$). The converse implication, ${}^{i}\overrightarrow{\Gamma_{C''}}{}^{k'} \subseteq {}^{i}\overrightarrow{\Gamma_{C'}}{}^{k}$, adopted concurrently, entails for the theory that just the same idents constitute the membership of both sets; the pair of commitments is equivalent to a commitment to a change of characterization in these idents.

(c) Similar considerations apply to <u>positional</u> characters under implicative as under constancy commitments (pp. permitting the construction of commitments involving changes of (relative) position or associated value.

(d) Where T takes the value '=' the formulation yields a simple implication within each of the chosen frames $F_g,\ldots,F_m$. The common case where $i = k = k'$ includes the important class of instances whereby some aspects of characterizations are recognized on the immediate perceptual evidence of others. We will call these instances of <u>recognitive extrapolation</u>. I shall assume that, once adopted, commitments of this sort hold, subject to revision of the theory, for all frames of adoption or assignment (the few exceptions are, I think, unimportant). I shall therefore write such a commitment simply '$\subset[C',C'']$'.

All such extrapolative commitments, being adopted for single frames, are immediately interpretable in terms of I-set enclosures. However, by introducing them only as special cases in the overall temporal ordering of a sequence, I aim to reflect the intuition, expressed above, that they rest on readings of experience over time.

Two special cases are of interest, namely those in which either C' or C'' is a unit set. Where $\subset[\{P\},C'']$, we say that C'' is an implied set of P; it plainly follows that $(\forall Q)(Q \in C'' \rightarrow \subset[P,Q])$. Where $\subset[C',\{P\}]$, we say that C'' is an inferring set of P; here, it follows that $(\forall x)(C'/x \rightarrow P/x)$. The principal interest of these cases is that they appear to capture the form of two types of theoretical commitment which are frequently met with in practice, and which may both be strongly intuitive and largely or wholly unconscious. For example, assignment to an ident of the character "man" may lead to a commitment to the assignment by implication of some particular set of anatomical characters which, being attended to in context, is a proper subset of C. In another context, the assignment of a single character may found a commitment to many others: the quality of a heard voice may be enough to permit the assignment of a considerable set of characters, relevant in context, associated with a "man", or even a particular person.

The general point is that in the schema, $\subset[(1),(2)]$, the character or set of characters in the antecedent position (1) is intended to be satisfied by features recognized directly on the phenomenal evidence, while the character or characters in the consequent position may be assigned, by virtue of theoretical commitment, only on evidence for those in position (1). Much of the importance of this class of commitments resides in that it is the natural path through which commitments of S-theory - which go in principle beyond the evidence of immediate perception - become incorporated in R-theories. One of its most fruitful developments, which may or may not incorporate S-theoretical elements - is a structure of extrapolative identification, next to be analysed.

(iii) Extrapolative Identification: the E-function

A class of extrapolative commitments which deserves special treatment: those in which the effect of the extrapolation is to complete an identifying characterization for a particular ident: i.e., where $[C',C''] \wedge C' \cup C'' = f_k(x)$ for some x and any chosen k. It amounts to a commitment to recognize some ident x uniquely, at any time, on the evidence of an assignment of some chosen subset of its identifying characters; as where we recognize a particular person from their voice. It is possible to represent this class of cases in terms of an extrapolative function, now to be defined.

F3. The function E from S into $\wp C$ is the extrapolative function (E-function) of the R-theory of an R-sequence defined on the R-frames $F_0,\ldots,F_n$, whose values are given by:

$$(\forall x,C')(C' \in E(x) \leftrightarrow (\forall i,k)(C' = \theta_i(x,k) \vee \subset_o^n [C',(\theta_i(x,k) - C')]_i).$$

Notes:

(a) $E(x)$ is non-empty for all x, by virtue of the term $C' = \theta_i(x,k)$; but it is understood to be typical of contexts in general that nearly all idents are recognized on partial evidence, at least in each frame. One virtue of defining this form of commitment over a sequence is that it reflects the way in which it may be supposed that we synthesize the total characterization for an ident from partial characterizations frame by frame over an extended period.

(b) Although the form is here defined for an individual ident, it is assumed to be common, for example, for all members of a Γ-set which are distinguished from one another only by positional character (in a given figure: see pp. 111f) or its associated value (for a given V-character) to yield values of the E-function which also differ only in PCs or values. In this way the notion of extrapolative identification is easily extended to cover sets of similar idents.

In some instances, a sufficiently bold (and therefore vulnerable) R-theory may adopt commitments involving the existence for the theory of a number of distinct idents even where their essential distinguishing characters (typically PCs or values) are not directly perceived, but extrapolated from evidence for their existence as a suitably structured set - for example, the cards in an apparently complete new pack. Generally this involves the incorporation of more or less sophisticated S-theory; and will emerge especially in contexts of probability assessment (Part III, Section P).

Summary of Sections E, F: R-sequence and Sequential Commitments

The R-sequence (E1, p. 148) has been presented as a sequence of R-frames, within which no change of characterization is recognized; each frame of the sequence being distinguished from the next by at least one change of characterization, called an 'event' (152). Recognitions of events, like recognitions of spatial boundaries between idents, are governed by the strategy of attention/neglect adopted in context (154). Significant events for an R-theory, called 'mark-events', are often changes of positional character in some figure, or value for some V-character, affecting chosen idents (153). With each frame of an R-sequence is associated a state-of-the-theory, comprising all assignments and commitments adopted under the R-theory at that frame for all frames of the sequence; typically accumulating new structures in successive frames, and capable of change under accepted revisions of the theory (147-152).

Continuity in an R-sequence is primarily recognized in terms of the identities of idents occurring in different frames; characters may also be recognized as "the same" in different frames by virtue of neglect of possible differences (comparison being impossible); but idents are typically recognized as persisting through changes in characterization, which may be total (149-50, and see 97ff). The principal "force" holding an R-sequence together is the structure of sequential commitments, inductive in character, adopted in the light of experience over time, and constituting the essential content of the R-theory in each case (related, of course, to the Reader's motives). All sequential commitments to be considered here are analysed in terms of two fundamental schemata, for constancy and all forms of implication, respectively (164-9). Many different forms of implication are exhibited as special cases of the given fundamental schema, including those involving positional and valuative characters. In particular, we explore the structure of extrapolative identification, regarded as reflecting the commonest form of identification over time (170-2); although deeper philosophical problems of identity are left to Part IV.

The strategy of attention/neglect is largely governed by the involvement of idents and characters in commitments of the relevant R-theory; which is also the main determinant of recognitions of composition (which typically changes from frame to frame), in particular that of Γ-sets (156). It has been shown that formulae expressing theoretical commitments in terms of the standard implication sign '$\rightarrow$' are logically equivalent to formulae expressed only in terms of relations of enclosure, overlap or exclusion between Γ-sets so determined; so that in principle all sequential commitments of the kinds analysed here, together with all associated structures of logical consequence, can be rendered in terms of such spatial relations as recognized, recalled or expected in context (162-3). That such commitments and consequences can, therefore, be interpreted as non-linguistic structures implies not, of course, that the use of language is unimportant in the construction of R-theories generally, but merely that in many simple instances, or at unsophisticated levels, it may be absent. The central role of language in the formation of 'group R-theories' (involving several Readers) is examined in the next and following Sections.

G. Group R-theories (GR-theories); and the Role of Language in Concrete Contexts

(1) I now extend the analysis of recognitive theories to structures of understanding of a common concrete context by a group of perceiving subjects - to be called the Readers of the context in each case. As indicated in the Introduction (p. 27), such an understanding is closely bound up with the notion of a language of the context. But the language of C-theory, as so far constructed, contains no structures specifically defined as even a first-order descriptive language of an R-theory - i.e., a language used in context to describe the perceptual recognitions of

members of the group to one another (though examples of such language have been used in illustration). Having thus failed to distinguish, in our analysis of the recognized structure of a context, between its linguistic and non-linguistic elements, we are now in a position to make the distinction in the terms suggested by the form of this analysis: namely, in terms of the *commitments* adopted for these classes of elements under the relevant R-theories.

Although we shall find that our formal treatment of a language system shows it as strictly independent of the structure of a group of Readers using the system, it is obviously appropriate to introduce it only after that structure has been defined, so that the two structures can be related. (The notion of a "private language", adopted by a "group" of one, will be discussed in some depth in Section IV.3.) The structure to be defined for a common R-theory (called a *group R-theory*, or *GR-theory*), shared by a group of Readers whose members are determined by their adoption of that theory, is by the same token strictly independent of their use of a common language; but since, in practice, the formation of such groups independently of language is a relatively rare occurrence in man, the two structures will be introduced in immediate succession, the group of Readers adopting a common GR-theory being called a *language-group* or *L-group*.

(2) GR-theory

G1.(i) A *group R-sequence* (*GR-sequence*) F^G is a sequence of recognitions adopted by a finite set $R^G = \{\ldots,R^\nu,\ldots\}$ of Readers (called the L-group of the context) in association with a set $F^* = \{\ldots,F^\nu,\ldots\}$ of distinct R-sequences (def. E1, p.148); each Reader $R^\nu \varepsilon R^G$ associates the sequences $F^G, F^\nu \varepsilon F^*$, so that (indicating elements and functions of the respective sequences by their appropriate superscripts):

(a) $(\forall\nu)(S^G \subseteq S^\nu \wedge S^{()G} \subseteq S^{()\nu} \wedge C^G \subseteq C^\nu \wedge C_2^G \subseteq C_2^\nu)$;

and

(b) $(\forall\nu,x,y,i,k)(x,y \varepsilon S_k^G \rightarrow (\theta_i^G(x,k) \subseteq \theta_i^\nu(x,k) \wedge$

$$\theta_{i2}^G(x,y,k) \subseteq \theta_{i2}^\nu(x,y,k)))\text{.}$$

(ii) A <u>Group R-theory</u> (<u>GR-theory</u>) is a structure of common recognitive <u>commitments</u> adopted by all members of an L-group R^G of Readers consistently with the assignments of a GR-sequence F^G.

<u>Notes</u>:

(a) Condition (i)(b) requires that, though individual Readers' assignments may vary as between their respective individual R-sequences, all Readers must agree on assignments in F^G; including, crucially, those pairs of assignments which determine events marking frame-changes in the GR-sequence.

(b) No special condition has been included to secure common structures of enclosure and contiguity in the GR-sequence. These are secured by common assignments of positional characters in $\hat{\eta}^G, \hat{\tau}^G$.

This definition is a gross idealisation for many contexts, in postulating total agreement between members of the L-group. But such agreement may be approached closely by sufficiently restricting the field of recognitions supposed included in the GR-theory; and it is claimed that this postulate does not differ essentially from an almost universal tacit assumption intuitively made by most people in what they take to be common contexts of concrete recognition. Neither intuitively nor for C-theory is such total agreement treated as infallible; its status for C-theory is that of a general commitment to be incorporated in the individual R-theory of _each_ member of an L-group, which, to the extent that it survives exposure to the perceptual evidence, holds for _all_ members. In practice, it remains unchallenged unless inconsistencies appear in the R-theoretical structure for one or more of the Readers. In the typical concrete context, confidence in a common understanding is sustained to a large extent by the assumption that differences between non-linguistic recognitions and commitments of Readers (including unproblematic extrapolations) are _negligible_, except for a few special points which call for the use of language. Thus for C-theory the role of language in such contexts is taken to be as much the maintenance and regulation of a consensus which is for the most part non-linguistic in origin, as the introduction of new material into a GR-theory.

In attempting now a fairly rigorous account of the structure of commitment under which it will be supposed that linguistic elements are distinguished and used, I shall start, as the principles of this analysis demand, with the tasks these elements are called upon to perform in context. The underlying intuition is that we learn to use language in much the way we learn to use other familiar constructional materials, to form structures serving recognizable purposes; and learn to understand their use for these purposes by others. So, languages are seen as socially and historically developed resources, consisting of components which can be put together in various ways: the least resulting structure capable of entering into an unequivocal relation of non-linguistic reference being a sentence. (Though this involves a marginally non-standard interpretation of 'reference', it does not appear to raise any serious conflict with more familiar uses.) Each sentence, as uttered, is newly constructed to take its place in a context. It is in this spirit that I shall suppose that 'natural' languages, which may have been developed over thousands of years to handle information in a bewildering variety of contexts, can be drawn upon to answer the particular needs of each new utterance as they arise. More will be said shortly on this aspect; and some deeper philosophical problems of language will be taken up again in Part IV. The underlying notions which will inform these discussions must first be defined, as rigorously as possible.

(3) Basic Language System

The definition of a basic language system for a concrete context must rest initially on an understanding of the forms of non-linguistic structure this system is supposed to reflect.

The elements of a GR-theory have been exhibited as common subsets of those of the R-theories of the Readers of the L-group; likewise the assignments and commitments of the GR-theory are taken to be held in common as parts of these individual R-theories. We have seen above that all such assignments and commitments are in principle logically equivalent to recognitions (either immediate or inductively extrapolated) of structures of enclosure, intersection, complement or mutual exclusion of Γ-sets (Section F, pp.155 ff.). These structures are themselves composed, and recognitions of the relevant spatial relationships between their bound-idents are equivalent to recognitions that these particular assignments and commitments hold in the R-theory concerned. It should not be forgotten that the recognitions of the Γ-sets themselves are generally motivated by association with commitments of the theory adopted for the relevant sets of characters in context. (To facilitate the treatment of negative statements of the base-language, we shall assume for the present that recognitions of some bound-ident relationships are logically equivalent to recognitions that the negations of particular assignments or commitments hold in the theory - e.g., $P \notin \theta_i(x,k)$, or

$\neg({}^{i}\overrightarrow{\Gamma_{C'}}^{k} \subset {}^{i}\overrightarrow{\Gamma_{C''}}^{k'})$. The preconditions for such recognitions will be discussed in Section J(1) below.)

All such recognitions that particular assignments, commitments or negations hold in the GR-theory will thus be supposed to have the form of complex η- and τ-configurations, whose structure will not be further analysed. They will be called H-configurations, represented by the set H^G defined informally by:

G2. Each member h of H^G is equivalent to a recognised assignment or commitment such that, exclusively, either h or $\neg$(h) holds in the GR-theory.

All significant utterances (spoken or written) occurring in a context will be assumed analysable as sequences of one or more distinct sentences, recognized as bounding complex configurations (heard or seen) of the relevant R-frames. Their detailed structure will, again, not be analysed here. (Some account of a possible method of analysis is given, however, in Section H.) More explicitly, a sentence is taken to be the bound-ident of a configuration of verbal forms, recognized as composed: so, its structure is determined by those of its components, and their manner of composition. No attempt will be made to analyse language use as a whole: attention will be concentrated on a somewhat idealised base-language defined for each context, supposed drawn from existing natural or specialised languages, and consisting of statements. These statements are related to the members of the set H^G comprising recognized assignments and commitments (or negations) by a reference function, also defined for each context.

G3. The pair L^G, Ψ is a basic language system for the GR-theory adopted by the L-group R^G for the GR-sequence F^G iff:

(i) $L^G \subset S^G$ is a set of recognized statements uttered or interpreted by members of R^G (called the base-language);

(ii) for each (ν,i) there is a recognized subset $L_i^\nu \subseteq L^G$ of statements made or interpreted by $R^\nu \in R^G$ at frame F_i^G (including the case $L_i^\nu = \emptyset$);

(iii) Ψ is a function from L^G into H^G (defined by G2), called the reference function: such that $s \in L^G$ is held true in the GR-theory iff $\Psi(s)$ holds in that theory, and:
$(\forall s,s')(s \in L_i^\mu \wedge s' \in L_i^\nu \wedge s \underset{L}{=} s' \rightarrow \Psi(s) = \Psi(s'))$, where $s \underset{L}{=} s'$ means that the two statements are linguistically identical. This is understood as saying that they are recognized as the same sentence, characterized identically except in terms of (a) utterer or interpreter or (b) mode of utterance (e.g. spoken/written).

Where statements of different linguistic structure (different sentences) are understood as associated by Ψ with the same member of H^G, this constitutes a further commitment of the GR-theory, that they are members of the same propositional set $L' \subseteq L^P$, defined as follows:

G4. The <u>propositional power set</u> $L^P \subseteq \wp L^G$ is given by:

$L^P = \{L' : L' \subseteq L^G \wedge (\forall s,s')(s,s' \in L' \rightarrow \Psi(s) = \Psi(s'))\}$.

The pair $\langle L',\Psi\rangle$ constitutes a <u>proposition</u>.

<u>Notes on G1 - 4</u>:

(a) The treatment in G2 of assignments and commitments as 'H-configurations' without distinction reflects the intuition that the distinction itself is, to some extent, artificial and subjective as well as highly context-dependent. Assignments are, in a sense, commitments of the theory: commitments take the form of patterns of possible assignments (where "possible" means consistent with the commitments). The fundamental distinction is between commitments supposed to rest on the immediate evidence of perception (assignments); and those, adopted on the basis of past perceptual experience, used to extend the Reader's understanding beyond the reach of immediate perception. An important part of the purpose of language in concrete contexts arises from the fact that what is an assignment for one Reader (R^μ) may be understood only as a consequence of a commitment for another reader (R^ν). A statement by R^μ may serve to set up, or confirm, this commitment for R^ν: if he takes the statement as evidence, he will accept the assignment as his own.

R^μ's statement, "The apples are red", may set up or confirm a commitment for R^ν, who cannot see the apples, that they are ripe at the time; it may at least be taken as evidence for the assignment of redness to the unseen apples. Few cases are quite so simple. But the claim is made that, in concrete contexts, all possible assignments, and therefore all valid commitments, are reducible to recognitions of configurations of enclosure and contiguity determined by the distributions of relevantly characterized idents in space and time. To take another relatively simple example: "All swans are white" amounts to a commitment expressible as '$\Gamma_{\text{"swan"}} \subset \Gamma_{\text{"white"}}$', for all frames of a context which specifies no restrictions of its universe in space or time, or in terms of the associated L-group (except, perhaps, that they should be speakers of English). To provide it with some rational basis, it may be supposed adopted inductively on the basis of the multiple assignment, "All swans I have seen, or of which I have been told, are white": where the L-group consists of the speaker and all those whose statements - in whatever natural language - he has heard or read and taken as evidence, and the assignments are restricted to all frames of the context up to and including the frame of utterance. It will, of course, be contradicted by any one assignment of "blackness", for example, to any ident which is still recognized as a "swan" (constituting a recognized intersect of the bound-idents of $\Gamma_{\text{"black"}}$ and $\Gamma_{\text{"swan"}}$; coupled with an exclusive structural commitment (C4, p. 80) for the pair of characterizations ("black", "white") as read in context).

(b) A somewhat relativised form of Quine's notion of 'referential opacity'* would have been introduced into C-theory, if we had sought to give general expression to the idea that, for each Reader R^{μ}, the reference of statement by any <u>other</u> Reader R^{ν} is a matter of interpretation, which could be seen as possessing a higher degree of 'theoreticity' - to adapt another Quinian term - than R^{μ}'s understanding of the reference of an utterance of <u>his own</u>. We may take R^{μ} to use an utterance of R^{ν} as <u>evidence</u>

<u>either</u> (a) of the relevant portion of the R-theory of R^{ν},

<u>or</u> (b) that the assignment or commitment, to which he takes R^{ν}'s statement to refer, holds for the GR-theory.

Choice of (a) would have involved the construction of complex expressions for notions like 'R^{μ}'s theory of R^{ν}'s theory' - carrying the threat of higher orders of meta-theory. Apart from its dangers of confusion, such an analysis strikes me as unrealistic. In using particular expressions to refer to particular aspects of his theory, takes them to be "correct", or at least effective for use, in the context of the particular GR-theory. This is a theoretical judgment on his part, no less vulnerable to error than is that of R^{μ} in seeking to interpret the reference of R^{ν}'s statement. So alternative (a) seems unlikely to yield an articulation of any structure of theoreticity offering greater clarity or plausibility to compensate for

* Quine (1953), pp. 142 ff.

its complexity. I have therefore chosen alternative (b) for the ordinary concrete context, along with an acknowledgement that such a strong theoretical position is open to error at many points. Questions of theoretical error in concrete contexts will be taken up later.

Note, in passing, that the same person may appear as a Reader of the L-group and an ident of the GR-sequence. Characterizations of a Reader may, for instance, be attended to where that person is a part of the essential subject-matter of the theory (say, in a doctor-patient context); or where the identity of the speaker is important for the truth-conditions of an utterance, as with the use of personal pronouns or positional indicators. No special problems seem to be created; nor is it necessary for all Readers to figure in this way.

(c) The basis of understanding defined for the reference function Ψ is that a sentence s is held true in the theory iff the corresponding assignment, commitment or negation, $\Psi(s)$, holds in the theory. This amounts to the assignment of an abstract (non-recognitive) property of 'held-truth' to s . But the association of this assignment with recognitive structures is so strong that we may regard it as incorporated in the GR-theory, at least in unproblematic cases - as strong, for instance, as that involved in recognizing a house as somewhere to live, and a particular house as the home of a particular family. The recognized association between the configuration s and the H-configuration $\Psi(s)$ has the character of a superconfiguration, analogous to that presented by a house and its inhabitants (learnt, in each case, from experience). Profound questions of the relationship of 'held-truth' to more familiar (if no less problematic) concepts of truth will be raised in Part IV, especially Section 7. The discussion will include consideration of the case where s is held false, i.e., the case where (Ψ(s)) holds in the theory.

(d) While the manner of presentation of the definition of a proposition in G4 is non-standard, its logical consequences are claimed to be very similar to those of more familiar accounts. We may start by deriving the consequence that the truth-conditions for all s ε L' such that L' ε L^P are identical under the GR-theory. (Linguistically identical statements (tokens of the same sentence) are trivially co-propositional.) This identity will carry over into the restrictions placed on a field of possible worlds by the truth-conditions of s : the contribution of s to the truth-conditions of any compound sentence in which s is recognized as implicated; or to the truth-conditions of any statement of propositional attitude of which s is the subject (strictly in the context of the GR-theory in each case). Again, questions about the contributions of verbal components to the determination of the truth-conditions of s , other members of L', and associated structures like those just mentioned, are likely to turn in the normal way on the linguistic rules of the natural or specialised language from which they are drawn. Some indications on this aspect of the matter are given in the next Section (H).

By way of summary, we may now define a GR-theory rather more fully and explicitly than in G1 above, as follows:

G5. A GR-theory adopted by an L-group R^G of Readers is a quintuple $(F^G, H^G, L^G, \Psi, L^P)$, such that:

(i) F^G is a GR-sequence consisting of the commonly agreed structures of the R-sequences recognized by the members of R^G under the R-theories of the context adopted by them; ... G1

(ii) H^G is the set of all configurations of Γ-bound enclosure, intersection or mutual exclusion constituting recognized assignments or commitments for F^G; ... G2

(iii) the base-language L^G (a subset of the set S^G of idents of F^G) is the set of all statements, being those sentences, recognized as bounding composed configurations of verbal elements, associated with members of H^G by Ψ; ... G3

(iv) the reference function Ψ is a function from L^G into H^G such that $s \in L^G$ is held true in the GR-theory iff $\Psi(s)$ holds in the theory; and ... G3

(v) $L^P \subseteq \wp L^G$ is the propositional power set, such that each set $L' \in L^P$ consists of statements held true by reference (under Ψ) to the same member of H^G; and the pair $\langle L',\Psi\rangle$ constitutes a proposition of the GR-theory. ... G4

While this formulation may seem like an enormously complicated way of saying something very simple, the manner of articulation is held to be important for an epistemological understanding of the construction of common contexts by groups using language. Although the emphasis of this study is on non-linguistic aspects of concrete contexts, there is nothing in the definition of Ψ to prevent its extended interpretation for references to assignments or commitments involving other linguistic elements, or even self-reference. Such references could include the assignment of the property of 'held-truth' to statements. These aspects will not be pursued further here. But, in view of the strong emphasis on language in much current philosophy, some indications must now be given of the relevance of this analysis to linguistic structures actually used in simple concrete reference itself.

H. Some Linguistic and Semantic Aspects of Concrete Reference

(1) The most striking difference between the present analysis and most earlier accounts of reference is that what we may call - for want of a better term - the 'unit' of reference is the pair consisting of a sentence (statement) and the recognized non-linguistic condition associated with it. In the tradition of linguistic analysis which, in this respect, has taken many of its fundamental concepts

from Frege (especially his (1892), trans. Geach et al. (1960), pp. 56 ff.), reference is assigned primarily to certain words or expressions within sentences, called 'referring expressions'. In general, each referring expression has been supposed to arrive on the scene equipped with a more or less determinate range of "ordinary reference": which can perhaps be identified with the extension of the concept to which it is taken to refer. (But Frege accepted that we never attain to comprehensive knowledge of this reference: op.cit., p. 58.) Such an expression, however, was often considered to lose this standard reference, when incorporated in a subordinate clause governed by any context of indirect or reported speech (which, for Quine, produced an 'opaque context', as mentioned above, p. 184). That is to say, loss of ordinary reference is seen to occur when a statement including the expression is explicitly <u>attributed</u> to someone - in our terms, a particular Reader of a particular context - as something he has said, believes, thinks, etc. In such cases the reference is supposedly to the <u>sense</u>, which Frege practically equated with the contribution made by the expression to the <u>thought</u> constituting the sense of the sentence. In C-theory, <u>all</u> utterances, together with the recognized conditions (assignments, commitments or negations) of which they speak, are explicitly attributed to particular Readers of particular contexts, as their utterers or interpreters. The circumstance that, for a particular utterance of a particular GR-theory, this attribution is not made verbally, does not change its theoretical status. No privilege attaches, for example, to the R-theory of a speaker as determining "the" references of the expressions he uses. The reference function Ψ is taken to be adopted by each member of the L-group as holding for the group as a whole. L-groups will generally draw on pre-existing forms of language, though they may adapt and add to them. But the suitability of such forms for the purposes of the context rests, in this analysis, not on appeal to some supposed general system of ordinary reference, but rather to a more Wittgensteinian notion of effective use based on recollections of previous uses of the chosen expressions in contexts perceived as similar. (No attempt will be made to refine the notion of 'similarity' appealed to here, for the generality of cases. But we shall find that this is another notion which takes on special clarity and precision in contexts of measurement.) Thus, the necessary commitments adopted in context for linguistic elements and their relationships are constructed inductively, as are all commitments of the GR-theory.

It is true that, of all human artefacts in familiar daily use, the forms of language are amongst the most closely controlled and therefore regular: the associated inductive commitments, built up socially and historically, are exceptionally stable in consequence (though not, of course, wholly static). But these considerations apply most strongly to internal rules of grammar and syntax, which have attracted most attention from philosophers. These are effectively unchanged from context to context: while relations of reference outside language need to be newly constructed (or reconstructed) for each use in each context, and are most open to both subjective and objective variation. I do not, of course, deny that component expressions of a sentence can be thought of as having distinct reference to specific elements in any structure of which the sentence speaks. But the view forced on me by the epistemological motives of my analysis is that such references can only be determined in the context of a complete sentence. Some complementary arguement for this view, from the purely linguistic standpoint, will be offered later. It will rest on the observation that a large proportion of component expressions in familiar languages are, taken alone, ambiguous in reference; and the first level of disambiguation is to be found in the structures of the sentences in which they are used. (Even if some terms appear to have a strict, universal reference, this does not affect the principle.) But, first, I want to point to the restrictions placed on the forms of language by the structures of recognitive theory to which they are used to refer.

(2) Analysis will be simplified by restricting attention to cases where a statement is used to add a single recognitive condition (assignment, commitment or negation) to the GR-theory - by bringing it forward for acceptance by the L-group. (It will be found that, in strictly recognitive contexts, negation is always used in association with a parallel positive condition).

Such statements are amongst those which have the effect of changing the GR-theory in some respect. So they necessarily involve a frame-change, marking a change in the state-of-the theory at the frame of their introduction into the context. This frame-change is not necessarily that brought about by the condition to which, in each case, they refer: which may be adopted for earlier or later sequences of one or more frames. This aspect is dealt with in many languages partly by the use of tense-forms: otherwise by distinct time-referring expressions, including calendar terms or names of specific events. These matters are mentioned here only as indications of possible means of analysis; they will not affect the main points to be made. Storage of information over time is, of course, another important use of language (written, recorded, remembered). Such use helps to secure the stability of a GR-theory, in the form of constantly maintained assignments or commitments. (These are not the same as 'constancy commitments' of the sort analysed above (F1, p.164) though they often coincide for the same characters). It also permits the recruitment of new members to the L-group, as interpreters of earlier utterance: and is virtually a precondition of long-lasting contexts of theory adopted by large L-groups. Such contexts have naturally dominated the subject-matter of philosophy, especially linguistic analysis. But the temporal structure of a context is largely irrelevant to our immediate concern with concrete reference.

(3) The analysis has been presented so far in terms of an idealised 'base language', intended to represent a minimal sentential structure assumed common to all languages capable of concrete reference. We may clarify the relationship of this idealised language system to other languages by saying, first, that any corpus of actual language - "ordinary" or "specialised" - will be taken to have <u>concrete reference</u> for C-theory to the extent, and in the form, that a translation is in principle available into one or more consistent statements of a suitable base-language, instantiating the terms and conditions of G3, p.181 . A "translation", for this purpose, may be a linguistically identical or logically equivalent structure in the same actual language. A "<u>concrete proposition</u>" can be taken as the set of all sentences which are translatable into the same set of base-language statements of some GR-theory. A subset in a single language of such a 'propositional set' would be similar in form to a set of Chomskian 'transformations' of one another.

I shall now concentrate attention on a fragment of English, either taken to be a translation, in this sense, of a part of a base-language containing only statements which change the GR-theory as explained above: or, equally, we may think of each example as itself a fragment of a base-language, drawn from English, considered as fulfilling the conditions laid down in G3. Amongst these statements, attention will be further restricted, for simplicity, to cases where a statement is used to add a single character, or set of characters describable by a word or simple phrase, to the characterization of a single ident, itself describable by a word or simple phrase: so as to modify that characterization at and for the sequence of frames immediately following its utterance. We shall still be able to bring out the main lines of the demands made by the task of concrete reference on the construction and interpretation of linguistic forms.

From a standpoint of linguistic analysis it would be natural to take our departure from the straightforward case of a sentence, designed to carry out the task described, consisting of a subject-predicate pair; of which the first element is a referring expression picking out or denoting the ident to be spoken of, and the second a predicative phrase containing an expression referring to the character to be added. Such cases are, indeed, typical, but far from universal. A few examples show that many natural usages appear quite anomalous in these terms.

A. It's raining.

B. Three statements accompanied by pointing to a table:

(i) This is a table.

(ii) This is marquetry.

(iii) This is a scotch (referring to the contents of a glass on the table).

C. The door is open (where more than one door is visible, but only one is open).

D. (i) Socrates is worrying the students.

(ii) Socrates is worrying the sheep (where Socrates is the name of an English sheepdog).

In all these cases it can be argued that the strict reference, in the sense of that which determines the identity of the ident whose characterization is to be modified, is fixed by the predicative phrase. Nor is it clear how any appeal to Chomskian transformational grammar, by modifying the content or relationship of 'noun phrase' or 'verb phrase', would assist our understanding of how reference operates in these cases.

It is important to recall that we are not merely concerned with the logical form of truth conditions of the sentences (though these are always relevant) but their informative force in changing the theory of the context and the nature of the referential commitments by which this force is applied. From this point of view, the interpretation of a statement must determine at least three factors in the situation:

1. The context of theory (in so far as this is not predetermined by existing non-linguistic commitments, or previous discourse).
2. The particular ident spoken of.
3. The character to be added.

This can be understood as involving a progressive concentration of attention; to more and more distinctively characterized spaces (at relevant times). The process is complete only when the necessary characterizations have been determined under both heads 2 and 3. Where these characterizations are simple enough, as in case A above, we need not be surprised that both these tasks are done by one phrase. Nor is its grammatical form critical: "It's rainy", or the single word "Rain!", would have achieved the same result. The time and place of utterance (not necessarily the speaker) are typically adequate to fix the space characterized and the time of characterization in this case, given the recognitive conditions and more or less universal non-linguistic commitments. But these factors do not fix the context of the theory, under head 1. It might appear, at first glance, that this does not matter, since it is not obvious how it would affect the truth conditions for the statement. But the context of theory is what settles the logical consequences flowing from a statement. In this case, it might clearly affect how much precipitation would count as "raining": enough to stop a football or a tennis match - or enough to spoil a new hairdo. This, in turn, will affect the truth-conditions in context. We may suppose that the context of theory is supplied, in such cases, by non-linguistic commitments or by previous discourse. (Davidson's treatment of this sentence as a paradigm of understood meaning is considered in some depth in Part IV.)

In most cases of deixis (like B(i) - (iii)) the demonstrative and accompanying visual indications serve only to restrict the space to be characterized, leaving all characterization to be carried out by the predicative phrase. In the three examples given, this phrase also does much to fix a context of theory. Since no competent English-speaker needs to be told that a table is a table, B(i) seems to belong to a context of language-teaching - or perhaps logical or linguistic analysis. In B(ii) we are interested mainly in the decoration on the top of the table, probably in a context of antiquarianism, or buying and selling. In B(iii), the consequences seem unequivocal. This is not, of course, a comprehensive analysis of reference in deictic contexts - merely an indication of possible trends in a C-theoretical analysis, as are all these illustrations.

General terms like 'door' (case C) supply a characterization sufficient only to restrict attention to a class (Γ-set) of idents. If more than one is available for reference in context, further characterization is needed to fix reference to one of the class. Here, this is done by the predicate, given the stated recognitive conditions. It might also be done, for instance, by deixis; or by an adjective - "The safe door is open", "The open door is ...". Many possible contexts of theory suggest themselves, but I shall not pursue them.

Proper names (cases D(i), (ii)) are designed to fix reference to particular individuals, however characterized in context, and problems under head 2 arise only when more than one individual bears the same name. Generally non-linguistic commitments of a context restrict alternatives within manageable bounds - especially with names as rare as Socrates (outside Greece). Where confusion is possible, little additional characterization is usually needed to determine which individual is named, and this is often supplied by the predicate which also adds the necessary character under head 3. I have, however, used case D to illustrate how abstract (non-recognitive) contexts like that suggested in D(i) can introduce ambiguities not dispelled by non-linguistic commitments - as they certainly would be in D(ii). Someone who has a dog called Socrates may find himself having to say, "Socrates, the philosopher, ...".

(4) The main motive of this discussion of examples has been to argue that the key factor in concrete reference is the association of elements of <u>characterization</u> with the particular words or expressions used: and that the complete sentence is the linguistic unit within which the definitive structure of characterization is built up. The grammatical structure of the sentence, important as it may be, serves the purposes of concrete reference only in so far as it helps to determine how the characters evoked by virtue of this association are related to one another and to the idents assigned them. More will be said shortly about the roles in characterization of different grammatical categories of expressions. But first I want to emphasize the importance of the structure of contextual <u>theory</u> (head 1 above) to the operation of

reference. Many proper names, and most common general terms (like the noun, 'table', or the adjective, 'open', for example) carry an enormous wealth of associated characterization from past use in a variety of contexts. Apart from the dangers of ambiguity, in many cases pairs of characters associated with a given term would be inconsistent if understood together in a single context. The shared commitments of a well-constructed GR-theory have the effect of restricting the field of associations selected for attention in context; essentially, to those involved in the commitments of the theory. It is the interaction of these commitments with past linguistic associations which makes adequately consistent and unequivocal reference possible.

A further important contribution of the GR-theory is derived from the -function, which ideally determines, by extrapolation, a consistent pattern of characterization in all individual R-theories of the context, in spite of all variations in the sources of information of different Readers (see F3, p. 170f).

(5) Accepting at least for the sake of argument, that the contribution to the reference-value (under 1) of a statement by a component word or expression consists of a set of characters, selected for attention in context from a larger field associated with it in memory, I want now to examine the relevance of the grammatical category to which a component term may be seen to belong.

While the supposed existence of character-fields of reference may seem relatively unproblematic for names, noun-phrases, and even adjectival phrases which can be clearly understood as having composite idents (Γ-sets) as contextual fields of reference, certain other types of expression may be thought of as raising special problems. I shall therefore say something more about two of these: verbs, and comparatives.

9. To prepare the ground for these considerations, I want to make a short detour into the grammatical phenomenon of 'parts of speech' as a feature of indo-european languages in general. By making more or less clearly recognizable structural (morphemic) differences between types of words distinguished as nouns, verbs, adjectives, adverbs, etc., and inflecting them in various ways to mark number, gender and tense, one can use these inflections (and 'agreements' between them) to clarify verbal relationships, and so, often, to disambiguate possible alternative readings of the referential roles of the inflected words. Much of this aspect has been lost in English: in the Chinese group of languages it is completely absent. (For all that I say henceforth about these languages, my source is "About Chinese", Richard Newnham, Pelican, 1971.) Interestingly, it is generally true that the less inflected a language, the more it relies on word order for disambiguation: the highly inflected structure of Latin gives almost total poetic freedom of word order, while the reverse is the case with Chinese, whose poetry glories in ambiguity.

It is sometimes said that Chinese has no verbs (or, for that matter, adjectives). This does not mean it has no words for the kinds of features of experience we refer to by verbs; merely that it makes no morphemic distinction between these words and, say, nouns. There is no visible or audible difference, for example, between the 'verbal noun' (e.g. English "running" - itself ambiguous - French "le courir", German "das Laufen") and other 'parts' of the verb. This suggests that the indo-european types of morphemic system, though useful, are not essential to successful reference. Bearing this in mind, I want now to point to some aspects of the usage of <u>verbs</u> in English.

Many verbs have concrete reference, which (apart from tensing) may be classified as behavioural or relational. By behavioural reference I mean reference to perceived change over time, assigned to any ident, e.g., "run", "boil". By relational reference I mean reference to any configuration assigned to two or more idents by the use of the verb in conjunction with expressions referring to those idents, e.g., "match", "support" (in concrete senses). The two kinds of reference may occur together in one verb, where perceived change affects more than one ident, e.g., "race", "hit", "assemble", "follow".

We have already seen (note (e), p.154) that change recognized as continual (even if not strictly continu<u>ous</u>) may be analysed as a persistent character assignable to an ident for any frame (or sequence of frames), limited only by the specific commitments of the relevant R-theory as to which forms of change are to determine frame-change. It is persistent characters of this kind - varying with context - which form the typical fields of reference of verbs like "run". "The trains were running normally out of Euston this morning", "I had to run for the bus", "The bath tap's running", are varied examples. There is no difference in principle, so far as I can see, between the recognition of persistent characters involving continual change in such ways as these, and of more "static" characters like those involving, say, shape or colour. Some examples of frame-change involving verbial characters will be considered shortly. Relational verbs can be simply analysed as referring to a specified type of configuration, defined as ident, composed of the idents associated in the assignment referred to - typically by drawing attention to a relation derived from the relevant figure (the relevant definitions are in note (c), p. 57f). Mixtures of these types of reference raise no additional problems. A verb like "to live" has for most of us a huge field of behavioural and relational character-reference: yet in a particular context just a few, or even one, of the characters it comprises may be enough to characterize an ident as "living".

The English habit of forming parts of verbs out of parts of "to be" coupled with verbal adjectives - "is running", "was broken", "has been following" - seems to reflect an intuition that "running", "broken", "following" are recognizable characters like any others. Verbal nouns like "runner", "breaker", "follower" may be seen as "naming" some ident by reference to a verbial character. "Runners" and "followers", in some contexts, become what might be called 'status-character' referent terms, belonging to the same type as "nurses" or "fathers". The last two are grammatically nouns, although the verbial aspects of their reference are attested by the verbs "nursing" and "fathering". English, again, has a habit of making verbs out of nouns with no immediate verbial connotations: "to water", "to bridge", and - much to the fury of British pedants, the American-born "to contact".

Recalling that Chinese does without most of these distinctions, the point is made (short of comprehensive demonstration): it may be plausibly claimed that all concrete reference, by whatever combination of parts of speech, can be analysed in terms of assignments of characters to idents.

(6) The reference of <u>comparatives</u> raises a different kind of question: very important in the context of a study of measurement, as a class of organised systems of comparison. The present analysis seems to lead to the conclusion that this is an aspect of language use in which <u>non</u>-linguistic factors of the particular context dominate the R-theoretical structure to such an extent that purely linguistic analysis is bound to fall short of an adequate general account of reference. Use of a comparative term,

if it is to achieve concrete reference, must direct attention to a specified comparative structure, within which a comparison figure is recognized, such that the pair of idents specified form a configuration of that figure, yielding a comparative relation in terms of values of an associated V-character, also to be specified in the relevant statement. The success of a statement, often quite simple in form, in specifying such a structure, plainly makes considerable demands on the L-group's common understanding of the reference function - even allowing for heavy reliance on the non-linguistic aspects of the context. The formal account of a comparative structure given above (D 8 and 9, pp. 123-4) calls for both a set of compared idents and a set of characters, values of a V-character, in terms of which they are compared (as well as the recognized comparison-figure). This account contrasts, on the one hand, with metamathematical accounts in the Suppes tradition, and on the other with linguistic, or semantic, accounts of comparison. The first approach, as we have seen (Introduction, p.28), starts with the entities to be compared, the compared qualities (attributes, etc.) and relations of comparison being defined simply as sets of such entities. Linguistic aspects are neglected. Linguistic approaches, on the other hand, take their departure from the fields of reference (extensions, or concepts) associated with verbal expressions, regarded as objective structures which are there to be discovered and analysed, rather than continually constructed and adapted in use, and the memories and records of use. Consequently they may be faced with problems arising out of verbal forms which are no more than accidents of usage - however practically useful - limited to a few, generally indo-european, languages. While C-theoretical analysis suggests that we should in principle distinguish, for each particular comparative structure, a V-character with a distinct set of values recognized, to a context-dependent standard of accuracy, in association with a chosen comparison-figure, ordinary English usage, for example, is rather less than perspicuous in describing such a structure.

If the V-character is "length", for example, so that each ident of the comparative structure is in some sense assigned "length", any one ident may be said to "have greater (or less) length than" another; or, say, to "have a length of n metres", for some specified number n. These locutions fit easily with C-theoretical analysis, using qualifications of the term for the V-character to designate members of the value set. But we may also use adjectival forms, saying that an ident is "longer" or "shorter" than another, or "n metres long", with no easily discernible difference in the reference from that of the previous usage. At the same time, however, we may call some idents of the structure "long" and others "short". In a particular context we may call an ident "short" (or "not long") which is quite consistently called "2 cm. long". (We cannot call it "2 cm. short" unless we want to say it is 2 cm. shorter than it ought to be: if we want to say it is 2 cm. longer than it ought to be, we must say just that, or perhaps "it is 2 cm. over length" or "too long".) Further, we may speak of idents "increasing in length", "getting longer", or "lengthening": these usages are generally equivalent, but the last seems unacceptable where no change in length is involved. The branches "get longer as you go down the tree"; they only "lengthen" (but also "get longer") as the tree grows.

Comparatives may also be applied to verbs, by adverbial qualification. From previous discussion, it is to be expected that for C-theory the structural distinctions from noun- or adjective-related comparatives is more apparent than real. A small group of examples, with brief analytic notes, illustrate the point:

(a) "A ran faster than B", "A ran at a greater speed than B": comparison between A and B in terms of their "running" and "speed" characters.

(b) "A ran faster and faster", "A ran at increasing speed": most naturally interpreted as referring to the assignment to A of a single 'persistent-change' character, possibly measurable by a single acceleration-scale. Alternatively, as in the next example, changes in speed could be related to frame-change.

(c) "A ran faster (A's speed was greater) before the damage to his knee": comparison, with respect to A, in terms of his "running" and "speed" characters, before and after a frame-change referred to an event: a change of characterization called "the damage to his knee". (The knee was "undamaged" before the change, "damaged" after.)

(d) "A's shirt was brilliantly white": an 'adverbial' qualification of the adjective "white", or of the verb-phrase, "was white"? Or equivalent to "brilliant and white", or, "the whiteness (of A's shirt) was brilliant"? Although not expressed as a comparative, this suggests at least a tacit, vaguely conceived comparison between A's shirt and white shirts in general - or, perhaps, a more definite comparison with other specified shirts in context, or with A's shirt in a pre-bio-wash frame - in terms of degrees of "brilliance" (or otherwise) of "white" (or a range of colours).

It surely should not surprise us that in face of the enormous variety of recognitive conditions calling for valuation, and especially comparison of values, English throws up a number of alternative grammatical forms of expression, some of which make analysis of the structure of reference problematic. Nor need we suppose English to be exceptional in this. In most everyday contexts, the "fit" between language and recognition is loose: no closer than it need be. In stricter contexts, including those of measurement, we make use of carefully constructed and controlled artefacts to supply standard values - standard colours, standard weights, clock times - and assign linguistic terms by registered convention. In English (and all indo-european languages I know about) such conventions use terms both for V-characters and their values which are grammatically nouns - though English, typically, often adapts them as adjectives: a "vermilion" tie, a "four-pound" salmon, a "two-hour" session.

(7) One semantic account of adjectives, however, draws attention to the association between comparative valuation and the logical problems of vagueness in a way which suggests interesting parallels with the C-theoretical version (Kamp, 1975). Hans Kamp argues that the attempt to deal with adjectival comparatives in terms of a many-valued logic, distinguishing degrees of truth-value for sentences using a single adjectival predicate, lead to confusions or even contradictions in the resulting combinatorial system (see op.cit., Sec. 2(10)). He therefore opts for a standard two-valued logic, which leads immediately to an analysis of the comparative in terms of the association with each adjectival predicate of a set of 'graded' models: distinct interpretations in each of which any one-place predicate either holds, or not, or is undecided, for each element of the universe in each interpretive model and choice of elements (as assignments of values to variables) for that model. These

interpretations may also be 'context-dependent' in terms of which assignments for the variables yield decided truth-values, and which not, in different contexts. There is some loose similarity of structure with C-theory in the choice of two-valued logic (for which I have offered no argument), associated with a set of discrete structures within which certain assignments to idents - in this case of characters which are values of a V-character - hold, or not, under a theory which selects certain idents and values as elements of the context. I shall not attempt translation between the two formulations, which have different aims. But both accounts accept the partial status of typical - and perhaps all - actual instances of comparative systems which might be susceptible to their analyses. Kamp treats the partiality of his semantic systems in terms of vagueness, associating each partial interpretation with a set of logically possible completions (in the sense of van Fraassen's supervaluation theory): in each of which some or all of the truth-values left undecided for the instantiating context (or situation) are decided one way or the other, in terms of the appropriate model and alternative choices of elements. For this set of completions he considers a probabilistic structure, for which an ideal measure is proposed: but rejects it, at least for the general case, on the ground that it fails to resolve contexts in which there is more than one criterion for the applicability of the relevant adjectival predicate.

The more epistemological perspective of this study suggests at least two different aspects of the theoretical structure under which notions of vagueness might be considered to fall. Both, I think, can be accommodated within Kamp's analysis. One is taken to occur strictly within the comparative structure of the relevant R-theory, for which I have proposed the association, in many cases, of a pair consisting of a standard and an approximate structure. In the standard structure, definite character-values and comparative relations are determined for the theory; further assignments are then more vaguely associated with these standard values, for elements of the approximate structure, for which the theoretical commitments are weaker, particularly in rspect of transitivity. A full account of such structures in non-metric contexts, with examples, is given above (D10, 11, 12, pp.128ff). This aspect of vagueness remains to some extent under the control of the L-group or individual Reader, where they can eliminate indeterminate value-assignments by neglecting them in the construction of the R-theory. In measurement contexts, not only are standard values and the limits of tolerated vagueness expressed sharply in numerical terms, but associated approximate values can be restricted to those which are well determined in the same terms, with some loss of transitivity. This determinacy may, of course, be limited or excluded in particular contexts by the unavailability of a recognizable comparison figure.

The second aspect of vagueness for C-theory arises at the point where comparative values come to depend on commitments of substantive theory - i.e., where we must go beyond what can be recognized on the phenomenal evidence using only the commitments of R-theory (we shall see that this conceptual boundary is itself dependent on the theoretical structure of the particular context). Here, choices between alternative readings of comparative values and relations, derivable under the various commitments of substantive theory, may be undecidable. There are obvious similarities between this account and Kamp's analysis in terms of possible completions of a partial model. Although, for C-theory also, probabilistic structures are not always appropriate to such cases, it has been found better to treat the whole question later, under the head of probability, and after the analysis of metric contexts.

Although Kamp's main choice of theme concentrates his attention on comparatives in relation to adjectives, he also gives some consideration to nouns in this context. C-theoretical analysis suggests that what determines whether or not comparatives can coherently be deployed in association with an expression depends not on grammatical analysis of parts of speech as much as upon the recognition - in concrete contexts - of an associated comparison-figure (and I suspect that an analogous criterion holds in abstract, or mixed, contexts). The Chinese, after all, dispense with these indo-european distinctions. Newnham suggests that this poverty of distinction makes Chinese "a poor medium for logical or philosophical ideas" (op.cit., p. 85): but perhaps he has a narrow view of philosophy. China has produced great philosophers, and may hope to produce more in future. But they are unlikely to be linguistic analysts.

J. Notes on Some Abstract Concepts in Contextual Theory

The exposition of C-theory, as metatheory of R-theories of particular contexts, has now been brought to the point at which the treatment of measurement contexts as special cases can be carried out. A few important background considerations, however, remain. I want to clarify the status of certain key abstract concepts for the theory, namely: non-affirmative (principally negative) assignments, objectivity, truth, and error. I shall suggest how each of these concepts may attain sharper significance for measurement systems.

(1) Non-Affirmative Assignments

(a) Negation. In the fundamental definitions of R-frame (A1 - 9, pp. 52-4) and R-sequence (F(1)(c), p. 81) - on which the whole structure of C-theory is built - all assignments of characters to idents are affirmative and unequivocal. This is claimed to reflect the actual intuitive experience of phenomenal recognition. For any P and x, either $P \in f(x)$ or not. If not, $(P \notin f(x))$, the judgment that P is not assigned to x does not rest in any simple way on the phenomenal evidence for the characterization of x alone. It is, indeed, a logical consequence of the definitions that this negation holds for each and every character in C and not in f(x). But it would be an entirely false account of perceptual recognition that required us to make this negative assignment for every such character - an impossible task in many such contexts. We do not use logic in recognition, but to derive the consequences which interest us from the results of recognition. (The only appearance of negation '$\neg$' in the fundamental definitions is in the definition of exclusion '$-\eta/a,b$'; AD1, for A7. The expression '$\neg(\exists c)(\eta/c,a \wedge \eta/c,b)$', which occurs there, does negate a formula which contains assignments of configurative characters, in the form of enclosure-relations 'η' (for this interpretation see discussion in Note (e), pp.66ff. But it must be strictly read as saying that <u>no such assignment</u> for any ident c is part of the R-theory, i.e., <u>attended to</u>. Again, the expression '$(\forall c)\neg(\eta/c,a \wedge \eta/c,b)$' is a logical consequence, but its recognitive testing would require us to check every ident of the context to see whether or not it satisfies these enclosure relations.)

The intuition to be captured is that we only <u>attend</u> to specific negative assignments; in C-theoretical terms, this restriction of attention is governed by the specified commitments (explicit or tacit) of the particular R-theory. Three key schemata have been introduced which may yield negative assignments. I will now discuss each of these in turn.

(i) $\Gamma_{C'\overline{C''}} =_{df} \Gamma_{C'} - \Gamma_{(C' \cup C'')}$ ... F1, p.160

To treat only the simplified case of single-membered sets of characters, and putting $y = \Gamma_{P\overline{Q}}$, we easily derive $\vdash (\forall x)(\eta/x,y \rightarrow \neg(Q/x))$. Γ-set construction, as defined, rests on the recognition of the composite ident (satisfying BD2,p.72) of all idents characterized by a particular set of characters for each Γ-set. In this simplified case, the relevant Γ-sets are Γ_P, Γ_{PQ}, and $\Gamma_{P\overline{Q}} = \Gamma_P - \Gamma_{PQ}$; e.g., the sets of "all birds", "all white birds", and "all non-white birds", recognized as "not white". This result rests, as we have seen, on two underlying commitments: first, to the recognition of certain particular features of idents in a given region as "the same character" when assigned to different idents; and, second, the recognition of certain particular sets of idents so characterized as enclosed in composite idents in such a way that complements, such as $\Gamma_P - \Gamma_{PQ}$, can be determined (see "characterization and the logic of assignment", pp. 155 ff.).

(ii) $\neq[P,Q] \leftrightarrow_{df} (\forall a,b)(P/a \wedge Q/b \rightarrow a \neq b)$

... distinctive pair, C3, p.80.

We derive easily $\vdash \neq [P,Q] \rightarrow (\forall a)(P/a \rightarrow \neg(Q/a))$: following the example given on p. 90 , we can say that anything "black" will be at once recognized as "not a swan". It must be insisted that such a negation rests on the specific recognition of this pair of characters ("swan", "black") as a distinctive pair in context; in another context we might lump all swans, or all birds, together, of whatever colour, in distinction from all other animals some of which may be black, others white, or any mixture of colours. (Such distinctive pairs are often important in determining boundaries: we can distinguish parts of a knitted sweater as exclusively "purl" or "plain", neglecting variations of colour common to both sides of the boundary.)

A distinctive pair of characters, as instanced here, may or may not be <u>values</u> of a third, valuative character - which would bring them under the following schema:

(iii) $V\{C',Q\} \rightarrow (\forall P,P')(P,P' \in C' \wedge P \neq P' \rightarrow S_P \neq S_{P'})$

... (value set: D7(ii) and notes, p.120)

That schema represents the general case where the values of a value-set are not distinctive (note (e), p. 88). But in many familiar cases, especially of metric values, values <u>are</u> distinctive: nothing can have more than one value of length, say, in any one frame. So, adding the condition $(\forall P,P')(P \neq P' \rightarrow \neq [P,P'])$, we have $(\forall P,P',a,b)(P,P' \in C' \wedge P/a \wedge P'/b \wedge P \neq P' \rightarrow a \neq b)$; whence $(\forall P,P',a)(P/a \wedge P \neq P' \rightarrow \neg(P'/a))$.

Except in highly restricted contexts such as those instanced above, there are few plausible examples of negative assignment where no covering V-character is recognized, if only tacitly, as assigned to the ident to which the assignment of a particular character, as one of its values, is negated. We are certainly committed, in some ill-defined way, to the implication that a "table", or a "piece of music", is "not a swan". Neither of these can readily be assigned a V-character of which "swan" is also a value. But it is also hard to think of any rational context in which such negations would form part of an R-theory. Cases of this kind, if they can be discovered, would be hardly generalisable and not very interesting. (In what context would it be apposite to point out that "The Dying Swan" is not a swan?) Underlying such a negation is, typically, such a tacit assignment as "this bird is a goose", or, "this bird honks", "so it can't be a swan"; would it ever be rational to make the judgement, "this motor car honks, so it can't be a swan"? The case of the honking bird leaves open the question what value of "bird" other than "swan" is to be assigned, if any; and illustrates how a distinctive pair ("swan", "honking") of type (ii), although not being a pair of values of any V-character, can be part of the basis for discriminating between values of a V-character of which one of the pair is a value.

One interesting use of negative assignments is to block extrapolative identification (see F3, and discussion, p. 170f). If a "bird" is assigned many of the characters which could lead to its identification as a "swan" on partial evidence, but is then found to "honk", a tentative identification would be blocked.

It is not claimed that this set of three schemata exhausts all sources of negative assignment. But it is comprehensive enough to form the basis for a discussion of the main metatheoretical problem raised for C-theory by such assignments. Every negated character must be a member of the recognized domain $\mathcal{C}$ of characters for the R-theory; there must, therefore (by A6, p. 53), be some ident recognized in the context to which it is assigned. So we could not deny swanhood to any ident in a context where no swan is recognized. This is not as restrictive as may at first appear, since it is not required that the excluded character should be recognized in the same frame: so, by extending the context far enough back in time, we may negate any character recognized in the past, for which a suitable distinctive commitment (for example) is adopted for the R-theory.

The "storage" facility of language (p. 192) is of major importance here, in increasing the domain of values, stable over long periods, which can be brought within R-theoretical accounts of contexts familiar to a particular L-group. We are still, however, restricted to characterizations which can be understood as resting on phenomenal evidence available at some frame of the relevant R-theory to some member of the L-group. What counts as such evidence, and how much the R-theory may "read into it", is within rational limits a matter of autonomous decision within the theory in each case (ultimately, too, what counts as "rational"). These limits can be relatively clearly stated for measurement contexts, and fuller discussion is therefore left for Part III. At this point, it may just be noted that the property of valuative commitments, whereby affirmative assignment of one value yields negative assignments of all others, is sharply defined for measurement systems.

(b) Other Non-affirmative Assignments

The C-theoretical analysis of disjunctive assignments of the form '$P/a \vee Q/a$' follows closely the analysis of negation, which has already been presented as generally the result of a choice between alternatives. In case (i), recognition of the sets Γ_P and Γ_Q leads at once to the derivation $\vdash (\forall a)(a \in (\Gamma_P \cup \Gamma_Q) \rightarrow (P/a \vee Q/a))$; only if $\Gamma_{PQ} = \emptyset$ is this an exclusive 'or', so that $P/a \rightarrow \neg(Q/a)$. In case (iii), looking back to the definition of a value set given by $V[\mathcal{C}',Q]$ (D7, p.120), we see that condition (i) requires $(\forall a)(\eta/ax \rightarrow (Q/a \leftrightarrow (\exists P)(P \in \mathcal{C}' \wedge P/a)))$. Putting $\mathcal{C}' = \{P_1, \ldots, P_n\}$, the consequent formula can be written: '$P_1/a \vee \ldots \vee P_n/a$', the simple alternative $P_1/a \vee P_2/a$ being the special case where has just two members. Only the distinctive condition for value set, or the recognition of a distinctive pair, yields the negation $P_1/a \rightarrow \neg(P_2/a)$.

Disjunction appears, therefore, as an intermediate stage in the process of recognizing assignments. It occurs naturally in questions, like, "Is this a swan or a goose?"; where the questioner has tacitly made a V-character assignment ("bird", or something narrower such as "large white long-necked bird") and is looking to another Reader's utterance for evidence leading to a specific value assignment. This form of question helps to determine the form of the context, by directing attention to a specified range of values, perhaps clarifying an accompanying gesture which might equally have done for "What is that swan doing?". Often, a simple question like, "Is this a swan?" will have the same effect.

Orders or statements of intention - "This wall will be white" - can be analysed, like predictions, as assignments under sequential commitment (pp. 163 ff.) - and equally open to disconfirmation at a later frame. Other syntactical forms, placing phrases referring to assignments in subordinate clauses, may assign special status to these assignments in the framework of a Reader's supposed psychology in terms of hope, belief, desire, etc. These attributions may fall within a psychological theory which is unlikely to be concrete in C-theoretical terms, but which may affect the acceptance of the assignment, or otherwise, for the Group-R-theory. But they do not affect the form of the assignments as such, if the relevant references can be assigned to the phrases in question; and will not be considered further. The statement that a

Reader "knows", or, in certain senses that he "sees", "perceives", or in similar terms "is aware that" some assignment holds (via the assigned reference of a subordinate clause), will carry some commitment to 'objectivity', which is next to be considered.

(2) Objectivity and Empirical Truth

The question whether this study can throw any light on general philosophical or metaphysical notions of reality or truth must be left for discussion towards the end. But, at this stage, limited informal definitions of these concepts for C-theory can be given in terms of an abstract property of 'objectivity' for idents and characters. Corresponding with the property ordinarily attributed to anything by the word "real", or to a property or character attributed as "really" possessed by anything, it is (usually tacitly) assumed for all idents and characters of an R-theory. If any phenomenon occurs in the spatio-temporal framework of a context, and is attended to in association with the context, from which the assignment of objectivity is withheld, it is generally given concurrently to some associated ident to which the appearance of the phenomenon is attributed (see examples below).

The conditions for its assignment vary from context to context; and, within each context, from

Reader to Reader, and from element to element of the R-theory. Its assignment brings with it certain types of theoretical commitment which also vary in this way, but which seem always to involve spatio-temporal aspects of the associated idents, i.e. (1) their extension in space, and (2) their persistence through time. The first is dependent on our general beliefs about the proper extensions of natural objects: "real" people cannot walk through the spaces occupied by "real" tables (an 'exclusive' commitment under Schema C4, p. 80) - though they may seem to walk through their "real" reflections in transparent glass. Commitments of this kind generally bring with them other, ancillary commitments about consistency with the perceptions of others. The commonest test of objectivity is to ask others whether their perceptions tally with our own: "Do you see what I see?" Commitments of the second kind are equally testable by a single Reader of the context: "real" elephants do not vanish on the instant or appear intermittently to the uninterrupted gaze (a 'constancy' commitment under Schema F1, p. 164). However, the same does not apply to the ordinary unscientific experience of seeing "real" flashes of lightning.

But an after-image, though it persists for the expected time in the perception of a single Reader, does not occupy the region of space suggested by this perception, nor are we surprised that it is not seen by others. We are inclined to think of it as "real", all the same, because such things are regularly perceived by people after looking at bright lights, and their accounts of the effects tally well enough (though we should not feel the same about pink elephants seen by heavy drinkers). We may go on to construct a theory connecting them causally with chemical effects in the retina; and we can imagine an experiment in which A recognizes the chemical effects by direct observation in B's retina whenever B recognizes an after-image. Regular correspondence reinforces both the theory and the attribution of objectivity to the after-image.

It seems clear that the conditions for the assignment of objectivity rest on a principle of induction, in the broadest sense of theoretical reliance on the persistence of regularities over time in the forms of "real" phenomena - an important aspect of empirical theory being the recognition and consistent expression (or tacit understanding) of these regularities. This is not the crude "inductivism" castigated by Popper, which holds that theory merely generalises from past observation: it is, on the contrary, implicit in his own "hypothetico-deductive" model of scientific theory, since all empirical hypotheses from which deductions may be derived, in such a way as to predict future observations, presuppose such regularities. Typically they involve hypostasis of unperceived structures which are typically supposed just as objective, or "real" for the theory as the phenomenal evidence on which they are recognized. The relevance of these considerations for this study is that contexts of measurement are constructed - physically as well as theoretically - to maximise assignments of objectivity to particular elements of the phenomenal evidence. The results of measured recognition are generally treated as the final arbiters for the objectivity of unperceived, hypostasized elements of the associated substantive (non-recognitive) theory.

Where conflicts occur between perceptual readings and inductive commitments respecting the objectivity of particular idents or characters, we seldom have difficulty in making the distinction, and preserving the commitments. Where doubt is unresolved, appeal is often made to minimal characterization, of the kind associated in epistemological writings with special entities, called sense data, qualia, etc. (but having no special status for C-theory). "Well, it may not be a real tomato, but at least I see a round red thing in my field of vision." Training in art or psychology may help, but this is no reason to suppose that untrained people cannot understand such things (as seems to be suggested, e.g., by Quine (1970, p. 1) and Quinton (in Warnock, 1967, p. 68)). Most of the time, in concrete contexts, the distinction between "appearance" and "reality" is, naturally and properly, neglected. The ordinary language of perception is forged in the experience of this general neglect; it has to use special devices - like those of the sentence about the tomato - when the need arises to point up the distinction, and it does not always do it very well. We talk of "seeing a house" and not of "recognizing it on the evidence of its visual appearance"; of course, but such syntactic observations cannot be used to found theories of perception, to which they have no relevance.

Although this concept of objectivity has been introduced here in the context of Group-R-theory, and a good deal said of the role of language in reinforcing its assignment, I do not want to leave the impression that it necessarily requires such reinforcement. Our ordinary conduct of life depends on a rich profusion of such assignments by each one of us, made without help from our friends, and much of it unsupported by language.

The abstract assignment of objectivity to the elements of a recognitive assignment (ident and character), together with the commitment that this assignment holds for the R-theory, has equivalent force in the logic of assignment to that of held-truth in the logic of the base-language, and of its actual-language translations, where they enter into the context (see discussion p. 185, note (c)). This is a concept of <u>empirical truth</u>, which functions as logical truth in the logic of the associated empirical theory. The common acceptance of consistent systems of assignment as empirical truth leads naturally to the abstract assignment of <u>error</u> to inconsistencies between assignments, since truth can never be inconsistent.

(3) Error

The underlying principle in the analysis of error for this study is the assumption that it shows up, if at all, in the form of inconsistency in the relevant GR-theory (or individual R-theory). The concept is thus firmly relativised to particular theories, and is to be distinguished from any universal or supratheoretical notion of falsity. Its relationship to this notion will be considered in Part IV, Section 7. The distinction in the context of a particular theory between error and falsity is not the same; it will be discussed at the end of this Section.

One important aspect of error was mentioned above (Section G, note (b), p.184): inconsistencies between Readers' understandings of the references of statements in the base-language. Since these references are to assignments, commitments or negations of the GR-theory, we can take them to be ultimately reducible to systems of conjunctions or disjunctions of conditions of the form $P \in \theta_i^{\nu}(x,k)$ or $P \notin \theta_i^{\nu}(x,k)$. This reduction points to five variable factors in the theoretical understanding of the context, in terms of which error may appear: the reference function Ψ for the particular context, associating a statement with a condition of one of these forms; the elements (P) of characterization; the idents (x) supposed to be characterized thereby; the R-frames (i,k) at and for which the condition is taken to hold; and the identities (R^{ν}) of the Readers in whose R-theories it is taken to hold. These same factors, other than Ψ, represent the possible loci of errors in recognition itself, independently of language. (I shall neglect the possibility of error through misperception - say, mishearing - of an utterance; and deliberate lying or other misuse of language. We have enough trouble without these.)

I shall distinguish between 'local' error, where inconsistencies occur either within the R-theories of relatively few members of the L-group, or between these and the R-theories of most of the group in factors for which these majority theories are mutually consistent; and 'group' error, where inconsistencies occur in what are recognizably the same factors for all, or nearly all, members. (There will be borderline cases, especially in less rigorously organized contexts.) Local error is generally relatively easy to diagnose and deal with; though, where discrepancy occurs between the theory of the many and that of the few (even the solitary one) it is not always that of the few which must be 'corrected'. The most serious difficulty arises in distinguishing between error due to inconsistencies in the reading of the symbolic function Ψ, (i.e., verbal meanings), and inconsistencies in the reading of one or more of the elements of the assignment determined by (P,x,i,k) for '$P \varepsilon \theta^{\nu}_{i}(x,k)$' (i.e., perceptual recognitions). This raises fundamental questions about the relationship between language and perception (or sensation), which will be further considered shortly. Some other special cases of local error follow:

(1) Where the value of P is a positional character of the form P^{x}_{α} for some figure $\hat{\alpha}$ of the context (see above pp. 106 ff.), an ident x, otherwise consistently 'read', may be assigned to a different configuration from that to which it is assigned by the rest of the group; for instance, a hand, or a voice, assigned to the 'wrong' person.

(2) The case where inconsistency, though, often, apparently due to local differences in character-assignment on direct perceptual evidence, can best be analysed as inconsistency in the understanding of an associated theoretical commitment. For example, recognizing an apple, wrongly, as unripe because it is green. Some cases of this type can also belong to type (1). The distinction between recognition by theoretical commitment and that by direct perception is not definable in general terms; it is context-dependent, and a matter for determination under the relevant R-theory. In measurement contexts, it can be of critical importance.

(3) We have seen (above, pp. 212 ff.) that what I have called cases of 'negative assignment' are often special cases of some form of theoretical commitment (falling under head (2) above), by which the

assignment of one character precludes the assignment of some other character to the same ident. Such cases are probably the most common source of error; they can be diagnosed and solved for a Group-R-theory either in terms of correcting the assigned character, or the identification of the ident, or by the revision or abandonment of the commitment (the correction being made either locally or for the group). If, for example, a group holds that all swans are white, and that white excludes black, the discovery by a member of a bird he takes to be a swan but black leads to inconsistency in the GR-Theory: this threat of error may be averted by (a) excluding him from the group; or deciding (b) that it isn't black - just a trick of the light; (c) that it isn't a swan; (d) that some swans are, after all, black; but probably <u>not</u> (e) that some things can be both predominantly black and predominantly white - or, for that matter, both swans and crows. As we also saw on pp. 215 ff commitments to sets of characters as sets of values of a valuative character are rich sources of recognitive negation, since assignment of one such value precludes assignment of any other value to the same ident at the same frame. Black/white and swan/crow are both pairs of mutually exclusive values under this analysis. Measurement systems are constructed with the aim of eliminating recognitive inconsistencies of this type for values of the valuative characters chosen in context; an aim assisted, as we shall see, by fixing levels of 'tolerance' below which inconsistencies are neglected for the context.

(4) Inconsistencies in the reading of ν - i.e., in the identification of the Reader R^{ν} whose statement may determine a change of assignment - should in general be safely negligible; in view of the commitment in definition G1(i)(b) (p. 176) that what holds for one Reader holds for all of the group, taken with G3(iii) (p. 181), which says that identical statements from different Readers refer to identical assignments. Apparent exceptions will occur where a speaker is involved in the reference of his own statement. "My foot hurts" is a simple example; more sophisticated ones will occur in scientific or technical contexts where conditions affecting an observer are factors in his observation report. For C-theory we must see the utterer in such a case as filling two distinct roles: (a) as member of L-group and source of utterance, and (b) as ident of the Group-R-theory to which characters - including configurative characters determining relations - are assigned for the theory (i.e., R^{ν} = 'x' in '$P \varepsilon \theta_i^{\nu}(x,k)$'). If these roles are clearly distinguished, we see that only the second is involved in the reference of the statement - loosely, in what the statement says. Errors (inconsistencies) in the understanding of this reference can then be

understood, once more, independently of the identity of the utterer. If John says, "My foot hurts", and someone else, thinking Peter spoke, reports "Peter's foot hurts", any third person believing this report will make the same error (with respect to the group theory) as the second in thinking that Peter's foot hurts, and not John's. The source of the error and its correction will best be determined by agreeing about whose foot hurts - i.e., the correct assignment to John as ident - and we can again safely neglect errors associated with the identity of an utterer.

(5) Not much need be said - nor could be, in general terms - about the tendency of error to be revealed as a result of increasing the coverage of a Group-R-theory in time (by the addition of frames) scope (by the addition of idents or characters), or by the addition of members to the L-group, as the context develops. Such an <u>expansion of context</u> increases in principle the opportunities for inconsistency (not least in the understanding of the reference function), to be read as 'error' unless and until 'corrected' in the manner illustrated above. In general, the more a context of theory is consistently expanded and the threat of error met, with or without correction or revision, the greater its claim to objectivity. Discussion of the relationship of this claim to that of empirical truth must, once again, be postponed to the end of the study.

(6) Two relatively straightforward types of error-source can now be considered as cases of group inconsistency in the understanding of the symbolic function I: namely, (a) redundancy, where more than one form of statement may be taken to refer to the same assignment; and (b) ambiguity, where the same form of statement may be taken to refer to more than one assignment.

(a) Redundancy. If, for simplicity, we suppose that we have clearly distinguished sets of expressions in the ideal base-language of C-theory, one referring to idents, another to characters, others to utterance or assignment frames, and so on, the existence of redundant expressions in any one of these sets should not give rise to error, so long as all members of the group understand them as referring in the same ways. Inconsistencies in such understandings will emerge in the same ways as would local errors for Ψ, as discussed above; in terms of conflicts about the readings of statements in relation to specific assignments. In non-ideal languages, however, the position is less clear - frequently because, for example, expressions taken to refer to the same entity may carry confused or conflicting associations with past characterizations. The understanding of two expressions as referring to the same entity amounts to a theoretical commitment; this would be the basis of a C-theoretical analysis of the notion of 'genidentity' as used by Carnap.

If either expression is limited in reference to the entity as characterized in a certain way, a further commitment is required, that this characterization obtains at all relevant frames of the context. The recognised Fregean test of referential identity of this kind is substitutability of the relevant expressions; and a favourite example is that of the morning and evening stars, identified with the planet Venus. The original theoretical commitment on which this identity is based is credited to an ancient Babylonian astronomer. Before him, no single entity corresponding to our planet Venus existed for any known theory. After him, the three expressions are commonly thought to have identical reference. But now consider the statement: "I can see the evening star from my window" - perhaps in a letter. Substitution of either alternative expression destroys the meaning; 'morning star' would lead to the implication of a falsehood, that the window faces East. We can rescue the essential thought (at the cost of romance) by analysing the sentence as equivalent to: "I can see the planet Venus in its evening phase ..."; thus revealing clearly the factor of frame-dependent characterization.

What we have to say, for C-theory, is that if two expressions are to refer to the same ident, they must be tied by the common understanding of Ψ, and the values of θ for the group theory, to the same sets of characters for each frame of the context in which it occurs.

On the face of it we should be on safer ground with redundant expressions for characters. Here, the favourite example is "bachelor/unmarried man" (not in our sense strictly a case of recognitive character, but this does not affect the argument). Again, care is needed. If we follow the O.E.D. - and, I think, common usage - by glossing "bachelor" as "unmarried man (<u>of marriageable age</u>)" (my emphasis), we might be cruelly misleading a young girl by describing an elderly widower, though certainly unmarried, as a bachelor. It is not, I think, pure chance that I am driven to use an old-fashioned and well-worn example. Frege's interest in the substitutability of expressions derived from his wish to assimilate expressive language with the language of mathematics, for which this facility is a major requirement. Ordinary expressive language, however, is evolved for use in day-to-day communication, where the last thing we want is that our words should be replaceable by others without loss of truth. Hence, although <u>partial</u> redundancies, in the sense of overlapping references, are common, perfectly substitutable synonyms are rare.

<u>(2) Ambiguity</u>. Where the same utterance is taken, say, by different Readers to refer to more than one assignment, error will only show up if the assignments or their associated commitments are in some way incompatible, so that a theoretical inconsistency is revealed. It will be assumed that the moment such an inconsistency <u>is</u> revealed, it can be corrected by revision of the function Ψ and the base language L^G so as to provide different expressions for

the different recognized assignments. For example, if two people in the group are called "John", we can distinguish between them in reference by the addition of a further name or a description so as not to understand - in conflict with the evidence - that the "wrong" John's foot is hurting. Similarly, if a particular "house" is described as "high", we may have to sharpen reference to the character, by specifying either the sense "lofty" or the sense "built on a hill", if inconsistency is to be avoided.

Such errors, then, are relatively trivial; they must be clearly distinguished from what we may call <u>concealed</u> ambiguity, where identical expressions are used to refer to what are taken <u>wrongly, by the whole L-group</u>, to be identical assignments. For example, suppose two different people were recognized as one, called "Lee Harvey Oswald", leading to potential inconsistencies in the history of J.F. Kennedy's assassination; or suppose the character "fungous" was wrongly taken by naturalists to be a value of the character "vegetable", leading again to potential inconsistencies in their theory; we may properly distinguish these as cases of <u>ambiguity of recognition</u>, leading to a real but unrecognized ambiguity of reference. Such cases are trickier than simple ambiguity of reference, since they reveal themselves only when potential inconsistencies are actualized, and diagnosis is then often problematic. This is because the consequent decisions involve choices between theoretical commitments, either at the level of basic recognitive assignments, or at the level of the dependence of some types of assignment on others, which may involve deeply ingrained beliefs.

Two types of recognitive ambiguity can be usefully distinguished:

A. Cases where a possible solution lies in the decision to replace a single ident by two (or more) similar but distinct idents, to which otherwise inconsistent characters can be consistently assigned (such, perhaps, as the hypothesis of the two Oswalds). So, we retain our commitment to the inconsistency of the characterizations at the cost only of the commitment to the identity of the original single ident. This involves, of course, the construction within the theory of two distinct, consistent and plausible 'life histories' for the newly recognized idents, where only one existed before (David Wiggins' account of identity in terms of 'life histories' will be discussed below, in Part IV, Section 6).

Cases where recognitive ambiguity reveals itself for a single character-assignment to a single ident are rare. Recognitive ambiguity for characters is therefore best considered as a special case under the second head.

B. Cases with respect to <u>composites</u> (A 9, p. 54): e.g., Γ-sets (BD1, 2, p. 72 ; or composed configurations (see esp. p. 71 above). Recognitive ambiguity for any such definitive characters may lead to inconsistencies in the recognitions of enclosures of supposed <u>components</u> in such composites until the error in the theory is corrected. Some examples: (a) Discovery that what was thought to be a single bird species of the genus phylloscopus, with two kinds of song, showed minor but systematic anatomical variations, corresponding to the songs; corrected by recognizing two distinct species (C-theoretical composites) names p. trochilus and p. collybita (willow warbler and chiffchaff); (b) discovery that fungi exhibit certain features, and lack others, that are respectively absent and present in plants generally; corrigible either by recognizing fungi as a sub-order, or as a distinct order, and modifying recognition of the order 'plants' accordingly (still, I believe, controversial); (c) discovery that whales, once thought to be fish, exhibit features common to mammals, and lack many common to fish; corrected by recognizing whales as belonging to (enclosed in) the class (composite) mammals, and not fish; (d) early-morning discovery that what I took to be my left shoe pinches my left foot; corrected by revising my recognition of its configuration to that of a right shoe.

It is claimed that the above account of error sources, if not exhaustive, presents the major types encountered in relation to recognition of, and reference to, idents and characters in C-theoretical terms. It emerges that, at least in those types of cases considered, group error shows up, if at all, as a conflict between locally inconsistent theoretical commitments, some of which the group is usually unwilling to abandon - regarding them as parts of the common structure of 'knowledge'. A special class among such conflicts are those which call in question the common understanding of the structure of the reference function Φ.

Ambiguity of reference, however, is sometimes adopted as a deliberate strategy, for greater flexibility or scope in speaking of classes of idents whose distinctive characterizations (marking each as unique) carry no critical differences of commitment for the GR-theory. Typically, though not always, this strategy is used where these distinctive characterizations are, for practical reasons, inaccessible to recognition in context. Because of the dangers of confusion if the resulting logical structure of the context is not strictly kept in mind, the strategy will now be carefully examined. It will be found that the theoretical consequences of this analysis are important in many cases of measurement and probability assessment.

Deliberate Ambiguity, Synonymous Sets and Tolerance of Error

Deliberate ambiguity involves the neglect of particular kinds of differences in characterization between idents, as part of what might be called R-theoretical strategy for the context; on one or more of the following grounds, of which the first and last are crucial:

(1) that these kinds of differences are irrelevant, i.e.,

they do not affect (through any form of theoretical commitment) factors of recognition which are important to the group in context;

(2) that attention to them may unduly complicate or confuse the group's understanding, or otherwise inhibit the development of the group theory;

(3) that they are in practice either partially or totally unrecognizable on the available phenomenal evidence in context.

A universal form of such neglect in concrete contexts is that which is operated in the choice of atoms, atomizing characters, etc. for the context (C2, 8, 9, p. 80-1). This may be read as a commitment to neglect potential inconsistencies, or <u>tolerate error</u>, for any differences of characterization involving proper enclosures of these atoms. But <u>ambiguity of reference</u> need not accompany such tolerance, since in many contexts reference is only made to idents whose characterizations are unique, as C-theory requires, so that one-to-one correspondence is secured. In other contexts, however, one or more characterized sets of atomic idents ('<u>atom sets</u>') may be so recognized that each set encloses more than one member, but every member of each such set plays what is seen as an exactly equivalent role in the Group-R-theory of the context. The members of such an atom set are taken as <u>indistinguishable</u> for the context, and all character-differences between them neglected. Such neglect is expressed in a deliberate ambiguity of reference, the same term or expression of the base-language being used to refer indiscriminately to each and every member of the set; which I shall therefore call a '<u>synonymous set</u>'.

In many scientific contexts, especially those of physics, this type of indistinguishability is interpreted as identity, according to what is known as 'Leibniz' Law'. I think this term is misleading. Leibniz, as I understand him, insisted that <u>each one</u> of his monads had a unique set of properties. In saying that if monad A and monad B were indistinguishable, they were identical, he was thinking in terms of an <u>ideal</u> distinction of properties such that any one monad is indistinguishable from itself and distinguishable from every other: <u>not</u> a form of practical distinction in which the property-differences of distinct monads can be neglected. My analysis parallels Leibniz in insisting that each recognized ident is assigned, <u>in principle</u>, a unique set of characters. Since, however, this is a theory of practical recognition, the words 'in principle' are there to signal an admission that, in many contexts, the principle is wholly or partly neglected for members of synonymous sets satisfying requirements (1) to (3) above.

It is nevertheless worth noting that the recognition that any synonymous set has more than one member involves the underlying acceptance of this quasi-Leibnizian principle. A physicist may treat all molecules of a given volume of hydrogen as "identical": if he now wishes to estimate their numbers in given conditions of pressure and temperature, he would, to be consistent, have to answer "one". In such a case, the only assignable character-differences will be positional, involving ideal and unperceivable (not measurable) space-time or mass-energy relations between the hypothetical multiplicity of atoms. We can best understand this for C-theory as the construction of an ideal, extrapolated frame-sequence structure, for which statistically-expressed variations in values of chosen valuative characters (say, of position or momentum) are used such that the estimated number of distinct atoms <u>could</u>, <u>if</u> the structure were at any frame practically recognizable, be consistently distinguished. Nothing in this account militates against the deliberately ambiguous use of synonymous reference to members or subsets of the atom set, for the purposes of free generalisation and abstraction.

At a simpler level, standard units of a measurement system often appear in context as members of synonymous sets - not all of which are necessarily atom sets. All "centimetres" of a context may be treated as mathematically "identical" (though recognitively "equivalent"). But the recognition or extrapolation of an interval of "100 centimetres" which is also a member of the distinct synonymous set of "metres" involves a commitment to their distinguishability in principle, if they are to be countable; and in many instances we may need in practice to distinguish a particular interval, uniquely characterized, as "a centimetre".

At the same time tolerance of error, such that character-differences between members of a synonymous set which is also an atom set are systematically neglected, is a necessary pre-condition of the use of synonymous reference, to any such set or any set composed of its members. In measurement contexts, this tolerance can be given numerical expression; an aspect which will be given further attention in due course.

Error and Falsity

A question seems to arise over the distinction between error and falsity, each as opposed to truth. Error is seen here not as the contrary of truth, but rather as a failure to determine truth within the theory, in the sense of a consistent reading of any particular utterance of the language of the context by all Readers as referring to an assignment which holds for the Group-R-theory. As we have seen, failure may occur as inconsistency in the group theory of the reference function Φ, or through defects in some or all of the component R-theories with respect to the assignment to which the utterance is taken to refer. Defects may arise either from lack of foundation (in recognition, or by implication under theoretical commitment), for an assignment taken as possible for the theory; or from the recognition or implication of contradictory assignments, of which no more than one can hold for the theory, and none holds unequivocally. In such cases no determinate value of truth or falsity is available in the theory.

We are left with various alternatives, of which the following are the most clearly defined: to reject the theory, to neglect one or more of the relevant possible assignments, or to construct a probabilistic theory assigning 'weights' to each possible assignment. The last alternative is considered in Part III in a context of measurement.

Falsity under a theory is determined by what the theory prohibits, by virtue of implication under definite commitment from unequivocal assignment. (It may, of course, apply to negative assignments, pp. 212 ff.) The question of the relation between truth or falsity under empirical theory, and logical - as it were, absolute - truth or falsity, will be left for discussion at the end of the study.

A STUDY IN EMPIRICAL KNOWLEDGE: THE PRECONDITIONS AND STRUCTURE OF MEASUREMENT

by

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III. MEASUREMENT IN CONTEXTUAL THEORY

K. The Structural Conditions of Measurement

1. Comparative Structure and the Notion of Quantity

A stage was reached in the development of C-theory where, within the R-frame, a comparative structure is defined in which sets of idents can be ordered with respect to sets of characters, namely value-ranges of a V-character, Q, by virtue of the recognition of a comparison figure, $\hat{\delta}$, associated in the comparative pair $\langle Q,\delta \rangle$ (D8-12, pp. 123-134). The structure so defined was not affected in any way by the subsequent definitions under the heads of R-sequence or Group-R-theory. Determinate ordering under the relation $\overset{\delta}{\underset{Q}{>}}$ was shown to be dependent on the selection or construction of a standard set (D11) within the comparative structure; with which an approximate set (D12) might be associated. These definitions were constructed without essential use of numbers. It was pointed out, however, that arbitrary sequences of numbers - most simply, a sequence of the first n positive integers or "natural numbers", for suitable n - could readily be placed in functional relation onto a standard set so defined, such that the ordering under $\overset{\delta}{\underset{Q}{>}}$ was homomorphic with that under $>$ on the numerical sequence. Further, the same numerical sequence could be associated with an approximate set.defined with respect to that standard set, such that a unique subset (or cluster) of the approximate set is associated with each member of the standard set, called the k-cluster, where k is the number assigned to that member, under the arbitrary function. (The sequence from 1 to n was in fact used, for convenience only, in the definition of these terms.)

However, doubt was expressed whether the construction, without further, of such an arbitrary function from a numerical sequence could in any sense be called measurement, since any conventional order (say, an alphabet) could be used in the same way, where no special property of numbers as such is involved. The same kind of doubt arises whether the associated V-character in such a case could properly be called a 'quantity'. Before further formal definition of theoretical structures, therefore, more will be said in an introductory sense about these questions.

Consider a row of books on a shelf, wrapped in plain covers numbered on their spines. So long as the numbers are all different, they can be used for identification. Though not essential, it will be more convenient if the number of each book is also given in some index which gives particulars of its author, title and subject; if the index observes one or more orderings, say, alphabetical; and if the books are shelved in numerical order from the left. It will now be the case that the order of the distances of the books from the left end of the shelf will correspond exactly with the order of numbers on their spines. It will also correspond with the alphabetical order chosen for the index, provided the numbers in the index follow that order. It does not seem reasonable, without further, to call the assignment of numbers in this case a form of measurement; although it appears to satisfy the conditions demanded by some writers, especially in the Suppes tradition, for what they call 'ordinal measurement'. Nor, I suggest, does it seem reasonable in general to regard the numbers as related in any way to distance-from-the-left as a quantity. (A rather odd exception would be the case where all the books were uniform - say, copies of the same book. We could then say that the left edge of the nth book would be n-1 book-widths from the left; and, if the books were numbered with successive integers from 1, we would have constructed a primitive and idiosyncratic measuring system for length or distance. Such systems will be briefly mentioned in later analysis.)

Supposing the books were not uniform, they could be re-ordered for height, so that the higher the book, the further right it would be shelved. The comparison figure here would be that recognized when two books are placed vertically side-by-side on the shelf, the relation $\underset{Q}{\overset{\delta}{>}}$ (where $\hat{\delta}$ is the figure and Q = "height") being assigned to any pair of which the first extends vertically beyond the second. (I am assuming that we shall not here distinguish any special subset of the books as standard, so that if ordering is to be unequivocal we must treat the whole set as standard, recognizing, if necessary, some least difference - however intuitively judged - below which pairs of books are assigned "the same height", and no books of intermediate heights recognized.) If the books are now renumbered in height order, books of the same height must be given the same number. Could we now choose the numbers so that their assignment could be called measurement, and so that they can be seen as related to height as a quantity? There appears to be no logical or theoretical difference, for this purpose, between this case and the former, where the comparison figure is one elaborated from the left-right figure discussed on p.137 (note (c)), and the V-character is distance-from-the-left. The two kinds of ordering could be combined, so long as our "unit" of left-right ordering becomes not the individual book, but the set of books-of-the-same-height. What function, then, if any, do the numbers perform? Well, if the books were taken down and disordered for any reason, they could be replaced in height order by reference only to the numbers and the left-right order, without repeating the height comparison. If the numbering-for-height now seems to have mysteriously taken on some of the quality of measurement of a quantity, I suggest it is because our somewhat arbitrary procedure has provided us with a crude ad-hoc <u>theory</u> by which we can extrapolate from the numerical ordering to height ordering. But it is a weak theory, in the sense of being poor in logical consequences, even for the restricted context. It is difficult to think of any use which could be made of such a theory.

Suppose we try to make it more interesting by adding a further theoretical commitment. Let us say that the left-hand support of the shelf is weak, and we are using height as a rough indicator of the weight of the books. On this basis, the arrangement will ensure that the centre of gravity of the books will be (just about) as far to the right as possible. But, if we really care, and we can get hold of some scales, we will use these to check the weight-order of the books (the appropriate comparison figure will be discussed later, under weight or mass measurement). We can expect a fairly close correlation between the two orders; but let us say that it is not exact, and a few books now turn up in different places. We re-number for weight. The correlation now becomes irrelevant. We will choose between optimum weight distribution, and the aesthetic effect of a smooth curve along the top edges of the books. We will, in effect, have distinguished between height and weight as two different quantities assignable to the books, by a procedure which could be called measurement. But if the orders had happened to coincide - as they well might - would we then have identified the two quantities? In this case, surely not. Nor can we say that the difference, in spite of identical ordering, depends in any simple way on the comparison figure, or its associated procedure, in operationalist terms (Introduction, p. 16f). We shall see that some ways of comparing weights are carried out, in fact, by comparing lengths (or heights) which, under the associated theory, do correctly indicate weights. In general, we shall find that the same comparison figure may be paired with different quantities (V-characters) in different regions of a context; and, conversely, the same quantity with different comparison figures. What determines the pairings is the associated (substantive, or non-recognitive) theory of the context in each case.

Consider now one further project for the books: an attempt to order them for intrinsic interest. As a first effort, we may place them in order of frequency of use, using past records in some associated context. As an indicator of interest, this frequency is quite plausible but subject to odd errors. The set book for some important exam may be frequently used, but thoroughly boring. We may try to check our results by asking a sample of readers to score the books on a scale, say, 0 to 5: not read, very boring, rather boring, no clear opinion, quite interesting, very interesting. This, again, is subject to error. Respondents may wish to impress the surveyor, or themselves, with their intellectuality; or they may treat all surveys as a joke; people have different ideas about interest anyway. But, if the two orderings turn out to be fairly closely correlated, there is a temptation to think that one or other - or some statistical amalgam of both - is thereby reinforced as a true measure of interest. But looking back to the height-weight correlation in the example above, we see at once that such confidence is illusory. If there _is_ such a thing as a true order of intrinsic interest, we have no reason to suppose on the evidence of our investigations _alone_ that it is nearer to either of the observed orders, or to a statistical average of both, or wide of either. We could, of course, improve the scoring system of the survey, introducing further distinctions and weighting factors. These would reflect our own concerns, as investigators, with various aspects of our idea of "interest", and what we intended to do with the results. Whether frequency of use would now enter into our calculations, and how much, would also depend on our concerns in context. I do not intend this as hostile criticism of such procedures, but as an introductory indication that what distinguishes these from physical measurement systems is not so much the accuracy of their observations (frequency counting and survey scoring yield unequivocal _numbers_) as the relative importance and quality of _non-recognitive theory_ in the measurement context. Further analysis will aim to give more precise meaning to this general comment.

Meanwhile, of course, for height, weight, and many other physical quantities we can point to a further stage in the development of _recognitive_ theory, which associates numbers with quantities in such a way that additive operations on numbers are taken to correspond, more or less precisely, with operations of composition on idents compared for these quantities. We shall show that this amounts to a major step in the development of a structure of logical consequence for the R-theory.

2. Formal Definitions of Metric Structures

In so far as the ordering of sets of numbers in functional correspondence with that of values of a V-character for R-theory can be termed 'ordinal measurement', the necessary structures are adequately defined above in terms of comparative structures (D8-10, pp. 123ff). As a first step to the definition of structures which are unequivocally metric, we define a composition operator, as follows:

K1. The composition operator o is an operator on the product $S \times S$, given, for all a,b in S such that $(\{a,b\}) \in S^{()}$, by:

$a \circ b = x \text{ iff } -\eta/a,b \wedge x = (\{a,b\})$

i.e., a and b exclude one another, and x bounds their composite. Although the operator is defined, in principle, for all composed pairs of idents, we shall only be concerned with its recognition in association with a metric structure, as now defined.

K2. $\langle G,K \rangle$ is a <u>metric structure</u> for the comparative pair $\langle Q,\delta \rangle$ iff:

(i) G is a standard set for $\langle Q,\delta \rangle$ and $K = \{P_1,\ldots,P_n\}$ such that:

$(\forall a)(a \in G \wedge P_1/a \rightarrow \neg(\exists b)(b \in G \wedge \delta/a,b)$, and

(ii) $(\forall i,j,k,a,b,x)((a,b,x\ G \wedge P_i/a \wedge P_j/b \wedge P_k/x \wedge a \circ b = x) \rightarrow i + j = k)$

(For 'comparative pair', see D9, p. 124 and note (b), p. 126 and for 'standard set', D11, p. 129. An ident a such that P_1/a satisfying (i) is called a <u>minimal component</u> (<u>m-component</u>) of G. Since each pair in G is necessarily composed under (ii), G itself will be composed; proof is omitted.)

K3. G' is a <u>data set</u> for $\langle G,K \rangle$ with respect to $\langle Q,\delta \rangle$ iff $\langle G,K \rangle$ is a metric structure for $\langle Q,\delta \rangle$, G' is an approximate set with respect to G for $\langle Q,\delta \rangle$ and:

$(\forall a,b,c)(a,b \in G' \wedge c \in G \rightarrow \neg(\tilde{\delta}/a,c \wedge \tilde{\delta}/b,c \wedge \delta/a,b))$

(For 'approximate set', see D12, p. 134). The final condition rules out the recognition of any determinate δ-relation between members of the same k-cluster as defined p. 135.

<u>Notes:</u>

(a) Evidently the demands made in terms of practical realisation by the structure of K2 are much more stringent than those made by the non-metric standard structure (D11, p. 129). It is assumed that in the majority of cases <u>construction</u> rather than selection of natural elements will be needed to satisfy the conditions; i.e. the physical assembly and shaping of materials, the making of marks, etc. Cases of selection of regular elements, either natural or constructed for some other, non-metric purpose, do occur, and these will be mentioned in the course of discussion of particular types of measurement context.

(b) The condition in K2(i) requiring that the elements of the metric sequence of values K shall correspond with the first n natural numbers for some n (necessarily finite, since the universal set C from which K is drawn is finite), and that, by (ii), these are required to have the normal additive property, will seem excessive if these elements are thought of as necessarily identical with the values of some standard unit. The intended commitment is the much weaker one that every system of measurement requires the provision of some standard structure the assigned values of whose elements can be placed in functional correspondence with such a sequence of numbers. The choice of function (which will be discussed in relation to particular types of context) allows such a standard apparatus to be used with an indefinite variety of theoretical interpretive structures. Again, the relation of what is chosen as a standard "unit" to the least recognized interval (characterized here by P_1) is a logically arbitrary practical decision for the particular context. Typically, a conventional unit is chosen, and the least interval expressed as a rational (decimal) division of that unit, or decimal multiple, to a chosen number of "significant figures".

At the same time, our commitment to a unique function from the standard value range onto a set of integers appears markedly stronger than that adopted by KLST for their weakest axiomatization of an additive empirical structure ('Archimedean ordered semigroup', definition 2, p. 44). This effectively posits a partial function into the real numbers, together with an 'Archimedean' condition, under which (roughly speaking) no element of the structure is 'greater than' a finite multiple of any other such element. However, some bold epistemological assumptions are implicit, both in the notion of the empirical ordering relation 'not greater than', and in the quasi-additive notion of 'concatenation' on which an effective "multiplication" of 'copies' of empirical elements is inductively defined (in the sense of mathematical induction). Both are frankly modelled on corresponding arithmetical properties of numbers, without serious consideration of the question whether, and if so under what theoretical conditions, their instances could be recognized for actual empirical structures. This question is no part of foundational analysis in the Suppes tradition.

More will be said about these aspects shortly. From the point of view of this study, KLST's assumptions are far too strong as the basis of a generalized analysis of the field of relations with respect to any concrete quantity - even, we shall see, one as fundamental as "length". By adopting stronger conditions for a standard structure, and much weaker ones for associated data (not, for example, involving "multiplication" of data), I shall try to show how the demands of rigour and flexibility in mathematical description can be reconciled both with each other, and with the limitations of actual recognition.

(c) The appearance of the exclusive '-η/a,b' in the definition of the composition operator o (K1) is necessitated by the use of the non-exclusive value-system V[$\mathcal{C}$',Q] (D7,8, pp. 120-3) for the basic comparative structure, which is not further restricted for the general case of a metric structure (K2). Only in a few special cases (of which mass or weight measurement against standard objects in an equal-arm balance is the principal) are we concerned only with comparisons between discrete objects. In spatial interval comparison - whether of length or angle, or as an indicator of some other quantity - comparisons are frequently made between two idents, one of which is enclosed in the other.

I must now go on to show how, in the terms of C-theory, it is proposed to analyse the conditions for the recognition of empirical structures, satisfying the commitments of metric and data structures (K2,3), in concrete contexts.

Since the composition of spatially bounded entities (rather than any operation on the values themselves) emerges as the key operation for measurement, we might expect to be landed with the conclusion that all measurement contexts are necessarily reducible to terms of spatial measurement. I suppose the truth to be fortunately less crude than this; it has to do with the empirical theory behind the choice of interpretive function in each context, mentioned in note (b), p. 252 f, and still to be left for later. Meanwhile I merely point to the central role in fact played by spatial concepts in the vast majority of measurement contexts, and the often remarked tendency to "spatialise" the concepts of non-spatial types of measurable quantity. At all events, we must start by dealing with some of the points which arise with regard to spatial measurement itself, accepting, for the moment, its central role.

3. Complementarity, Continuity and Contiguity in Recognitive Structures

If any structure of spatial composition is to yield homomorphism with arithmetical addition, it must at least exhibit complementarity - in the sense that, given positive numbers x,y,z such that $x + y = z$, y is the complement of x in z. But the attempt in C-theory to reflect the actual structure of perceptual recognition has led us to abandon the general principle of spatial complementarity for idents. This has been taken to reflect the intuition that every actual recognitive context is so constructed as to leave out a number of neglected spaces, about which the R-theory has nothing to say. In consequence, the language of C-theory as so far formulated has no general means of describing a "full" space, in which every proper enclosure of a given ident has a spatial (rather than merely set-theoretical) complement in that ident. It is no good, for instance, writing of some x that:

$$(\forall a)(\bar{\eta}/a,x \rightarrow (\exists b)(\bar{\eta}/b,x \wedge (\forall c)(\eta/c,x \wedge -\eta/c,a \leftrightarrow \eta/c,b))) \quad \text{(see C2, p.80)}$$

This says no more than that for each recognized proper enclosure a of x there is another proper enclosure b, outside a, which encloses all recognized proper enclosures of x outside a. It is satisfied, e.g., by

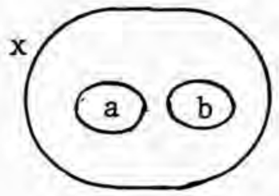

Diagram 3

If we recognize the composite ($\{a,b,x\}$) - e.g., as an enclosure configuration - the pairs $\{a,b\}, \{x\}$; $\{a\},\{b,x\}$; $\{a,x\},\{b\}$ are set-theoretically complementary, and may be used to generate corresponding pairs of composites; but they are not spatially complementary, nor are the bound-idents of the corresponding composites. So there is no evident basis here for a generalizable homomorphism from operations of spatial composition into addition on the integers, corresponding to simple addition of the numbers of members of the associated sets.

If we now seek to overcome this difficulty by revising the language of C-theory so as to express a general notion of spatial complementation, we shall find that this results in complication, without serving a useful purpose. To show this, I shall introduce the necessary terms temporarily, numbering the definitions distinctively to signal that they form no part of C-theory proper.

What we will need is a means of referring to at least some uncharacterized spaces, assuming for the moment that they can be recognized without the assignment of recognized characters. One scheme of definition is as follows:

X1. In recognizing any ident a as properly enclosed in an ident x, (i.e. $\bar{\eta}/a,x$), we necessarily recognize its boundary (whose dimensionality depends on that of the space defined by the field of attention, having one spatial dimension less than that space). That boundary divides the space in x into two parts, one of which is a itself, recognized in terms of the unique set of characters f(a). The other space is not necessarily recognizable in terms of any characters in $\mathcal{C}$, but is determined by recognition of the boundaries of x and a, and its exclusion by a. I shall call it the antident of a in x, written: $\bar{a}(x)$.

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Every ident has an antident in the maximal ident m , which will be written just $\bar{x}$ for each x. Antidents, as such, are not recognized spaces; but they may enclose, or even coincide with, recognized idents, where the necessary characters are assigned.

X2. We define an operation '□' on bounded spaces, recognized or not, which forms from any two such spaces which share some part of a recognized boundary a single space, determined as enclosed by all boundaries of the two original spaces except the shared region of boundary. There are several possible cases, but the interesting case at this point is the formation of the enclosing ident from any proper enclosure and its relative antident (spatial complement), given by:

$(\forall a,x)(\bar{\eta}/a,x \rightarrow a \,\square\, \bar{a}(x) = x);$

which follows from X1 and X2. In particular:

$(\forall x,y)(x \,\square\, \bar{x} = y \,\square\, \bar{y} = m\ ).$

X3. We now suppose that in specific instances it is possible to identify particular idents with the antidents of other idents, as having identical boundaries, so that:

$\bar{\eta}/a,x \wedge (\bar{a}(x) = b) \leftrightarrow a \,\square\, b = x.$

Looking at the equivalence proposed in X3, we seem to have successfully described a case where the space occupied by x is completely "filled" (or, "exhausted") by recognized idents a and b. If we could construct more complex equivalences yielding identities of the kind:

$x = a_1 \square\, a_2 \square \ldots \square\, a_n$

we would appear to be well on the way to describing a spatially "full" ident such that for any $\bar{\eta}/a_k,x$, we could identify $\bar{a}_k(x)$ with some combination formulated by repeated use of □ with sequences of idents a_1 to a_{k-1}, a_{k+1} to a_n. But, bearing in mind that antidents remain unrecognized unless and until they are identified with fully characterized idents, and that in the general case idents may be scattered discontinuously through neglected spaces, the new terminology does not, in fact, make such a construction notably easier. Even with the simple case of diagram 3 (p. 256), we can "fill" x with a □ $\bar{a}(x)$ <u>or</u> b □ $\bar{b}(x)$; but, if we want to express the total space in terms of both a,b and antidents, we shall have to write, say, a □ b □ $\bar{b}(\bar{a}(x))$ - and, unless we can identify this last term with some coherently characterized ident, we shall still not have filled the space with idents. As the number of enclosed idents increases, such a construction becomes rapidly less manageable in recognitive terms.

Can we do better with the original, unextended language of C-theory? The notion of an antident was defined in terms of <u>boundaries</u> and the operation & in terms of <u>shared boundaries</u> which were said to be eliminated by the operation. The space of an ident could be filled by a properly enclosed ident and its relative antident because the enclosed ident shared the <u>whole</u> of its boundary with the enclosing ident - and so the whole boundary was eliminated by the □-operation, leaving us with the complete enclosing space. In the original unextended language, the notion of shared boundary is expressed by the relation of contiguity τ; but this provides no ready means of expressing the condition in which one ident shares a whole boundary, or any specified part of a boundary, with one or more others.

There is, however, a structure already defined within the theory which does exhibit coincidence of boundaries under τ, and hence a correspondence between set-theoretical and spatial complementation: the <u>Γ-set</u> structure. Recall that the necessary coincidence of boundaries is derived from the common <u>characterization</u> which distinguishes, from all other elements of the context, each and every member of a Γ-set (including the bound-ident), and its composite.

What we seek is therefore a system of Γ-sets associated with a V-character so that, in any Γ-set $\mathcal{U}$ of the system, where one of its values picks out a subset $\mathcal{U}'$ of $\mathcal{U}$, its complement $\mathcal{U}''$ in $\mathcal{U}$ is also assigned a value. These values are to be associated with numbers so that the number associated with each composite is the sum of the numbers associated with those of any subset and its complement. There is one, and apparently only one, simple general condition under which this aim is achieved; namely that the numbers associated with the values of the sets $\mathcal{U}, \mathcal{U}', \mathcal{U}''$ are precisely the numbers of members of these sets.

These values are, however, primarily determined with respect to a comparison figure, recognized under the instantiating R-theory of the context, so that

(a) the values of <u>standard</u> elements are associated with numbers directly in the manner just set out; i.e., they are determined by the number of atomic elements of which each element is composed;

(b) it follows that these values, which typically differ as between one standard element and another, cannot belong to the common characterization (the Γ-determinant character set) of any standard set - though the common V-character (say, the quantity Q) may be understood to do so;

(c) datal values are recognized, under the comparison figure, as approximating those of the relevant standard composites;

(d) datal elements, whose values are thus determined, are distinguished from one another, and from standard elements, by characters other than the members of the Γ-determinant set which distinguishes the standard structure; and other than values of Q (otherwise no two data could have the same value).

Assuming that data are disjoint as well as distinct from standard elements, the following conditions also obtain:

(1) Comparisons between standard and datal elements are for boundary-characters, and occur at <u>external</u> boundaries of the standard composites (i.e., at the Γ-bounds of these composites; for the distinction between external and internal boundaries, see note (c), p. 76).

(2) Value-comparisons between standard elements themselves typically occur at <u>internal</u> boundaries between

components of standard composites. In the light of point (a) above, this means that internal boundaries between <u>minimal</u> standard components (characterized by P_1 under K2(i) above) are <u>constructed</u> so as to secure that the value of each such component is understood to contribute an <u>amount</u> of value corresponding to the addition of 1 to the numerical value of any composite to which it belongs (represented by the subscript k to the value P_k of the composite).

The following definition exhibits a simple structure of contiguous idents in which these conditions are fulfilled:

K4. S' is a <u>τ-series</u> (written, '$\tau(S')$') iff:

(i) $(S') \in S^{()}$, and the members of S' are ordered as a sequence $\langle a_1,\ldots,a_n\rangle$, $n > 1$, such that:

(ii) $(\forall k)(1 \leq k \leq n{-}1 \rightarrow \tau/a_k,a_{k+1})$;

(iii) $(\forall i,k)(1 \leq i,k \leq n \wedge i \neq k \rightarrow a_i \neq a_k)$; and

(iv) $(\forall i,k)(1 \leq i,k \leq n \wedge i \neq k \wedge i \neq k{+}1 \wedge k \neq i{+}1 \rightarrow \neg(\tau/a_i,a_k))$.

(We may also write: $\tau\langle a_1,\ldots,a_n\rangle$).

Note that a τ-series has the properties of a <u>counting series</u>, as defined for a progressive 2-character; the special definition being necessitated by the fact that τ is not progressive: see D4,5 and note (d), pp.104, 109. Where the same set S' is both a τ-series and a counting-series for a 2-character yielding a comparison figure for a quantity, the same numbers can be used for positions in both series.

(S') under this definition, is a "full" ident (containing no neglected spaces, and whose sub-composites exhibit spatial complementarity) if and only if S' coincides with a Γ-set. But since the Γ-set is the only form of composite so far defined as recognized under the fundamental definitions, which can generally satisfy the conditions of a τ-series, it will be assumed that each such series is also a Γ-set for some Γ-determinant set of characters in context. Nevertheless, it is not possible to lay down general conditions in terms only of τ-series which will reliably fill the whole recognized space of a context (except for the trivial and uninteresting case where the context consists solely of a single τ-series). Consider the following definition:

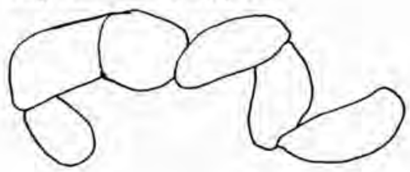
K4'. x is <u>τ-connected</u> iff

$$(\forall a,b)(\bar{\eta}/a,x \wedge \bar{\eta}/b,x \rightarrow (\exists S')(\tau(S') \wedge a,b \in S')).$$

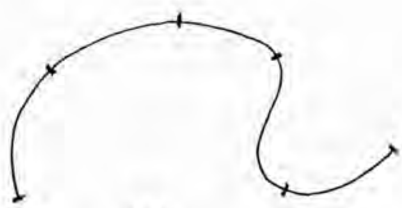
This will be satisfied by an ident x all of whose proper enclosures are connected by τ-series - making x in that sense "continuous" - but without exhausting the space in x: we may still have lacunae of neglected space, although a τ-connected ident cannot be "discontinuous" in the sense of being enclosed in a number of wholly disjoint boundaries (like swans on a pond).

Even so, such a τ-connected space does not capture the typical structure of measurement contexts. The τ-series was introduced above to answer the requirements for the additivity of standard sets in metric structures, as defined in K2 above, and is not commonly found in data structures or other elements of these contexts (though it may, of course, occur). I therefore adopt the alternative strategy of proposing that, in the general case, these requirements are confined to standard sets which are constructed or selected for the purpose in context; and go on to consider what further conditions may be necessary for their satisfaction in this restricted field. Formally, this restriction may be accomplished by defining the Γ-sets with which standard sets are identified on a suitable space-restrictive ident all of whose enclosed spaces are τ-series.

The conditions for a τ-series may be satisfied by recognized structures of any spatial dimensionality (which, to be perceptually recognizable must be $\leqslant 3$); the diagram gives a straightforward example of dimensionality 2 (Diagram 4(a)).



(a)



(b)

Diagram 4

Such a series will also satisfy the conditions for a metric standard set iff commitments of R-theory obtain according to which the values of some quantity, for any composite of members of such a series, correspond functionally with the number of minimal components of that composite. But no geometric theory is to hand according to which a τ-series of enclosed volumes or areas satisfy the necessary conditions for values of volume or area; other than euclidean systems (or their derivatives) in which amounts of volume or area are calculable from values of length (or distance) and angle. I find, therefore, that the only theories available to satisfy our requirements concern the class of special cases in which the enclosed spaces of the components of the relevant τ-series are <u>negligible</u>: i.e., what we may call 'one-dimensional' series (as in diagram 4(b)) whose <u>external</u> boundaries are the phenomenal correlatives of <u>lines</u>; while the <u>internal</u> boundaries between their components are recognized as having <u>no</u> spatial dimension, and are the phenomenal correlatives of <u>points</u>. We shall find that a wealth of theories are available whereby functional correspondences exist between the numbers of minimal components of such series, and values of physical quantities (not only length or distance). Note that such a one-dimensional series may be an open curve of any shape, though euclidean straight lines or circular arcs predominate in contexts of physical measurement; for which it will be argued that theoretical systems of the type just described form a complete basis.

Recalling that character-differences associated with assigned values of a standard metric τ-series are to be recognized at internal boundaries between its components; and that these values are functionally associated with numbers of m-components in composites separated by these boundaries; I now introduce the definitions necessary to integrate the τ-series fully with a metric structure.

K5. $\langle I,J\rangle$ is a <u>marked interval series</u> (written: $M\langle I,J\rangle$) iff:

(i) S' is a τ-series, $I = S' \cup \{x: S'' \subseteq S' \wedge x = (S'') \wedge \tau(S'')\}$;

(ii) $J \subset C$ is a set of characters (called '<u>marks</u>') such that:

(a) $(\forall A,B)(A,B\varepsilon J \wedge A \neq B \rightarrow (\exists x)(x\varepsilon I \wedge AB/x))$

(b) $(\forall x)(x\varepsilon I \rightarrow (\exists A,B)(A,B\varepsilon J \wedge AB/x \wedge A \neq B \wedge (\forall C)(C\varepsilon J \wedge C \not/ x \rightarrow (C=A \vee C=B)))$

(iii) For all $x,y\varepsilon I$ and $A,B\varepsilon J$:

(a) $AB/x \wedge AB/y \rightarrow x = y$

(b) $AB/x \wedge BC/y \wedge x \neq y \rightarrow (\exists z)(z\varepsilon I \wedge AC/z \wedge (x=(\{y,z\}) \vee y=(\{x,z\}) \vee z =(\{x,y\})))$

(c) $A/x \wedge A/y \rightarrow \tau/x,y$.

(The members of I are called '<u>intervals</u>').

<u>Notes</u>:

(1) Condition (iii)(c) is to be read as saying that the members of J mark the shared boundaries of those members of I which touch. Putting $(S') = (x_1,\ldots,x_n)$, then since $S' \subseteq I$ (by (i)) and each member of the series touches the next by K4, it follows that the members of J can be arranged in a series corresponding with the first n + 1 natural numbers (including 0 (i.e., 0 to n), two for each member of S'. The first and last members of J mark the unshared boundaries of x_1 and x_n, which are the <u>ends</u> of (S').

The shared boundary-mark for (x_1,x_2) is thus numbered '1'; for (x_2,x_3), '2'; for (x_k,x_{k+1}), 'k': so, each $y_k \varepsilon I$ such that $y_k = (x_1,\ldots,x_k)$ is also a marked interval series (MIS) having k members, whose end-marks are numbered '0' and 'k'. We can therefore assign to each such y_k the '<u>interval-value</u>' k, which coincides both with the <u>ordinal</u> number of its non-zero end-mark, and with the <u>cardinal</u> number of members of *S'* of which it is the composite under 'o'. Taking y_i, y_k such that $0 < i < k$; $y_i = (x_1,\ldots,x_i)$; $y_k = (x_1,\ldots,x_i,\ldots,x_k)$: we may also recognize $z = (x_{i+1},\ldots,x_k)$, the cardinal number of its component members of *S'* being k-i. Thus, each contiguous series z of members of *S'* may itself be recognized as a MIS to whose maximal interval we can assign an interval-value coinciding with the cardinal number of its component members of *S'* <u>and</u> with the difference between the ordinal numbers of its end-marks.

(2) Though nothing is said in the definition about the dimensionality of the series, and other cases are possible, only the one-dimensional case will be of interest in any instantiating context to be considered in this study, for the reasons stated above p.265

(3) Again, although it is not stipulated that the members of the basic τ-series *S'* are atoms of the context, so that proper enclosures are not ruled out, every interval in *I* is either a member of *S'* or a composite of a sub-series of its members, so that no <u>interval</u> is recognized as a proper enclosure of any member of *S'*, whose members are therefore recognized as the minimal constituents of

the series. In measurement contexts, numerical significance can be given to this aspect.

S' will therefore be called the '<u>constituent series</u>' of $\langle I,J\rangle$, and its members the '<u>constituent intervals</u>' of I.

Now, let $\langle I,J\rangle$ be a marked interval series and $X \varepsilon J$ an end-mark of I. Then for all $A,B \varepsilon J$, where $A,B \neq X$ and $A \neq B$, the available orders are $\langle X,A,B\rangle$ and $\langle X,B,A\rangle$. Putting $x,y,z \varepsilon J$ such that $XA/x \wedge XB/y \wedge AB/z$, we have either $x = (y,z) \wedge \bar{\eta}/y,z$ or $y = (x,z) \wedge \bar{\eta}/x,y$. We can therefore define a relation $\overset{X}{\underset{I}{>}}$ on J by the abbreviation:

$$B \overset{X}{\underset{I}{>}} A \leftrightarrow_{df} (\exists x,y)(x,y \varepsilon I \wedge XA/x \wedge XB/y \wedge \bar{\eta}/x,y))$$

The transitivity of $\overset{X}{\underset{I}{>}}$ follows from that of $\bar{\eta}$ since:

$$C \overset{X}{\underset{I}{>}} B \wedge B \overset{X}{\underset{I}{>}} A \rightarrow (\exists x,y,z)((XC/z \wedge XB/y \wedge XA/x \wedge \bar{\eta}/x,y \wedge \bar{\eta}/y,z)$$

whence $XC/z \wedge XA/x \wedge \bar{\eta}/x,z$. If $Y \neq X$ is also an end-mark (the other end) then the order $\langle X,\ldots,A,\ldots,B,\ldots,Y\rangle$ for which $B \overset{X}{\underset{I}{>}} A$ will also give an order for which $A \overset{Y}{\underset{I}{>}} B$, for each pair (A,B) in J.

It might seem natural to say, therefore, that for any x in I such that $AB/x \wedge B \overset{X}{\underset{I}{>}} A$, x is 'positive on X in I', while x such that BA/x is 'negative' on the same end-mark. But this will not do in the present analysis, since by definition $AB/x \leftrightarrow BA/x$. Positive or negative 'sign', therefore, emerges as dependent on a specific <u>order of assignment of marks</u>. The role of marks as characters was referred to in Note (b), p. 56-7, and the notion of sign will obviously be important in the context of the order of assignment of marks in measurement. But more fundamental matters must be dealt with first.

4. The Structal System

We now associate the marked interval sequence with the metric structure.

K6. The pair $\langle \mathrm{I},\mathrm{J}\rangle$ is a structal system for the comparative pair $\langle Q,\delta\rangle$ iff:

(i) $\mathrm{I} \subseteq \wp S$; $\mathrm{J} \subset \wp C$; and for each $I \in \mathrm{I}$ there is a unique $J \in \mathrm{J}$ such that $\langle I,J\rangle$ is a marked interval series; and a value set $K \subset C$ such that $\langle J,K\rangle$ is a metric structure for $\langle Q,\delta\rangle$: and

(ii) for all x,y such that $x \in I$; $y \in I'$; $I,I' \in \mathrm{I}$; and $I \neq I'$:

(a) $\delta/x,y \vee \delta/y,x \vee \overset{\approx}{\delta}/x,y$;

(b) $\delta/x,y \leftrightarrow (\exists z)(z \in I \wedge \delta/x,z \wedge \overset{\approx}{\delta}/y,z)$.

(N.B. The term 'structal' is used to signal the intuition that the extremely rigorous conditions demanded for this structure are such that they will normally only be satisfied by a system of specially constructed - or, in rare cases, selected - standard idents taken by the R-theory of the context as elements of a measuring apparatus.)

Notes on Structal System

1. The order generated by $\underset{I}{\overset{X}{\geqslant}}$ on a chosen end-mark X

for a given structal sub-system $\langle I,J\rangle$ is the same as that generated for the same elements of I by the relation $\underset{Q}{\overset{\delta}{\geqslant}}$ associated with the relevant comparison-figure and V-character Q. (This is a simple consequence of K6, putting $\langle I,J\rangle = \langle I',J'\rangle$. Each such sub-system thus satisfies the conditions for a structal system.)

2. In any sub-system $\langle I,J\rangle$, numbering one end-mark X as '0', and the remaining members of J as '1', '2',...,'n' in the order of $\underset{I}{\overset{X}{\geqslant}}$, we can choose corresponding numerical assignments to the values K of the quantity Q, such that the value of Q for any member of I will correspond with its interval-value: i.e., with the cardinal number of its component members of S'. As we have seen, the values so assigned are not taken to be necessarily identical with any particular range of values of a standard unit specified for Q, but are to be in functional correspondence with such a range of values under an interpretive theory of the pair $\langle Q,\delta\rangle$ in the context. At the very least, the interpretive function is to be so constructed that the numerical values of all standard equivalence sets under $\underset{Q}{\overset{\delta}{\approx}}$ for each sub-system shall have equal numerical values for the structal system as a whole (see D10, p. 128, and notes; and notes (4), (5) below).

3. The numerical value for $x \varepsilon I$ under this system depends solely on the number of members of the constituent series of I of which x is composed; not necessarily on their length, which will depend on factors associated with

the comparative pair $\langle Q,\delta\rangle$ in the context. (Where, for example, the marked intervals to be compared are those of an arc swept by a needle in a voltmeter, equally-valued intervals are not necessarily equal in length.) In the simplest case, where Q _is_ "length", and $\hat{\delta}$ is the ordinary comparison-figure (in which line-like features of the relevant idents are aligned, and x $\underset{Q}{\overset{\delta}{>}}$ y iff x extends beyond y at either end and y does not extend beyond x, as interpreted in the context) then the equivalence-classes in standard interval series under $\underset{Q}{\overset{\delta}{\approx}}$ will be of "equal length" - including, of course, the constituent class of minimal length for which the value for Q is equal to $P_1 \varepsilon \mathcal{J}$ such that P_1 = '1'.

In other cases, the equivalence of marked intervals is not recognized by the direct comparison of the intervals themselves, but by some procedure which, under the R-theory of the context, ensures that marked intervals are equivalent to mutually equivalent differences in the value of Q. This type of procedure, called by engineers "calibration", will be discussed under particular examples later. The fact that some of these procedures are technically highly sophisticated does not prevent them fulfilling the role of 'comparison figure' in the terms of this analysis. Once the procedure is established, the actual structure of the perceptual recognitions involved in comparisons is typically direct and simple.

(The assignment of numbers to values, and the comparisons of values, in contexts of _non-physical_ measurement is a different matter again. It will be considered shortly in the general discussion of the order properties of the relevant structures, as well as under specific examples later.)

4. As a metric structure, each $\langle \mathcal{I}, \mathcal{J} \rangle$ will support a <u>data structure</u> (K3) which, as a special case of an approximate structure, collects non-standard elements of the comparative structure into unique subsets of $\mathcal{S}'$ (the 'k-clusters') characterized by approximation to distinct values of Q (see D12, p. 134), as assigned to the standard equivalence-classes under $\underset{Q}{\overset{\delta}{\approx}}$ determined for the members of $\mathcal{I}$, by the approximation of relation $\underset{Q}{\overset{\delta}{=}}$.

5. The choice of end-mark X = 0 for each $\mathcal{J} \varepsilon \mathbf{J}$ is, as has been said, dependent on the interpretation of $\langle Q, \delta \rangle$ in the context, and must be made so that the order under $\underset{\mathcal{I}}{\overset{X}{>}}$ is the same as that under $\underset{Q}{\overset{\delta}{>}}$. In the case Q = "length" and δ = the standard alignment figure, it is intuitively clear that choices of end-mark are restricted only by convenience, since δ-comparisons of intervals yielding $\underset{Q}{\overset{\delta}{=}}$ or $\underset{Q}{\overset{\delta}{\neq}}$ relations are in principle readily carried out between any pair which can be brought into alignment. For other V-characters the comparison procedures mentioned above place considerable restrictions on the choices of end-mark and order.

There is, of course, no necessary connection between the value X = 0 as defined here, and any ultimate zero-value for Q as interpreted by any overall theory of the context. But the conditions of these definitions will only be met by a system of measuring apparatus which includes a perceptual scale (or scales) consisting of marked intervals which, in the R-theory of the context, exhibit the additive properties for values of Q (including, where appropriate, negative values or subtractive differences) which, in correspondence with compositions of the intervals, permit the construction of a numerical function from a chosen subset of these values onto the appropriate sequence of positive integers, for each scale in the apparatus.

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5. Order Properties: Comparison with Foundational Analysis

The contrast outlined in the introduction (pp. 3-7) between the approaches of this study and of the Suppes tradition in the analysis of the foundations of measurement (as represented especially in KLST) can now be made more precise. That tradition seeks, as was said, to provide a variety of axiomatic systems for what are called 'empirical relational structures' (ers), presumably on the ground that the material for analysis in these terms is taken to be the product of empirical investigation. Particular projects in measurement are then supposed to be testable according to whether or not their results (however obtained) can be shown to satisfy one or more of these axiomatic systems. No questions are asked, for example, about how the elements of the theoretical structures of these projects themselves are distinguished from, or related to, the totality of human experience, even in a particular context; nor about the perceptible forms taken in different contexts by the fundamental ordering relation $\succsim$, of which nothing is said but that it "corresponds" to the numerical relation $\geqslant$. Such questions are, as we have seen, the central concern of this study, and this contrast of approach leads to major differences in formal structure, which will now be considered.

(1) Connectedness. KLST's fundamental relation $\succsim$ is defined as connected (Definition 1.1, p. 14); i.e., the relation is determined, one way or the other, on every pair of elements drawn from the self-product of a single fundamental set. The C-theoretical relation $\underset{Q}{>}$, whose role in the structure appears broadly comparable, is determined only on those pairs of idents to which the relevant characterizations are assigned by virtue of the recognition of a particular configuration in context. Again, KLST's definition of the unequivocal relation $\succ$ by: $x \succ y \leftrightarrow_{df} x \succsim y \wedge \neg(y \succsim x)$: suggests a parallel between their $y \succsim x$ (by virtue of connectedness, equivalent to $\neg(x \succ y)$), and my own $\neg(x \underset{Q}{>} y)$. But this last negation only excludes the recognition of the relevant configuration, and fails to assert, in the general case, any other relation. The virtue of connectedness, for KLST, is that it leads to a representative function from the ers into the real numbers as a theorem (their Theorem 1, p. 15). This result strikes me as unrealistic.

KLST's underlying theory directs attention exclusively to those pairs of elements in any context on which the fundamental relation is taken to be determined; this appears, ultimately, to be the meaning of the commitment to connectedness. Since the structure so connected is said to be empirical, we must suppose that the relation is empirically determined in every case: the testing of a particular measurement project against the axiomatic system would proceed by considering its structure of results as a model of the system. The theoretical structure connecting these results to the phenomenal evidence is outside the system altogether: which appears to restrict its usefulness as a programme for assessing the status of particular measurement projects.

In this study, connectedness is assumed only for standard and approximate structures, of which metric structal and datal structures are special cases; and for fictal structures extrapolated from these. Because the language of C-theory is rich enough to distinguish between these $\geq_Q$-connected structures and others in context (whether or not Q-valued), we are able to analyse the theoretical structures within which Q-values are determined. The main source of this richness is, of course, the inclusion of terms for the characters by which the different sets of idents are distinguished; permitting analysis of the structures of characterization by which these different sets are recognized as related in context. The ${}_Q$ relation is itself a character relation, and the resulting orderings are determined on sets of idents by virtue of character assignment.

(2) Equivalence. KLST define a fundamental type of equivalence by: $x \sim y \leftrightarrow x \succsim y \wedge y \succsim x$ (Definition 2, p. 15). No similar simple equivalence is available in C-theory, since $\underset{Q}{>}$ is non-reflexive and antisymmetric. The basic equivalence here is given by: $x \underset{Q}{\approx} y \leftrightarrow \neg(x \underset{Q}{>} y) \wedge \neg(y \underset{Q}{>} x)$; which, once again, is defined only on a standard set for Q.

(3) Order. KLST's definitions produce a derivable order on the fundamental set which is proved homomorphic to an order in the real numbers with the numerical relation $\geqslant$. Again, the notion of an order is first introduced into C-theory at the level of a standard structure, and it is argued that in the general (non-metric) case this order cannot be associated with numbers in any way that raises useful consequences from the properties of numbers as such, other than conventional order.* This standard order rests, not on connectedness and equivalence as in KLST, but on the stringent conditions of the definition of the standard set (see D11, and associated theorem, p.129 ff.). This collects the members of the standard set into equivalence sets under $\underset{Q}{\approx}$, with which members of the approximate set are associated into clusters under the weaker

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* There may seem to be a tacit implication in KLST's presentation that any particular instance of a weak order generates a homomorphism with a unique set in the real numbers under $\geqslant$; if so, it is misleading.

equivalence $\underset{Q}{\sim}$. These conditions are justified in terms of the construction (or selection) of a standard set S'' of idents conforming to this condition; which I take to be a much more plausible basis for the determination of order in actual contexts of comparison (whether or not metric).

(4) Transitivity. KLST's definitions product transitivity of $\succ$ on the fundamental set. In C-theory, transitivity of $\underset{Q}{\geq}$ on a standard set is determined only for a single (subjective) Reader and a single R-frame (during which his assignments do not change) (Note (d), p.152). The extension of such a commitment to more than one frame - i.e., in spite of the possibility of change - is understood as a commitment to the stability of the ordering, permitting past comparisons to be incorporated in present judgments (essential where successive measurement readings are to be combined). The extension to more than one Reader is understood as a commitment to the objectivity of the ordering (Section J(2), pp. 220 ff.). I claim that in actual contexts of comparison such commitments to stability and objectivity are severely restricted by the (tacit or explicit) theory of the context for its Readers. If so, it is important that our metatheoretical language should be rich enough to allow expression to be given to such restrictions; which KLST's is evidently not.

(5) Additivity. While the previous features of theoretical structure have been exhibited as restricted to our standard structures and their associated approximate sets,

it should be emphasized that, although these may be supposed to include such systems of empirical relations as those studied by KLST, they are much broader, and include contexts of comparison which could not remotely be classed as measurement (such as that instanced on p. 139). KLST's own definition of 'standard sequence' is much more restricted, being introduced to handle the concept of additivity in metric structures; and demanding amongst other more sophisticated requirements a structure of concatenation. I have pointed out (p. 4) that this notion is frankly modelled on the numerical operation of addition with which it is taken to be homomorphic. This would not be surprising if I am right in supposing that KLST's empirical structures are to be identified with the (numerical) end results of measurement projects, rather than the evidence on which they are constructed. Their somewhat cursory explications, in terms such as those of placing rigid rods end-to-end for the measurement of length, play no essential part in their analysis.

The problem of additivity in empirical structures has been approached in this study through the quite different concept of composition, with its roots in the most fundamental level of analysis, designed to render recognitions of whole/part structure in the phenomenal world. Additivity, as such, is traced back to the properties of complementarity of countable sets; and the problem of securing additivity in empirical structures is seen as that of selecting or constructing just those systems of complementary sets of idents whose membership counts can be theoretically associated in context with amounts of the quantity to be measured.

In the light of this enriched account of metric structures, we are now in a position to address the question, posed at the outset (p. 1), what it is that gives measurement its powerful status, as a basic component of many branches of human understanding.

6. The Status of Measurement: Clarity, Precision and Accuracy

If we now recall what was said about the logic of assignment (pp. 155ff.), and about the structure of possible correspondence between this logic and the logic of associated utterances (p. 186, note (d)), we see that it is at least plausible to suggest that such a field of shared commitment within a Group-R-theory can be "locked into" the truth-functional structure of ordinary propositional logic governing linguistic expressions of that theory. The cautionary double quotes signal an admission that there seems little hope of putting together a complete demonstration of this thesis.

A major strengthening of the logic of a Group-R-theory accrues in the context of measuring systems satisfying the conditions of a metric structure, by virtue of the operational homomorphism between (1) recognitive operations on an empirically ordered system ($\langle \succsim_Q, \circ \rangle$), and (2) arithmetical operations on an associated numerically ordered system ($\langle >, \pm \rangle$), which with reasonable care can be made derivable from the definition of that structure (condition (ii) of K2, p. 251; and see note (2) on the structal system, p. 270). By this means, most if not all commitments under the group theory of the recognized compositional structure of the empirical system (1) can be locked into the truth-functional structure of mathematical logic governing descriptions in mathematical language of that system under the theory. Again, the general demonstration must be lacking; a point which must lead to greater caution in the assumptions of homomorphisms for all sorts of quantities than has been shown by the foundational analysts so far. The adoption of the strong types of commitment needed to sustain this major step is seen as dependent on the organization of conditions of clarity and precision for recognitions, by members of the L-group, of values of the quantities chosen for each context, in the light of consistent motivation and understanding (as defined in terms of the GR-theory).

(1) Clarity is a matter of consistent understanding, by members of the L-group, of the structure of the reference function Φ: that is to say, of transparency of reference of the terms of the language of measurement to corresponding elements of the measuring apparatus (the structal system - allowing for the use of synonymous sets, where appropriate (pp. 239). For instance, "Size 2 eggs" distinguishes a particular class clearly, provided a well-constructed convention exists, and is known to the addressee. Clarity of

this kind is claimed to be a salient property of actual measurement systems; a clarity which is to some extent transmitted to patterns of reference to the associated <u>data structure</u> whose values for the chosen quantities are approximated to those of the structal system by the use of the same comparison figure in each case. This aspect will be further explored in considering particular examples of types of measurement system, below.

(2) <u>Precision</u> is interpreted for C-theory in terms of the standards of consistency of value-assignments adopted by an L-group for the group theory of a context (and is, therefore, immediately dependent on clarity of reference to idents and values). It is to be distinguished from <u>accuracy</u>, which will be interpreted in terms of the <u>tolerances</u> adopted by the group for the context. Thus, I can say that the sun is precisely 93 million miles from the earth, to the nearest million: not very accurate, but I can confidently expect that it will be consistent with anyone else's reading to the same accuracy. By contrast, people are not very precise in their judgments of relative temperatures of objects by feeling with their hands. Where precision fails, accuracy cannot be determined. Both precision and accuracy can be increased if technical facilities can be improved: but, for a given level of technical sophistication, precision will tend to decrease as the chosen level

of accuracy increases - with "indeterminacy" as the limiting case.

Perhaps the most unusual (and probably controversial) feature of this analysis is that it leads to the view that the required combination of clarity and precision with the possibility of constructing an operational homomorphism in the structure $(\langle \succsim_Q, \circ \rangle, \langle >, \pm \rangle)$ is, with few exceptions, available only for physical quantities which are either *themselves spatial* in character, or can be *indicated* by means of some *mechanical* system yielding a *spatial analogue* which can be calibrated (p. 271 and below p. 327). This is seen to be a consequence of our biologically-evolved systems of perceptual recognition. So, though metaphysically contingent, it is practically necessary as a condition of these kinds of recognition; and appears to be linked, in ways merely suggested here in terms of Γ-set structures, to the systems of logical inference we use in understanding our phenomenal experience. What I shall try to analyse, for certain particular cases, is the structure of theoretical interpretation used to represent the relevant *indicator* systems in terms of *interpretive functions* from values of the measured quantity onto values of the *recognized* spatial intervals. We shall find that this necessarily involves the incorporation into the recognitive theory of the context of prior substantive theories of the measured V-characters (below, p. 326). The vital point here is that the prior determinants of these structures lie in the overall interpretive theory and not in the mechanical or recognitive operations. This theory determines the choice of operations (often more than one) for the context in each case. Meaning comes from the theory and not the operations.

As well as examples to illustrate these proposals, some real and apparent exceptions will be discussed in the following sections.* I shall ignore recently-developed systems which contain a further mechanical linkage to a digital display, where these amount to a mechanisation of the procedure of perceptual judgment of approximate datal values. But these must be distinguished from digital displays linked to mechanical counting systems for discrete entities (such as Geiger counters), which, in C-theoretical terms, are not strictly measurement systems - since they do not involve comparative structures. Some methods of psychological or sociological quantification are, in this sense, counting rather than measurement systems; others assign numbers to non-physical parameters by the use of ad hoc scoring systems which, again, do not conform to C-theoretical definitions of metric systems exhibiting the necessary operational homomorphisms. Such systems are, however, often operated in conjunction with substantive theories in which their results are treated analogously with those of physical measurement, using mathematical

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* For reasons of space, much of this analysis will be presented in summary form only. See Note on Sections M-R, p.294

structures developed in _spatial_ contexts (and often explicitly using spatial concepts or graphic representations). One purpose of the present analysis is to exhibit as clearly as possible the structures of such analogies, and the limitations on their reliability for ambitious programmes of extrapolation.

Any such assessment - which need not be derogatory or illiberal - must rest, for C-theory, on its understanding of the relationship between recognitive and substantive theory in each context.

7. R- and S-theories in Measurement: an Ontological Analysis

C-theoretical analysis of the distinction between recognitive and substantive (roughly, non-recognitive) theory within the group theory of any context will rest, in turn, on an intuitive three-fold distinction between types of supposedly existent elements, according to whether or not they are specially constructed (or selected) to conform to some commitment of the theory of the context; and, if so, whether or not they are elements of an R-theory of the context.

A. _Data_ are those elements of phenomenal experience which are not recognized as specially constructed (or selected) to conform to theoretical specifications formed for some purpose determined for the context. If, like Hamlet, we know a hawk from a handsaw - or a hill from a house, a rock from a clock, a man from a measuring stick - we know that the second member of each pair has literally built-in characters connected with its normal human use; while the first of each pair must for the most part be accepted on its own terms. We may select from its characters those which interest us in context - specially those to which we attach theoretical commitments - but our theoretical constructions with respect to a datal element are passive. That is, if they do not conform to our theoretical commitments, we change the theory rather than rejecting or attempting to reconstruct the data. (Not that this principle is always followed.)

B. Structa are those elements which - while remaining elements of the Reader's recognitive theory of the context - are physically constructed (or, in rare special cases, selected) to conform with theoretical commitments adopted for that context. Since they share an R-theoretical structure with data, the consequences of their conformity can be extended in various ways to commitments for suitably related data. With respect to structa, our theoretical constructions are active, in that, if they fail to conform with our commitments, we may seek to reject and replace, or reconstruct them, to ensure consistency within the R-theory. In the above pairs of examples, the handsaw, house, clock and measuring-stick are of this type. In the context of use for which they are constructed, their assigned characters are almost exclusively determined by prior design. (The rare cases where selection rather than physical construction is adopted for the determination of structa, concern either natural structures whose regularities are found adequate for the purpose of the context, or human constructions whose regularities can be adapted for uses for which they were not designed. As with most intuitive distinctions, there are borderline cases. But in contexts of measurement, the distinction is generally sharp. At the most sophisticated level, the use of the wavelength of caesium radiation as a standard for length is a clear case of selected structa - though its regularity as data had first to be established by the use of physically constructed aparatus. An intermediate case would be the use of standard bricks in a wall as a basis for the determination of length; and, at the most primitive level, the use of parts of the human body to determine units such as "spans" or "cubits" was once considered adequate. We need not be surprised to find that man himself - the measure of all things - may turn up as a borderline case.)

The distinction between structa and data was already implicit in the definition and discussion of the structal system (K6, pp. 269 ff.). The third classification of sets to be analysed in the terms of this distinction is now mentioned for the first time.

C. Ficta are elements of human construction whose use, if any, is not completely determined by any R-theory of any context - though we shall be interested only in those types of ficta which are used in theoretical correspondence with data or structa of such R-theories. Again, there are certain borderline cases, such as works of art, for which questions may arise as to whether their use is wholly exhausted by the recognition of their phenomenal structure. The use of ficta in concrete contexts (especially those of measurement) includes a class of special cases of the use of fictal structures to carry or store information in theoretical correspondence with concrete contexts of recognition which may or may not coincide in space or time with the immediate acts of recognition used in forming the relevant R-theories. The type of theoretical correspondence involved can be broadly characterised as reference, the associated fictal structures being broadly characterised as symbolisms - whether linguistic, algebraic or graphic (i.e., diagrams, maps, plans, etc.). Beyond their use for the simple carriage or storage of information, these structures, by virtue of their potential detachment from the immediate context of recognition, can be used for fertile schemes of extrapolation, by the application of implicative commitments beyond the description.of immediately perceived concrete contexts. In particular, they are used for the theoretical "prediction" of data, and for the design of structa.

The term 'ficta' may be thought unfortunate for its possible associations with "fiction". The distinction is deliberately blurred for the same reasons as underlie the treatment of all empirical knowledge as 'theory'. Ficta are human constructions employed in the contexts of theories, and are exactly as reliable (or not) as elements in the construction of truth (as a metalinguistic class in Tarski's sense) as are the theories for which they are used. Many theories, as has been said, are well enough founded to be accepted unhesitatingly as 'knowledge', and their derived statements as 'true'. There appears to be an unbroken continuum of reliability or acceptance from these to the most interestingly speculative, or plain scatty, that people can devise - the present study not excepted.

(The terms 'datum', 'structum', 'fictum', will be used for single elements of the structures falling under these concepts as just informally defined; and the adjectives 'datal', 'structal', 'fictal' for associated terms, structures, systems, etc.)

The use of fictal construction, especially in measurement contexts, may go beyond the transmission, storage or extrapolation of information referring to assignments for characters or idents belonging to the Group-R-theory. It may involve theoretical commitments by the L-group to the existence of entities or properties not held recognizable by any Reader of the group in any frame of the associated concrete context, but "locked into" that context by further commitments of the same theoretical structure, such that assignments of properties to entities within this fictal structure are derivable under the logic of the group theory from those of the Group-R-theory. Any adequately coherent, and consistent, fictal structure of this kind belongs to the S-theory of the associated concrete context. Notions of adequacy, coherence and consistency are taken to be context-dependent in exactly the same way as they are for R-theories. So, also, are many decisions of the L-group as to what is to count as the boundary between R- and S-theories, in terms of the entities and properties governed by these theories (only those of R-theories being categorized as idents and characters). Entities, properties and structures of commitment belonging to S-theory in one context may become <u>incorporated</u> into the interpretive structure of the R-theory in another. For example, the substantive theory of the expansion of mercury and glass under heat is incorporated into the recognitive theory of the measurement of temperature by mercury-glass thermometer, as a tacit or neglected factor in the adoption of the thermometer as <u>indicator</u> of temperature values. These considerations are not, I believe, trivial. They reveal an important aspect of the role of measurement systems in the construction of successively more sophisticated forms of empirical theory, each being incorporated in the recognitive theory of the next.

One important area of decision in this respect is, however, pre-empted by the fundamental assumptions of C-theory. The essential point was made in reference to atomization ($K[C']x$, note (d) p. 85). In every region of an R-theoretical space, R-atoms exist of which no proper enclosure is recognized. Similarly, in measurement contexts, a least recognized interval-value (P_1) is specified as minimal (K2, p. 251). Thus no smaller values than P_1, and no intermediate value between P_k and P_{k+1} can be recognized, since no ident so characterized is recognizable. Still less can any irrational value - e.g. for the diagonal of unit square. But, once a suitable interpretive function has been constructed for an S-theory of the context, there is nothing to prevent the extrapolation within that theory of such unrecognizable values. If necessary, the objective existence of entities possessing these values as properties can also be extrapolated. Such existence cannot, however, be checked by measurement. The recognized metric value may therefore be the meeting-point for two sets of approximation-conditions: those specified for the data structure in terms of the standard apparatus and comparison figure, and those specified by the mathematical rules for the numerical structure of the associated substantive theory.

There are interesting consequences for the notion of _continuity_ in the variations of value for any quantity, such as intervals of space, time or temperature. For concrete structures, any such notion involves fictal extrapolation of intermediate values under the substantive theory of the quantity in context. We are in no way obliged to assume that entities or properties posited within a purely mathematical accounts of continuity - such as the countably infinite sets of Cantor, the notion of pointwise 'density', or the calculus of limits - correspond with elements having objective concrete existence. My own intuitions of perceived continuity in the real world are to the contrary, but argument on this point is beyond the scope of the present study.

Two further aspects of fictal construction are among those which will be exemplified for particular measurement systems:

(1) _Calculated_ as distinct from directly recognized (measured) quantities - of which the classic example is the calculation of values of velocity from readings of distance and time. One aim of the analysis will be to distinguish such cases from those of indicated measurement, as described above; some earlier writers (notably N.R. Campbell) have lumped these cases together under the head of 'indirect measurement'. It is worth noting, for example, that there are cases of indicated, as distinct from calculated, measurement of velocity, such as by spectrographic readings of "red-shift" in astronomy. I

shall try to avoid confusion over the various meanings of the word "measurement" by speaking of direct readings from a metric structure in terms of '<u>registration</u>' and '<u>registered values</u>'.

(2) <u>Probabilistic</u> and statistical <u>extrapolation</u>, often using the notion of synonymous sets (pp. 239 ff.) - especially in contexts of analogies between physical and non-physical measurement systems.

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8. Summary of Measurement Theory

No further metatheoretical structures are to be defined at this stage. It will be found that a very wide range of measurement systems are interpretable as special models of those already defined, in association with appropriate substantive theories. Yet more are interpretable by analogy in terms of the same structures. I therefore list the principal structures below, with summary indications of their theoretical relationships.

(i) <u>The structal system</u> (K6, p. 269) consists of a set of subsystems, each of which associates

(ii) <u>A Marked Interval Series</u> (K5, p.266) with

(iii) <u>A Metric Structure</u> (K2, p. 251). This, in turn, associates

(iv) <u>The Composition Operator</u> (K1, p. 250) with

(v) <u>A Comparative Pair</u>, consisting of a <u>comparison</u>

figure for a valuative character (called, in measurement, a quantity), common to all subsystems of the structal system (D8, 9, and notes pp. 123 ff) for which

(vi) One or more standard sets are constructed or selected in context (D11, p. 129). These qualify as Marked Interval Series ((ii), above) by satisfying the conditions for

(vii) The τ-series (K4, p. 262); and being characterized by an appropriate sequence of marks, on which the composition operator ((iv), above) is determined.

(viii) The Data Set (K3, p. 251 ; interpreted for the structal system in note (4), p. 272) is a special case of an approximate set (D12, p. 134)

(ix) A numerical structure is associated with the system, such that each interval of each marked interval series is assigned a numerical interval-value equal to the difference between the ordinal numbers assigned to its end-marks; these being assigned to the marks according to a system of interpretive functions constructed to reflect the group substantive theory of the quantity in context. This interval-value also corresponds functionally, under the definitions of the structal system, with the cardinal number of members of the constituent series of least recognized intervals under the Group-R-theory. All approximate values, and values interpolated under the substantive theory, are strictly related to these interval-values. (See notes to K5, p.266 ff).

NOTE ON SYNOPSES IN SECTIONS L - R

Considerable research has been done on applications of C-theory to a large number of instances of actual measurement contexts, with the aim of exploring the varieties of theoretical structure it can be used to analyse, and the kinds of conclusions it may yield. A full account of this research would exceed the limits apporpriate for a Ph.D. thesis, but it seems desirable even at this stage to give some indication of the scope and character of the insights which may result. I have therefore presented much of this work below, in synopsis. Where conclusions are presented without argument, they are in fact the product of careful reasoning. References for the main publications consulted will be given in the bibliography. But the work has been unsupervised, and is strictly not part of the thesis proper.

Only for the analyses of length measurement (Section L) and probability assessment (Section P) have the fundamental principles been set out in reasonable detail, as integral parts of the argument of the thesis; further work on these subjects, also, being indicated in synopsis.

INSTANCES OF MEASUREMENT CONTEXTS

L. Length or Distance: (i) the alignment system

1. It has been pointed out that much of the existing work on the fundamental principles of measurement, up to and including that of KLST,has placed great reliance on the presentation of simple length measurement, as carried out on a basis of ordering structures supposed to consist of equivalent standard elements laid end-to-end, sometimes called 'rigid rods'. Analysis has then proceeded on the assumption that a principle of additivity in empirical structures, taken to be obtained by the assembly or 'concatenation' of numbers of 'copies' of standard elements, can be extended by analogy from this literal interpretation to measurement contexts in general; although no argument has, as far as I can discover, been offered for this assumption (other than a few evidently special cases, such as the assembly of numbers of standard weights, which will be mentioned in synopsis in Section O).

Argument has been offered above, particularly in the last Section, for the view that the only soundly generalizable basis for attributing a property analogous to numerical additivity to directly recognizable structures is in terms of numbers of members of a standard set, held under the (R- and S-) theory of the context to exhibit, in a chosen comparison figure, values of the relevant quantity proportional to their numbers in composition. The basic structure exhibiting this property is the standard one-dimensional τ-series of marked intervals. Where the quantity to be measured is itself "length", it is obvious that there are simple applications in which the comparison-figure directly yields "equal length" for each interval (by means which will be investigated shortly). But, even for the measurement of length or distance, there are many contexts of a quite simple kind in which the intervals are <u>not</u> of equal length, and the criteria for their equivalence in the structal system rest - as in all other cases, like those of temperature, time or velocity - on the commitments of the incorporated S-theory associating each interval with equal amounts of the measured quantity, for which equal length of these intervals is not a consideration. Thus, the circumstances that one very literal interpretation of this formulation can be provided for length measurement by the rigid-rod model - and, possibly, a few other special cases of 'empirical additivity' suggested - cannot coherently be supposed adequate for a general account. By contrast, C-theory will present this particular model as itself a special case in which R-theory, without incorporation of S-theory, provides an instance in terms of a standard set of idents which can be directly recognized as <u>both</u> equivalent for the measured quantity <u>and</u> readily composable in the required manner. So the general case, for length as for other quantities, is that in which we must appeal to incorporated S-theory for a structure of commitments according to which the standard intervals satisfy the conditions for equivalent value and composability. It will be found that this approach leads to greater flexibility (in all senses), combined with rigour, than an analysis based on analogies with abutments of rigid rods (the theoretical use of the 'rigid rod' concept in relativistic analysis is quite a separate matter).

We shall return to the theoretical status of 'rigid rods' in sub-section (i)5. I must start, however, with more detailed consideration of the literal instantiation of structal and data structures, putting the quantity Q = "length" and the figure $\hat{\mathcal{S}}$ = "alignment" as the comparative pair. The rationale for the choice of such a system as fundamental was given in para 1, p. 255. The basis of the system was further characterized in note 3, p. 270, on the structal system, where it is stated: "The numerical value for $x \in \mathcal{I}$... depends solely on the <u>number</u> of members of the constituent sequence of $\mathcal{I}$ of which x is composed" (where $\mathcal{I}$ is the set of all intervals of one of the marked interval series (MIS) of the structal system, and its 'constituent series' is the τ-series (S') composed of its least recognized intervals for the context; every member of $\mathcal{I}$ is either a member of S', or the composite of a subset of S'. p.267). The note goes on to point out that the value of x in this case has no necessary relation to the <u>lengths</u> of these constituent intervals, which are to depend on "factors associated with the comparative pair Q, for the context". Where Q is "length", the values of the constituent intervals and their composites will be read as "the lengths of" the respective marked intervals in the structal system; and hence those to which the lengths of the associated data structure are approximated. The comparison figure $\hat{\mathcal{S}}$ = "alignment" is described in the same note, as a figure "in which line-like features of the relevant idents are aligned, and x $\overset{\mathcal{S}}{\underset{Q}{>}}$ y iff x extends beyond y at either end, and y does not extend beyond x, as interpreted in the context". (The most important factor for 'interpretation' in this case is the decision as to the degree of accuracy with which 'extending beyond' is to be interpreted.) Since all relations of equivalence and approximation are defined in terms of '$\overset{\mathcal{S}}{\underset{Q}{>}}$', the note concludes: "the equivalence-classes in standard interval sequences under $\overset{\mathcal{S}}{\underset{Q}{\approx}}$ will consist of idents of 'equal length' - including ... the constituent class of minimal length for which the value in $\mathcal{K}$ is equal to $P_1 \in \mathcal{J}$ such that P_1 = '1'." (where $\mathcal{J}$ is the set of marks assigned to the members of $\mathcal{I}$; $\mathcal{K}$ is the set of all interval-values of Q for the structal system). So the interval-value, or "length" of any ident x of the structal system is given simply by the number of minimal constituent intervals (to be called, 'm-intervals') of which it is composed, which is equal to the difference between the ordinal numbers of the end-marks by which it is characterized.

Since no further interpretation is required, no difficulty is created by the fact that _any_ sub-series of _any_ standard interval series satisfies the conditions for a standard series, _either_ of whose end-marks can be set at '0'. Further interpretation will, however, be needed as soon as we wish to use the resulting numerical values, in conjunction with the logic of arithmetical addition and subtraction, to derive commitments regarding constructions defined in these terms, involving two or more idents of the datal system. This will be dealt with below under the head of "composition and sign" (p.310). Generally, we shall find that restrictions are placed on the assignment of ordinal numbers of marks by _any_ interpretive theory which goes beyond the simple assignment of values of "length" on the basis of counting the number of m-intervals of which a standard ident is composed - or, in the case of a datal ident, the number of such intervals in a standard ident to which it is approximated.

But I must first go on to clarify, as far as possible, how the basic 'alignment' figure is to be further interpreted for the measurement of length.

2. What is meant by a "line-like feature" of a relevant ident was more fully described on p. 265 , as part of a solution to problems associated with the "filling of space" in such a way that fully complemented - and therefore additive - compositions of idents can be constructed and recognized. The type of feature required was there described as "that in which we restrict attention to what

we call one spatial dimension of a particular region of an R-frame. Such a region may be regarded as a τ-series whose spatial 'enclosure' (as a limiting case of this concept) is recognized in terms of a single boundary shared with neighbouring idents, or as a pair of closely-aligned boundaries whose enclosed space is neglected. It is the phenomenal correlative of a line-segment. ... The shared boundaries of (these segments) are recognized as having no spatial dimension: they are the phenomenal correlative of points." It is, of course, these "point" boundaries which are recognized by their characterizing marks (or, more literally, by their configurative relations to these marks, which necessarily occupy space "alongside" them).

The 'alignment' figure $\hat{\delta}$ can be seen in these terms as a figure whose instantiating configurations are constructed and recognized as "a pair of closely-aligned boundaries whose enclosed space is neglected"; each of these boundaries being recognized as a distinct region of one of the two idents whose "lengths" are compared; all other regions of the boundaries of these idents being neglected in this recognition, for this R-frame of the context. (The time restriction is essential here, as we shall see.)

In the structal system, the boundary-regions which instantiate MIS are constructed so as to be readily recognizable, unequivocally, by all members of any L-group for any context in which they are used. It may be helpful to think of them as distinct quasi-idents in their own right, 'enclosed' in the pieces of apparatus on the boundaries of which they are constructed (but seldom consciously or explicitly distinguished). In ordinary English parlance they are called "scales": but, to avoid ambiguities from varied uses of this term in the literature, I shall call them <u>structal interval series</u> (SIS).

The sense of "dimension" in the above description needs careful interpretation. It is to be understood <u>only</u> in terms of the SIS-boundaries themselves, and must not be taken to suggest that, even in the same context, there may not be a further interpretation in terms of substantive (in this case, geometrical) theory under which these boundaries are describable as embedded in, or part of, a structure of more than one dimension: for example, if an SIS is taken to be "curved", or if two distinct SIS (or the same SIS in different frames) are taken to be related so that their interval-values are to be assigned to different geometrical dimensions. Interestingly, the very notions of "straightness" and "rigidity" (the last being a combination of commitments to "constant straightness" and "constant length", often blithely assumed in accounts of measurement theory) will emerge as belonging to the substantive, geometrical theory of the space, and not to the recognitive theory of measurement itself.

Geometrical theory will enter our analysis at a very simple level, when we come to discuss the use of triangulation systems for length or distance measurement in subsection (ii)2. We may neglect for the present (but not in any complete development of this analysis) relativistic accounts in which S-theory calls for rigid rods whose length varies with conditions of observation. But only if we understand the structural role of the 'incorporation of substantive theory' at this fundamental level, will we be able to develop our analysis successfully for structures whose sophistication is limited only by the intellects of the theoreticians engaged in the construction of the context, and the technology at their command (in that order).

3. Returning, now, in the light of these considerations, to the 'line-like' SIS in its own terms, we may speak of it, if necessary to avoid ambiguity, as being of 'one R-dimension'. We start by looking at the intervals of a single SIS, compared by alignment with one another to the extent that their boundaries are aligned - i.e., that they overlap. (In this section I shall, for simplicity, write '$>, \approx, \sim$' for '$\overset{\delta}{\underset{Q}{>}}, \overset{\delta}{\underset{Q}{\approx}}, \overset{\delta}{\underset{Q}{\sim}}$', since the values of δ and Q are constant throughout.)

(a) Each interval x of such an SIS is aligned with itself, and trivially satisfied the definition given for $\approx$/x,y (putting y = x) for all standard structures (D11(ii), p.130);

(b) The relation $>$/x,y for any x,y in the SIS will be satisfied iff:

  (i) the whole boundary of y is aligned with the boundary of x;

(ii) some non-neglected part of the boundary of x is not aligned with that of y;

(iii) treating the SIS as a 'quasi-ident', x and y are also quasi-idents enclosed in it, and y is a proper enclosure of x.

(c) x and y are distinguished as enclosures of the SIS by reference to their characterizing marks. It is by reference to these marks that parts of the boundaries of x and y are recognized as aligned, or not.

(d) It is convenient to restrict attention to the case where, for some marks P_i, P_j,$P_k \varepsilon \mathcal{J}$ for the SIS, P_iP_k/x and P_iP_j/y (i.e., these marks are members of f(x), f(y), respectively, for the R-frame: A5 (note), p.52. That is, P_i is a common end-mark of x and y; P_j, the other end-mark of y, is a mark of x; but P_k, the other end-mark of x, is not a mark of y. (We say that a mark P_j is "between" marks P_i and P_k on a SIS iff there are intervals x,y of the SIS satisfying these conditions.) Relations of other types of interval-pairs can easily be reduced to conjunctions of cases of this type. It follows in this case that $P_j \neq P_k$, and that there is an interval z of the MIS such that P_jP_k/z; no part of z is aligned with any part of y; and $\overline{\eta}$/x,z. Also, since each SIS is part of the metric structure, x = y o z; and x,y, and z are composed of integral numbers a, b, and c of m-intervals such that a = b + c, and a = k — i, b = j — i, c = k — j, where the ordinal numbering of the marks $P_{i,j,k}$ is 'i', 'j', 'k'.

The interpretation Q = "length", $\hat{\delta}$ = "alignment", also determines that of the equivalence-relation $\approx$ by which idents of the structal system are read as "of equal length" iff they are composed of equal numbers of m-intervals. Since, in general, there is no question of every pair of m-intervals of a structal system being aligned in any actual context, this essential condition must normally be the subject of a (usually tacit) commitment of the R-theory that _if_ any two m-intervals of the structal system are aligned, either with each other or with a third m-interval, they will exhibit the relation $\approx$. (Again, since, in general, it is not practical to align two distinct intervals of one SIS, it is best to think of this commitment as referring to the δ-equivalence of each such interval with any m-interval of another SIS of the system. If no other SIS is, in fact, present, the commitment is to the possibility of constructing one, which need not be composed of more than one m-interval). In a world where SIS for length measurement are mass-produced, such matters are normally taken on trust, together with a commitment to their conformity - to a tolerance acceptable in context - with some institutional SIS.

4. All datal values are recognized on evidence of a relation $\sim$/x,y, where x is an ident of the data structure and y of the structal system. In the present case, this depends on the construction of an alignment configuration in which a _line-like_ feature of x is aligned with y (which is _defined_ as line-like) so as to satisfy the defined conditions as interpreted for the context. Since

datal idents are not in general provided with specially-constructed line-like features, such a construction normally involves a judgment by the relevant Reader under his R-theory of the context - which is not always trivial. It is not possible to generalise about this, except to say that in most cases the matter can be adequately resolved at the levels of precision and accuracy required for the context. Equally, the recognition of datal _marks_ of x as _point-like_ features, not specially constructed, such that the conditions for ~/x,y are satisfied, is a matter for theoretical judgment, normally assisted by established rules which are epistemologically trivial. Alignment of the datal interval (line-like feature) with the structal interval to which it is thus approximated, must be such that any space enclosed by the aligned boundaries is negligible in context. In practice, the configuration will always be constructed so that the datal interval bounded by one datal and one structal mark is negligible, approximation being judged by the relative datal intervals - if any - recognized between the other datal mark and the nearest (as judged) structal marks.

5. _Constancy_ of length, from frame to frame, of intervals in the structal system is an essential commitment of the R-theory which is, again, normally tacit and unproblematic. In some special cases, corrections based on substantive theories about variations with temperature or stress may be required. From an epistemological standpoint, the interest here lies in distinguishing the types

of theory involved, and their roles in the theoretical structure as a whole. (Much intuitive reliance is certainly placed on commitments that structal or datal elements of linelike form, when made of known flexible materials, do not change in length (within certain tolerances) when bent.)

Straightness, as we have seen, does not enter fundamentally into the theory of length-measurement by alignment. It is enough, for example, for a flexible tape to be in contact with a datal ident in such a way that the space enclosed by the relevant line-like features, datal and structal, is negligible. In those cases where straightness is important, we have seen that for C-theory it belongs to the substantive geometrical theory of the context. Commitments to straightness may be adopted for datal as well as structal elements. An interesting case for C-theory is that where I wish to measure the length of a wall, which I take to be straight, with a metre ruler or tape, the wall being at least several metres long. Foundational analysis tends to suggest that I "concatenate" the necessary number of "copies" of the ruler by placing them end-to-end, assuming that they are "rigid". This is at best misleading, and would create problems with a flexible measuring-tape which do not arise in practice. What I actually do is to assume that the wall is straight - if this is important in context - and mark off successive portions of it, metre by metre, until the last section for which I read off an approximation to the desired accuracy. This procedure satisfies the condition that all data are approximated to structa, if we regard the marked-off metre intervals along the wall as 'temporary structa' for the context. This can be done, since they can be constructed to conform as accurately as required to the relevant definitions; but the last section, which is approximated, remains datal. The commitment to straightness is adopted for the wall as a whole, independently of whether any part of it is assigned structal or datal status. (The main difference between 'temporary structa' of this kind and regular elements of the structal system is that the end-marks of their component intervals (which may not be minimal) attract no theoretical commitments beyond the immediate operation for which they are constructed; they are neither specially constructed in advance, nor have they any further practical or theoretical relevance. They return to be indistinguishable parts of the data structure, as do the temporarily-marked intervals they bound. Indeed, as the next example shows, the "marks" themselves may be purely notional, having no objective existence.)

There are cases also where the greatest interval is - at least initially, datal, alignment is used, and straightness is not material. For instance, wheels equipped (for convenience) with counters can be used to measure the lengths of curves of any shape and any practical length: big ones for measuring travel along a road, small ones for their representations on maps. Here, the circumference of the wheel is the initial structal interval, best seen as an open curve whose ends happen to coincide, again for convenience. The intervening datal marks can safely be omitted - though we must be committed to saying that _if_ they were drawn in, each circumference-long datal interval so constructed _could_ be converted into a 'temporary structal interval'. Total contact - negligible enclosed space - over the _whole of the greatest interval_ is the key to the epistemology of the theory. Not even the circular geometry of the wheel is fundamental - another mechanical convenience. (Counted paces along a "straight edge" or "curved path" provide a more primitive example.)

6. _"Length" and "distance"_. We have seen that geometrical properties like straightness enter into the epistemology of the theory of measurement only where we go beyond the use of a simple alignment configuration in the measurement of length. An even simpler case than those we have considered is that where an interval of the structal system is placed so that a pair of datal marks can be found to satisfy the conditions for approximation, but no line-like feature is recognizable in the datal structure of which the datal marks are end-marks; e.g., the diameter of a circular table, or a gap between two stable structures. If we adopt the commitment that the structal interval is geometrically "straight" (and a taut tape will do as well as a rigid ruler), we can say that "the _distance_ in a straight line" from one datal mark

to the other approximates the value of the structal interval. No datal quasi-ident exists for the R-theory of which the "length" can be equated with this distance. The distance is thus strictly a fictal construction - going beyond the phenomenal evidence, and calling in aid our (very simple) geometrical theory of the space. However, it is normal to regard such a distance between datal marks as an element of the datal structure: a datal interval. For C-theory, this means incorporating our substantive theory of the space into our recognitive theory.

This "distance" would not necessarily cease to exist, as such, for the R-theory if we were (then or later in the context) to discover or construct some recognizable line-like feature aligned with it, whose "length" could be equated with it. Can we say that with every length a distance is associated? Or that length is a special type of distance? Note that distances need not be straight. They may have any regular geometrical shape associated with line-like structures - such as the distance travelled by a ball swung round on a string - or no regular shape. Having measured the length of a road with a wheel, we know the distance travelled by the measurer.

It seems to me that the distinction we draw between these terms will vary with our theory of the context, and other factors associated in that context with the values concerned (say, its relation to the time of travel at a given velocity). For this study, I shall keep "length" for values of spatial intervals assigned to line-like features recognized as (parts of) idents whose identity is completely determined by the assignment of other characters; and "distance" for all other recognized spatial intervals.

7. Composition and sign. To satisfy the conditions defined for alignment-length measurement, all data must be line-like features with point-like end-marks approximated to recognized structa; and any assignment of interval-values to data by arithmetical calculation from summation or subtraction of their approximate numerical values must be analysable so that the resulting value can - at least in principle - be assigned to some recognized datal interval satisfying these conditions. The simplest case is that of a configuration of data satisfying the conditions for a marked interval series (K5, p.266), though lacking the theoretical commitment to correspondence between the ordinal numbers of marks and cardinal numbers of constituent series which distinguish an SIS. That is to say, a contiguous series of line-like features recognized as bounded by shared end-marks, and therefore exhibiting the strict spatial complementarity of the τ-series (K4, p. 262). For further exposition I will adopt - mostly for this section only - the following terminology and symbolism:

(a) A configuration of data satisfying the above conditions will be called a datal interval series (DIS).

$\overline{a,\ldots,m} =_{df}$ the sequence of idents a,...,m form a DIS.

The letters g,h,j,k will be used as variables representing any members of such a DIS. Where $\overline{g,\ldots,k}$ designates any sub-series, including $\overline{a,\ldots,m}$ itself, gk denotes the total interval composed by g,k, and all intervening intervals.

(b) $\odot$,A,...,M will be used to denote the marks of a DIS given by a,...,m; ordered so that $\odot A/a$, $\odot K/ak$, and $\bar{G}K/gk$ (where $\bar{G}$ stands for the predecessor of G in the order, such that $\bar{G}$,G are the end-marks of g, so that $\bar{G}G/g$; a convention to avoid identifying some particular letter in the series with this end-mark). G,H,J,K will be used as variables representing members of this series of marks.

(c) We can now follow the normal convention by writing $\bar{G}K$, for gk, or $\bar{G}G$ for g, for all values of these variables; i.e., naming an interval by its end-marks. I shall use x,y,z as variables for members of the associated SIS, and N_0, N_g, N_h, N_j, N_k as variables for the ordinal numbers which are their end-marks, writing N_g^k for x where $N_g N_k/x$. The convention will be that the order of end-marks denoting N_g^k is the ordinary numerical sequence, such that $k > g$; and the order of datal marks $\bar{G}K$ will be that of $\odot$ to N, which will be uniform for each DIS for each context. But where we have two (or more) distinct DIS in any context, say $\mathcal{D} = \{a,\ldots,l\}$ and $\mathcal{D}' = \{a',\ldots,m'\}$, there is no general commitment to the relationship between the orders $\odot$ to L and $\odot$ to M'.

(d) There is plainly a functional relation which will be written $\mathcal{L}$ = "the length of "; such that for each gk = $\bar{G}K$ there is some structal ident, say x = N_h^j, such that ~/gk,x; the conditions for MIS give $\mathcal{L}(x) = j - h$; whence $\mathcal{L}(gk) \triangleq j - h \triangleq (\bar{G}K)$. j − h is necessarily an integral number which is the cardinal number of m-intervals in x, and is positive under the above convention that j > h. I shall consider shortly what meaning can be given to negative values, either structal or datal; meanwhile there is plainly no sense in the idea that x may contain a negative number of m-intervals. (Strictly speaking, we should say that $\mathcal{L}$ = "the length, expressed as a number of m-intervals of the structal system, of"; an m-interval, here, is not necessarily a conventional unit. A further, interpretive, function, converting these numbers into units or part-units of the context, and assigning consistent values for all datal intervals in context, must normally be constructed under substantive theory.)

(e) The conditions for the comparison figure of alignment require that, where data are aligned either with structa or with other data - say, $\bar{G}K$ of $\mathcal{D}$ with x = N_h^j or with $\bar{H}'J'$ of $\mathcal{D}'$ - the end-marks are placed so that the question which, if either, 'extends beyond' the other, at one or both ends, is determinate. This is in practice only possible if at least one pair of end-marks is <u>matched</u> so that the spatial interval between them is <u>negligible</u> in context. This would yield the value, say, $\mathcal{L}(\bar{G}N_h) = 0$ or $\mathcal{L}(\bar{G}\bar{H}') = 0$; and will be written as a configurative relation, $=(\bar{G}N_h)$, or $=(\bar{G}\bar{H}')$, as the case may be.

Only for a pair of structal intervals can both pairs of end-marks be so matched; but, if $\bar{G}K \sim N_h^j$ and $=(\bar{G}N_h)$, the pair (K,N_j) will approximate to this configuration. Such an approximative matching configuration will be written $\triangleq(KN_j)$.

I shall now use the above notation to discuss a number of consequences and questions which arise for the interpretation of alignment-length measurement.

(1) Consider the possibility that, within any one DIS there exist two marks K, K' such that for some J,x, $\sim/JK,x$ and $\sim/JK',x$. Let $\mathcal{L}(x) = n$; then $\mathcal{L}(JK) \triangleq n \triangleq \mathcal{L}(JK')$. Since, by K3 (p.251), $\neg(>/JK,JK')$ and $\neg(>/JK',JK)$, $\mathcal{L}(JK) \triangleq \mathcal{L}(JK')$. All such JK, JK',... form a k-cluster as defined for D12, p.135. So all such marks K, K',... can be regarded as indistinguishable for the context, and the intervals between them neglected; i.e., $\triangleq(KK')$, etc.

Further, for all idents x,y, of the structal system such that $\approx/x,y$ we have $\mathcal{L}(x) = \mathcal{L}(y)$; so that if GJ, H'K' of two distinct DIS $\mathcal{D},\mathcal{D}'$ satisfy $\sim/GJ,x$,$\sim/H'K',y$, and $\approx/x,y$, we have $\mathcal{L}(GJ) \triangleq \mathcal{L}(x)$, whence $\mathcal{L}(GJ) \triangleq \mathcal{L}(y)$, i.e., $\mathcal{L}(GJ) \triangleq \mathcal{L}(H'K')$.

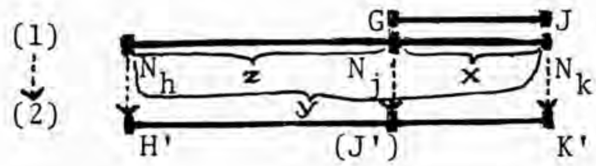
(2) Since, within any DIS, datal intervals satisfy the conditions of spatial complementation for the τ-series, we can extend the definition of the composition operator o; so that for GJ, JK of DIS $\mathcal{D}$, GJ o JK = GK. By the definition of a DIS, it follows in such a case that there exist x,y,z of some SIS such that $\sim/GJ,x$, $\sim/JK,y$, $\sim/GK,z$, and x o y = z.

Any calculation, under this extended definition, of an approximate length for a composition of data from different DIS - say, GJ o H'K' - carries with it the commitment that datal and structal intervals exist such that the above conditions are satisfied and appropriate approximations and equivalences hold: e.g., there are J", H", K" of " and x,y,z such that $\mathcal{L}(GJ) \triangleq \mathcal{L}(J''H'')$, $\mathcal{L}(H'K') \triangleq \mathcal{L}(H''K'')$, $\sim/J''H'',x$, $\sim/H''K'',y$, $\sim/J''K'',z$, and $x \circ y = z$; whence $\mathcal{L}(GJ \circ H'K') \triangleq \mathcal{L}(z)$.

With reasonable care - which may involve the construction of "temporary structa" such as those proposed for the measurement of a wall (above, p.307) - such conditions are normally satisfied, quite literally, for ordinary contexts of alignment-length measurement, with the help of certain unproblematic substantive commitments to the stability of the relevant structures, to be considered shortly. But for many contexts, including some of measurement systems for spatial intervals, the recognition of both structal and datal intervals equal or approximate to the greatest composed interval whose value is used in calculation is not realisable readily, or at all. Error and inconsistency may then arise from the cumulative effects of successive approximations, unchecked against the structal system.

(3) Proceeding now to the consideration of sign, let us ask at the outset what sense can be given, within C-theory, to an operation of "negative composition" on perceived spatial intervals such that, say, the "length"

$\mathcal{L}(GH) - \mathcal{L}(H'K')$ has negative value? We will expect to proceed by aligning GJ and H'K' with x,y,z such that $x = N_j^k$, $y = N_h^k$, $z = N_h^j$, and (1) $\hat{=}(GN_j)$, $=(JN_k)$; (2) $\hat{=}(H'N_h)$, $=(K'N_k)$; as in the following diagram:



(If GJ, H'K' are not, and cannot be, aligned, these alignments must be done <u>successively</u>, by <u>moving</u> the MIS $\{x,y,z\}$. Some consequences are discussed shortly.)

We now have $\mathcal{L}(GJ) \hat{=} \mathcal{L}(x)$, $\mathcal{L}(x) = k - j$; $\mathcal{L}(H'K') \hat{=} \mathcal{L}(y)$, $\mathcal{L}(y) = k - h$; whence $(\mathcal{L}(GH) - \mathcal{L}(H'K'))$ $\hat{=}((k - j) - (k - j))$; i.e. $\hat{=}\ h - j$, which has negative value. But what meaning can we give to this result in C-theoretical terms? What <u>ident</u> is assigned this value? The structal ident z is composed of $j - h$ m-intervals, which is a <u>positive</u> number - whether you count them from N_h to N_j or from N_j to N_h. If we wish to identify a datal interval approximate to z, we can place a mark J' in H'K' such that $\hat{=}(J'N_j)$: so that H'J' is the spatial complement in H'K' of J'K' such that $\mathcal{L}(J'K') \hat{=} \mathcal{L}(GJ)$. But what is negative about that?

It becomes clear that negative values, if any, must be assigned in the terms of a <u>substantive theory (S-theory)</u> of the context, under an <u>interpretive function</u> which takes the values of 'h', 'j', 'k' (which are always positive for C-theory) into corresponding values in a numerical structure where negative values are given meaning which goes beyond the perceptual evidence. Thus, e.g., if in C-theoretical analysis N_h = '0', N_j = '3', N_k = '5', we can construct a simple interpretive function 'minus 3', taking N_h into −3, N_j into 0, and N_k into 2. But note that what we are doing here is not to change the marks on the MIS N_h^k - which would not affect any of the points made above - but to assign them values in the substantive theory such that the direction of counting has a specified meaning for that theory. So the reference above (p.268) to the dependence of sign on the "order of assignment of marks" can now be clarified to mean their order of assignment within an associated S-theory.

The S-theory must offer some rationale according to which counting from N_j to N_k and from N_j to N_h shall be done in opposite directions - called, conventionally, positive and negative. Moreover, that rationale must also determine whether this counting-rule shall apply for all alignments or, if not, for which alignments and on what principle.

As noted in our fundamental consideration of 'counting series', an order of counting is associated with an autonomous choice of one of any pair of relations which are in principle derivable from a progressive binary figure (Section D, pp. 103 ff.). Selective attention to a particular relation can only be determined by some theoretical commitment attached to that relation; and, where the figure is a comparison figure for a measurable quantity, the decisive commitment is likely to be to a functional association of variations in that quantity with those of some other factor of importance in the context. Such an association gives critical meaning to the direction of any particular variation. This theory-dependence is strikingly true of contexts of measurement of length or distance; since, notoriously, spatial directions have no significance in isolation, being wholly relative not only to conditions of recognition, but to choices of particular directions and alignments as decisive with regard to the motives of Readers of the context. This is so even if the motives are purely intellectual and abstract in content, as has been my arbitrary choice of illustrative direction above.

Thus, the present analysis provides a substantial epistemological base for the conventional view that negative numbers are a symbolic introduction into mathematical representations of empirical quantities, yielding the subtraction operations required by the demands of the empirical theory of the context and the purposes of the associated calculations. This has been done without appealing to any distinct concept of 'difference measurement', as invoked by KLST and many others: since <u>all</u> our metric elements are presented in the form of intervals, represented by numerical differences between numeral marks, which may be interpreted as positive or negative according to the commitments of the associated S-theory.

(4) A minimal form of S-theory (which may or may not

involve the assignment of negative values) is already implied in the mention above of successive alignments, typically involving the moving of structa with respect to data. If we are to make sense of compositions arising from successive readings - located, necessarily, in different R-frames - our S-theory of the spatial structure of the context must include commitments to stability of length from frame to frame of all structa; and stability of spatial position for at least one datal element, with respect to which other datal structures can be located. This notion of position is, of course, context-dependent, and, as I have said, a matter for substantive commitment, whose only criterion of validity is that it forms part of a consistent theoretical reading of the context as a whole. The least possible commitment to datal stability would appear to be to stability of position for one end-mark of one DIS, composed of at least one datal interval with one other end-mark; this carries a further commitment to the existence of at least one structal interval to which this datal interval is recognized as approximate. For each such 'stabilised' DIS I shall suppose that one end-mark is chosen as the primary locus of stability, from which all numerical values are counted: to be called the 'origin' of that DIS and symbolised '$\odot$'. Where both negative and positive values are to be assigned under the S-theory to intervals of a single recognized τ-series, they will be interpreted as two distinct DIS sharing a common origin but to be read in opposite directions -

their geometry, as parts of a "straight line" or not, being also a matter for S-theoretical commitment. So long as all datal idents of the context are enclosed in one such stabilised DIS, a simple S-theory can be constructed without further commitment - even to stability of length-value, where the identities of the DIS and its enclosed intervals are recognized from frame to frame. (A simple and intriguing example, which dispenses even with a stable origin, is the measurement of someone's waist from time to time over a long period. Our commitments are to the stability of the structal tape and its marks, and the identity over time of the person and his/her waist. Our assumption is that the length of the closed curve is independent of the chosen origin. As soon, however, as we wish to distinguish different intervals of a closed curve - say, the circuit of a lake - one or more stable origins must be identified.)

It is worth noting at this point that a datal interval or DIS - whether stabilised or not - must retain its <u>identity</u> over time, as a basis for sequential commitments from frame to frame, although its <u>value</u> for a measured quantity may vary: while, although structal intervals must retain stable quantitative values, their identities are less crucial. They form a system of <u>synonymous sets</u> (above, pp. 239), whose differences of character, except values for the measured quantity, are neglected. It follows that the sequential commitment to the identity of features forming an ident, from frame to frame, attaches to non-metric characters. For example, we do not just measure "lengths", but the lengths of objects, identified by characters other than their lengths. It is with these non-metric characters that the commitments to physical stability (like those which determine a direction of counting) are associated.

As the S-theory of the context becomes more sophisticated, more commitments with regard to the geometrical relations of the data are added. No general theory of this development seems possible, but increasing geometrical complexity may be expected to be associated with the introduction of more datal or structal elements into the context, and some familiar lines of development should be mentioned. Straightness may first appear as the "shortest distance between two points", or as a schema for common alignment of several datal or structal features. Straightness itself is generally an important component of Euclidean schemes leading to commitments regarding relativities, and especially congruences, of length-values. At an early stage commitments to the stability of particular datal angles, and circular arcs, are typically introduced; more will be said on this aspect shortly. Human constructions for use - especially furniture and buildings - must, historically, have provided a rich source both of motivation and realisation for increasing sophistication of this sort; including the use of level and plumb-line, and the Egyptian 3-4-5 triangle. Descartes' coordinate system can be seen as a generalisation and abstraction from the resulting structures.

The more recent, non-Euclidean geometries are constructed, in practice, as purely mathematical modifications and elaborations of the Cartesian scheme. Tests of the resulting theoretical commitments by measurement of specified data require the reduction of the results of calculation to a Euclidean form: in the light of C-theory, this appears as a necessary consequence of the epistemological preconditions of measurement.

Such reduction often involves approximations within the mathematical structure, which, from our metatheoretical standpoint, must be clearly distinguished as belonging to different theoretical systems (the S-theories) from the recognitive approximations involved in the theory of measurement as such (the R-theories). Mathematical, S-theoretical, approximation may also be necessary to reduce calculated results - e.g., of irrationals - to a number of significant figures such that they can be functionally related to an integral number of recognized m-intervals of the structal system in context. It was in these senses that it was said above (p. 290) that the measured value may be the meeting-point of two systems of approximation. The tendency to speak of the simpler surviving S-theories, and their associated measurement systems, as "approximations" to, say, Einsteinian theories of spatial structure, regarded somehow as the "real" geometry of space, must at least be treated with caution. But this must be left for later discussion. In practice, for reasons which further analysis will suggest, the measurement systems associated with the more sophisticated spatial theories always involve the <u>incorporation into R-theory</u> of (often tacit) commitments derived from earlier, established <u>S-theory</u> - of the kind accepted in context as common, or background, "knowledge". Some examples will now be offered.

<u>(ii) Distance measurement using incorporated substantive theory</u>

1. There is one aspect of the substantive theory of the structure of space which is tacitly incorporated in so many contexts that its acceptance as "knowledge" is beyond question - though just what is known by it, and what, under the term "space", it is known <u>about</u>, may be matters for debate. What I am speaking of is essentially a commitment to the quantitative uniformity of spatial structure, independently of the operations of measurement - most obviously, of the units used, or, in C-theory, of the constructed m-intervals of the context. This has been dealt with, in the Suppes tradition, under the name of 'uniqueness' (see especially KLST, pp. 9 ff.) in terms of 'permissible transformations' of the 'numerical function' at the core of a 'representation theorem'. At its simplest, the notion can be formulated, for what is there called a 'ratio scale', as the commitment:

$$(\forall\phi,\phi')(\exists\alpha)(\forall a)(\phi(a) = \alpha\phi'(a)) \;;$$

where a ranges over the basic set of the 'empirical relational structure', ϕ , ϕ' are 'numerical functions' associated with 'scales' using different 'standard units', and α is a positive real number. This simple form of uniqueness commitment (which can be proved as a theorem for many cases under KLST's assumptions, and would certainly be taken to hold for all simple cases of length-measurement) explicitly applies to the numerical theory of the context; though it is not precisely clear what is credited with "uniqueness".

For C-theory, each of two structal systems, not constructed so as to be defined in terms of specified functions on the values of their standard intervals, presents itself as part of the data structure of the other. While certain not very interesting regularities could no doubt be derived from the commitments, in each case, to systematic inequalities, it is not generally possible to give a unique value to which would satisfy the above formulation, and which could be precisely tested in measurement. This would be the case, for example, when translation was attempted as between the language of the metric system (based on the metre) and either some unconventional ad-hoc system such as the "book-widths" or "bricks" instanced earlier, or the anglo-american inch-based system. All measured relationships between these systems are approximations, to the tolerance adopted in context. The assigned value of the parameter α is thus context-dependent, and the statement that it has a completely determined value in the real numbers is an idealised commitment of the substantive theory with respect to _pairs of structal systems_. KLST's assumption that the system of units can always be referred to some "sufficiently fine-grained standard sequence" is therefore unrealistic, unless interpreted within some system of tolerances. Any notion of an ultimate limiting value which is progressively approached will founder - as part of a theory of _measurement_ rather than of an abstract structure of space - at molecular distance scales, if not before. I have accepted that such commitments are unproblematic for the measurement of spatial intervals, at least at ordinary scales. But the extension of such commitments to systems incorporating higher-level theories, and especially to systems in the human sciences employing analogues of spatial structure for hypothetical quantities, it becomes important to analyse as closely as possible the theoretical structures employed. This I take to justify my somewhat pedantic analysis of this type of commitment as a case of the incorporation of S-theory in R-theory.

2. A perfectly practical case of this kind of commitment (for which I will in future use the simple term '_incorporation_', where unambiguous) is the incorporation of Euclidean, Pythagorean and trigonometric theory in R-theories for the measurement of spatial intervals by various forms of _triangulation_. These incorporations occupy the whole of the middle ground of spatial

measurement between lengths and distances measurable by alignment with surveyors' tapes, and astronomical distances for which the Earth's orbit is too short a base for accurate assignment. They include the use of optical rangefinders, and of various kinds of navigation equipment to fix positions and distances at sea or in the air, typically using radio or radar beacons or beams: in all these cases they therefore also incorporate a theory of the straightness of 'rays' of electromagnetic radiation. It is no part of the task of this study to go into detail on the structures of any of these incorporated theories in their own fields; I merely note their presence as structures of tacit commitment within the R-theory of associated measurement. To qualify for tacit incorporation of this kind, commitments of substantive theory must be well established and unproblematic, accepted as knowledge. The commitments underlying systems of triangulation are paradigms (in the pre-Kuhnian sense) of this kind of knowledge. Overlaps occur between the ranges of distance so measured and those measured, at the lower end, by alignment, and at the higher, by more sophisticated forms of incorporation. Consistent readings by different systems in these overlapping ranges add inductive support to the incorporated commitments - hardly necessary in the ranges mentioned here, but becoming increasingly important at extremes of scale, even in spatial measurement. For some other types of measured quantity, this aspect can become crucial.

A further concept which must be introduced, if the notion of incorporation is to be clearly understood, is that of an indicator system - in cases of physical measurement, always a physical linkage system - in which perceptually recognized intervals of one type of quantity are taken to 'indicate' values of another. We shall find that this concept has application in the analysis of the vast majority of measurement systems. I introduce it in the context of spatial measurement incorporating triangulation theories, because here it has particularly fundamental application. Here, both the recognized intervals - which I shall call 'registered' - and those to which values are assigned - which I shall distinguish as 'measured' (by indication) - are spatial. (For reasons already sufficiently stated, we shall expect nearly all registered intervals to be spatial.) The essential notion is that some feature of the measuring apparatus, which I shall call the 'standard variant', presents an analogue of variations in datal intervals which the apparatus is used to measure: approximations between changing (registered) values of the standard variant and structal intervals constructed in the same apparatus are read - without additional calculation - as values of the measured quantity for the datal intervals themselves. The structal intervals are marked (calibrated) with measured, not registered, values.

In an optical rangefinder, for example, images from two sight-lines at either end of the instrument are brought together by reflection to the eye of the Reader, in such a way that his adjustment of a marked wheel in matching the images <u>registers</u> the <u>angle</u> between the sight-lines. The marks on the wheel, as it is adjusted, sweep out a circular arc aligned with a graticule, in such a way that the position of the graticule with respect to the marks <u>registers</u> the angle; but the marks are so <u>calibrated</u> that the position of the graticule is read as an <u>approximate distance</u> from the instrument to the datal mark whose two images are matched by the Reader. The calibration - i.e., the placing and numbering of marks - of the instrument is carried out in terms of measured distances.

Note that the two quantities are not simply related: the registered differences in angle for equal differences in measured distance are not equal angles. (Strictly, what are registered - as with all measurement of angles - are intervals of circular arc: the incorporation of simple Euclidean theory is obvious.) The rangefinder may be calibrated <u>either</u> by matching its readings with values of distance obtained by alignment-systems; or, more likely, by calculating the appropriate angles for various distances using the principles of the incorporated S-theory.

<u>Calculations</u> used, as in this case, in determining the positions and values of structal marks for calibration in an indicator-system, are to be clearly distinguished from calculations made on registered <u>or</u> indicated values,

after readings have been taken. The results of the second kind of calculations are classed not as readings but as findings from readings, though in common usage such findings are often called 'measurements'. In the above example, distinguish either (a) the determination of the same distance, not by the rangefinder, but by measuring the angles subtended by the object at each end of a base-line of measured length, and calculating the distance from these readings; or (b) the determination of the distance between two remote objects by measuring the distance to each by rangefinder, and the angle between the sightlines, and calculating the required distance from these datal values. In case (a), triangulation-theory is used in the calculation but not incorporated in the measurement system; in (b), it is both used in calculation of findings, and incorporated in the indications of the rangefinder from which readings are taken. These further distinctions elaborate somewhat the points made above about the conflation of concepts in Campbell's terminology of 'indirect measurement' (p. 291 , note (1)). Such distinctions may become important in analysing the theoretical structure of many more sophisticated cases of interrelationships between registered and indicated values and substantive theories, and findings calculated from them.

Synopsis of Further Work

(1) A main purpose of further exploration of the variety of incorporated S-theories, and associated procedures,

for the measurement of spatial intervals, would be to bring out the point that the level of sophistication depends less on the type of measured quantity (here simply 'length' or 'distance') than on the remoteness of the context of attention from ordinary human modes of perception.

(2) The following examples of spatial measurement systems are among those cited:

Type of system	Order of scale (approx.)	Incorporated theories include:	Means of reading or obtaining values
(i) Increasing distances			
(a) Radar	Up to 250,000 miles	Electronic information theory and speed of radiation	Direct reading of deflection of oscilloscope beam, indicating time-lapse, indicating distance of object
(b) Triangulation on base of earth's orbit	Up to 250 light years	Relativistic geometry of orbit and recorded light	Calculation from recorded angular readings
(c) Spectroscopic	? million light years	Expansion of universe and red-shift effect	Calculation from spectral line displacement (tables)
(ii) Decreasing distances			
(d) Micrometry: Vernier screw	10^{-5} m.	Geometry of micrometer	Lateral and angular displacement of screw head (direct reading)
(e) do. by diffraction gratings	10^{-6}m.	Geometry of micrometer	Electronic counting of 'moiré fringes'
(f) Interferometry	10^{-10} m.	Light interference	Electronic counting of interference fringes

(3) The following general observations, among others, are based on discussion of these cases:

(a) Use of photographic and other recordings (e.g., case (b)) or electronic counting (cases (e), (f)) require a small conceptual leap for C-theory, in that they replace human perception by mechanical devices at various points in the system. But the essential limitations on the performance of these devices differ from those on human perception only in scale, and they exist so to transform their "recognitions" that they can be read by people. Again, the theoretical commitments by virtue of which they "recognize" certain aspects of their input as "significant", and "neglect" others, are of course built in by human designers. The notion of a 'comparison figure' needs elaboration to embrace these complexities. It is to be elucidated, in general, by reference to physical analogue relations supposed to obtain between variations of the measured quantity and positions of standard variant, by virtue of physical linkage.

(b) Below the limits of scale considered above, we now predict that a succession of limits will be reached as we approach subatomic scales, beyond which, first, all indicator systems will break down for lack of any form of physical linkage, yielding an analogue; and, second, S-theoretical models of space-time structures, used in calculating spatial intervals, will progressively lose application to the point at which, thirdly, our theories of space-time itself lose application in the domain of particle physics.

(c) Our examples exhibit a huge range of 'standard units' from parsec (order 10^{16} m) to angstrom (10^{-10} m). In each context, measurement restricts attention to a small number of 'significant figures', in decimal terms. Each decimal step may be considered as marking an 'order of magnitude' in terms of the chosen unit. Regarded as setting limits to regions of theoretical attention, these 'orders of magnitude' can be interpreted in terms of the C-theoretical notion of C-scale, associated with that of 'proper enclosure'. Their actual quantitative expression in decimal terms must be supposed to have biological origins; they obtain independently of whether or not the units used, or the mathematics of computation, have a decimal structure. The notion can be generalised, by analogy, for all measured quantities, unless special theoretical considerations apply.

M. Temperature (Synopsis)

(1) Temperature is taken as typical of a large class of physical quantities for which no comparison figure is recognizable, in terms of the quantity itself, capable of generating a marked interval sequence which can be put into correspondence with a metric structure. For this class of quantities, one or more indicator systems have been developed such that, by incorporating S-theory, variations of some other quantity - usually spatial intervals - for which

a comparison figure is recognized, can be read as analogues of variations in the quantity to be measured. We have seen that this principle applies to the measurement of length/distance over certain ranges; but temperature seems the simplest and most illuminating example of a quantity whose measurement calls for the application of this principle over its whole range. It is also a quantity associated closely with a directly perceived comparison figure - that for values of "felt heat, warmth, cold", etc. But it is argued that what is felt is not temperature, but heat gain or loss at the skin surface (important for the survival of the organism). Felt warmth etc. do not in fact yield an unequivocally transitive ordering with respect to that of measured temperatures. Different methods of temperature measurement can, by contrast, be adjusted to show transitive orderings with respect to each other (within accepted tolerances) over regions of overlap between their ranges: a precondition of their being understood as measuring the same quantity.

(2) The history of temperature measurement is used to show how increases in the precision of measurement march step by step with the development of theory: with theory always one step ahead, defining what is to count as precision - and deepening understanding of what thermometers measure. The contributions of Boyle, Bernoulli, Charles, Gay-Lussac are cited; but the crucial step was the interpretation of heat as a form of energy (Joule), leading to that of temperature in terms of thermal equilibrium.

(3) Almost all systems of temperature measurement rely on setting up thermal equilibrium between the measured body and a standard variant linked to visual SIS (scales). Standard variants exploit 'thermometric properties' of various substances or systems: amongst those cited are:
(a) Expansion systems using liquids, solids (e.g., pairs of metal strips in contact), or gases at constant pressure;
(b) Pressure systems using gases measured at constant volume;
(c) Electrical resistivity systems, registering the varying resistances of conductors of chosen materials with temperature;
(d) Electrical potential generated at interfaces between different metals;
(e) Change-of-state methods registering relative amounts of liquid and vapour in closed systems;
(f) Systems registering the varied magnetic responses of paramagnetic salts;
(g) Various methods of registering the frequencies and amounts of radiation given off, mainly at visible wavelengths, called 'pyrometry'.

Types (e) and (f) have been developed for very low temperatures, and (g) for very high temperatures; but the other methods overlap over very large ranges.

The fundamental problem in calibration (constructing standard sets of marked intervals) is that you cannot strictly compare intervals at different temperatures. Repeated operations generating similar amounts of heat, progressively raising the temperature of a measured body, is a good procedure, but ultimately not good enough in theoretical terms. The same may be said of the search for correlations and comparisons between the results of different systems: but assumptions of random deviation have, of themselves, no sound foundation (see under 'probability', below, Section P). Finally, confidence in the validity of temperature measurement is to be sought in the coherence of the S-theory which is used to explain them. The last step cited is Kelvin's interpretation of heat transference in terms of the ideal 'Carnot cycle', as it affects the understanding of temperature values. All systems can, by application of this principle, be shown to be more or less in error, and their particular errors assessed against the theoretically 'predicted' shapes of phenomena. Fortunately the performances of actual gases under method (b) above are close enough to that of the 'ideal gas' (as analysed by Kelvin) to give expression to this confidence in practice, over its range from about 2° to 500°K (approx.); though the method is not itself practical for general use, and is used to check and correct an 'international practical scale' based on a combination of electrical devices (extending from about 90° to 1300°K).

(3) This account is contrasted with KLST's naïve appeal to "laws of similitude", which reflects their disinterest in epistemological concerns. The notion of such "laws" is hard to interpret in terms of actual theory, and is absent from metrology, which is the practical expression of the epistemologist's search for sound foundations in measurement theory.

(4) Discussions of measurement beyond the region 2° to 1300°K (cryogenics at the lower end, pyrometry and astronomical methods at the other) cannot be summarised here, in spite of their special interests for C-theory: nor can accounts of our understanding of the discrepancies between real and ideal gases. It is noted that, once more, as we move towards extreme values, direct readings from indicators are replaced by the use of mechanical "recognition" systems and marked by increasing use of calculation from registered values. Also, since all temperature measurement depends on conditions of relatively stable thermal equilibrium, all transient temperatures are necessarily obtained by calculated extrapolations within sophisticated theoretical models.

(5) Heat, then, is a form of energy, distinguishable in practice by its effects on the 'thermometric properties' of systems, measured as temperature. It is tempting to consider a possible generalisation from this principle,

according to which all systems of physical measurement are specialised ways of recognizing the structures of energy in matter, the forms of such recognition being restricted by the available means of organizing perceptible effects. It is at least clear that all recognition involves transfers of energy of some form between a recognized and recognizing system - a point which finds special expression in the uncertainty principle of Heisenberg. These more fundamental aspects will be given further consideration later.

N. Time (Synopsis)

(1) At one level, time appears as a quantity readily measurable by a wide variety of indicator systems, which provide analogues of what are recognized as intervals of time, registered in the form of spatial intervals according to the normal principle just exemplified for temperature. At a more profound level, it is a focus of (appropriately) perennial metaphysical problems, classically seen in terms of reconciling the notion of time as a continuous flow, with those of instant or duration; or with the experience of time as an all-embracing medium, through which one appears to move at varying speeds, encapsulated in an ever-changing "now", where an ever-receding past is never quite invaded by a relentlessly arriving future. C-theory is in principle neutral on these deeper questions, but our understanding of measurement has relevance to the "now" concept, through its involvement in relativistic accounts.

(2) Time plays a dual role in the C-theory of the structure of recognition: as a quantity measured by the recognition of associated comparative and metric structures, used as indicators; and as part of the structure of the basic R-theories. To measure time, we must choose, or build, some regular sequence of events on which we can raise the necessary commitments under which it can be adopted as structal for the context. Datal intervals are then recognized by the approximate coincidence of their end-marking events with pairs of events marking the boundaries of structal intervals, by virtue of the comparison figure adopted for the context, and the associated tolerance rules. The events which mark frame boundaries - datal or structal - are changes in character-assignments (including those of positional characters in various figures). If successive structal intervals are to be distinguished, the characterizations of their start- and finish-boundaries must be different from one another. If there is no intrinsic difference - as in the case of atomic oscillations - the mark-events must be distinguished by their relations to some other, differentiated sequence of frame-marking character-changes - such as those generated by a quartz-regulated electrically-driven clock, as in the 'atomic clock'.

So, even at this abstract theoretical level, recognizable changes within successive frames, not counted as frame-changes, are necessary constituents of any comparative structure for time (for the recognitive status of such changes, see note (e), p.154). Such changes are to be termed 'T-processes'. An obvious illustration is the change of position of the hand of an ordinary clock in moving from one minute-mark to the next. A sequence of such T-processes characterizing successive m-intervals of a structal system for time measurement is to be called a standard T-sequence. or STS. Each (least recognized) m-interval of an STS is thus bounded from the next by a frame-boundary recognized as a shared end-mark. All longer structal time-intervals in the same context are bounded by pairs of these end-marks, and recognized as composed of the set of intervening m-intervals, by analogy with spatial SIS. Datal time-intervals are approximated to structal intervals of the context, as stated above.

It has proved possible to reconcile this account with a wide variety of instantiating contexts. (Interesting special cases like that of archaeological time-measurement must be omitted from this synopsis.) Concepts of time as a continuous flow or medium, of which m-intervals are seen as subdivisions, are shown to belong to S-theoretical extrapolation. As with all 'marks' for measurement, the 'mark-events' of STS are distinct from the intervals they mark, and so do not affect the issue.

(3) Two kinds of T-process have figured largely in the development of time measurement: natural periodicities, and mechanical systems. The function of any T-process is seen as the 'metering-out' of successive, equal amounts of energy in work on repeated operations, as precisely similar as possible. Astronomical processes do this, as it were, naturally. The important step in mechanical systems is the production of the 'escapement' (first devised for water-clocks in China before 1100 AD): which allows relatively large, energetic systems to be regulated by systems of relatively small mass/energy. The resulting process is relatively robust with respect to perturbations (which affect all such systems more or less). Unlike temperature, and most other 'indicated' quantities, there is no indication that repeated operations have different effects over different intervals (a factor which is associated with the observation that for time, as for space, there is no natural resting-place for the metric 'origin').

Until recently, all precise measurement of small intervals has used the theory of mechanical systems themselves to determine their own standards of precision; but now, these too are based on a group of natural periodicities - frequencies of electro-magnetic radiation (EMR). Another natural periodicity, the resonances of quartz crystals, is used to provide the energy-generating system to which the EMR frequency acts as "escapement". But astronomical processes - involving the largest distributions of mass recognizable as belonging to the same entities over periods of time - remain the ultimate sources of metric standards, even for 'atomic clocks'. The second is now defined as 1/31556925.9747 (precisely) of the standard 'tropical' year.

It remains to ask what is being measured: plainly not the amounts of energy expended in the T-processes, which vary so much as between systems. Our best answer comes from relativistic accounts (see head (5) below).

(4) Conceptual confusions associated with the "spatialisation" of time (which, it is argued, are responsible for most of the ancient puzzles) are at least partly resolved by C-theory. The detail cannot be given here, but the main point is that attention to distinctions of theoretical level, parallel with those already urged with regard to the spatialisation of quantities in general, assists the process of understanding. Spatialisation is a common feature of indicator systems for all physical quantities: spatial (e.g., graphic, or analytical) representations are also common in association with S-theoretical accounts of all types. Primitively, the Greeks just did not have the means to distinguish short intervals of time from the spatial distances covered by moving objects; nor the 'mark-events' bounding them from the physical marks reached or passed.

(5) It turns out that C-theoretical analysis, with its emphasis on the relativism of recognitions to the individual Reader, is well suited to the discussion of the epistemology of relativistic accounts - rooted as they are in the interpretation of metric recognitions. The word "accounts" here emphasizes the point that the introduction of relativity theory does not affect our analysis of the recognition or measurement of time or space intervals in any fundamental way. What it does is to modify the structure of associated S-theoretical commitments which, at most, may lead us to accept one alternative reading rather than another as more precise in a particular context. Philosophically, a radical effect of this modification is its abolition of the absolute framework of universal time, and in particular of the universal "now".

Sophisticated argument - not readily summarised - shows how the literal interpretation of a Reader or 'observer' as a particular human perceiver can readily be generalised, under certain S-theoretical restrictions, in terms of metric 'origins' to which structures of EMR (typically, light) can be referred in temporal or spatial terms, in such a way as to accommodate a Minkowskian interpretation. Importance is attached to a formulation by Milič Čapek (in Fraser, J.T., ed., 1968) in terms of 'world intervals' associated with 'world lines': and its interpretation in an entropic framework, at least for macrophysical systems. What emerges, however, is a relatively simple idea: the proposal that what is "metered out" by T-processes is a structure of intervals of EMR - for which the natural frequency-based metric is almost obligatory. For a particular 'observer', what count as temporal intervals are those of EMR arriving 'successively' from what is recognized as "the same source" (or sources along the same geodesic line). The notion of energy-transfer from 'recognized' to 'recognizing' system (mentioned above, Section M(5)) is central to this account. Geometry (ultimately relativistic) provides the S-theoretical structure by which spatial intervals are recognized in terms of EMR received from different sources at the same time. This answer removes some of the fundamental questions from the domain of metaphysics.

O. Special Cases in Physical Measurement

(1) The main purpose of this Section is to consider real or apparent exceptions to the dominant role in physical measurement of indicators using marked spatial intervals, as predicted by C-theory. It is emphasized that this prediction, which is not absolute, rests only on observation of the contingent restrictions imposed by the limitations of human perception, and not on fundamental properties of measured quantities. This accounts, too, for the associated dominance of incorporated S-theory.

The cases considered are mass (or weight); hardness; and the relationship between types of colour measurement and that of frequencies of light.

(2) Mass, or weight, can be measured in many conditions with an equal-arm balance, using the principle that for these quantities the measured value of a whole set of idents (suitably composed) can be taken equal to the sum of the values of the members of the set (space- and time-intervals being, almost certainly, the only other quantities for which this obtains).

This system is compared with others used for the same purpose, but without using this principle: the sliding-weight balance, the spring balance, and centrifuges or oscillators used in the absence of a suitable gravitational field. Many factors of interest are involved, but the main point made is that mass is not recognized perceptually as such, only registered in terms of 'forces' (natural or artificial) operating on it. (In this, it is distinguished from 'weight' - which is consequently a vague and "impure" concept. The use of the term 'force' is taken up again in Section Q.) From this point of view, it is seen that even the equal-arm balance makes use of the equal lengths of its arms to indicate equal moments (incorporating a simple S-theory familiar to Aristotle). It can thus be most perspicuously analysed as a special case of the sliding-weight balance, in which the standard weights, rather than being slid to vary the distance-component of the moment, are constructed to give as many distinct values of the mass-component as may be practical or necessary in context. The exception, however, is real, to the extent that it incorporates the 'additivity' of mass, and hence of gravitational 'force', in its S-theory.

(3) Hardness is often cited (E.G., by KLST, 131) as a physical quantity whose measurement yields only non-additive values - sometimes called a system of 'ordinal measurement'. Criticism of this concept was given above (p.244), making the point that where the only relevant property of assigned numbers is their order, an alphabet would do as well, negating the essential purpose of measurement. This account of hardness measurement rests on 19th-century work by Mohs, which could not assign definite approximate values to datal samples of materials, and has apparently had no practical use (as C-theory would predict).

Systems (not cited in the foundational literature) have since been devised, starting at the turn of the century, which do employ indicator- mechanisms, do yield values which are effectively additive over short ranges, and are used in practical engineering contexts. It has to be admitted that the levels of S-theoretical interpretation in these contexts are not very sophisticated. Each type of instrument covers only quite a narrow range of materials - which is generally adequate for practical purposes. However, quantitative correspondences between the readings of different instruments in overlapping parts of their ranges are close enough to justify a comprehensive system of conversion tables, to two or three significant figures. The situation is in many ways similar to that of temperature measurement, up to quite a late date. What is missing is any S-theoretical development, parallel with those of Joule or Kelvin, on the basis of which we could say which instrument, of two giving readings for the same set of material samples, is the more precise in relation to some underlying concept of "what hardness is". But some aspects of the subject do seem to suggest lines of advance. The variation of hardness with temperature, for instance, is quite precisely measurable for some materials, using the existing systems. It shows a particular pattern of correlation which closely follows that for the variation of hardness with tensile strength. This last correlation is precise enough for hardness to be used in some contexts as an indicator of tensile strength, where measurement of the latter as such is impracticable.

Principal sources have been Dieter (1961) and Tweedale (1964).

(4) (i) Colour measurement is of philosophical interest mainly because of debates about whether colour is an external, inherent property of objects, or an internal, subjective phenomenon or state-of-affairs constituted by perception. Criticism is given elsewhere of arguments for the first position from 'ordinary language' (pp. ,). But these are sometimes backed up by suggestions that there is some scientific explanation (not the concern of the philosopher) in terms of which, e.g., what we see as "red" is identical with, or corresponds to, a uniquely describable physical structure or attribute which is the same for all objects we see as red. Such suggestions are decisively contradicted by the structures of theory developed in association with the practice of measurement, on the one hand, of colour, and on the other, of properties of light as generated by, or reflected from, objects. The subject is intricate (and might intimidate non-technical philosophers), but a selection of simple, bald observations, each uncontroversial in its own context, may be enough to indicate the indeterminacy of the relationship between external stimulus and internal response.

(a) The recognition of spectral colours is irrelevant for the recognition or measurement of frequencies of light - a black-and-white photograph of a spectrograph reading contains all the information required.

(b) The colour associated with any given frequency can be matched exactly in an indefinitely large number of ways, including many using little or no radiation at that frequency.

(c) The great majority of colours actually recognized are not associated with single pure frequencies, but with mixtures of many frequencies; and any such colour can be (ideally as well as practically) matched by an indefinitely large number of mixtures of different frequencies.

(d) The validity of experimental results and systems of measured tests carried out with systems of colour analysis or reproduction is not critically dependent on any theory of human colour vision: and, in any case,

(e) while the physical analysis of retinal response to light of different frequencies provides us with an S-theoretical account of colour vision, colour as such does not enter into the associated investigation.

The essential point is that human colour vision depends on the relative responses to light of three sets of cells in the retina; each set responds to a wide range of frequencies, without differentiation except in terms of the total electrical response of that set. That is why we can say that precisely the same colour can be experienced as the effect of responses to an indefinitely large number of different frequency-combinations, provided the total response elicited from each set of cells is the same: "pure" spectral colours can be mimicked as well as any others.

This is the basis of colour reproduction, e.g. by photography or television. Such systems make use of sets of 'primary colours' (or 'complementaries') - chosen entirely with regard to the practicalities of each particular system. As we have seen, our own colour responses make no use of primary colours: their distribution over the visual band is also far from well-balanced - which is why in general yellows "look brighter" than blues.

(ii) When we turn attention from measurements of light-frequencies or retinal responses to attempts to measure values of colour itself, we find again that the most empirically adequate representations appear as three-dimensional. But the three "dimensions" are quite different from those of the trichromatic analyses we have been speaking of: typically they are 'hue'; 'lightness' (or 'value'); and 'chroma' (or 'saturation'). Values are obtained by visual comparison between datal and standard samples, the latter themselves the products of subjective comparisons by panels of judges. Different systems are used in different contexts, but most are now based on an analysis by the painter A.H. Munsell in 1907, which employs a range of 100 standard 'hues', constructed on the principle of 'least noticeable differences' at similar values of lightness and chroma. This principle is familiar in schemes of psychological quantification; being explicitly acknowledged to be founded on subjective reports, it is clear that the Munsell test is indeed such a scheme. Questions concerned with how far such schemes are properly classed as 'measurement' in the same theoretical sense as physical measurement are left for Section R. Here, we note only that the values are in no sense "additive". No mathematical operations can be significantly carried out on them. They are effectively no more than catalogue index numbers.

(iii) The relevance of these considerations to the fundamental question mentioned at the start of this subsection (4) is not so simple to analyse. There seems little doubt that the consciousness by which we recognize colours as characters of particular idents outside the body is located in the brain. This consideration applies, of course, to our recognitions of any properties whatever of external objects, or of these objects themselves, by any sensory means: it is not a peculiarity of colour. C-theory has little to add, beyond asking the question: if we can extrapolate the position and boundary of the back of a house by virtue of what we recognize of its front, why not the position and boundary of the front also?

(The main sources for this subsection are Cornwell-Clyne (1951) and Pirenne (1967).)

P. Probability

(i) Theoretical structure and general principles

A substantial part of the literature on this subject proceeds on the supposition that, since relatively reliable assessments of the probabilities of certain types of events can be reached on the basis of empirical observations, it is proper to regard such assessments as a form of measurement, and probability itself as a measured quantity.

This supposition is a feature of two clearly distinguishable approaches, sometimes adopted together: a pragmatic approach, which points to the existence of many contexts associated with counting or measurement in which numerical values assigned to probabilities prove empirically adequate within tolerable limits; and the search for a rigorous, typically formalised articulation of the logic or syntax of statements about probabilities, in a way which yields numerical representations. The first approach concerns itself primarily with the mathematical theory involved in methods of calculating probability-values of complex states of affairs from those of simple events. In the second, philosophers have sought to elucidate the notion of probability as a rational concept, by showing what logical or syntactic properties probabilistic statements are to have, if they are to satisfy the axioms of a mathematical theory. My analysis will suggest that each of these approaches tends to obscure an important distinction in theoretical level between measurement and the assessment of probability. Since this proposal places such assessment outside the main scope of this study, I shall not discuss the logic or mathematics of theories of probability, except in the most general terms. However, probability theory is closely bound up with many contexts of measurement, where it is commonly associated with the theoretical treatment of statistics - the line between the two concepts being variously drawn. In such contexts it can be regarded as a precondition, not of measurement, but of successfully calculated extrapolations from measured values. A conceptual analysis will therefore be given in which the relationship in different contexts of probability between different theoretical levels, as understood in C-theory, will be indicated.

The essential principle of this analysis is that probability is an abstract, <u>relational property</u> of pairs or larger sets of <u>theoretical models</u>: possible structures of extrapolation, from partial evidence, under the relevant theory of the context. "Possibility", here, is understood in terms of consistency with the commitments of a theory. This property of probability is understood in terms of <u>strengths of commitment</u> to alternative extrapolations; and to take numerical, in some sense"objective", values only where the relevant theory yields, by extrapolation under its commitments, clearly articulated alternative models, for which equal (or rationally assessed) relative weights, or strengths of commitment, are adopted. It will be argued that the resulting account is both simpler and more unified over the whole field, without sacrificing rigour; at the same time raising little or no essential conflict with established probabilistic or statistical theory, except in the analysis of theoretical levels and the consequent epistemological indications.

The concept of probability has generated some confusion amongst philosophers and other analysts; not only over the question whether or not it is a measurable quantity, but also over (a) how many kinds or categories can be distinguished, and on what terms, and (b) the related question, what kinds of entities or structures are the objects of assessment for probability. Under head (b), Brian Ellis lists "events, states of affairs, theories, propositions, etc." as amongst possible objects for assessment (1966, p. 165). His "etc." might have included such entities as values, errors, or statistical deviations, whose probabilities are often assessed in practical contexts. Under head (a), we can, I suggest, assimilate most available typologies in terms of three vague categories: (1) intuitive, non-quantitative, or subjective probability; (2) logical, mathematical, or analytic probability (Carnap's probability$_1$); and (3) empirical, statistical or synthetic probability (Carnap's probability$_2$, which he identifies with a 'frequency concept' (1950)). Often these different categories of probability are thought of as relating different categories of object. Carnap, for example, considered his probability$_1$ (P_1) as governing propositions, and his probability$_2$ (P_2) as a theory of the relative frequencies of specified types or classes of events; but he also felt the need to distinguish a theory of estimation (of values of quantities), based on his analysis of his P_1 (op.cit., and Schilpp, 1963, p. 74). Some current analyses assume that all categories of objects of probability-valuation can be reduced to events, on which a set-theoretical algebra can be constructed (e.g. KLST, pp. 199 ff.). But this term requires very broad interpretation, if all or most contexts in which a notion of probability is invoked are to be covered. I shall try to show in due course how some of these different approaches can be reconciled with a single account of probabilistic contexts, without any loss of rigour in the analysis of the particular types of context described under these special categories. But first I must give a fuller analysis of probability in the terms of C-theory itself.

I propose the following semi-formal definition of a unified concept of probability, based on a prior definition of a structure of extrapolative models with respect to any (G)R-theory (where '(G)R-theory' may refer either to a GR-theory, or to the R-theory of a single Reader, whether or not it forms part of a GR-theory). For simplicity of exposition I shall define the proposed structure with respect to the state of a given (G)R-theory at a given frame of its R-sequence, leaving it to be understood that, as with (G)R-theories in general, the commitments of the theory and its associated models may change over time with the 'state-of-the-theory' (above, p. 149).

P1. (i) By an extrapolative set (E-set) of E-models with respect to a (G)R-theory Θ, we are to understand a set $\Theta^X = \{E^{X,1},\ldots,E^{X,n}\}$, having at least two members, and satisfying the following conditions:

(a) Each $E^{X,k} \varepsilon\ \Theta^X$ has a theoretical structure to a set $\mathcal{S}^{X,k}$ of entities; a set $\mathcal{C}^{X,k}$ of properties; an assignment function $\theta^{X,k}$; a logic identical with that of Θ; a consistent set of theoretical commitments defined on the elements $\mathcal{S}$ of idents and $\mathcal{C}$ of characters in Θ; and a frame-sequence structure defined on that of Θ.

(b) $\mathcal{S}^{X,k}$ is a non-empty set, each of whose members is either an ident of $\mathcal{S}$, or an entity not in $\mathcal{S}$ called an X-ident, whose existence for $E^{X,k}$ is extrapolated under a commitment of $E^{X,k}$.

(c) $\mathcal{C}^{X,k}$ is a set, having at least two members, each of which is either a character of $\mathcal{C}$, or a property not in $\mathcal{C}$ called an X-character, whose existence for $E^{X,k}$ is extrapolated under a commitment of $E^{X,k}$.

(d) Each $E^{X,k}$ of Θ^X may be called a possible model, or structure of extrapolation, under Θ; and each member x of an E-model $E^{X,k}$ is assigned a unique subset of $\mathcal{C}^{X,k}$ given by $\theta^{X,k}(x,i)$ for each frame F_i^X for which x exists.

(ii) An E-set Θ^X under a theory Θ is an alternative E-set (AE-set) iff the commitments of the members of Θ^X are such that no two such members are logically consistent with one another.

(iii) Probability is an abstract valuative property (the term 'valuative' being used analogously with its use for characters in R-theories) whose values, called p-values, may be assigned, by any Reader $\mathcal{R}$ of the L-group adopting Θ,

(a) to the members of any AE-set Θ^X of Θ, so that the p-value $p(E^{X,k})$ of each such member $E^{X,k}$ for $\mathcal{R}$ is to reflect the strength of $\mathcal{R}$'s commitment to the particular structure of extrapolation from Θ which $E^{X,k}$ represents; or

(b) by virtue of assignments under (a), to any simple or compound assignment determined by any member or members of Θ^X. (By 'compound' assignment is meant any structure of conjunctions or disjunctions of simple assignments of the form $P \in \theta^{X,k}(x,i)$ or negations of the form $P \notin \theta^{X,k}(x,i)$. For considerations affecting the assignments of p-values under (a), or relationships between such assignments and those under (b), see notes below.)

(iv) The following relations are introduced, as an exhustive set in this analysis, on pairs of p-values of E-models or assignments in $(\Theta^X)^2$; to be understood, by analogy with recognitive valuation, as reflecting the judgments of the relevant Reader(s) as indicated:

$p(E^{X,j}) > p(E^{X,k})$, as reflecting the judgment that $p(E^{X,j})$ is <u>greater than</u> $p(E^{X,k})$ (taken to be logically equivalent to $p(E^{X,k}) < p(E^{X,j})$);

$p(E^{X,j}) \overset{o}{\sim} p(E^{X,k})$, called <u>ambivalence</u>, as reflecting <u>indecision</u> by R as to the relative strengths of commitment involved; and

$p(E^{X.j}) = p(E^{X,k})$, as reflecting R's accord of <u>equal</u> strength of commitment.

The relations $>, <, \overset{o}{\sim}$ are taken to be in principle to be determined for all AE-sets for which p-values are assessed; the first two being asymmetric and transitive for each Reder. $\overset{o}{\sim}$ is taken to be symmetric for each Reader, but is not assumed transitive in the general case. = is undetermined in the absence of additional special commitments, to be discussed in note (e) below.

(v) An AE-set Θ^X of Θ is <u>closed</u> (and called a <u>CE-set</u>) iff all cases where no member of Θ^X holds are <u>neglected</u> under the commitments of Θ and Θ^X. An AE-set which is not closed is <u>open</u> and called an <u>OE-set</u>. It will be assumed that the p-value of a (simple or compound) assignment $\underline{a}^X$ is that of a disjunction of the set $E^X(\underline{a})$ of all E-models in which it holds; and that, when Θ^X is a CE-set, the p-value of the <u>negation</u> of $\underline{a}^X$ is that of a disjunction of the complement of $E^X(\underline{a})$ in Θ^X. In an OE-set, the p-values of negations are not in general determined.

Notes

(a) The assignment of p-values in any context will be called p-valuation. Any terms not explicitly defined above are to be understood in the sense defined for GR-theories in Section G, pp. 174 ff.

(b) For simplicity, I shall consider in the notes which follow only the structures of relative p-valuation of models and assignments for a single frame F_i^X of the (G)R-theory (but see note (f) below).

(c) This is an explicitly psychological account of probability, but psychological in exactly the same way as are the C-theoretical accounts of recognition and measurement. So, although it is acknowledged that the p-valuation of any AE-set by any Reader is dependent on psychological factors unique to that Reader and context, variations due to these factors may in many contexts be neglected in an analysis of the valuation under an appropriate metatheoretical description. Like the C-theoretical account of recognition, this expresses a fundamentally empirical observation respecting the actual behaviour of people in the relevant contexts.

(d) Following the proposed analogy with recognitive valuation (Condition (iv) above) I shall assume that the simplest and most fundamental forms of p-valuation for E-models or assignments are not numerical, or readily provided with a significant numerical representation - any more than are our commonsense, unmeasured judgments about the relative sizes of animals or weights of rocks. Our means of judging relative p-values are fundamentally introspective rather than perceptual (even where they are concerned with extrapolations for recognizable elements), and are thus less commonly decisive than those for recognitive valuation, except in special cases: some of which are considered in the next note.

On the same principle, I shall not suppose that they typically determine a unique, unequivocal order over the whole of any particular AE-set or its associated assignments. Thus the relation of 'ambivalence', $\underset{\sim}{\circ}$, is intended to reflect large class of cases in which a Reader is unable to decide between the relative strengths of commitment appropriate to a given pair of E-models or assignments. (The class may include cases which are considered in standard analyses under the head of 'indifference'. But any commitment to transitivity for such a relation seems to me to call for additional special conditions which I shall not explore, apart from the special case of equiprobability below.) No <u>general</u> rules hold as between p-values assigned by different Readers; the fact that two people disagree about the relative p-values within an AE-set does not in general lead us to say that they are operating different theoretical contexts, nor that their views are logically inconsistent (there is nothing that one holds unequivocally true and the other false).

Two assumptions of general interest do, however, remain plausible even under the very weak conditions I have supposed for the general case. The first is the transitivity of the relations $>, <$, reflecting unequivocal judgements of relative p-value by the individual Reader. The second can be expressed as the principle that the p-value of a disjunction of a set of E-models is in some sense the same as the combined p-values of the members of the set. In the absence of numerical values we cannot speak of the "sum": though this interpretation becomes available wherever numerical values can be assigned. Both the above assumptions can, however, be given clearer meaning in the simple and common case where each member of an AE-set extrapolates a single, simple assignment: for example, alternative weather forecasts for a particular time and place. In such a case, it seems immediately rational to equate the p-value of each assignment with that of the E-model in which it holds (the epistemological significance of the E-model being that it provides the theoretical commitment under which the assignment holds). The transitivity of $>, <$ on p-values can in such cases be simply transferred from the set of E-models to the set of assignments (in a way which would not be possible for simple assignments which are components of compound assignments associated with E-models). Also, the p-value of a disjunction of assignments can be assumed to be that of the combined p-values of the separate assignments. In conditions where the AE-set can be taken as closed (A CE-set, under condition (v) above), this conclusion can be given even clearer meaning; in that the p-value of the negation of any disjunction of a set of assignments is that of a disjunction of all the remaining assignments. This can be given its sharpest meaning for ordinary intuitive contexts in the observation that the p-value of the negation of any one of a closed set of alternative assignments is that of a disjunction of all the others; especially where one assignment is taken to be clearly more probable than all others put together (in which case the other p-values can, often, be neglected).

It remains important to note that the notion of the negation of an assignment, excluded by C-theory as a form of direct recognition, is also permissible here precisely where, and only where, it is adopted as a consequence of the commitments of the theory as applied to other, positive assignments of the context.

Where E-models determine compound assignments we may suppose that any one simple assignment $\underline{a}^X$ (of the typical form $P \in \theta^X(x,i)$) may hold in more than one E-model as an element in a conjunction. In such a case, the p-value of this assignment may plausibly be supposed the same as the combined p-values of all the E-models of the set $E^X(\underline{a})$ in which it holds. But $\underline{a}^X$ may hold in two models $E^{X,j}$ and $E^{X,k}$ such that $p(E^{X,j}) > p(E^{X,k})$ - so that no simple relationship exists between the p-values of all assignments of Θ^X and those of its E-models. We may nevertheless reasonably hold $p(\underline{a}^X) > p(\underline{b}^X) \leftrightarrow p(E^X(\underline{a})) > p(E^X(\underline{b}))$, and similarly for $\underline{\Omega}$ (so long as neither assignment is involved in a disjunction in any model, in which case the relationship is unclear). Sense can also be given to the p-values of some negations where Θ^X is closed. The more complex the AE-set, and the ambivalences or disjunctions of assignments in models it involves, the less clear is the meaning to be given to such formulations. The lines between confusion, guesswork and rational judgment seem impossible to draw, the only clear distinctions being, on this view, those indicated in note (e) below.

But first, a simple illustration may help to clarify what has been said so far. Suppose that in a simple language of weather forecasting only the following characterizations of weather are assignable to a particular region at a particular time: s = sunshine, r = rain, n = snow, f = fog, c = cloud, excluding cases r,n, and f. A probabilistic theory using such a language constitutes a CE-set iff all forms of weather-forecast are either forced into these categories or left out of account. If not (the OE-set case) it makes sense, for example, to set (for a particular place and time) $p(r) > p(n)$ and $p(c) > p(r)$ - whence $p(c) > p(n)$; but not to say, "It will probably be cloudy", if what is meant is that

$p(c) > p(s \text{ or } r \text{ or } n \text{ or } f)$; since we have not ruled out the possibility that, say, hail, which has not been classified, might either attract a greater strength of commitment on its own, or, together with other conditions, provide a disjunctive alternative which would tip the balance of commitment. For the closed case, we must suppose that all assignments such as that of hail are either forced into one of the chosen categories (say, rain or snow), or considered so rare as to be properly excluded from our forecasting theory. In such a theory there is some sense in a forecast of cloud as more probable than all the others put together; but it remains a somewhat vague and intuitive theory, corresponding rather to the oracular pronouncements of a weatherwise old shepherd than to a public weather forecast. If such a forecast were to be printed in a newspaper, we would have a right to suppose that it is based on a well-articulated and sophisticated system using numerical p-values based on accurately measured observations, of pressure, temperature, wind-speed, etc., within which strengths of commitment to a wide variety of complex conditions can be clearly expressed and assessed. Some possible preconditions for numerical p-valuation will now be considered.

(e) In spite of the similarities between p-valuation and recognitive valuation which have been invoked above, there is one aspect of the theoretical structures involved for which the analogy crucially breaks down, when we come to consider questions of the significance of numerical values, or even determinate order. In recognitive valuation, we found that determinate orderings were available only for specially constructed standard sets, with respect to which approximate sets could be assembled into associated clusters for each standard value: numerical values being confined to metric structures for which additional stringent conditions were imposed. Plainly, none of these structures are available for the valuation of strengths of commitment to E-models or their associated assignments. (Constructions involving 'standard distributions' are themselves probabilistic systems of extrapolation, of quite different theoretical status. Some of the epistemological considerations raised will be discussed in due course.)

There is, however, one very simple and common form of probabilistic context in which numerical p-valuation can be given precise and plausible meaning, and which will be our point of departure for the discussion of all such p-valuation. It is closely associated with systems of recognitive valuation (calling for extreme care by the epistemologist to distinguish the levels of theory involved). This is the variously exemplified class of contexts where extrapolations consist of distributions of finite sets of values of particular V-characters as assigned to a finite set of idents. It includes cases with only one ident, but at least two characters - such as the guess whether the coin under my hand shows head or tail: and indefinitely complex cases. Our earlier analysis and discussion of V-characters shows that the notion is extremely versatile, since it covers, as defined, virtually all cases of alternative assignments amongst a range of possible characterizations under a (G)R-theory. Extrapolative assignments of values of such characters in the manner set out above will automatically satisfy the conditions for an AE-set, in that just one value may be assigned to each ident for each frame. Cases in which this is taken to be a CE-set include (1) some in which values are in principle recognizable as directly assigned to each ident for each frame, like the marked values on the faces of a conventional die; and (2) some in which approximate values are extrapolated for the members of an approximate set by virtue of their supposed positions in a suitable comparison figure (D12, p. 134) - including the case of a metric data set (K3, p. 251). (Distributions of values in standard sets, including metric structal systems, are by definition recognized and not extrapolated. The sense in which, in certain contexts, particular extrapolated values are treated as 'standard', cannot be explored at this point.) Similar principles can be extended, with care, to cases where either X-idents or X-characters are unrecognizable, their existence being accepted only under the commitments of associated S-theory; provided that an adequate basis for numerical p-valuation holds in that theory (as in statistical mechanics).

The simplest basis for numerical p-valuation occurs where, in a given CE-set, each assignment of the form $P \in \theta^{X,k}(x,i)$ receives the same strength of commitment as every other (where P is a value or approximate value of the relevant V-character or quantity, which may involve X-characters; and x ranges over idents and X-idents of Θ and Θ^X). Some epistemological considerations respecting such assessments of equal strength of commitment will be discussed below. Here, we point out only that, given such an assessment of equiprobability, it will result in the accord of equal p-values to each E-model $E^{X,k}$ giving a complete, distinct distribution of assignments of values of the V-character or quantity amongst idents and frames. Given n members of the CE-set, the p-value of each $E^{X,k}$ is conveniently set at $\frac{1}{n}$; that of any disjunction of m different E-models is naturally assessed as the numerical sum of their p-values, i.e., $\frac{m}{n}$; and that of their complement in Θ^X, $\frac{n-m}{n}$ (i.e., $1 - \frac{m}{n}$, the p-value of the disjunction of all members of ^{X} being unity, so expressing the commitment to closure of the CE-set). Further, the p-value of any simple or compound assignment consistently derivable in such a system can be numerically equated with that of the disjunction of just that subset of E-models in which it holds; and that of its negation with that of the complement of that disjunction. The p-value of any assignment which holds in all models is, of course, 1. This account makes no explicit appeal to any literal notion of 'frequency'; though, of course, there will be many instances in which these principles will apply to extrapolations concerning the frequency of particular types of event in a system. But it is difficult to draw a clear line between such a literal interpretation and a more figurative notion associating the p-value of a pattern of assignment with "how often it occurs" in the theoretical structure of E-models; suggesting that any such notions can be somewhat liberally applied.

Patterns of probabilistic extrapolation of this general form, and many forms constructed by mathematical developments or modifications of it, are commonly associated with patterns of R-theoretical readings by counting or measurement, such that both observational readings and structures of extrapolation are adopted in common by large L-groups sharing long-established contexts of theory - though individual gamblers may use highly idiosyncratic systems of this type. The senses in which probabilistic systems of this kind may be considered "objective" will be discussed below.

(f) The broad, and perforce tentative, claim is made that this account of the way in which the simplest forms of numerical p-valuation can be analysed in terms of relative strengths of commitment to members of CE-sets, involving assignments of values of V-characters or quantities, can be extended in principle to all forms of such p-valuation which can themselves be analysed as developments, extensions or modifications of this form. One simple development which can be briefly mentioned is that

that in which conjunctions of assignments of values of two specified quantities are extrapolated for a single set of idents; or similarly for X-characters or X-idents. If it is supposed under the commitments of Θ^X that, for each assignment of a value of each of the quantities to an ident, an equiprobable set of E-models exists in each of which one value of the other quantity is assigned to the same ident, then the total number of E-models is the product of the number of values assigned for each of the two quantities; and the p-value of a conjoined assignment of a value of each quantity to an ident is the product of the p-values of each assignment taken separately. The effect of specified restrictions on such a structure can often be given mathematical expression which reflects the commitments of the empirical theory of the context. Again, a simple modification of the simple case analysed above is one in which E-models are given unequal p-values designed to reflect the commitments of the (G)R-theory with respect to certain factors supposed to affect the R-theoretical readings on which the extrapolations are raised: these factors being represented by numerical coefficients or 'weightings' so chosen that the total of all p-values remains unity, and all or most of the above conditions hold as for the equiprobable case, given care in the mathematical treatment.

Moreover, recalling that the analysis so far has been restricted to systems of extrapolation for single frames of a (G)R-sequence, there seems no reason why extensions of these principles to extrapolations for sequences of two or more frames should not be given similar analysis in terms of strengths of commitment to E-models. These may include cases where mathematical expression can be given to the effect of chosen restrictions, excluding certain conjunctions of successive assignments for successive frames, associated with the C-theoretical concept of 'events' determining frame changes.

Something must also be said about proposals, first associated with the work of Ramsey, for numerical valuations reflecting judgments of probabilities, by individuals or groups, in contexts where no basis exists for commitments to equiprobability or specific weightings on the lines just discussed. Such projects can be approached in two ways: first, as an account of what should be supposed rational assessment of probabilities, independently of the vagaries of individual judgment; second, as an attempt to quantify (in the numerical sense) the actual strengths of commitment of individual Readers in particular empirical cases. In the terms of this study, the first approach would be seen as an introduction of fresh systems of commitment, over and above those involved in actual determinations of probability in practice, as to the structure of probability as a rational concept - which would be beyond our present scope. The second, on the other hand, must be classified as a form of psychological "measurement": a topic which will be considered (in synoptic form only) under the head of non-physical quantification in Section R below.

No further analysis can be given here of possible developments or modifications; but it is hoped that enough has been said to give at least prima facie support to the tentative claim at the start of this note.

Before discussing the relevance of this analysis to some earlier accounts of probability, I want to draw attention to a few general points which will assist in this discussion.

(1) In the many cases in which E-models are so constructed that p-values are closely associated mathematically with the numerical structures of the systems of idents and characters involved in the context (whether the results of counting, measurement, or calculated from such results), there is an insidious danger of conceptual confusion about these relative theoretical levels. It is often convenient, for example, to plot p-values against values of the relevant quantities in the same graphic display. In ordinary contexts it is natural, and usually harmless, to regard the derived p-values as 'results' of the same kind as those on the basis of which they are arrived at. Our analysis draws attention to the need for care, in epistemological analysis, to distinguish between theoretical levels at which questions of meaning, truth,

objectivity, etc., may arise.

(2) The aim has been to exhibit the conceptual framework of probability as capable of a unified analysis over a wide range of applications. So, although it is acknowledged that all p-valuation has a psychological aspect, this account of the conceptual framework is taken to be independent of any psychological theories of its origins in human motivation. Variations due to psychological factors are reduced to a minimum for an L-group adopting, for whatever reason, a (G)R-theory of objective measurement, with which is associated a probabilistic structure of E-models incorporating a well-defined logical and mathematical theory. Under this analysis, rather than distinguishing three kinds of probability - such as subjective, mathematical and empirical - it is more perspicuous to retain 'probability' as a concept common to all kinds of E-model valuation. We may then distinguish, on the one hand, psychological theories of the emotional drives or forms of subjective bias governing human use of this concept; and, on the other, logical and mathematical theories used in association with it in particular contexts.

(3) It has been assumed that the concept of probability is definable only within a metatheory of concrete contexts, because it is in just such contexts that we have reason to form extrapolative commitments to alternative models in situations of which we have incomplete evidence from perceptual recognition. In the vernacular, the word "<u>statistics</u>" is generally used for assemblies of

numbers derived from empirical investigations involving measurement or counting; or to calculations made on these numbers, under the commitments either of substantive theory, or of the mathematical theory of associated p-valuation. In the present analysis this restricted sense appears the best and least equivocal.

(4) The objects to which probability values have been assigned are E-models, and it has to be admitted that these are remote from general usage. But, if this is indeed the form of the underlying theoretical structure, it may account for the fact that usage, rather than acknowledge this esoteric foundation, has attached the term to a wide and potentially confusing variety of objects, derivable in various ways from theoretical models. I have pointed to particular sets of alternative <u>assignments</u> as the aspects by which alternative E-models are often most easily distinguished; and also said something about the cases in which pairs of such assignments can be recognized or understood as <u>events</u>. In more complex or vaguer cases, C-theoretical analysis of R-theories in general (in Part II) has provided a basis for reducing all aspects of potential E-models to structures of character assignments and their associated commitments - which can readily be extended to X-characters and the commitments of extrapolative models. <u>States of affairs</u> may be analysed as more or less complex systems of assignment involving specified sets of idents or X-idents. Assignments, and many reasonably closely-associated structures of related

assignments, such as events, can be expressed by <u>propositions</u> (Section G, note (d), p. 186).

Some other objects which have been proposed for p-valuation need little modification to permit a translation into the language of E-models: for example, hypotheses (e.g. by Parratt (1961), as one of several types); or multivalued implication (e.g. by Reichenbach: see references below). These terms plainly refer to what we should here call alternative theoretical commitments.

(5) The C-theoretical account of measurement leads to the conclusion that all <u>registered</u> values (i.e., direct readings from the relevant comparison figure, as opposed to results calculated from these) are derived from <u>integral numbers</u> of standard m-intervals - though these may be expressed as <u>rational</u> fractions of conventional units. Again, the numbers of values actually registered in this way are necessarily integral; and the same considerations apply with greater force where the results on which extrapolations are raised are arrived at by counting. Thus probabilistic extrapolations based on such registered values rest on an empirical foundation not involving irrationals. But note that, since E-models are supposed constructed as fictal, not recognized as datal, structures, there is no fundamental restriction to finite numbers for CE-sets, nor to rational numbers for p-values. Equally, since E-models may assign unrecognized X-characters as well as recognized characters, whether to idents or unrecognized X-idents, neither $\mathcal{S}^X$ nor $\mathcal{C}^X$ has been

restricted by definition to a finite set. Plainly, the assignment of continuously variable p-values, and the consequent use of the mathematics of the real numbers on such values in terms, e.g., of differentiation or limits, is dependent on the extrapolation of infinite sets of E-models, or E-models containing infinite sets of X-idents or X-characters, and hence of non-measurable values of the relevant quantities. In such cases, inductive support for the validity of extrapolations may involve numerical (not metric) approximations to rational numbers which can be compared with measured values.

(6) There are some very exceptional but important kinds of p-valuation (especially in the field of quantum theory) where the conditions for the construction of a single CE-set appear to be prohibited, in principle, under the overall theory of the context, including the associated theory of measurement and extrapolation. Two main types of extrapolative context, classified according to their AE-set structures, may be selected for attention here.

(a) Single AE-set,not closed: e.g., in conditions of indeterminacy, where values of two quantities, say position and momentum, may be extrapolated for the same set of X-idents, say electrons; but the theory prohibits the assignment of values of both quantities to any one X-ident in the same (theoretically extrapolated) frame.

(b) Two mutually inconsistent CE-sets: e.g., in the measurement of "spin" for subatomic particles, where successive readings of what are held to be equivalent inputs into the same measuring apparatus (though in different conditions) yield two inconsistent probabilistic CE-sets, taken together; but either reading alone gives a good probabilistic distribution. The associated S-theory forbids, in principle, a means of reconciliation or choice between them.

Significantly for us, it is the nature of measurement itself - active not passive, theory-bound not theory-free - which justifies these theoretical prohibitions. Conditions of either of these types would be excluded from our account of coherent structures of extrapolation, the first as incomplete (or 'open'), the second as, at best, equivocal, if the idents and characters involved were in principle recognizable. But in these cases we are dealing with X-idents or X-characters which are wholly extrapolated under the commitments of S-theory, and distinct from any idents or characters recognized in the R-theoretical system of observation on which they are based. They are clearly articulated and inductively supported by recognized results in testing and use. Some doubt must remain, however, as to whether we are entitled to regard such incomplete or equivocal accounts as coherent descriptions of actual conditions which can properly be held true: it seems more appropriate to speak, with van Fraassen, of 'convenient myths' on which useful extrapolations can be built (1980). Questions of this sort must be left for further discussion in Part

IV; noting here only that any resolution in these cases may well depend partly on clear distinctions between the theoretical levels at which p-values are assessed and extrapolated assignments constructed.

(7) There is a simpler, perhaps more fundamental sense in which no probabilistic account purports to be a true description of actual conditions; namely that any such account is an idealisation to which the system described is not, in principle, supposed to conform exactly. For example, however many times a regular die is actually thrown, the occasions when exactly one-sixth of the throws yield, say, a six, are comparatively rare. The law of large numbers convinces us that perfect coincidence between actual throws and our ideal extrapolations is approached as a limit with continued repetition, but this limit is never, in principle, reached for all relevant outcomes. This being so, inductive support for probabilistic theory must appeal to strategies of tolerance and neglect which, though based on careful mathematical reasoning, differ from those of approximation in measurement (as defined above) both in principle and in theoretical level. Thus, the results obtained may be supposed accurate enough for differences between extrapolations and actual conditions to be, in some cases, neglected, and in others assumed with a certain degree of confidence to fall within certain limits: these estimations being themselves idealisations based on supposedly equiprobable deviations from an ideal standard distribution.

In general, "randomness" as a property of empirical distributions is a theoretical idealisation: the distinction between randomness and disorder, as a concept associated with related phenomena, was emphasized by Carnap (e.g., in Schilpp, 1963, p. 76), though without marking any difference in theoretical status.

(8) There are at least two senses, to be carefully distinguished, in which objectivity can be attributed to probabilistic accounts: first, the sense of intersubjectivity, associated with the strategy of creating conditions for p-valuation in which possible vagaries of individual judgment are eliminated as far as possible; second, the sense in which it may be supposed that such an account describes actual conditions involving real entities and properties. Objectivity in the first sense can be readily attributed to all cases of numerical p-valuation which are inductively well-supported (the sense of 'induction' here will be further explored in Part IV). The conditions may also be partly satisfied by less rigorously articulated forms of extrapolation based on common-sense understanding of regular patterns of behaviour, for example in natural kinds or artefacts: but the line here is hard to draw. The second sense of objectivity may perhaps be extended beyond extrapolations of recognizable events like throws of dice to include, say, the unobserved molecules of classical statistical mechanics. But our analysis suggests it must be withheld from some of the extrapolated entities or properties constructed in

incomplete or equivocal S-theories.

There may be a further sense in which E-models themselves may be thought of as having objective existence, along with their associated propositions; but not, in the absence of further theory, the strengths of commitment accorded to them. Such questions must be left open here.

(ii) Critical comparisons with other analyses

1. The general line of this discussion will be not merely that, with few exceptions of detail, existing theoretical treatments of probability can be reconciled with the above account; but that many of the conceptual frameworks they use can only be given clear meaning within a metatheoretical structure of the kind there described. In the case of Carnap, almost certainly the most influential writer on this subject in recent times, a great deal of the work has been done for me by Kemeny, who produced a formal structure, in collaboration with Carnap himself, for the latter's concept of 'degree of confirmation', or 'probability$_1$' (P_1). (See Schilpp, 1963, pp. 711 ff., and Carnap's broadly approving comments, ib., pp. 75, 974-80). Carnap's own main centre of interest in this context has been the construction of a logic of induction, on the basis of a concept of confirmation (for hypotheses, whose theoretical status is almost self-evident). It was therefore left to Kemeny to explore more deeply the possible formal (and metatheoretical) structure which such a concept might generate

at the level of the hypotheses themselves. What emerged was the analysis of 'hypothesis' and 'evidence' as elements of a system of wff. (well-formed formulae of a formal language), each identified with the set of models in which they are true (op.cit., 721). Models are defined, for this purpose, as semantic interpretations of a specified formal language, to which probabilistic weights may be assigned: the measure of each wff. being the sum of the weights of all models in which it is true (ib., 717). It is made clear that the formal language (or logistic system) under consideration is intended to be a scientific language, assumed to be "fully or in part empirically interpreted" (ib., 715): so that the whole analysis is placed in the context of a scientific theory (though Kemeny's discussion indicated that the word 'scientific' may be very widely interpreted: the emergent principles are intended to hold for quite ordinary contexts, like "rain tomorrow", as well as Einstein's Field Equations). Indeed this empirical reference seems a necessary condition for the analysis to contribute to a logic of induction. We are plainly already close to my own style of analysis, but a number of loose ends need tying up.

Though Carnap himself uses the same formal style as Kemeny, the notion of 'models' is subordinated by him to that of 'state-descriptions' of the relevant language, which are in turn (eventually) reduced to systems of 'families of predicates' (such that for each individual exactly one of each family holds). Although the reduction can be interpreted for C-theory in terms of propositions referring to assignments (concrete or extrapolated under S-theory), there is here a strong tug back towards the notion of probability as a property of sentences, which, although certainly a permissible reading of the formal system, tends to obscure the vital connection with the theoretical models in which the sentences (which may be extremely complex) may be embedded. Kemeny seems much clearer than does Carnap that sense can only be made of these families of predicates and their degrees of confirmation, in a context of fully understood theory in which 'hypothesis' and 'evidence' possess defined status, in a system of comprehensive and coherent models.

2. There is, indeed, a fruitful zone of confusion around the question of what sorts of relation are supposed to hold between hypothesis 'h' and evidence 'e' in the Carnap-Kemeny system. The m-functions from which probabilistic weights are derived are defined in terms of numbers of models in which both h and e have definite truth-values; as befits propositions of a formal logical language. No formal restrictions are placed on the interpretations of h or e, beyond their being defined as well formed formulae of the language in which models are constructed. Clearly, then, their distinct interpretations as hypothesis and evidence, respectively, must have some connection with their status in empirical interpretation. But it is not clear what this connection is. It seems

plausible to claim that all examples offered in the Carnap-Kemeny account can be given a C-theoretical interpretation in which e refers to an assignment (or complex structure of assignments) under recognitive theory, while h refers to one of a number of alternative (structures of) assignments extrapolated in a set of E-models constructed under some overall S-theory of the context. But, as far as I can see, nothing approaching this degree of explicitness occurs in the literature which has grown up around that account. Indeed, in answer to critics, Carnap has acknowledged that his earlier specifications in terms of 'degree of confirmation' or 'rational belief' are "vague and ambiguous"; offering in their place (or in further explanation) three other alternative formulations, in terms respectively of a fair bet, estimation of relative frequency, or rational calculation of utility (ib., 967). But, apart from remaining somewhat vague, these do not together make up a very coherent account of the notion of the probability of a hypothesis with respect to evidence, let alone of how we are to distinguish 'hypothesis' or 'evidence' among the wff. of a 'logistic system'. (I am using single quotes to mark Carnap's technical terms; double to identify direct quotations.)

That this confusion is not confined to myself is amply confirmed in the literature. Reichenbach took the natural meaning of confirmation of hypothesis by evidence to be that, if h logically implies e, and e is verified, h is (to some extent) probable; an inference which he attributes to "some logicians" (Neyman, ed., 1945-6, p. 15; Carnap's system stems from work in 1941). The theory of probability, he said, knows no such inference. Indeed, Carnap's system employs neither this nor any other appeal to implication, other than the defined consequence that if e implies h, the probability (P_1) of h on e is 1. This is logically equivalent to the statement that in every 'permissible model' where e is true, h is true, so that the definition is derivable from that of the 'model-function'; it tells us no more about the categories of evidence and hypothesis or their theoretical relationship. Reichenbach himself proposed a version in which probability is analysed in terms of many-valued implication; a concept to which he gives an interesting, weak form of axiomatisation under which his frequency principle is defined as a special case (ib., 1-2). This is also an early - perhaps the first - account which takes the objects of p-valuation to be classes of events, whose relations of inclusion define the implicative relations between associated sentences; most current accounts treat of 'event-sets' in much the same way. From this he develops a probabilistic logic for sentences, pointing out that this can be interpreted under the frequency principle "by the device of counting sentences about events instead of counting events" (ib., 6 ff.). How he understands his notion of probabilistic implication is perhaps best expressed thus: "We shall not claim that the individual assertion is true; we shall assert it in the sense of a posit ... A posit is a statement with which we deal as true although we have no knowledge about its truth ... The probability appears as a rating of the posit, which we call its weight." (his emphases, ib., p. 17). Though this notion is given clearest expression in the frequency interpretation, he claims that it is also defined for the more general case by his system of probabilistic logic (ib., 18). It may plausibly be claimed that this account can be given its clearest interpretation in contexts of empirical theory as analysed above, and that there is here a considerable area of agreement with the C-theoretical account. Carnap seems not to have reacted at all to this notion of Reichenbach's, to whom he attributes only a form of what he calls the "frequency theory".

Popper seems to have conducted a running paper battle with Carnap over the nature of probability and confirmation (which he equates with his own notion of 'corroboration'). Carnap complains that he and Kemeny have repeatedly sought to explain to Popper just what they mean by these terms, without success (Schilpp, op. cit., pp. 995 ff.). Undoubtedly any problems caused by the admitted vagueness and ambiguity of the terms were powerfully exacerbated by the diametrically opposed views of the two men on the subject of induction, in the context of which Carnap's account is constructed. Popper fiercely rejected what he called "inductivism", and derived his notions of probability from an account of scientific theories as conjectures. More will be said shortly about the resulting analysis.

Brian Ellis also complained of ambiguity in Carnap's account of the status of evidence in probability (1966, pp. 179-80). His complaint is explicitly placed in the context of the logical/empirical (analytic/synthetic) distinction which Carnap proposes as the main difference in theoretical status between his P_1 and P_2. Part of the difficulty arises, I think, because Carnap's overriding interest in the relevance of the logical structure of P_1 to induction leads him to neglect almost entirely the analysis of P_2 - to which he constantly refers simply as 'the frequency theory' - and its relationship, if any, to P_1. Ambiguity is increased when frequency principles are apparently cited as integral parts of the definition of P_1, as noted above (p. 370; the model-function itself is a frequency-based concept). Considerations already noted lead to the view that Ellis is fundamentally right in feeling that the discrimination of evidence <u>as evidence</u> is intrinsically empirical. In Carnap's system, a sentence of the form, "on evidence e, the probability of h is p" is analytic (or linguistic-conventional): and it is difficult to see how empirical contexts are to be understood in these terms.

3. The idea that probability might be thought of as a measurable quantity, even by analogy, seems to have arrived late in the long history of the subject. Brian Ellis is the first of the writers so far mentioned to

consider it in the main context of a theory of measurement - which accounts partly for the importance he attaches to the concept of evidence. His argument rests on purely pragmatic reasoning from demonstrations that numerical values can usefully be assigned to probabilities. Although he clearly acknowledges the crucial role of theory in the assignment of p-values, seeing it as the basis of a choice between 'inductive rules and procedures' which in effect determine these values in many contexts, he declines to offer any theoretical account of p-valuation itself, seeming to doubt that one is possible (op.cit., pp. 176, 161).

Ellis' pragmatic approach is typical of the literature where the association of probability with measurement is discussed. Neither from the practical point of view, nor from that of purely formal analysis of the resulting structures, are the crucial epistemological questions raised. Practical works on probability, like Parratt's (1961), tend to accept p-valuation as measurement, on grounds similar to Ellis', without asking philosophical questions of any kind. Pfanzagl (1971) can still write a theory of measurement in which the concept of probability appears only in association with wagers and subjective utility-judgments. The notion of an objective system of p-valuation appears here, not as a centre of interest, but only as a limiting case where subjective assessment is wholly constrained by the physical conditions (as in tossing a coin). On the other hand, KLST, writing on the foundations of measurement in 1971, follow Ellis' pragmatic reasoning, quoting him with approval (p. 201): though they provide a very much more sophisticated formal analysis, involving distinct structures of ordering for 'objective' and 'subjective' probability. Neither structure shows very close analogies with their analyses of orderings for measurement in the ordinary sense, and they give no clear account of the relative theoretical status of the different kinds of ordering.

One salient difference between p-valuation and measurement in C-theoretical analysis is that the entities being evaluated - strengths of commitment - offer no prospect of understanding in terms of 'incorporated S-theory' by which such notions as addition and multiplication of numerical values can be given concrete meaning. In this they have more in common with structures of valuation in non-physical schemes of quantification. KLST acknowledge that their concept of 'concatenation' - on which they construct the relevant operations in the analysis of ordinary measurement - cannot be applied here: pointing out (correctly, but rather surprisingly) that they cannot be applied, for example, in many cases of length measurement either - and leaving it at that. It may be hoped that the present account offers at least some prospect of clearer articulation in these matters.

4. Before concluding this discussion of the probability literature with a consideration of Popper's position, there are two general topics connecting probability with

measurement on which something more must be said: randomness, and the relationship of probability 'space' to the space-time of physics. To start with the second, it is interesting, for instance, to find A.I. Khinchin, in introducing the subject of statistical mechanics (1949, pp. vii, 44 ff.), troubled about the legitimacy of using 'phase-space' averages (representing an indefinitely large distribution of values of variables in many dimensions of probability-'space' involving a number of physical quantities, including those of physical space) to replace time-averages, in computing extrapolated 'trajectories' of hypostatic entities in this space. It is clear, however, that the structure of phase-space is entirely probabilistic and therefore, if our analysis here is right, metatheoretical. Any use, or test, of this type of theory of molecular systems in a context of actual measurement involves the extrapolation from the measured evidence, using commitments of the theory, of a very large number of unobserved (or partially or indirectly observed) X-idents, to which complex structures of extrapolated X-characters are assigned. Even if time does not appear, as such, in the systems of quantities in terms of which these assignments are made in the theory, it is clear that the whole basis of the theoretical structure rests on an underlying commitment to the random distribution of these assignments over time - within the restrictions imposed by the conditions of observation.

I have already noted Carnap's insistence on the distinction between randomness and disorder, and interpreted the distinction for C-theory (p. 219). The status of randomness as a theoretical structure, with varied applications in probabilistic extrapolation, may be obscured by a tendency to regard it as a given physical property of particular systems. It is doubtful whether even Carnap appreciated this aspect of the distinction. Again, Parratt attributes it to the "whim or caprice of nature, both in errors of measurement and, in some cases, as an inherent characteristic of the measured property" (1961, pp. 4, 5). A remark of Campbell's, made in a different context, is interesting in this connection. He says we regard a hand of thirteen trumps of a 'coincidental' result of a 'fortuitous' deal, not because it is any less probable than any other 'named' hand, but because it is much more probable on the theory that the deal is not fortuitous but arranged (1928, p. 263). Two aspects of this remark call for comment. First, it points to the status of randomness, or the 'fortuitous deal', as one of a set of alternative theories, which is itself rendered improbable where observed distributions fail to conform. Secondly, it is evident that by 'named' hand, Campbell means a hand completely specified, except in terms of dealing order - temporal sequence being, again, arbitrary. This, I suggest, points to an important possible ambiguity in the sense of the term 'most probable distribution', which may not always be attended to. The description of the thirteen-trump hand as not specially improbable, on the fortuitous-deal theory, rests on the fact that every card of the pack _is_ individually named. If we neglect this aspect, and attend only to suits, for example, a thirteen-trump hand is obviously less probable than a hand of either (1) thirteen drawn from any one suit, or (2) twelve trumps and one of another suit. In a typical context of probabilistic physics, such as statistical mechanics, findings rest precisely on the condition that the ident-elements are neither named nor separately identified. Their distinguishing space-time characters are systematically neglected; they are treated as _synonymous sets_ (above, p. 239). This is one of the most important uses of the principle of synonymous sets, and helps to explain the attention I paid to it, and the so-called "Leibniz' Law" by which it is usually justified. It is a general feature of theories of random distribution in physics, and in many non-physical contexts.

5. Popper's approach to probability starts from the opposite ground of interest: the structure and status of the _theories_ in which random distributions are posited, and from which p-valuations take their empirical meaning. He announces that it is an "elementary consequence" of the mathematical structure that the probability _of a theory_ varies inversely with its informational content (Schilpp, op.cit., p. 219; and generally in his writings). This principle can be fairly precisely translated into the language of C-theory if we equate the notion of 'informational content' with a function of the number of

alternative sets of E-models, and the number of models in each set; such that the inverse of the product of the p-values of any set of assignments determines its content-value, and the content-value of the theory in any context is the sum of those of all its extrapolated assignments. This suggests that Popper's principle holds so long as by 'content' we mean 'extrapolative content'. This will clearly be reduced by 'corroboration', i.e., by observed readings which (to an accepted tolerance) match values previously extrapolated under the theory. If I am right to propose the probabilistic assessment of rival theories in a context, in terms of the relative amounts of the deviations of their observed error-distributions from randomness, then increased corroboration will indeed increase the "probability" of a theory expressed in this way - while reducing its content. But the above analysis makes quite clear the difference in status between this type of assessment (which should, perhaps, be given a different name), and p-valuations of E-models within a single overall theory (above, p. 346). Increases in corroboration do not increase p-valuations of particular E-models; rather they <u>may</u> raise the probabilistic assessment of the theory as a whole by reducing the scope of probabilistic computation under that theory. Corroboration in this sense is clearly quite different from confirmation in Carnap's, which appears to apply (oddly, I think) to the evidence from which hypotheses are extrapolated. But there is a sense in which Popper-corroboration of earlier extrapolations may become Carnap-confirmation for later ones.

I am not sure if this is the kind of result Popperians want, but the analysis is plainly more sympathetic to Popper than to Carnap. This sympathy extends to Popper's general charge that Carnap's "inductivism" relies unduly on an insupportable principle of "uniformity of nature": the apparent failure to see that randomness is a theoretical posit, rather than a given property, is symptomatic. C-theory supports the Popperian stance, in saying that we cannot appeal to any general uniformity: we can only propose theories with respect to particular, specified regularities and seek the verdict of nature through measurement. But it seems reasonable still to call this process induction.

Synopsis of further work

The principles outlined here are also relevant to the following contexts, which have been researched, but whose full treatment must be omitted.

(1) Analysis of the subsidiary notion of possibility: seen as relativised to extrapolative theory.

(2) Differences in theoretical properties of forms of extrapolation distinguished as (a) interpolation, where, e.g., extrapolated values are inserted between R-theoretical readings, and (b) ultrapolation, where assignments are extrapolated of values, or to entities, hypostasized beyond the limits of possible readings; with discussion of such associated notions as 'curve-smoothing' and 'degrees of freedom'.

(3) Further analysis of theoretical properties of commitments to equiprobability (a) necessitated by ignorance or lack of control, (b) secured by close control in a strong theoretical framework.

(4) The importance of the notion of 'independence' as

between extrapolated assignments; and the introduction of the notion of 'probability amplitude' for cases (in quantum mechanics) where this cannot be secured - involving the introduction of irrationals among p-values.
(5) The roles and theoretical structures of Gauss and Poisson distributions as fictal approximations to ideal binomial distributions (in the sense given above, p.290).
(6) The application of probabilistic theory to the analysis of the structure of time, involving the concept of entropy; including criticism of a proposal for time-reversibility.

Q. The Concept of a Dimension (Synopsis)

Three different senses of the concept of a dimension are distinguished:

(1) Metric dimension, i.e., the set of numerical values of a specified quantity determined in a particular context by measurement, or by calculation from measured values, which may include extrapolation.

(2) Coordinate dimension, i.e., the set of numbers, taken as values of a specified quantity, as represented in any one coordinate of a coordinate space, in graphic or analytical geometry, in an abstract context of mathematical analysis; whether or not some or all of the values represented are determined by measurement.

(3) Analytical dimension, i.e., the abstract notion of a dimension itself as a numerical expression of a specified quantity; especially in contexts where the theory and technique of dimensional analysis are employed. (This technique is principally used as a means of establishing or testing the theoretical consistency of any equation or set of equations expressing theoretical equivalences between functions of constants or variables taken to represent values of different physical quantities.)

Type (1) (metric) has been a major topic of this study, and further clarification is omitted from this synopsis. Type (2) (coordinate) is explicated as a form of mathematical expression of the S-theoretical structures adopted in each context. Rigorous argument has been prepared in criticism of KLST's account of some such structures in terms of a formal system called 'additive conjoint measurement', which is a major part of their analysis. This, too, must be omitted, since it cannot be effectively summarised. It points to a serious danger that formal sophistication, combined with neglect of precise empirical interpretations, may lead to grossly misleading assumptions: particularly for contexts where the S-theoretical structures on which coordinate representations are based are themselves weak or absent (as in some of their cited instances).

But the main purpose of this Section is to consider the relevance of the theory of measurement to type (3) (analytic). The principles of dimensional analysis of macrophysical quantities have been developed from the observation by Bridgman (1931, Ch. 1) that a large class of physical quantities are theoretically defined so that the values of each in any context are equivalent to a specified multiplicative function of the corresponding values of one or more of three 'basic' quantities - mass, length (or distance) and time. The relationships of these quantities, or analytical dimensions, whether complex or basic, are found to be in principle independent of the choices of either metric or coordinate dimensions in any particular context. In spite of a certain vagueness in its fundamental conceptualisation, Bridgman's system proved extremely fertile, both for the critical analysis of proposed new 'laws' based on observation, and even for the creation of new laws _a priori_, subject to empirical testing which was often favourable.

The promise which this observation seemed to hold captured the attention of seekers after fundamental principles, especially Ellis (1966) and KLST. But the strategic neglect by these writers of the precise requirements of empirical interpretation has prevented them from seeing the central relevance in this context of the limitations on perception, and the consequence dependence of our understanding of observation on the incorporation of S-theories. This has led KLST to postulate six 'basic' dimensions: adding electric charge, temperature and angle to Bridgman's original mass, length and time. None of these is exhibited as of special significance with respect to the others; nor, it is argued, is there any clear, unified account of why just these quantities should occupy a privileged theoretical position.

For C-theory, just two quantities occupy special positions in this context: energy and space: the second as that in which the recognition of structal interval series is directly available, uniquely. The domain of dimensional analysis, as here understood, is found to be just that in which a substantial body of S-theory has been built up (socially and historically) such that a unified account is given of the various indicator systems and schemes of calculation by which values of other quantities in the field are read in terms of spatial intervals. Apart from the conceptual problems associated with angular measurement, the demonstration that all the key quantities concerned are simply related to forms of _energy_ is not difficult. Time, like space, has been shown above to be measurable in terms of the structure of radiant energy. Temperature, electric potential (charge) and electro-motive force are unproblematic in this regard; and the relativistic identification of mass with a form of energy provides an essential link.

With regard to angle, it has proved possible to propose a form of geometrical analysis (which cannot be reproduced here) in which angular intervals, along with those of space and time, are incorporated in a relativistic account (already indicated in Section N), according to which space and time intervals are distinguished in terms of the mode of reception of radiant energy by a recognizing system; and spatial intervals are distinguished as angle-like or distance-like under the incorporated S-theory. Thus, the total emerging picture is that dimensional analysis exhibits the structure by which our quantitative understanding of the macrophysical distribution of energy in the universe is mediated by S-theory through recognitions of spatial intervals.

The philosophical message that emerges from this account, constructed in the light of current scientific insights, is that the structure of our recognitions of physical phenomena, and hence of our knowledge of the physical universe, is determined by the conditions of our participation in that universe, and not vice versa - or *a priori.*

R. Non-physical Aspects of Measurement

In contexts of physical theory, one of the principal roles of measurement systems has been seen as the elimination of social or psychological factors from structures of recognition; in the sense of reducing variations due to such factors to the level at which they can safely be neglected in context, even though they cannot be removed from the total situation in any case. An immediate conflict appears to arise, therefore, as soon as we try to apply the concept of measurement to the evaluation of these factors themselves. The subject is enormous in scope, and its exploration has revealed many aspects in which C-theoretical analysis can suggest useful distinctions of theoretical level. All I can do here, however, is to reproduce the summary appended to my original exploratory account.

The use of analogues of measurement procedures may be appropriate where the intention is to assign numerical values to sets of responses by living organisms, in such a way as to describe (rather than represent) a structure of relationships between them; according to the commitments of some theory which gives specified empirical meanings to arithmetical relations between the numbers. These meanings, though always in these contexts recognized in, and assigned to, responses, may be taken under the theory to refer to relationships between the respondents, or the stimuli and conditions which elicit them, as the relevant idents of this theory.

Such numerical value-assignments are analogous to measurement in that values are assigned to each of any relevant set of idents so as to describe the amount attributed to that ident of some quantity (V-character) recognized under the theory. They remain no more than analogous, at best, primarily because of (1) a lack of understanding (generally even in broad principle) of the complex organic processes involved in producing the responses (if, indeed, these are wholly responsible); associated with (2) lack of control over these processes, and often also over stimuli and conditions. This lack of understanding and control is compounded in social contexts where cultural, historical, economic or political factors are involved. Another serious lack, arising out of the preceding, is of any basis for well-specified strategies of neglect, according to which the factors considered can be safely or rationally simplified. These deficiencies result in the absence (1) of precisely specified standard structures, as a basic for comparison, analogous to the structa of physical measurement; and (2) of theoretical structures in which empirical relationships are clearly expressed in arithmetical terms. The attempt to fill these deficiencies by sophisticated probabilistic structures, built on sets of numbers which themselves lack theoretical significance, leads to mystification, often covered by exaggerated (and uncheckable) claims.

Only in the simplest contexts of physiological mechanisms, animal behaviour, or human perceptual or reactive response, does there appear to be any prospect of a firm enough theoretical structure to support confidence in the arithmetical properties of the assigned numbers as an element in precise description. Beyond this level, the most that can plausibly be asked of such analogues of measurement is the broad indication of relative values of comparatively well-understood non-physical characteristics; complemented, on a mutually corrective basis, by common sense and informed intuition. To the extent that mathematical sophistication may interfere with this mutually corrective process, its use for apparent technical advance must be approached with caution. This caution is reinforced by the absence of sound theoretical grounds for the extrapolative principle implicit in all probabilistic reasoning. Formal analysis can do little or nothing to assist in stiffening the theoretical structure; it rather requires such stiffening as a prior condition of its useful application.

In any event, all thought of non-physical measurement in the life or human disciplines as an analogue of physical science "in its infancy" - based always on an absurdly simplistic account of that science - is, fortunately, misconceived. If it were not, there would be strong moral arguments for infanticide. The adult, if permitted to develop, would be dependent on a kind of control over one another which human beings show little sign of being able to exercise responsibly over the objects of physical science itself.

(Principal sources for the investigation, though not for the views expressed, are Cicourel (1964), Coombs (1970), Lader (1975), Nunnally (1970), Pfanzagl (1971), Stephenson, W. (1953), Stevens, S.S. (1936, 1951, and in Stevens, G., ed., 1975).)

PART IV. SOME GENERAL PHILOSOPHICAL PROBLEMS: the relevance of contextual theory

A. Language and Theory

(1) Knowledge, Meaning and Truth

From the point of view of more general philosophical interests, the main insight which may be claimed for the line of reasoning developed in this study is to be found in the relatively detailed articulation of the structures of *theory* taken to be fundamental at least for the recognition and understanding of perceptual experience; including the way in which non-perceptual, substantive theories may be incorporated in the context. More generally, the concept of empirical knowledge at all levels has been exhibited as a structure of partial theories, interlocking to the extent that they are taken to involve the same elements, and accepted as true accounts of reality by those who adopt them as known. This view of the structure of empirical knowledge impinges on most aspects of philosophical inquiry, not only in philosophy of science where it is more or less common ground. Given our special concern here with theoretical structures at the frontiers between linguistic and non-linguistic elements in particular contexts, we may look for some contribution to the understanding of the relationships between knowledge, meaning and truth: regarded as fundamental by many philosophers of language.

A major distinction has to be drawn between my approach in this study, and that mainstream tradition in semantics associated primarily with Donald Davidson. That tradition has concentrated attention on the analysis of one side only of the frontiers which concern me: the linguistic. That analysis has been carried out largely in terms of the syntactic or grammatical structures of particular languages or language-types (especially English). My own analysis treats the understanding of language in concrete contexts as just one structural element - though an exceptional and important one - in perceptual recognition: appealing to the supposed construction of more or less fully-developed empirical theories, from context to context, as the basis for the functional association of linguistic with non-linguistic elements in particular concrete situations. This approach has been found essential, in particular, for the clear understanding of measurement contexts.

From the point of view of the mainstream, the recognitive aspects of language itself are irrelevant: nor do I attach importance to these, except to facilitate a unified analysis by emphasizing that linguistic structures are, indeed, parts of perceptual experience for both utterer and interpreter. Within such a unified analysis I am nonetheless able to give some account of the structural properties which are required of language, if it is to fulfil its various roles in the reporting or recording of perceptual recognitions, and their associated theoretical commitments. This account is independent of the actual conventions of any historical language; but, as far as I can see, consistent with those of which I know anything.

Mainstream analysis, on the other hand, is forced to neglect non-linguistic factors in recognition (not only the recognition of language itself), except by reference, in general terms, either to typical forms of sentences supposed to speak of them, or to the understanding of a 'natural language' as a whole. This prevents the introduction into the analysis of any structure, distinct from language itself, which may be involved in the formation of understood correspondences between linguistic and non-linguistic elements in experience.

It is, of course, acknowledged that assessment of the conditions for the truth or falsity of many sentences depends on some kind of knowledge of "the way the world is", "the facts of the case", or the like: matters of which all our knowledge comes ultimately from perception. One way of dealing with the difficulties for analysis generated by such vague global phrases, is to suppose that it is part of <u>the linguistic competence of speakers of a given language</u> to be able to determine just what <u>non</u>-linguistic conditions are, in such cases, relevant to the assessment of the truth or falsity of any particular utterance. The underlying intuition here is that of Tarski, whose recursive definition exhibited the concept of truth as a property of sentences. His own view was that the recursion could only be carried out for a rigorously constructed 'object language', the truth-values of whose sentences were in principle determined. He held explicitly that this programme could not be carried out for natural languages, the precise forms of whose sentences could not be exhaustively known, let alone their truth-values. Though he discussed the possibility of constructing suitable modifications of such languages, he did not attempt it - nor has anyone else. He was certainly aware of the relevance to this concept of questions of meaning; he freely used the notion of translation (as a meaning- and therefore truth-preserving transformation), and categorized his work as belonging to semantics. But it was left to Davidson to make the essential connection between knowledge, meaning and truth. Michael Dummett acknowledges that Davidson made a step forward in recognizing that the problem of meaning is concerned with understanding - knowledge of meanings - but also asks what kind of knowledge this may be (What is a theory of meaning? in Evans & McDowell, 1976, esp. pp. 69, 70). Here, and in his (1978), he explores a number of approaches to an answer; and seems to set most store by the notion that it includes a "practical ability" which must be acquired. However much this acquisition may depend on innate capacities, the conventional forms of any particular language must be learned, and Dummett argues for an investigation into the structure of meaning through the means of acquisition. More will be said of this approach below.

Davidson, however, has introduced other factors into the problem: context of utterance, and the notion of a language community, both of interest for this study. To quote a version of his formulation given in his Thought and Talk (in Guttenplan, 1975, p. 17):

> ... for speakers of English an utterance of 'It is raining' by a speaker x at time t is true if and only if it is raining (near x) at t. To be armed with this information, and to know that others know it, is to know what an utterance means independently of knowing the purposes that prompted it.

In order to make this account of the matter plausible, it is necessary to restrict illustrations drawn from natural languages to those which make limited, simple demands on the knowledge of the interpreter, beyond that of the grammar and syntax, and the most general and unproblematic of the lexical conventions, of the language in use: 'Snow is white' and 'It is raining' are typical. It is reasonable to say that all one needs to know to assess the truth or falsity of an utterance of 'It is raining', apart from English, are the two factors mentioned. Knowledge of the speaker's identity is generally enough, for this particular sentence, to raise an inference to the particular region of space referred to (but not unexceptionably: we need to know where the speaker is at the time, and whether he refers to rain seen (heard, or felt) directly, or by telescope or even television). The presence, or not, of rain, such as determines the truth or falsity of the report, 'It is raining', is a phenomenon whose characteristics are exceptionally constant over different times, places and contexts of recognition. Most other phenomena of which we commonly speak are less uniform in character, and the relevant utterances call for more complex contextual analysis, as we shall see.

It is, of course, possible to argue that _any_ judgment associated with the use of language, whether by speaker or interpreter, is linguistic; and, indeed, it must be _partly_ a matter of linguistic competence. But wherever assertions are made about non-linguistic subject-matter, the context necessarily includes non-linguistic recognitions (past, present or future), and demands from utterer and interpreter the competence to make the relevant associations between these and the linguistic structure used. The way in which this competence depends on memory of past usage and its non-linguistic associations is quite fully discussed in Section IV.4 below. Meanwhile the simple claim is made that, however important linguistic competence may be (and this, too, is more fully discussed shortly), all use of language to speak of non-linguistic elements or structures, and all understanding of such use, calls also for _non-linguistic recognitive competence_. A person blind from birth lacks competence to speak of colour (or interpret colour-words) only from prior lack of visual competence: the nature of the linguistic incompetence is independent of the person's linguistic skills as a whole, and strictly determined by recognitive incompetence. Again, we may ask whether an English-speaking Eskimo who has never seen rain, or an

English-speaking African who has never seen snow (even on television), can be fairly accused of linguistic incompetence if they do not understand "It is raining", or "Snow is white". In the terms of the present analysis, the claim is that the interpretation of linguistic elements in concrete contexts necessarily involves non-linguistic elements of the relevant R-theory in every case.

To take an illustration which reveals more of the factors likely to be involved, consider two possible uses of the sentence 'The water is clear', spoken by one of the crew of a boat entering harbour. Suppose, first, that the crew-member is looking forward from a position of vantage: he means that the surface of the water is clear of large obstructions likely to call for changes of course. Second, suppose that he is holding a glass tube up to the light, containing a sample of water he has just hauled up from the bottom as part of a hydrographic survey: he means that the internal volume of the water in the tube is free of visibly undissolved matter. The meaning is sharply determined in each case; the use of language typical and unproblematic; the further verbal description given here quite unnecessary in context.

At least three types of non-linguistic skill may be distinguished as involved in the acts of interpretation here:

(1) <u>restriction of attention</u>, within the total perceptual field, to elements relevant to the supposed common GR-theory of the context. For example, in the first case,

it is not enough for an interpreter to know that the speaker refers to the water ahead of the vessel - as any English-speaker might, from the direction of his gaze: he will be understood by a competent interpreter to speak of the channel, recognized by visible marks known from familiarity or marine convention. The competent interpreter will also know what kinds of possible obstruction are concerned in the statement that the water is clear.

(2) acknowledgement of the relevant commitments of GR-theory, both within the immediate frame, and sequential. For example, in the second case, it is reasonable to suppose that the competent interpreter will take the utterance to refer not only to the water in the tube; but also to that in the part of the channel from which the sample was drawn; and he will also draw proper inferences, under the GR-theory, with respect to the significance for the survey of the observation of clarity (the sense in which such an inference may be part of the meaning of the utterance is considered below).

(3) extrapolation from one's own (past or present) recognitions to those of which the utterer speaks. For example, extrapolation from the interpreter's knowledge of the harbour, and of the rough position of the boat as judged, say, from a view through a porthole, to what portion of the channel 'The water is clear' refers; or from past experience of taking and interpreting water-samples to the conditions of a particular case; or the kind of

extrapolation we have supposed unavailable to our Eskimo to the perceptual truth-conditions for 'It is raining'.

Since no two people can make use of identical structures of recognitive experience, this third type of skill is taken to be necessary not only for any intelligible use of language to speak of recognitive material, but for any coherent understanding of the non-linguistic behaviour of others. It plainly goes beyond the realm of linguistic competence, being independent in its form of the conventions of any particular language.

None of the skills or faculties attributed above to a competent interpreter are linguistic in any natural sense. They are certainly not possessed by the generality of English speakers. One way in which it might be argued, at most of the first two, that they are still within the domain of linguistic competence is by supposing that a group of people involved in such a context are using a special language in whose interpretation they are, exceptionally, competent. But this means relativising not merely truth-conditions but languages to contexts (however defined); and almost all of us are exceptionally competent in some usages not familiar to all speakers of a language (in the ordinary sense). In the absence of any more clearly articulated structure either of a 'context' or a 'language', we are left with a loosely-constructed proliferation of "languages", with respect to which the notion of 'linguistic competence' is arbitrarily, and indeed circularly, attributed.

Moreover, to define 'truth in English', say, by recursion over a sequence of sentences chosen by reference only to the supposed competence of 'English-speakers', itself involves an element of circularity in its determination of the class of sentences with respect to the class of speakers. No prior theoretical structure is available to determine either class, except in terms of the other. Tarski's own 'object language' was, of course, formally constructed, so avoiding circularity at a cost of remoteness from natural language. My own analysis, by contrast, retains the vernacular notion of what constitutes 'a language' (apart from the idealised 'base-language' defined for a particular GR-theory): and defines contexts in terms of the theories adopted by their participants. It relativises linguistic structures to contexts only in terms of the Φ-functions determining their local systems of reference: supposing that the GR-theory of a context may draw freely, for its linguistic structures, on pre-existing language forms.

It is, of course, no part of the burden of my thesis that linguistic competence is unimportant, or that the analysis of particular types of linguistic structure in relation to truth or meaning can be neglected. Although the level of linguistic competence involved in the interpretation of such sentences as 'It is raining' or 'Snow is white' is minimal, we shall shortly consider forms of linguistic structure whose interpretation generates greater interest for analysis. But first I want to look at two aspects of interpretation which might be regarded as borderline cases with respect to linguistic or non-linguistic skills.

(a) The way in which the understanding of deictic gestures, directions of gaze, etc., affects interpretation of accompanying words, or is affected by it. Both cases occur. In the above example, direction of gaze helps to tell the interpreter which of several possible bodies of "water" is spoken of. In an illustration given earlier, pointing to an elephant will be differently interpreted according to whether the pointer says, "That is a rogue bull", or, "Look at the damage to that left ear".

(b) The way in which dispositions, habits or social role of the utterer may affect, or be affected by, interpretations of his or her words. Again, both cases occur. A West End model and a Scottish crofter could be understood to have different standards about what counts as a truth-condition for 'It is raining'; but if the speaker adds '... on a broad front from Land's End to John O'Groats', we may infer that he or she is a meteorologist, and that the truth-conditions of the utterance are not naïvely perceptual.

These two cases emphasize the interdependence, and sometimes mutually supporting roles, of linguistic and non-linguistic skills in the interpretations of utterances, of which more will be said in Section IV.4. However, some factors involved in such interpretations are unequivocally matters of linguistic competence with respect to the language in use (neither this list nor earlier ones are claimed to be exhaustive):

(1) Standard lexical conventions rooted in past usage.

(2) Syntactic or grammatical rules determining the contributions to meaning of different elements in the utterance: for example, those governing the formation of compound sentences, or the effect of indexical elements such as pronouns, tense-forms or demonstratives.

(3) Lexical or syntactic conventions determining the dependence of the meaning of any particular utterance on the understanding of elements of previous discourse.

We shall see that the roles of these various factors in the determination of meaning and truth can best be understood in terms of various forms of meaning-dependence, analysable as occurring within the structure of a language, either within a context, or from context to context.

Meaning-dependence

It is sometimes said that no sentence has meaning in isolation from other sentences, and that this leads to a 'holistic' view of meaning in a language: but this notion needs careful interpretation. There may be a sense in which, once we accept the interdependence of meanings of different sentences, our understanding of this interdependence cannot stop short of comprehending the whole language; or, conversely, that 'a language' is recognizable as such precisely in terms of the mutual meaning-dependence of its sentences. But, from the analytical point of view, what matters is the form of dependence we attend to.

The forms of meaning-dependence studied in mainstream semantics have largely been those determined by syntactic relationships between sentences, parts of complex sentences, or other types of sentential form, characteristic of particular natural languages, or groups of such languages. Developed from Davidson's account of meanings in terms of the truth-conditions of sentences, this tradition can be traced back, through Tarski's definition of truth itself, to roots in Frege's and Russell's attitudes to language. These attitudes were more explicitly mathematical in inspiration than those of others of the period, even including Wittgenstein's. Frege's original motivation, in particular, had been to assimilate the logic of operations on numbers to standard verbal logic. Tarski developed a notion drawn from elementary mathematics - that of the satisfaction of an equation by constant values of its variables - by giving it a recursive definition, in association with an enriched language capable of non-mathematical interpretations (see especially his (1966), pp. 190/1).

The mathematical genesis of these forms of analysis is reflected in their search for formulations which hold good independently of particular interpretations of some of their terms; in just the way that, in mathematics, algebraic formulations can be found to hold independently of substitutions over a given range of values for their variables (the notions of recursion and satisfaction). Algebraic forms are, indeed, often used in semantics: and, when particular sentences or sentential forms are quoted, it is generally understood that many of their terms are to be regarded in the role of constant values belonging to a range or sequence over which recursion is in principle possible. To take a very simple and familiar case, the structure of the meaning-preserving transformation from 'John hit Bob' to 'Bob was hit by John' is independent of the meanings of any terms replacing 'John', 'hit', 'Bob' in such a way as to form intelligible sentences in each of the two syntactic constructions. Thus, the forms of meaning-dependence revealed are those generated by operations of logic or reasoning generalisable, without loss of truth, over a wide range of contexts: just as, in mathematics, algebra is used to analyse and develop systems of mathematical logic and reasoning. Prominent amongst those structures studied have been: (1) dependence of the meaning of a compound sentence (such as a conditional) on the meanings of its components, as revealed by the syntactic form; and (2) the extent to which the use of similar syntactic constructions with terms involving different ontological commitments (typically analysed within a recursive framework) may reflect similarity between the systems spoken of. The most successful projects so far in this field, as it concerns natural languages, use a development of model theory, drawn from mathematical logic, and initially devised by Richard Montague. Hans Kamp has intimated that he is engaged in model-theoretic analysis of a third form of meaning-dependence; namely (3) that obtaining within structures of <u>integrated discourse</u> involving two or more distinct sentences in each instance. The logical relationships of each integrated set or sequence of sentences are shown to be reflected by those of specifiable syntactic devices (to take simple examples, the use of 'Thus ...', or '..., therefore, ...'). It is likely that this last form of analysis will be most relevant to the study of syntactic structures associated with chains of reasoning developed in contexts of empirical theory such as those considered here (though from a different point of view). The associated definition of truth, in terms of the embedding of representations of fragments of integrated discourse in real-world models (which I shall not try to interpret), also points in this direction.

These forms of analysis are concerned primarily, then, with the effects of syntactic operations on the <u>previously given</u> meanings of elements of particular languages. It is plain that such a procedure does not exhaust the analysis of meaning. An equally important aspect of meaning is that which attaches to particular values of the semantic 'variables', which are not given in a general knowledge of the language, but derived from recognitions in context. For this purpose, the last thing we want is that the contributions to meaning of some of our terms shall be replaceable by those of others without loss of truth. This is especially obvious in the case of measurement, where a particular value of some quantity, recognized for some particular object, is what is to be captured in words. For the illustrated use of 'The water is clear', the interpreter's concern is with just what body of water is clear, and of what it is clear. For 'It is raining', we want to know in each case just where it is raining: the time of utterance ("now"), like the all-important position of the speaker at that time, is typically a matter of non-linguistic recognition, unique to the occasion. For these kinds of meaning, we must look for a different kind of dependence.

Dummett has shown interest in another form of meaning-dependence: that associated with what he considers as the sentence-by-sentence process of acquisition of the ability to use a language (Evans and McDowell, op.cit., esp. p.79: see above, p.390 , and below, Section IV.4). Like the syntax or grammar-based analyses already mentioned, however, this process seems to be understood primarily from the point of view of the rule-governed structures of the languages themselves.

But though more will be said about the relevance of such structures to the concerns of this study, the contrasting perspective of our primary interest, here, in recognitive structures leads directly to an analysis in terms of a quite different form of meaning-dependence, that between sets of sentences distinguished by being used in the contexts of particular empirical theories; especially GR-theories and their associated S-theories as adopted by particular L-groups on particular occasions. The contents, structures and limits of these sets are decided autonomously by the theories themselves, in terms of the more or less coherent systems of assignments and commitments adopted within them. The types of linguistic structure involved in this form of analysis are (1) utterances stating particular assignments or commitments in the 'base-language'; and (2) deductive systems of utterances whose truth-values are related in consequence of adopted commitments. More will be said later about the second type of structure, which consists essentially of systems of empirical conditions under which particular assignments are held true in accordance with the commitments of any given theory. At this stage I want only to point to the kinds of linguistic structure which are likely to figure in such an analysis. Recall, first, that the kinds of recognitive structure to be reflected consist of: (1) assignments of particular characters to particular idents; (2) assignments of particular idents to Γ-sets determined by particular determinant sets of characters; and (3) implicative commitments which, it has been shown, are all expressible in terms of recognitions of the structures of intersection of Γ-sets. It has been argued that the forms of all three of these types of recognitive structures are reflected precisely, through the Φ-function, by the sentential forms of utterances of a base-language to which a large part of the linguistic structure of any recognitive context can be in principle reduced: in that sense the analysis accounts for the fundamental status of sentential forms in all languages, independent of lexical, grammatical or syntactic convention. We may now further note that, as each Γ-set is typically spoken of by means of a single word or simple phrase (of suitable grammatical or syntactic type in the convention of the language in use, and independently of the degree of complexity of the determinant character-cluster), we may expect the structure of Γ-set intersection to be <u>to some extent</u> reflected in the <u>lexicon</u> of the language. Thus, while a 17th-century compendium lists 'whale' as 'the largest of the sea-fishes', a modern dictionary gives it as 'any of the large marine mammals of the order <u>Cetacea</u> ...' - reflecting the intervening theoretical developments. An exchange between Quine and Davidson at the Woolfson lectures of 1974 (not recorded by Guttenplan) revealed that they agreed that there was no hard and fast line between dictionary and encyclopaedia as an account of the current state of a culture's general knowledge. But we should not go too far in placing reliance on a language as encapsulating a society's "world view": it is only too easy to construct totally opposing theoretical accounts in the same natural language of the same community, whose contradictory statements are held true with utter conviction and at the highest levels of understanding of which people are capable, without any linguistic impropriety. The present analysis more closely parallels Putnam's (1970), in which he points to the force of specific empirical theories in determining 'stereotypes' - roughly, dictionary entries for words referring to 'natural kinds', in terms of descriptions of their normal members: sharply distinguishing the role in semantic analysis of such lexical forms of meaning-dependence, from that of grammar and syntax in the formation of sentences. More will be said of this in Section 4 of this Part.

The relationships between theoretical commitments, Γ-sets or classifications, and logic are subtle and interesting. Frege, for example, wrote (in an attack on Schröder's account of classes), "... classes are determined by the properties that individuals in them are to have ... only so does it become possible to express thoughts in general by stating relations between classes; only so do we get a logic." (Geach and Black, 1969, p. 104: my emphasis). Tarski said of the 'general theory of classes', which he took as the basis for a 'language of infinite order', "... it suffices for the formulation of every idea which can be expressed in the whole language of mathematical logic. It is difficult to imagine a simpler language which can do this." (1966, p. 242). We might now say something similar of set theory. But Tarski's plan was for a less highly abstracted structure whose elements were unspecified objects classified according to their assigned properties: an infinitary system which might readily be supposed to have restricted interpretations in the Γ-set structures of particular concrete contexts, according to C-theory. My own analysis leads to the view that, in any given context, theoretical commitments adopted by its Readers, in association with a Φ-function fixing references, give meaning for those Readers to the system of assignments - and hence to the structure of Γ-sets according to which classification of idents is carried out: these sets being the natural objects of reference of the principal linguistic terms from which sentences are constructed. Logic governs the operations which build, from the meanings (so given) of the component elements of the discourse - the recognized assignments and commitments - a total structure of interpretation for the context, especially the structure of reasoning from sentence to sentence within it. In the associated linguistic structure, the first aspect is likely to be mainly reflected in structure of classification embodied in the lexicon, as suggested above: while the second is likely to be mainly reflected in the structure of syntactic and grammatical operations, under the rules of the particular language (or class of languages) for the relevant words and sentences. We might expect a full analysis of the linguistic structure of a given context to combine a syntactic analysis on the lines of Kamp's treatment of integrated discourse (mentioned above, p. 401), with an analysis of lexical meaning-dependence between particular terms and expressions, under the empirical theory determining the relevant Γ-set classification. To extract the fullest meaning from the notion of 'classification' used here, it must be thought of not as a static taxonomy, but as a dynamic structure capable of reflecting sequential commitments, governing the understanding of motion or change, as proposed in Part II. (It will be recalled that recognitions by measurement have been exhibited as particular types of systematic classification.)

To illustrate the contrasting effects of the two approaches to analysis I have in mind, we need a relatively sophisticated example. If the doctor, after full examination, tells me I have infectious hepatitis, I know that it is true just in case I have infectious hepatitis, but that does not say I know what the sentence means. I may have no idea what 'infectious hepatitis' means. The point is that I cannot be given the meaning just by knowing the form of words. It may be that Davidson accepts this; he certainly accepts that we may know a speaker holds his utterance true, without knowing its meaning (Guttenplan, 1975, p. 14). But if we point beyond the form of words to its meaning, the inference appears to become circular. The converse inference, from knowing the meaning to knowing the condition for truth (if any), seems safe; but hands us back our problem of what it is to know the meaning.

The analysis begins to become more tractable if we suppose that a condition for truth at each utterance may be a <u>proper part</u> of the meaning of any sentence (which is capable of making a statement). It then follows that to know the meaning of an utterance of such a sentence is to know the condition for its truth at that utterance; and that the converse is false - which fits the form of my intuitive account. To pursue the question what else may be contained in the meaning, we may ask what else we need to know, if we are to understand it. We need to know enough of the language in which it is uttered to know that it is a sentence capable of making a statement; and to know enough about the past use of the words of which it is formed to know how, when put together in this form, they may be properly used in the immediate context of utterance. In C-theory, this last is to construct an inductive theory of the Ψ-function of the context as applied to the particular utterance: giving an assignment or commitment as the truth-condition, namely, that which is to hold iff the utterance makes a true statement. Plainly, this condition is context-dependent, as are all other aspects of the structure of meaning other than the form of words (which belong to the trans-contextual conventions of the language, and now appear as distinct from the truth-conditions).

It is the Ψ-functional aspect of the meaning which I have supposed missing from my understatnding of the sentence in my example. I know at least that the doctor is uttering a sentence in English: this comes from my general knowledge of the language. I may go on to infer that 'infectious hepatitis' is the name of a known disease, which has some relevance to what the doctor learnt in his examination, and from my answers to his questions: this flows from my theory of the recognitive and social context, my understanding of the doctor's role, the kinds of things he is likely to attend to, and the kinds of S-theoretical commitment he is expected to make. Does this inference contribute to an understanding of the meaning? It certainly does not involve a knowledge of the conditions for the truth of the diagnosis. My memory of linguistic usage may give me the further information, that it means my liver is inflamed, and that I caught the condition from someone else. If I am right, these are among the truth conditions, but seem to supply only a small part of the meaning: it does not amount to my knowing what the diagnosis means to the doctor, with his (her) knowledge of the causes, symptoms, probable course, and treatment (if any). If he is a good doctor, he will tell me as much of these things as he thinks I can handle and will be useful to me. I may come to know that it means (as we normally say) that I picked up a virus from someone's excreta, probably in food prepared with unwashed hands; that I shall go yellow, or yellower than I already am; that there is no known treatment other than drinking a lot of water, and no alcohol; but that it does not mean - as some forms of hepatitis may - any association with cancer. But I shall still not know all that it means to the doctor.

The evidence on which he holds his diagnosis true is unknown to me, and of no importance so long as I am satisfied that he has good evidence, and a good theory by which to read the evidence. I may suppose, therefore, that the truth-conditions for his diagnosis are satisfied, without knowing what they are. But, after his explanation, I do seem to know something of what it means to have infectious hepatitis: to me, the important aspect of what it means is its consequences to me. It seems that the meanings of some sentences may have a number of different aspects, of which those given by their truth-conditions are not attended to in all contexts.

Where truth-conditions are attended to, how do they contribute to meaning, and how do they figure in the linguistic structure in a context of this kind? Suppose that the doctor's recognitions of a particular set of symptoms constitute a valid set of truth-conditions, under the relevant medical theory, for a particular diagnosis: how is this to be represented? In general, I take the following equivalences to hold, with respect to the English verb 'to mean', for English sentences X and Y:

(1) 'X' means that Y $\equiv_{df}$ $X \rightarrow Y$

(2) 'X' means the same as 'Y' $\equiv_{df}$ $X \leftrightarrow Y$

N.B. The implications indicated are, of course, to be read in each instance as strictly derived under the utterer's or interpreter's theory of the context, not as expressing some independent logical entailment (see Part II, p.410). So read, they may be contributions to the GR-theory of the context. Condition (1) may at first sight seem an excessive claim, since most analyses in this field have so far concentrated on the search for a theory giving the _whole_ of the meaning of sentences. But the intention here is no more than to say that if $X \rightarrow Y$ (under the relevant theory of the context), then 'Y' is _part_ of the meaning of 'X' _in context_. The claim is that the expression ''X' means that Y' - often phrased 'If X, then it means (or, _that_ means) that Y' - is commonly used as a way of stating the commitment. The fact that roughly equivalent constructions can be found, at least in other European languages, may be thought to reinforce the claim. The relationship between this notion and more general notions of meaning and truth will be further explored below.

The equivalence proposed in (2) seems to me to conform with Tarski's use both of the concepts of logical equivalence (identity of logical consequences) and of translation, in general and in its application to the semantic analysis of truth.

To apply these equivalences to the chosen illustrative example, I introduce the following simplified _ad hoc_ notation: let A,B,...,F be recognitions of a doctor's R-theory assigning _symptoms_ to a patient x; and let H be the commitment assigning a _diagnosis_ of hepatitis to x - this last may be regarded as an S-theoretical commitment of medical theory, _incorporated_ into the relevant R-theory. A,...,E are supposed assigned at the "present" frame of the context: F is assigned _at_ the present frame for a "future" frame - for which it represents _prognosis_.

In any case where <u>utterances</u> are understood under the Φ-function to state any of these assignments or this commitment, implications hold between them under the theory, as between the conditions stated. I shall use "A",...,"H" to represent such utterances.

Initially, then suppose the doctor's R-theory to include the following commitments:

(3) $H \rightarrow A \wedge B \wedge C \wedge F$

(4) $(A \wedge B) \rightarrow H$

(5) $(A \wedge C) \rightarrow H$

(6) No implication holds under the theory from the single conditions A,B,C, or F, to H.

(The converses of (4) and (5) follow from (3).)

It follows from (1) and (2) and the definitions - including those for the special notation - that "$A \wedge B$" or "$A \wedge C$" or "$A \wedge B \wedge C$" mean the same as "H"; and "H" means that "$A \wedge B \wedge C \wedge F$". A,B,C and F are necessary, but not sufficient, conditions for H to hold - i.e., for "H" to be held true in the theory; they are, I say, parts of the meaing of "H". On the other hand, if any <u>set</u> of these conditions is both necessary and sufficient for "H" to be held true, then that set is logically equivalent to H; and has some special claim to be called '<u>a set of truth-conditions</u>' for "H" under the theory. This requirement is satisfied in the example by {A,B}, {A,C}, {A,B,C}, and, trivially, by {H}. Our illustration suggests, therefore, that it may not be untypical for a given utterance to have more than one set of truth-conditions in a context, trivially including, in all contexts, the unit set comprising that condition which is taken to be stated by the utterance itself. We may call the set of all such sets of truth-conditions the 'truth-conditional set' for the utterance in context.

To bring out some of the critical consequences of this analysis, suppose now that a second opinion is called for, and that another doctor, observing the same symptoms in the same patient, diagnoses serum hepatitis - a disease with similar initial symptoms, but different aetiology and prognosis, from what is called "infectious" hepatitis (the terminology is, in fact, somewhat confused). We may represent this situation as yielding, in the first instance:

(7) $(A \wedge B \wedge C) \leftrightarrow H_1$ (Doctor 1) (9) $H_1 \rightarrow F$

(8) $(A \wedge B \wedge C) \leftrightarrow H_2$ (Doctor 2) (10) $H_2 \rightarrow (F)$

Where H_1 and H_2 diagnose mutually exclusive forms of hepatitis for x, having contrary prognoses. The doctors agree on (9) and (10); and decide that they can settle the matter by a further test, under conditions to be set out below. That is, they revise their initial R-theories to adopt a joint GR-theory holding, in place of (7) and (8) above:

(11) $(A \wedge B \wedge C \wedge D) \leftrightarrow H_1$; (12) $(A \wedge B \wedge C \wedge E) \leftrightarrow H_2$; and (13) $\neg(D \wedge E)$.

N.B., the doctors' GR-theory, whose commitments these conditions set out, is much more comprehensive than the patient's; the patient need not understand all these aspects of the truth-conditional structure, in order to extract those parts of the meaning which concern him.

In terms of the equivalences (1) and (2), the doctors' GR-theory, "$A \wedge B \wedge C \wedge D$" means the same as "$H_1$"; "$A \wedge B \wedge C \wedge E$" means the same as "$H_2$"; "$A \wedge B \wedge C$" means the same as "$H_1 \vee H_2$", or "H"; "$H_1$" means that F, and "$H_2$" means that $\neg$(F) - i.e., that x will not have the symptom assigned by F. Note also that, for example, "A B C E" means that x will not have this symptom; is there, then any sense in which the <u>naming</u> of the disease by "H_2" (or the simple assignment H_2) carries a meaning beyond the recognition of the four present symptoms - or is it no more than a convenient shorthand, and something for the patient to tell himself and his friends? We might say that it specifies a particular virus associated with the name - an element which may be hypostasized under the incorporated S-theory, being itself not recognized by direct perception, but by the symptoms it is taken to cause (as, we said, a fruit is recognized as "ripe" by its colour). In this case, the presence of the virus would itself belong to the truth-conditional set. Alternatively, we might say that in some sense the name "H_2" specifies a particular, larger context of medical theory in which the immediate context of diagnosis becomes embedded in some way. This aspect will be considered further below (p. 462), in the context of certain proposals about the semantics of 'natural kind' terms; here I want only to note that the important part of the meaning for the patient - and for the doctor as therapist, rather than diagnostician - is the prognosis derived under the theory. This strikes me as a principle, with regard to the structure of meaning in concrete contexts of recognition and understanding, which is widely generalisable.

Note also that the structures of truth-conditions for the two possible diagnoses do not differ essentially in the syntactic or grammatical forms which would naturally be generated for any utterances in which they might be stated. The critical difference resides in the structure of medical theory in context, and, therefore, to the extent that it is reflected in the linguistic structure, it will be in the lexicon - the medical dictionary or textbook. That is to say, the critical structure is that of meaning-dependence under the empirical theory of the context: again, I would claim, a widely generalisable principle.

At the same time, it is plausible to insist that the syntactic structure of any fragment of discourse associated with the context should, to be correct, reflect in some definite way the system of meaning-dependence taken to hold under the relevant theory - though each structure of meaning-dependence (syntax- and theory-based) is underdetermined by the other. If verbal argument were to develop between the differing doctors, critical analysis of their syntax may well reveal important aspects of their system of reasoning: these aspects of their meaning are, however, to be read in conjunction with the lexical meanings of the words, and the associated theoretical commitments. My own conclusion from the discussion so far is that it would be mistaken to look for a single, comprehensive Theory of Meaning; there is a sense in which we can say that each R- and S-theory of each context contains a theory of meaning for that context, comprising the Ψ-function and the structure of commitments. Such a claim would belong to a partial meta-theory of meanings. To complete such a meta-theory, we need at least some account of the structure of transcontextual meanings within particular language systems. This aspect lies mainly outside the present scope, but more will be said of it in Section IV.4.

It might be suggested that I have used 'meaning' in a different sense from that intended by Davidson - that I don't mean what he means. But this might mean (imply) that we have to look for a meaning (sense) of 'meaning' which involves no implications or presuppositions appealing to background knowledge, beyond some minimal interpretation of the specified sentence. But can we really be said to attach meaning to a sentence which neither has any understood consequences, nor is itself a consequence of anything? I have noted that has been the search for inference-free constituents of experience which has inspired appeals to such elements as uninterpreted sense data, qualia, erlebnisse, etc.; but a main line of argument in this study has been that recognitions of even such apparently simple elements can only be understood in a framework of theory (in at least the weak sense defined in the introduction, p. 19). Quine and Ullian's 'observation sentences' may seem to offer an example of a parallel search for the inference-free, in the field of linguistic structures, in which case a similar argument would apply to them (see their (1978), passim). My treatment of the doctors' diagnosis suggests, rather, that analysis of sentences in terms of the theoretical structures of the contexts in which they are used yields a well articulated, and subtly graded, account of different aspects of the meanings of those sentences: in which the relevant theory in each case determines the total meaning for each Reader in context, including his understanding of the relevant aspects of transcontextual meanings in any natural or specialised language on which he draws.

More will be said about this transcontextual aspect below (Section IV.4). Meanwhile we can say, briefly, that the meaning of a particular utterance is given by its contribution to the structure of theory in context. Davidson has explicitly rejected any project of 'relativizing a T-sentence to a proof or theory' (1973, p. 326). This rejection, which may be addressed to intuitionistic accounts of meaning quite different from the present analysis, is not very strongly or closely argued. Two possible motives for such a rejection may be considered. At a superficial level, there may be a fear of producing unmanageable complexities for analysis; and I hope this study might do something to allay such fears. More deeply, it may be thought that relativizing concepts of knowledge or meaning necessarily leads to a relativist doctrine of truth or reality. This is not a consequence of C-theory; but I shall leave consideration of this point until I have dealt with some other approaches to such questions, including those of Popperian philosophy of science.

This much can, however, be said at this stage about the concept of truth. Tarski restricted his definition to formal languages for which it is possible to construct precisely defined metalanguages; expressing doubt that such a precise analysis could be extended to natural languages. Various important writers, including Davidson, Quine and Popper, have wished to suggest that such an extension is at least in principle possible. But I have not found that this suggestion has ever been fully and formally argued, and the line of argument given above leads me to echo Tarski's original doubt, at least as it concerns any natural language, such as English, unrestricted by any well-defined structure of understanding. The common formulation of the Tarskian equivalence:

's' is true in L iff p ... (1)

(where 's' is the name of a sentence of language L, and p a proposition which it states, or which constitutes its truth-condition) is meaningless in any instance to any person who does not understand either s or p. If I am right in saying that meaning is given in every instance by a theory of the context of utterance, equivalence (1) is context-dependent in just this way. In Tarski's own formal treatment, an abstract meaning with no external reference is given by his theory of the defined linguistic structure, in terms of the notions of substitution and satisfaction: this is the theory of the context of his utterance (1956, passim). A much weaker version, based on that given above (Section II G)

$s \in L^G$ is held true in Θ iff $\Phi(s)$ holds in Θ ... (2)

(where is an utterance of base-language L^G of group-theory Θ, of which Φ is the reference function) holds, at least for recognitive contexts, without restriction as to which (natural or other) language L^G may be drawn from. This equivalence relativizes the property 'held true' of utterances to particular theories, whatever their linguistic structures, while reltaining a Tarski-like form. This modification of his convention can thus be used rigorously and rather unproblematically not only for the languages of scientific theories, as he hoped, but for much less sophisticated contexts using natural languages.

The sense here of the phrase 'held true' appears to be subtly but importantly different from that used by Davidson, when he says that "at an intermediary stage ... in giving form to a theory of interpretation ... the attitude of <u>holding true</u> ... , as directed towards sentences, must play a central role ...", and that "... it is the pattern of sentences held true that gives sentences their meaning" (Guttenplan, ed., 1975, p. 14 - his emphasis). But he contrasts this property of sentences, as determined by individual attitudes, with that of being "socially", or "in fact", true (ib., pp. 17, 22): a contrast he points more sharply in Synthese (vol. 27, 1974, esp. p. 321) as that "between sentences held true by individuals and sentences true (or false) by public standards". He thinks this distinction "essential to ... interpersonal ... communication" (Guttenplan, op.cit., p. 22). In so far as a propositional attitude is involved in my own account of 'held truth', it is only that which is implied in the adoption of a particular empirical theory (such as an R-theory of a context): my method is to analyse the theory independently of the attitudes or motives of those who adopt it. Where a GR-theory is adopted by the majority of some society, so as to acquire the status of a "public standard", it is no less appropriate to qualify its assignments or commitments as held true in that theory. The relationship of this notion to a more general or absolute concept of truth will be considered below, in Section IV.7, as part of a full discussion of theoretical structure in Part IV.B. This discussion goes far, I think, to articulate the concept of 'held truth', and to show why it is "central" to any account of meanings.

But something must be said at this stage, as part of a specific discussion of the role of language, to justify the introduction of the concept of 'theory' as logically prior (in some contexts) to the understanding of language; which may be thought to accord too fundamental a status to that concept.

(2) Theories as Fundamental Structures in Understanding

The notion being used here, of a 'fundamental structure in understanding', is to be sharply distinguished from the notion of conceptual or logical 'primitives' (as unanalysed components of a system); though closely associated with it. It involves the prior intuition that we cannot be said to understand any aspect of experience merely in terms of collections of primitive entities or properties, regarded as constituting the subject-matter of our understanding (say, at a particular time and place). Understanding, on this view, necessarily involves apprehending some minimal array of relationships between these elements: the degree to which these relationships are apprehended as forming related structures (as distinct from arbitrary sets or collections) may be plausibly put forward as a criterion of the extent to which these elements in experience are understood. This leads naturally to the notion of minimal structures, constituting the least complex systems of primitive relationships between primitive elements, in terms of which an aspect of experience can be said to be understood. Frege's contention, for example, that the sense of a word is only determined in the context of at least one sentence, can be seen as a proposal to define the sentence as a fundamental structure in the understanding of language: involving the consequence that not only the words, but the system of grammatical relationships in a language, according to which collections of words are formed into sentences, are essential primitives in an account of the language.

My analysis of the R-theories of concrete contexts can be seen as proposing the assignment (of character to ident) as a fundamental structure in this sense; which may be prior to, and even independent of, the introduction of linguistic structures into the context. Formally, a single assignment of a single character to a single ident constitutes a 'theory' - though a very weak one - since it satisfies the definition of an R-theory given at the outset of Part II, which itself satisfies the informal criteria for a theory given in the Introduction. The way in which such a minimal 'theory' might be constructed, and how it might be enriched by the introduction of a linguistic structure, can now be illustrated.

Since such a one-assignment theory contains no linguistic structure, however, it cannot be brought into the context of this writing, by way of example, without being already extended by the addition of at least a one-sentence "language". I shall therefore form such a theory out of my own, immediate, wordless experience: and then add a specific sentence, drawn from English for convenience - 'There is a red patch'. But this is not enough to form a complete GR-theory within which you, as reader, can understand the one assignment which formed my original R-theory. We need also a Ψ-function associating the utterance constituted by my writing the sentence above, with the original assignment. This involves, in some way, the further tacit implication, "... here, near me as I write" - a sort of Gricean implicature. In so far as we now have a GR-theory, its one commitment is to the assignment of "redness" to some "patch". The existential commitment carried by the words, 'There is ...', is implicit in the definition of an R-theory, so that its introduction in order to complete a grammatical sentence adds nothing to the structure (this aspect is discussed further under 'ontology' below, Section IV.5.)

I am not characterizing the red patch as a sense datum, quale, irreducible percept, or any other type of entity belonging to any theory other than an R-theory in the terms of this study. I could as well have written 'There is a red bus'. But my aim here is maximum simplicity and minimum restriction on the potential generality of the R-theory in the context of a more extensive theory. Nevertheless, in trying to bring my one-assignment theory into the public domain, I have had to introduce further elements into the context: the language, English, from which my one descriptive sentence is drawn; the paragraph, and indeed the whole study, in which its meaning is located; yourself as reader. There appears to be <u>some</u> meaning in the sentence in this extended context, since we both know the lexical meanings of the words. This knowledge, I suggest, comes for each of us from his or her experience of the use of English, alongside our own experiences of "patches" which can be called "red": which, in your case, does not include the patch I meant. The particular shade of red, the shape and texture of what I mean in this case by a patch, all of which are precisely recognized in my original one-assignment theory, are not determined by the words. Someone with me in the room as I write, on the other hand, would have access to another source of information, that of immediate perception. For her (let us suppose) this may be enough to complete, with the single sentence alone, a coherent GR-theory adopted by her and me as L-group (neglecting differences between our perceptual experiences). To make more certain, I might point, add further description, ask questions: none of which will greatly assist a reader remote in time and place from this immediate context, who has access only to the linguistic elements and their transcontextual meanings.

My main point is that the R-theory I instanced above - before its linguistic enrichment - was extremely simple. As a fundamental structure, it was simpler than the same structure enriched by the addition of even one sentence and at least one other Reader. Nor do I see any reason to suppose that the linguistic enrichment increased my own understanding of the original one-assignment theory. The sentence <u>by itself</u> is also very simple as a fragment of language: a natural example of a fundamental structure in linguistic theory. But its meaning in isolation from actual recognition is only partially determined. Linguistic theories - lexical, syntactic or semantic - are concerned with aspects of meaning specifiable for all contexts, or at least a large class of contexts. The missing component, with respect to the meanings of particular uses of sentences as recognitive reports, is the aspect of complexity, subtlety and detail in perception which varies from context to context, and from frame to frame: whose understanding is supplied not by the words but by perception itself. We are close here to the problems raised by Wittgenstein's questions about the possibility of private experience or a private language: and these must be addressed before we attempt further analysis of the contributions of language to concrete contexts.

(3) The Private Language Question and the Limits of Language

(References to 'PLA' in this subsection are to The Private Language Argument, ed. O.R. Jones. The initial quotations are from the late Notes of Wittgenstein, translated and edited by Rhees.)

> It is as though, although you can't tell me exactly what happens inside you, you can nevertheless tell me something general about it. By saying, e.g., that you are having an impression which can't be described.
> As it were, there is something further about it, only you can't say it; you can only make the general statement.
> It is this idea which plays hell with us.
> (PLA 233)

> Can one say: 'In what I say of someone else's experience, the experience itself does not play a part. But in what I say of my experience the experience does play a part.'?
> I speak about my experience, so to say, in its presence.
> (ib. 235)

It seems that Wittgenstein continued, at least until very late, to be troubled by the common suggestion that there are some elements or aspects of experience which cannot be expressed, or at any rate told to others, in language. One way in which he addressed the problem was through the question whether, if some experiences are inexpressible in ordinary language, there might be some other form of language in which they are expressible.

This immediately raises the further question how such a language might differ from ordinary languages. Wittgenstein's own first and simplest idea seems to have been that it differed in that the objects of reference of its referring expressions belonged exclusively to the experience of its user, so that these expressions would be meaningful to him but meaningless to others. These objects of reference were thought of as 'private sensations'; hence the supposed language was called a 'private language'. It is clear that Wittgenstein wished to dispose of the common belief in the first - which fitted ill with his general position - by showing that the supposition of the second is incoherent. The project failed: partly because showing that particular accounts of a private language are incoherent does not prove the general principle; but, more seriously, because even the general principle would not dispose of the idea of inexpressible sensations, which most people find quite easy to grasp, if hard (of course) to put more explicitly into words. But the resulting explorations have generated much interest, and throw light on various possible accounts of the relationship between language and the rest of our experience.

One obvious objection to the idea of a private language is that it would lack the most important property of languages, a power to communicate. Is this objection answered by pointing to languages of private record, such as da Vinci's notes, or Ayer's notion of a language invented, Adam-like, by Crusoe to name the unknown creatures of his island? Such examples largely miss the point, since we may reasonably expect that, in general, records which are 'private' in such ways as these will nevertheless speak of publicly recognizable objects and properties, and to this extent could be made public by being translated or taught to others. But the question remains whether there might be residues in such a language which speak of areas of experience inaccessible to others (because in principle uninterpretable in terms of common experience, perceptual or other): so that their descriptions in that language would be untranslatable into communicative forms. If the idea of a private language is to be made coherent, then it must surely take the form of language of private record. This notion has been attacked (by Wittgenstein and others) on the ground that it places undue reliance on an individual memory, not only of the apposite use of an expression, but of earlier private experiences supposed to correspond with that for which it is immediately used (see e.g. Kenny, PLA 217). But this weakness also undermines the use of public language to describe experience remembered by isolated individuals. I shall later discuss further the ways in which memory is involved in the construction of the linguistic elements in recognitive contexts generally.

One possible source of confusion is the use of the term 'private' both for experiences and for the language in which they are described, when it is by no means clear whether the concept of privacy applies to both in the same way. Kenny, for example, offers two interpretations of the term (ib. 215 f.). The legal word '<u>inalienable</u>' suggests a property vested permanently in a named individual. Of experience, it might be read as saying that it cannot be given or transferred, in itself, to another. This is contingently true in our own world, at least for the generality of our sensations (to use Wittgenstein's term). I shall not explore the intriguing possibilities of a capacity, say, to feel the pain of others, whether in this world or some other. Sensations which could be literally shared could become public objects describable in public language, and would not present Wittgenstein's problem in the way our own world does so. Of language, 'inalienable' has no obvious meaning, but might suggest the impossibility of transfer by teaching or translation. Kenny's second interpretation, '<u>incommunicable</u>', seems equivalent to 'inexpressible' for experience: for language, unless it is an odd way of saying 'useless for communication', it can, I suggest, only point to something like a silent language of thought - which will be briefly discussed below. An interpretation of privacy of experience we have already noted is that such experience is inaccessible to, or unrecognizable by, people other than those to whom it occurs. It does not follow that, to describe such experience for its "owner", a language must be inaccessible to others (in the sense given above p. 427), or even unrecognizable as language. In C-theory, we can point to the special case of a GR-theory consisting of just one R-theory, the L-group consisting of just one Reader adopting that theory, and the base language being a language of private record devised by that Reader as a means of maintaining and monitoring theoretical consistency throughout an extended R-sequence. There can be no a priori basis for an argument that such a language should be inaccessible to inspection by others; or even that they could not recognize it as a language, especially if the Reader tells them that is what it is. They still may not be able to learn to understand it, if some or all of its objects of reference are in principle publicly inaccessible or unrecognizable.

A model of such a language is provided by H-N. Castañeda's "Privatish"; a language which makes use of logical and other auxiliary terms from ordinary language, but all of whose concepts associated with objects of reference and their properties, and entailments involving these concepts, are publicly inaccessible (PLA 137 f.). Possibly to fend off such a development, Wittgenstein had progressively imposed restrictions on his own version of a private language: that it should share no terms with public language, and even perhaps have a different logic (ib. 136, 158). Our analysis must lead us to agree with Castañeda that such restrictions are arbitrary, and make nonsense of the whole idea. It is impossible to see how such a language could be constructed or understood by anyone; moreover, the restrictions are quite unnecessary for a language merely to speak of publicly inaccessible subject-matter.

In its most extreme form the notion of a private language could, as I suggested above, be applied to a silent language of thought - which might most easily accommodate publicly inaccessible subject-matter. This would almost certainly not meet Wittgenstein's requirements, since he presumably would not have accepted that anything could be 'expressed' in such a language. Such a language is indeed "private" in the sense of "inaccessible to others", given above, but the public attribution of thoughts to ourselves and others is common, and forms the basis of at least two existing approaches to the analysis of thought in terms of language: by Davidson (in Guttenplan, (1975), S.C.Brown, ed., and Synthese, vol. 27; (both of 1974) and Gilbert Harman (1973). Davidson contends that the attribution of thought by one individual to another requires that both shall be interpreters of language - though apparently not necessarily the same language. This contention is raised in the context of a broad programme of 'radical interpretation' of the behaviour of others in terms of attributions of beliefs and desires: which sees this task as one of establishing correlations between such attributions and observed behaviour (i.e., intentional action). Loose analogies are offered with Ramsey's analysis of correlations between attributions of beliefs and desires, as expressed in terms of 'evidence' and 'perceived utility', in observations of betting behaviour. The central proposition is that only the interpretation of <u>verbal</u> behaviour can yield any "detailed" understanding of another's beliefs or desires (Synthese, op.cit., p. 312). Davidson himself does not hold out much hope of a practical issue for such a programme, as he frames it, and cannot claim it as more than a broad indication of a possible rationale supporting our common attributions of thoughts to people.

My own strategy, seen in this context, is to concentrate analysis on <u>beliefs</u> (in terms of theories adopted from time to time by individuals or groups): leaving motivations (desires) to be understood only as expressed in terms of the choices of elements, assignments and commitments for <u>attention</u> in context. Attributions of thoughts to others enter this scheme only peripherally, except to the extent that a Reader assumes that he and other members of an L-group are adopting a common GR-theory in some context: a policy dependent on the availability of a rational strategy of <u>neglect</u> of differences of belief and motivation (which can hardly be absent in any normal context). This account may be thought to give some precision to Davidson's firm acknowledgement of "... the need to view others, <u>nearly enough</u>, as like ourselves" (Brown, op.cit., p. 52; my emphasis).

But, if extrapolations from our own thoughts to those of others may be rationally undertaken, the argument that detailed understanding of others' thoughts depends on language, which is in general reasonable enough, does not involve the denial of non-verbal thought to others. Davidson is silent on this point, as far as I have been able to discover. But there is nothing in his analysis to exclude the possibility of non-verbal thought: he denies only that it can be precisely attributed to others. He points out, indeed, that all "standard ways of testing" interpretations of decision or preference make use of language (Guttenplan, op.cit., p. 15): but it must be common ground that attempts - however successful or otherwise - to construct quantified theories of such matters must employ verbal instructions, and verbal (or verbalisable) responses: together with that crucial assumption of near-enough likeness between human beings.

Harman, by contrast, though he restricts analysis on pragmatic grounds to attributions of verbal thought, contends only that we sometimes think in words, saying explicitly that "... not all human thought is in words. Our conception of ourselves in the world is more like a map than a story; and in perception our view of the world is more like a picture" (op.cit., p. vii; and see also pp. 84, 182/3). He rejects, as I would, Ayer's pronouncement that it is "analytic" for him that unless a thought were expressed in words he would not allow that it was a thought (Ayer (1969), quoted op.cit., p. 58). It may be that less visually-minded people will find Harman's account strange. But I am in no doubt that if I try to work out a way to reorganise the furniture in my room for some practical or aesthetic purpose, and even to consider changing or adding items, I do so wholly or largely without using words; and that this activity is properly called thought. If I wanted to make my thoughts accessible to others, it would be natural to supplement words with plans or drawings, to bring out aspects not readily or precisely captured in words: "I thought of something like _this_." Nor need non-verbal thought, on this account, be restricted to visual subject-matter; the "something I thought of" might be a musical phrase (if I were talented that way).

At first sight, however, it may seem impossible to carry out a serious analysis involving non-verbal thought, rather than merely to seek to invoke general intuitions about it. Harman, indeed, makes no such attempt, confining himself to speaking "as if" a language of thought exists, whose "sentences" are "mental states" attributable by statements about attitudes of belief, fear, questioning, etc., with respect to publicly utterable sentences, supposed to represent thoughts (I shall not discuss his notions of 'representation' or 'symbolism'). He claims, plausibly, that the logic of such attributions clearly follows that of attributions of attitudes with respect to sentences which are quoted from actual public utterance by those (including oneself) to whom these attitudes are attributed.

But it is a short step from the lines of argument considered here, to a less than bold supposition that the relationships between linguistic and non-linguistic structures in thought are logically parallel to those between similar, publicly perceptible structures of which they think, in any particular context. Broadly speaking, these relationships have been exhibited, for what I have called concrete contexts, in terms of a function, formed as part of each particular GR-theory, from conventional rule-governed structures (drawn from one or more socially and historically constructed languages), into structures of assignment and commitment built of elements _not_ belonging to such languages. Both sets of structures are understood as perceptually recognized in context. If, as I suppose, we typically rely in such contexts on verbal and non-verbal sources to _supplement_ each other in our common understanding (each potentially providing information not given by the other), the same may be reasonably assumed typically true of thought about verbal and non-verbal aspects of concrete situations. (This assumption, I can say from personal experience, is fundamental to film-makers' understanding of their craft. We _think_ of - and in - pictures, sounds, words and music as distinct and complementary resources from which to generate and assemble meanings.) Thus, many recognized assignments or commitments in a typical context are not functionally associated with any utterance: and, where some utterance is functionally associated with a non-verbal condition, it is typically used to impart additional information derivable from further associations with past uses of the words. I take this to hold for thought as for perceived conditions.

Being conventionally constructed, the linguistic structures and their potential relationships with other structures must be learned, whether by practice or from instruction - whatever innate faculties may be involved in the process. In any particular context, the precise functional correspondence must be specifically constructed within the relevant GR-theory, using the participants' memories of past usage.

In this account of the role of language with respect to non-linguistic recognitions, anything which might be called a 'private language' would appear as a special case: in which the social 'group' constructing and using the language would consist of just one person. While this may strain the use of the word 'convention' - interpreting it as a set of decisions by this person to stick to certain rules of association on all occasions of using the language - there seems to be no obvious principle, in what has so far been said, to exclude this case. While interpersonal communication is unquestionably the most important role of language, private record is also a perfectly valid one: not depending in any obvious way on previous use of the same structures in communication. Learned associations with past usage are no less available in such a case.

I should be inclined to follow those who, like J.F. Thomson (PLA 168) suggest that Wittgenstein himself did not take private language very seriously. He just wanted to deny the possibility of private experience, but found that every argument against forming a coherent theory asserting this possibility was also an argument against a theory denying it (ib., 103). The most he could do was to point to the apparent hopelessness of trying to discuss it. But discussion does not seem to be as hopeless as he thought (and perhaps Davidson, also). The essential question, in the light of the above considerations, is whether <u>some</u> (especially perceptual) information, which is part of individual experience, is <u>in principle irrecoverable</u> from past associations with the use of language (public or private, uttered or thought) - whether or not the language is being used, in its presence, to fix attention on it, allowing perception to supplement the verbal information. If so, we may go on to ask in what sense it may be appropriate to regard these non-verbal aspects as contributing to the meanings of the words or sentences involved. How, for example, do we know whether a colour-word 'means' the same colour for different Readers, or for the same Reader at different times? The answer seems to be that we do not: but that we find that in many cases we can <u>neglect</u> whatever <u>differences</u> of meaning there may be, without being faced with contradictions. Now suppose that in some cases the difference cannot be safely neglected: how would contradictions show up, and what, if anything, would they tell us about the meanings of the associated linguistic structures? I shall draw an illustration from the context of colour-blindness, a phenomenon which is quite well understood.

Colour-blindness is not a form of blindness any more than tone-deafness is a form of deafness. It means only that the affected person fails to distinguish certain colours from one another, either as well as those with optimum colour vision, or, in extreme cases, at all. The commonest form is red-green blindness, which affects about 8% of European males to some degree, 2% of males totally, and females less frequently. To screen candidates for jobs where colour discrimination is important, there is a system called the Ishihara test, consisting of a set of cards each of which is printed with an all-over pattern of small patches of colour so that, to take the simplest case, someone with optimum colour vision will distinguish one outline, say a letter A; while someone with total red-green blindness will see another, say a letter B. (Incidentally this brings out something about boundary discrimination which is relevant to my general thesis about the recognition of ident boundaries by character-differences: its explanation may also help to make sense of the example. To distinguish a particular segment of visual boundary it is necessary that <u>both</u> (a) there is a perceptible <u>difference</u>, e.g. of colour, between the regions on either side; <u>and</u> (b) on at least one side there is enough perceptible <u>similarity</u> between different sub-segments for the boundary to be seen as continuous. If Bill is red-green blind he will fail to see the A on our Ishihara card because he fails to see that the colours on one side of its outline are on the whole redder, and on the other generally greener. Less obviously, Alf, with good colour vision, will fail to see the B that Bill sees, because his discrimination breaks up the boundary into irregular reddish or greenish blobs which are lost in the general mixture of colours.)

For my example I suppose a possible world which is like our own in almost every way, except that there is only one red-green blind person, Bill. Everyone else, including Bill's friend Alf, has perfect colour vision. Since they have no trouble with colour-blindness, there is no Ishihara test in this world. Bill, not wishing to seem stupid, has so far got away with pretending to see like other people, having learned that leaves are called 'green' and bricks 'red', and so on (as many people do in our world). Then an Ishihara card drifts into this world from a passing space-ship, and Bill picks it up.

ALF: 'What is it?'

BILL: 'It's a funny-coloured card with a B on it.'

ALF (looking): 'That's not a B, it's an A.'

What do they make of that?

1. The example brings into sharp focus something that Wittgenstein refers to when he says, "... people, as we say, sometimes see different <u>things</u>, colours, e.g., looking at the same <u>object</u>" (PLA 271/2, my emphasis).

(He goes on to argue that 'sense datum' language is unhelpful in such a context.) In my example we should surely say, quite naturally, that Alf and Bill are both seeing the same object, the card; but that they see on it not merely different colours but different things, the A and the B. (In the language of C-theory these would be idents; the A in Alf's frame and the B in Bill's.) This suggests to me that the ordinary language of 'seeing', perfectly adequate for most ordinary contexts, neglects a great deal of information about what, when we study the matter, we find to be involved in visual recognition; information which may be important, and therefore not to be neglected, in particular contexts. Generally, apart from pure research, it is only when things go wrong - as with Alf and Bill - that we need to go beyond simple statements of who sees what, to more sophisticated theories to account for apparent discrepancies between individual recognitions.

Sense datum language belongs to a vague philosophical theory about the structure of perception, including vision. Its relationship, if any, to current scientific theories - psychological, physiological, physical - is not at all clear. It would presumably say of Alf and Bill that their dialogue becomes easier to understand on the supposition that they each construct their perceptions on distinct sets of sense data, such that they agree that they see a variously coloured card, but disagree about a major feature of it, namely the letter to be seen printed on it. Wittgenstein rightly says that we should not speak of 'seeing a sense datum' (same, or different, for that matter - see PLA 273); the alternative, 'having a sense datum, and saying ...' (ib.), leaves out anything the theory may have to say about how propositions about objects, etc., may be constructed upon sense data. That is to say, this alternative language neglects the main thesis on which the theory is built; understandably, in Wittgensteinian terms, since there seems little or nothing to be said about it, except to assert that it occurs, as Russell did.

Wittgenstein further suggests (272) that if we use a 'theory of sense data' to explain discrepancies in the 'language-game of seeing', we ought to apply it in <u>all</u> cases, whether or not there is a discrepancy. This is misleading if it is taken to mean that, if we accept the theory, we must modify our language of seeing for all contexts. It is as if, after Copernicus, we could not admire a sunrise without saying, "How beautifully the earth's rotation reveals the sun's disc this morning." We use the simplest language that meets the needs of the context, neglecting irrelevant complications. He is also wrong to imply (if I understand him) that the adoption of a sense-data theory would generate some doubt or systematic scepticism which is not already implicit in the ordinary language of seeing. If we <u>sometimes</u> see different 'things' while looking at the same 'object', why not always? Theoretical entities like sense data are introduced into the context not to generate doubt, but to make sense of situations where doubt has already been generated.by discrepancies in the naïve account. There are arguments against sense-data theories - in particular their vagueness and lack of empirical content - but the creation of doubt is not one of them.

There is nothing, indeed, to prevent us from forming theories about each other's sensations involving publicly inaccessible entities. Whether we call them sense data, retinal images, stimulations of the visual cortex and corpus collosum, or whatever, will depend on what theory we are using; as will whether or not we think it relevant to call them 'private'. Evidence for their recognition, or for the truth of statements about them, may be thinner than for simple public objects; and will vary in doubtfulness with different theories about different types of sensation. In this they are on a par with theories about the atoms, quarks, etc., of which all public objects may be supposed physically constituted. Current theories of perception or sensation, rich in entities which are not publicly accessible in any ordinary sense, are now increasingly well established. They do nothing to disturb our ordinary use of the language of seeing, or to sow doubt of our capacity to see, but help us to understand the structure of visual recognition, including the phenomenon of colour blindess; even if they do not say just what 'sensations' _are_.

Wittgenstein himself seems not to have been averse to speaking of 'sensations' as distinct from public objects. If Kenny is right (PLA 224) he thought the language-game to be 'precisely the game of expressing sensations'. What he thought he should forbid was any reference to them apart from their public expression, by means of propositions claiming to be 'pictures' of them, or perhaps facts about them. I shan't discuss his reasons for the present, but return to Alf and Bill in their troubles.

2. They quickly establish that Bill has not gone crazy, and that he can still tell A's from B's in the ordinary way. He is driven soon to admit that he has always had a little difficulty in understanding what 'red' and 'green' were about. So they go to a local science-teacher who sets up a few tests. They reveal that Bill can't distinguish what other people call red or green, or, for that matter, their mixtures in the yellow-brown range, though he can distinguish any of these from blue. Released from his pretence, he invents the word 'gred' for what he sees when others say they see red, green, etc.: Bill's colour-world consists of gred and blue, and intermediates which he calls greddish-blue and bluish-gred. His readings of blue are close to those of the general population (judging by his reports under test), and he can of course distinguish light from dark in gred as well as in blue. His use of these terms is consistent.

Alf, then, can soon learn to use 'gred' correctly, and talk to Bill about it: he just uses it for any colour which is not blue or neutral (white-grey-black). But does he know what Bill _means_ when he says "gred"? As I understand it, Wittgenstein would have to say No. At least Bill can surely not be said to know what others mean by "red" and "green". But even he is eventually taught to use these words appropriately, with the help of a pair of filters prepared by the science-teacher and marked R and G. If a patch of gred is darkened by looking through R and not G, he calls it "green"; if by G and not R, "red"; if equally, "yellow" or "brown" according to whether it is initially light or dark. With practice he astonishes everyone by coming up, correctly, with "olive-green", "reddish-brown", "orange", and even "purple" (bluish-gred darkened by G). But does he _now_ know what others _mean_ by these words? And if not, how can he know what the _words_ mean? For Wittgenstein, this would induce an odd mismatch between 'meaning' and 'use'. We must look further for the full account of meaning.

But Bill surely knows what _he_ means by 'gred'. Is he speaking a private language? Certainly not one which meets all Wittgenstein's stated criteria; but one which could count as a very diluted 'privatish', à la Castañeda, relying heavily on public elements, but _fully_ understood only by Bill himself. The full meaning of Alf's colour-language is equally inaccessible to Bill. Bill's language counts as privatish and Alf's as public just because Bill is in a (very small) minority. Of course the reasoning in this account depends critically on empirical conditions (contingent truths) found in our own world, and transferred selectively to Bill's. But it is the role of language in worlds which are like our own, in respect of the determination of meanings, which concerns us here.

3. If I am rightly convinced by Kenny's analysis, the 'picture' theory gives the relevant account of Wittgenstein's own understanding of what he called the 'fit' between language, and 'facts' in the extralinguistic world. It relies on an analogical notion of comparison; we compare our sentences with features of the world just as we compare (representational) pictures with the features they represent, to see if they are "true pictures". Another analogy he uses is that of measurement: "Proposition and situation are related to one another like the yardstick and the length to be measured" (Notebooks, 29 Nov. 1944, quoted by Kenny, PLA 221). Even "the sense in which an image is an image is determined by the way in which it is compared with reality" (Blue Book, quoted by Kenny, ib., 223: my emphasis). But the analogies are very loose. A proposition is typically perceived in quite a different way from the perceptual situation, if any, to which it may refer. We cannot lay one against the other and register correct matching even as directly as we do between a picture and what it represents; which is less directly than we match a yardstick with anything it may be used to measure. The case of the image is the more tricky for being more plausible, at least in some senses of 'image'; a visual image, say on the retina, may

be very like a picture. But even this cannot be compared alongside that which it may be supposed to represent (though there are some interesting cases in which visual recognitions can be compared, in some sense, with, say, tactile recognitions of what are taken to be the same objects). But Wittgenstein was in fact considering an "image" of remembered pain. His implication is that this example is radically more problematic than the image called up by the expression 'a black eye' (or, perhaps, the expression itself, considered as an image). Suppose the visual image is meant: how is it compared with reality? The expression 'the letter on the particoloured card' calls up different images for Alf and Bill. They yield different referring expressions, 'A' and 'B'. To check (as if they would!) they compare with 'reality' - presumably the card. They still differ. Are they both right? How could they be wrong? What sort of a test is this? It seems natural to say that, in some sense, they perceive different 'images' of the card; what each checks is a remembered (evoked) image against an immediately perceived image. But how are these compared? And what does the result tell them? There seems to be no way in which either image can be compared with any reality beyond the perceived images of Alf and Bill - each of them comparing only his own images. If we call these images 'reality', we have to say that reality is different for Alf and Bill. If anything is logically impossible, this is. We are, I suggest, inevitably thrown back on some theory which provides a structure linking features of reality, images and expressions as different types of elements; restricts 'comparison' at most to elements of the same type; and explains the 'fit' or correspondence between elements of one type and another in some other way.

In scientific contexts, it is reasonably well understood that correspondences in each context are recognized within the framework of a chosen theory of that context, with explicit assumptions and internal rules. Comparisons may be made between perceived images, of carefully selected features in rigorously controlled conditions; and between expressions. Three kinds of expressions are of special interest here; particular statements about image-comparisons, which we may call 'readings'; more general statements about the (supposed) 'real' situation, which we may call 'findings'; and statements of the hypothetical assumptions or 'commitments' of the theory (generalized over a class of contexts). In the terms of this study, readings are to be understood in terms of a base-language of a GR-theory of which the relevant perceptual image-comparisons are carried out within the recognitive structure of the context (with all its incorporated commitments). Connecting these three types of statement are the logico-mathematical rules of inference. Higher-order terminologies integrate the base-language of the readings into the total language of the theory. Comparisons occur (inter alia) between readings, between findings, and most critically between findings and readings, especially where findings entail 'predictions' for readings.

The more developed and sophisticated the theory, the longer and more complex, in general, are the chains of reasoning between readings and findings. My suggestion is, of course, that a similar structure, greatly simplified, informs our ordinary contexts of recognition, reasoning and inference, and our use of the expressions of ordinary language in these contexts: the place of 'images' being taken by non-linguistic recognitions.

4. In the light of this suggestion, we may go on to explore Wittgenstein's problems about the special status of pain. How does the recognitive evidence for the finding 'Bill is in pain' differ in principle from that for 'Bill sees gred' - or, for that matter, 'Alf sees red'? It seems to me that the steps to 'Bill is in pain' from 'Bill says he is in pain', 'Bill looks (or sounds) as if he is in pain', or 'Bill is having 150 volts put through his arm' have precisely similar theoretical structures to those for 'Alf sees red' from 'Alf says he has a sensation of red', 'Alf is looking at a red object', or 'Alf is having light at 55^{o}A shone on his retina'. (Note that the case of the 150 volts would require no expression of pain from Bill.) In respect of inaccessibility to outside inspection, Bill's sensations of colour do not differ as proper subjects for theoretical findings from his sensations of pain, or, for that matter,

the state of his liver - or the contents of a closed book or box. The critical differences are between the structures of theory used in each case to reach these findings.

As has been pointed out, some aspects of others' sensations are covered by theories (of more or less technical sophistication, or just commonsensical) which are more or less secure. Verbal reports by the "owners" of sensations are the largest single class of sources of evidence for these. These may be supplemented or superseded to a greater or less extent by collateral observations (say, of neural electricity or retinal chemistry) for whose relationships to sensations some causal or statistical theory has been developed; they do not only relate to non-verbal 'expressions' or behaviour by the owner. Pain is more problematic than most sensations in this respect, for purely contingent reasons. The rules of the corresponding language-games aim to conform with the relevant theories of the contingent 'facts' in each case. No such theory, however, can rule out the existence of aspects of others' sensations not covered by that theory. This bears out the common intuition (which Wittgenstein seems to have unwillingly shared) that there are some aspects of our own sensations - in whatever sensory mode - which are not caught by either our own verbal reports, or correspondences with other theoretical correlates.

This restriction presumably applies to our own verbal reports to ourselves (memories, records, diaries, etc.) as well as to others'. Nor can non-verbal memories or records (including photographs or sound recordings) be exempted from this limitation. My analysis suggests that all these, too, can only be understood at any frame of any context in terms of the state-of-the-theory at that frame. Such a theory may, presumably, be "private" in the sense that at least some of the entities and properties with which it deals are in principle inaccessible to others. If, as this would imply, there are typically aspects of our own sensations which are not covered by any such "private" theory, the problem (in this world) of the inaccessibility of _all_ sensation to any but immediate individual inspection is both vastly greater in scope, and much less disturbing, than Wittgenstein thought. Notwithstanding this limitation, we have been able to construct a considerable network of more or less coherent and reliable theories about the world, including theories about other people's sensations. We are forced to use our own sensations as the only evidence for the character-structure of others'. But the cumulative evidence for the recognition of an apparently objective and consistent structure of idents, in all contexts, by virtue of these character-structures (with rare anomalies like the case of Alf and Bill) strongly supports confidence in these theories. Other theoretical structures, especially in psychology and biology, do much to reinforce this confidence.

It must be faced, I think, that nothing but our individual direct experiences of them is available to inform us of what these recognitive character-structures consist, or how they are generated (presumably in the brain). But one avenue remains for fruitful exploration: the question how, within these experiences, reliable theoretical correspondences are built up between verbal and non-verbal structures.

(4) The Understanding of Language in Concrete Contexts

In a paper on Grades of Theoreticity (in Foster and Swanson, 1970, pp. 2,3) Quine offers a 'black box' theory of human observation in which the 'input' to the box consists of neural 'stimuli', and the 'output' of 'testimony' - in the form of 'observation sentences'. The process of observation itself takes place inside the box, inaccessible and unanalysed (elsewhere he uses the same fairly well-known metaphor for the hypostasis, in physics, of unobservable entities like neutrinos or quarks). He prefaces this proposal by excluding from consideration, as the raw material of observation, the kinds of units of sensation, "red patches" and so on, we have already met as features of various attempts at the ultimate atomic analysis of perception or appearance. His argument against them (one also used by Anthony Quinton in Warnock, 1967, p. 68) is that they do not usually figure, as such, in our understanding of what we observe; they tend to be recognized only in rather specialised contexts of art, psychology - or even philosophy. But this argument does not go against the account of perceptual recognition given in this study, where the structure of recognitive 'atoms' is determined by the R-theory of the context in each case. "Red patches" appear here, as do "red buses" or "red traffic-lights", each in their appropriate contexts, functioning as 'R-atoms' if and only if the Reader neglects their proper enclosures as distinct elements. No expertise is needed, for example, to recognize a red patch on a white shirt, in a suitable context, as evidence for a wound. From this point of view, it may come to seem perverse to regard the elements of ordinary perceptual recognition as more abstruse than neural stimuli, which only occur in rather specialized neurological or psychological contexts. Stimuli are themselves unobservable, the evidence for their recognition depends on sophisticated instrumentation and incorporated theory. We do not see or hear stimuli any more than sense data; most of us do see red patches, hear dogs bark - and must use our eyes to read, or our ears to hear, sentences before we can even recognize them as language, let alone as statements reporting observation.

It is neither gratuitous nor unhelpful to distinguish the perceptual from the theoretical factors in the recognition and understanding of language. In concrete contexts - where each individual's learning of language begins, and language itself almost certainly originated - this analysis places language alongside the structures of which it speaks, as parallel but recognizably distinct aspects of the same general type of experience. The black box is still there, but its input and output are now more theoretically compatible; language occurs both as input (heard, read) and output (spoken, written). This, of course, is how the system has been presented above (Part II, Section G). The relationship between verbal and non-verbal experience has been exhibited, not as comparison in Wittgensteinian terms, but as the recognition and adoption of a reference function Ψ between elements of these two types of structure, from context to context. The '<u>practical ability</u>' we learn, as Dummett put it, is not some overall function Ψ; though part of it is the <u>capacity to form Ψ-functions</u> for contexts as they arise and develop. Dummett accepts, more than most, the dynamic aspects of meaning. "Not every aspect of use is sacrosanct," he says; all usages are "open to revision". Further, "the grasp of the content" of a statement "is not, in general (a matter of) verbalisable knowledge" (1978, p. 220). We learn to use language as we learn to use other constructional materials - to choose their elements and to put them together - to serve each new purpose of our own; and to recognize the purposes and meanings of these assemblies when put together by others.

Materials and methods of assembly vary from culture to culture, as well as from context to context. Within each culture, most of the materials and methods of construction for language (its lexicon and grammar) become a matter of conventional agreement, though the system is not static. It is a conventional structure of this kind, developed for use over a wide variety of contexts, which has come to be called 'a natural language'. Each individual's understanding of such a language is built up from nothing - except perhaps an innate capacity to construct such an understanding - starting, as I said, from experience of parallel recognitions of verbal and non-verbal structures. Though much is common, each individual's experience of language use is different, and associated with different structures of non-linguistic experience. Most important, each new context, and to a lesser extent, each new frame of each context involving language use, calls for an appropriate form of the Φ-function to be at least reconstituted from memory, and frequently extended by analogy with previous usage. Non-linguistic experience plays at least as fundamental a role as linguistic in this process of constant construction and reconstruction. Communication between speakers of the same natural language becomes increasingly difficult as their non-linguistic experiences diverge; in spite of the power of convention, linguistic usages tend also to diverge. It seems inevitable that large parts of non-linguistic experience escape capture in the resulting structure. What might rather surprise us is that linguistic convention builds strongly and consistently enough to allow the foundation of philosophical theories of human understanding on the results. Where, on this account, are we to look for these foundations?

An early casualty of this somewhat Heraclitean account of the development of language, coupled with an almost Protagorean insistence on learning from the world of sense, might be thought to be any Platonic or realist approach to the forms of language. The relevance of such an approach to the theory of meaning is critically discussed by Dummett in a paper of 1963, published in his Truth and Other Enigmas (1978), but partly repudiated in the introduction (ib., xxx). He identifies it in the original paper with a contention that meanings, especially those of mathematical statements, may be known independently of any evidence for their truth. Such a position does not appear to be wholly excluded by the above analysis. We could say that Platonic forms, or their modern equivalents, belong to an ontology of substantive theory which claims to be an account of aspects of the reality underlying and informing the world as experienced through the senses: not to the ontology of recognitive theories of the contents of sense experiences themselves. There could be some sense in which knowledge of such substantive theory could be shown to be constructed independently of the evidence of sense, as pure speculation constrained only by reason and the honest search for truth. But empirical knowledge is taken in this study to consist of systems combining substantive and recognitive theory, relating the elements of the two corresponding ontologies by accounting for recognized appearances in terms, often, of unrecognizable, hypostatized structures claimed as real. Ideally, these last exhibit principles of constancy, conservation and rational perfection, typically expressed in mathematicl form, and reconciled with recognized appearances only by strategies of approximation and neglect of minor error. This relationship between two kinds of knowledge is not far remote from the thought of Plato himself; though he only allowed Socrates to accord the title of knowledge to an idealised form of the first. This emerges clearly in the Theaetetus (where idealised and measured equality of lines are specifically contrasted), and in the Sophist. More must be said later about the two kinds of theory we have distinguished, how they may be combined and to what extent one may be independent of the other (esp. Section IV.5). At this point we are concerned only with the relevance of such an analysis to our understanding of language.

We may suppose a fully developed natural language, as understood by suitably educated speakers, to include terms and constructions appropriate to both kinds of theory - which we may for the moment call substantive and recognitive language respectively. Some terms and many constructions are likely to belong to both. But we might expect to find some terms or constructions of substantive language to speak only of elements of the ontology of substantive theory, and relationships between them, and so to be understood independently of any understanding of recognitive language (but not independently of sense experience of substantive language itself). No natural language, however, can be supposed to consist only of substantive language, excluding all terms or constructions of recognitive language: and, if not, no platonic-realist theory of meaning of the kind criticised by Dummett could be expected, even at best, to apply to more than a restricted part of natural language. This is not the place to discuss a possible world in which a substantive language could be not only understood, but formed, independently of sense experience of the forms of language themselves; not surprisingly, it is easier to frame the proposal in words than to imagine its fulfilment. Our business here is with the language of concrete contexts, and for this purpose we can find some aspects of the intuitionist account (as discussed by Dummett, op.cit., 215 ff.), which we should readily adopt, though we are not directly concerned with the mathematical questions it was devised to deal with.

We must agree with the intuitionists that language is to be considered first and foremost (of not exclusively) as a means of communication between individuals; and that much insight is gained by looking at the ways in which it is learned by individuals. But most important for us is Dummett's insistence that it is implicit in the intuitionist account that the ultimate foundations of meaning are to be found outside language itself, if we are not to become trapped in an infinite regress (ib., 217). To put it slightly differently, they are to be found at the interface between language and extralinguistic experience.

This last implies that, at least for the language of concrete contexts, the ultimate structure of meaning resides at its interface with non-linguistic recognitive structures (whether or not analysed in the precise terms of this study); and it is here that we might therefore look for any fundamental restrictions on possible structures for language itself. These restrictions will cut across all particular conventional systems of natural language, in so far as they speak of perceptual recognitions. Two complementary approaches suggest themselves, corresponding to those discussed in Section IV.1: start with particular languages (or groups of languages, such as the indo-european) and hope to discover a common structure suggesting how their speakers organise their understanding of concrete experience; or start with the analysis of recognitive structures, including the recognized forms of language in context, and investigate their systems of correspondence between linguistic and non-linguistic elements.

Consider the well-known situation described by Quine: an anthropologist goes to live with a previously isolated tribe and sets out to learn their language. If he sees his task as primarily one of compiling a dictionary matching terms of the unknown language with those of his own, he is already assuming certain similarities of structure which may or may not obtain. Beyond what might be called the rock-bottom theory of meaning, that the natives' vocalisations (or some of them) are meant to say something about the content of their experience, including perceptual recognitions, just how much, and what, similarity of structure is he entitled to assume? I am not about to attempt anything like a comprehensive answer to this question, but certain pointers based on C-theory may be of interest.

So far my account of the structure linking language with non-linguistic experience has been limited to the supposition of a function Ψ taking statements of an ideal base-language into (simple or complex) assignments or commitments in the non-linguistic structure of the GR-theory of any particular context. These statements have further been supposed to have a fundamentally sentential form, which has remained unanalysed except for the proposal that it should be understood as normally constituted, at the simplest level, of a suppositive and appositive phrase in each atomic statement (not necessarily in that order). The suppositive phrase is that part of the utterance which will have the effect of picking out and drawing attention to a set of one or more idents for each R-frame of which the statement speaks: the appositive phrase, that which either adds or subtracts one or more characters to or from the set previously assigned to the set of idents picked out; or expresses a more general commitment with respect to the characterization of idents so picked out, over one or more R-frames. (I am neglecting, in this general account, the complications regarding negation, discussed above, pp.212f.)

From the linguistic point of view the most important aspect of the analysis is the resulting claim that all structures of assignment or commitment can be shown equivalent to recognitions of specified structures of intersection between Γ-sets (of idents), each of which is completely determined at each R-frame of a context by a specified set of characters (or character-cluster). The suppositive phrase of any utterance must determine a sufficient cluster in context to pick out what may be loosely called the "subject" Γ-set: while the appositive phrase in that utterance determines one or more Γ-sets and their structures of intersection with the subject set (together, if necessary, with elements determining a time-sequence structure) so as to constitute a "predicative" construction. It has been claimed that all relevant syntactical categories (such as the nouns, adjectives, verbs and adverbs of indo-european languages) may be in principle subsumed in such an analysis. Thus, the main task of constructing, developing or learning a language for recognitive contexts is seen to be the progressive association in memory with each of a chosen set of terms (or expressions) of a chosen set of characters.

The construction of this set of terms (that is, those directly involved in characterizations) and of the set of characters associated with each term will be generally (but not exclusively) cumulative; as will the construction of a set of R-theories which will be (more or less well) remembered as having determined particular subsets of the characters associated with each term as being involved in particular ways in certain kinds of context. (In Section IV.1 it was argued that the subset or cluster of characters associated with any term in a particular context is typically only a small part of the totality of its associated characters; nor is such a totality necessarily consistent or unequivocal, for any one term, except under contextual restrictions.) This kind of memory-structure, together with the recall of associated forms of syntactic construction, is here supposed to be essential to the fulfilment in practice of any individual's native capacity to form Φ-functions for recognitive contexts involving language use, as they arise and develop. (Any individual may, of course, acquire memory-structures of this kind for elements of two or more natural languages, with or without associated schemes of translation; and for more specialised languages.)

There may be a loose sense in which an individual's memory-structure for a particular language may be called a 'theory of meaning' for that language. But, except perhaps in the case of a specialized technical language, the structure is likely to be very diffuse, and never held in the mind as a complete or coherent system. Completeness and consistency of understanding is reserved at best for the linguistic structures of particular contexts, as parts of entire GR-theories of those contexts. Such understanding is not, in any case, to be regarded as a philosophical theory of meaning, which should rather, I suggest, be concerned with principles governing relationships between linguistic and non-linguistic structures in general; complemented by philosophical studies in the semantics of particular languages or language-types.

These considerations direct attention immediately to the nature of language as a social enterprise: implicit in the intuitionists' emphasis on its function as a means of communication. Our interest here in the structures of meaning in particular recognitive contexts has thrust this aspect rather into the background; though it was acknowledged, for example, in the analysis of GR-theory, by the stated assumption that the individual Reader draws upon pre-existing resources of language which, to some degree at least, transcend particular contexts. Mainstream philosophical semantics has also, for different reasons, had little to say about the social aspects of language construction or development; accepting existing languages rather as ready-made structures, specified in terms of the competences of particular speakers or interpreters. I have suggested that this specification is circular (above, p.396): in that speakers and interpreters are defined for this purpose only as members of communities using the relevant language in each case.

The more purely linguistic aspects of the pattern of social and historical construction involved are beyond the scope of this study. But enough has already been said to suggest ways in which C-theory might offer a new perspective on such questions, leading to a greater stress on the relevance of the associated historical development in a community of systems of empirical theory (in the broadest sense). This relevance seems to have been most clearly invoked, so far, in Putnam's account of 'natural kind terms' (1970, cited above, p. 405 , where it was emphasized that these developments are expressed in the lexical rather than the syntactic structures of a language). It is in terms of such a developing body of theory, associated with particular lexical terms, that we should account for Kripke's observation that certain properties can come to seem necessarily true of any object describable by a given natural kind term (Davidson & Harman, eds. 1972). But our emphasis on contextual variations in the level and content of theories associated with particular lexical terms would point to a looser association, placing less universal reliance on 'experts' than Putnam, and restricting Kripke's concept of necessity to certain special contexts - a position closer in spirit to Searle's account of the associations of proper names with 'clusters' of characteristics forming a 'descriptive backing' (1969, pp. 170/1). Natural kind terms are distinguished - explicitly by Putnam - as those which are most closely associated with well-established bodies of theory; and the specially tidy patterns of meaning which thus accrue cannot be generalized to other types of usage.

Again, Chomsky's important observations on the facility with which infants acquire linguistic skills, pointing to possible genetic factors in the construction of languages, would be understood in C-theory rather in terms of innate capacities to form theoretical structures in which words and sentences have specific tasks to perform, embodied in Φ-functions: the requirements of these tasks being seen as the only universal restriction on actual linguistic forms (apart from certain motor and recognitive skills). Some indications of the way this might work have been given, but I can pursue the matter no further here. The topics which remain for discussion will therefore be considered not so much in terms of linguistic structures, as of the relationships between the theories expressed.

B. The Understanding of Theoretical Structures

(5) Ontology

I take the ontology of a theory to be the structure of elements of whose relationships it undertakes to articulate an understanding, and whose existence it therefore either presupposes, or deduces under its logical system. (It is convenient to speak of 'a theory' doing all these things, although in this study we must never quite forget the persons who acquire these commitments in adopting particular theories.) Ontology, as a philosophical topic, would thus be the study of theories in terms of the types of elements they seek to account for, and the broad relationships supposed to obtain between different types, in the case of any theory which concerns itself with more than one. It is in principle possible to distinguish different kinds of existence, according to the kinds of theory concerned. For example - remembering Kripke's interest in the question - there seems no reason to discriminate against the notion of a fictional existence, accounted for in works of fiction by more or less well articulated theories whose structures are much like their counterparts in non-fiction, or in ordinary attempts to understand what is going on.

C- and R-theories are concerned solely with what I shall call objective existence. This may be subdivided into recognitive and hypostatic existence, our interest in the second being restricted to the hypostasis of elements involved in explanations of recognitive structures. Extrapolated or fictal elements may be recognitive (in principle recognizable, if they exist) or hypostatic. The claim to objective existence is taken to be always present in these theories, though its strength depends on the structure and performance of the (G)R-theory in each case, and the relevant characteristics of the Reader or L-group adopting it. R-theories are concerned only with recognitive existence, except in so far as hypostatic elements may be treated as recognizable in virtue of incorporated S-theory (e.g., the counting of electrons in a Geiger-counter). C-theory is committed to the existence of R-theories constructed by Readers in all well-understood recognitive contexts (each with its own ontology): in the special conditions of the writing of this study, this involves extrapolation from my own experience, which is claimed to be objectively analysable in this way as far as it is relevant. The question of the relationship of objective existence, as understood in C- and R-theories, to any suggestions about what 'really exists' in an absolute sense - and therefore located in some ultimate theory of reality - will be briefly considered at the end of this study.

Particular ontologies are on this account completely relativised to theories; but so as to lead to some different general conclusions from those of Quine's account of 'ontological relativity' (1969). Quine, presumably wishing to dispel any illusion that some absolute, theory-independent categorization of ontic commitments is possible, says at the outset that "... it makes no sense to say what the objects of a theory are, beyond saying how to interpret that theory in another ..." (p. 50). This might appear to rule out not only any extra-theoretical statement of "what" the objects of a theory"are", but also any statement of this sort <u>within the theory itself</u>. If this is to hold good, some restriction must be placed on the kinds of categorization to which the rule applies, or we shall prohibit any theory from classifying its own elements, which cannot be intended. If one theory is used to comment on the elements of another, the ontologies of the two theories may overlap or not. Theory A may say that, for A, none of the objects of theory B exist; that some do, but not all; or that all do (as is the case with C-theory's treatment of R-theories). A can say nothing more about those elements of B that do not exist for A - though there is, of course, nothing to prevent anyone who understands both theories (or thinks he does) from saying which B-elements do not exist for A, and why, or even attempting "translations" (as it were) of B-elements into A-elements, if necessary in some third theory whose ontology is understood to cover both. The question of overlapping ontologies can obviously generate great theoretical complication, especially where different forms of existence may be in question; but it is unproblematic for C- and R-theories.

But although (for example) the elements of the ontologies of instantiating R-theories exist, in principle, for C-theory, C-theory says nothing about them in the language (if any) of these R-theories, except by way of illustration. Nor does it adopt, as commitments of its own, the commitments of these R-theories with respect to their elements, many of which could be in contradiction as between one R-theory and another. This would exclude from the obligatory commitments of C-theory, amongst other things, R-theoretical classifications: C-theory can compare R-theories in which "a whale is a fish" and "a whale is a mammal (and therefore not a fish)", without being involved in contradiction, although whales, fish and mammals all belong (in principle) to the ontology of C-theory as elements in its analysis of these theories. If (as seems plausible) we can generalise from this to all metatheoretical analysis of ontologies, it is only in the contents of their ontologies that analysed theories need be 'subordinate' (Quine, op.cit., p. 51) to the analysing metatheory: not in terms of their classifications of elements of these ontologies. The language of the meta-theory is typically more abstracted (as is C-theory), using general terms or variables for which elements of subordinate theories are in principle substitutable in particular instances. These substitutions are governed by the ontic commitments of the 'subordinate' theories, which to this extent determine the ontology of the meta-theory. On this account we should look for ontological analysis to sophisticated theories of this kind - mostly in philosophy books - rather than, in Quine's terms, the "full interpretation" of a theory"relative to our own words and ... to our overall home theory" (op.cit., p. 51) or, in an earlier version, our "over-all conceptual scheme, which is to accommodate science in its broadest sense" (1960, p. 17). These notions seem to owe much to the idea that natural languages somehow encapsulate definitive systems of theory, of which I have already noted that it would prevent people from arguing for opposing theories in the same language (so putting philosophers out of business).

On the other hand, the ontic commitments of empirical theories generally involve more than questions of language. We may suppose that the ontologies of particular theories can be analysed out by so formalising its statements that we can identify those variable-predicate pairs which come under the scope of existential quantifiers. But the implication is clear: to find the values of the variables and the meanings of the predicates, we must look outside the language of the theory, to the structures of elements it seeks to account for (unless it is a metalinguistic theory whose ontology includes only linguistic elements - which excludes nearly all science). But the relationships between the language and other aspects of a theory are not clearly articulated by Quine, nor are those between broad phenomenalistic, physicalistic or other conceptual schemes and particular theories with different ontologies which may be formed within them. If these aspects of our intellectual resources are kept distinct, it becomes clear that a theory may draw freely on common natural, or even scientific, languages without being thereby restricted to any prior commitment, ontic or otherwise; and that it may appeal to any available conceptual scheme, or invent one of its own, to categorize the form of existence it claims for its elements. This is not, of course, to say that a theorist can hope to be taken seriously, or even to increase his own understanding, if he builds without close attention to accepted language uses and the relevant theoretical claims of others. But the flexibility of language - the extensibility of its meanings through analogy and metaphor, in particular - is a condition of human creativity, including the invention of new and possibly better theories to account for objective phenomena. It carries the price that the most determined pursuit of the unequivocal cannot succeed outside narrow limits. It will be argued that much of the authority of physical measurement comes from its relative success in this pursuit. Leaving questions associated with more sophisticated theories till later, we must now return to the consideration of ontology at its simplest, recognitive level.

It seems to be common ground for many that the ontology of a theory can, indeed, be determined by analysis revealing the scope of existential quantification in that theory. To this, Lesniewski adds the interesting claim that any simple predicative statement (or 'thesis') of any theory can be shown, when analysed in terms of the canonic language of his systems, to carry some existential commitment for that theory. It is on this ground that he categorizes a system of predicate logic, carried out in this canonic language, as an 'ontology' (Luschei, 1962, esp. 144 ff.). The underlying intuition here seems to be that it makes no sense, within a theory, to make simple predicative statements about entities which do not exist for the theory. To capture this intuition, the canonic language ('L') of the system has certain special features: (1) A basic semantic category of terms whose reference is unequivocally fixed by the context, so that each is said to 'name' a 'distributive class' of one or more entities which exist for the theory under analysis (not for the predicate logic itself, which carries no ontic commitment). Though called 'nouns', these cut across all syntactic categories capable of determining appropriate classes. This suggests a sympathy with the view, expressed above, of the partly arbitrary structure of many syntactic forms, as actually developed; and an emphasis, accordingly, on the structure of what is described rather than that of natural languages. (2) A simple predicative statement is of a higher semantic category, being formed from two nouns of the first category by the use of a 'verb' term as functor. This verb-term effectively acts as copula, so that the first-named class is stated to be included in (or identical with) the second. (3) Though for convenience (and within the canonic rules) I have used the language of 'class' here, it can be dispensed with in L without loss of meaning; all classes named in this way are also 'individuals' of the system. A predicative statement need not mention classes, and its interpretation does not necessarily involve concern with them. (4) The 'distributive' class named by a noun has as elements just those elements named; in this, it is contrasted with a 'collective' class, which includes as elements or 'ingredients' all parts of those named elements without restriction. Lesniewski's system of <u>mereology</u> concerns only collective classes. (5) In line with Lesniewski's existential claim, no empty distributive or collective class is permitted in the system. Although L contains an expression for 'null concept', which has the status of a noun, it names neither individual 'non-entity' nor 'empty' class (Luschei, op.cit., p. 164). It cannot, therefore, appear in any simple predicative statement of L.

I give here this highly condensed account of some of Lesniewski's principles (which are explicitly 'constructivist' and 'contextualist') because they draw attention to certain points of common interest, in a framework which combines the utmost rigour with some intuitions shared by C-theory. In this study, the attempt has been made to reflect these intuitions in the language of standard set theory. The Γ-sets of C-theory are to all intents and purposes distributive classes of Lesniewski's system, and although no terms of a contextual base-language L^G correspond with his canonic nouns, the sentences of L^G are taken to state assignments or commitments which are in principle analysable in terms of intersects of these Γ-sets - although, like the statements of L, they need neither be stated nor interpreted in these terms. Further, for all x, every simple statement of the form P/x must be read with the axiomatic conditions that f(x) is non-empty, and $(\forall P)(\exists x)(x \in S \wedge P/x)$; so that every such statement brings with it existential commitments, and an utterance stating the assignment carries the same commitments for the utterer. Though the sentence 'The rose is red' does not in itself entail the existence of any rose or any property of being red, its truth conditions in any context include the existence of some rose and a character red which satisfy it, so that their existence is part of its meaning; and <u>use</u> or <u>utterance</u> of it is standardly understood, and intended, to imply that existence. Again, where the expression '∅' appears in a commitment as analysed in terms of Γ-sets ("No rose is blue": $\Gamma_{rose} \cap \Gamma_{blue} = \emptyset$), this cannot require the existence of any <u>recognized</u> empty set (∅ is not a Γ-set, no Γ-set is empty). What is meant is that a Γ-set intersection formula which under some specifications determines a set of idents, under some particular specification in some particular context determines what it is formally convenient to call a 'set', but with no members, hence the 'empty set'. No ident so specified is recognized in context; either no such entity is present, or, if present, it is neglected; either way, no such ident exists for the R-theory concerned, so that the use of '∅' carries no existential commitment. '∅' never appears in any simple assignment.

Though this analysis is restricted here to contexts of perceptual recognition, carrying commitments to objective existence for their elements, there seems no reason to doubt the possibility of extending a similar analysis to theories of other kinds, as it affects their ontologies. In comparison with Lesniewski's systems (which, to some extent at least, were invented to exorcise Russell's paradox), what stands out is that analysis of the whole/part structure (that of 'composition', especially of Γ-sets) is carried out within the logic of what he calls distributive classes - in contrast with his mereology (of collective classes) which he categorized as 'extralogical'. Again, though Lesniewski claims that his two systems can be used together in the analysis of many kinds of structures, including what I here call concrete contexts, he provides no indication, such as is given here, of the structure of contextual theory relating composition to characterization.

A special feature of the ontology of C-theory which may, finally, call for further justification, is its use of two distinct types of primitive elements: the idents and characters. It is unusual (but not unique) to quantify over properties, and even more so over both entities and properties. There has been a natural impetus towards a single fundamental class of elements in terms of which all others are defined, whether ident-like 'objects' or 'individuals' (in Quine's term, physicalistic), or character-like 'qualia' (similarly, phenomenalistic). Some pragmatic justification has already been offered for the dual system; but something more fundamental is needed, specially as I have now explicitly claimed for both the same, objective, form of existence. The ultimate basis of justification lies in the distinct form of relationship of these two types of elements with respect to time and space.

Given that R-theories are to a large extent inductive, like all empirical theories, and that induction appeals to past experience for the understanding of present and future, many commitments of these theories by necessity attach themselves to structures recognized as time-persistent. All major implicative commitments of an R-theory - those which give it meaning and internal cohesion - are attached to (sets of) idents, and rest on the primary commitments to the identity through time of these idents. (The form of these primary commitments will be further considered in the next Section.) Structural commitments with respect to (sets of) characters serve only to determine the boundary structures of idents, frame by frame. Although many characters are indeed recognized as "the same" in successive frames (or in different idents), it was pointed out above (p.97f.) that this sameness is not to be interpreted as strict identity, but as recognized by neglect of differences. This neglect was further justified (ib.) by reference to the logical consequences of assignments of the relevant characters to idents, as carriers of inductive commitments. These assignments typically vary from frame to frame; time sequence itself being recognized in terms of <u>changes</u> in character-structure associated with <u>persistence</u> of ident-structure.

Spatial structure, on the other hand, is recognized solely by discrimination of characters. The description of idents as 'spatially-bounded entities' is not to be read as implying that spatial-boundedness is some sort of additional, universal property of idents; rather that we gain our notions of extension in space from recognizing the manner in which these entities, whose boundaries are determined by their changing character-clusters, are distinguished, disposed and separated from one another at any one time. Since the set of idents assigned a given character, and the set of characters assigned to a given ident, both vary, no attempt at reducing either to the other promises an increase in formal elegance to balance the certain loss in intuitive clarity.

The way in which this dual structure of recognition has in practice affected the structures of associated languages is far from simple. I shall give only brief indications here. It has been argued above (p. 459) that words are integrated into the structure of recognition by association with clusters of one or more characters. Since the function of language includes the handling of temporal as well as spatial information, semantic categories (where these are clearly distinguished) tend to be developed accordingly. Some terms become associated with large clusters of characters such as are typically assigned to only one, or a few, ident(s) in each context of their use; others with clusters of one or a few character(s), typically assigned to larger sets of idents; yet others with typical changes of character-assignment, including that of positional character, to one or more idents. Syntactically, in indo-european languages, the first are, broadly, names or nouns; the second, adjectives or process-verbs; the third, verbs of change or motion, and their modifiers, including adverbs. Time-related information may be partly handled, as in indo-european languages, by use of the third category; or, as in sino-japanese languages, Lesniewski's semantic/syntactic categories of terms (like 'in two years' time', 'yesterday', or 'at Easter') which modify whole sentences rather than particular words; or by means of more complex constructions. It is perhaps for this reason that the relative simplicity of the recognitive structure, suggested by its analysis in terms of the dual ontology of idents and characters, has to some extent become obscured. In particular, the structure of individual characters is not generally or readily reflected in words; hence the borrowing of the term 'character' from zoology, where (as pointed out above, p. 100) pictorial illustration is often used to complete the vaguer indications of the written word.

6. Identity

The relativising of the concept of identity, as it appears in this study, to particular contexts of theory, in which stress is laid on the theoretical autonomy of the individual Reader, may seem to involve a denial of the important metaphysical role accorded to it by many philosophers. Special interest has recently been stirred by David Wiggins (1980); and I have devoted attention to many of the questions he raises, as they impinge on my analysis. A full account of the emergent issues would go beyond my present scope. But a few of the main points will now be discussed.

(1) The apparent relativism of my account is purely epistemological. Within each context of theory the proposed structure of identity is strictly Leibnizian - more strictly, it seems to me, than most in the literature. Underlying the rigour of this account is an intuitive commitment to the view that it is a precondition of the <u>truth</u>

of any theory as an account of any aspect of reality that its understanding of the structure of identity, for the elements of its ontology, is fully articulated and self-consistent. The account takes on a relativistic aspect because our theories handle only the contents of very limited fields of awareness and attention, in each of which our understanding is partial and fallible.

This aspect is largely irrelevant to Wiggins, who is primarily concerned to elucidate the concept as it affects the proper use and understanding of language in statements of identity, whatever the epistemological grounds (if any) of such statements. Nevertheless, there are considerable areas of common interest, and much agreement in these areas. One of Wiggins' main motives is to defend a strictly Leibnizian account against what he categorizes as a form of 'identity relativism', but one very different from my own. This view, originating with Geach, supposes, roughly, that it may be proper to say of two entities (or one entity?) x and y that they are (it is?) identical under one concept and not another. To take the classic case, something called 'Cleopatra's Needle' may be the same "landmark" or "monument", but not the same "piece of stone" as occupied the same place 100 years ago - if the stone has since been replaced, gobbet by gobbet, with concrete.

Such a proposition, for me as for Wiggins, is an example of the way language can be used to generate confusion. I would agree with him that it is to be elucidated by pointing to an ambiguity in the use of the name. If by 'Cleopatra's Needle' we mean an archaeological relic, whose importance as such reposes in its continuity of material and precise shape, then today's Needle is not the same as the one originally brought from Egypt. If we mean only a structure recognizable more or less by its striking outline, whose property is to stand continuously in the same place as a guide to mariners and other visitors, then it remains the same.

The difference between our perspectives leads me to be less surprised or concerned than Wiggins and others in face of such ambiguities. Our use of language generally relies heavily on surrounding clues (recognitive or otherwise) to determine which of many characters associated with each of the principal terms we use are to be understood as selected for attention under our theory of the context. In difficult cases, this may call for a good deal of qualification or explanation: "This is the same monument, but its material has been completely replaced. We still call it 'Cleopatra's Needle'."

(2) It has already been pointed out that 'natural kind' (NK) terms are specially distinguished as those with which, at any particular social/historical juncture, a well-understood common theory is typically associated. It is therefore to be expected that they will be relatively unambiguous when used in a straightforward, non-figurative sense. Thus, a statement understood to assign any particular ident to membership of a NK goes a long way toward

determining the whole of its relevant characterization in any context (independently of which specific characters are selected for attention). It does not, however, precisely fix its identity, if more than one ident of the context is a member of the kind. Wiggins' arguments against 'identity relativism' lead him to concentrate on the point that an assertion of identity under one NK concept is inconsistent with its denial under any other concept: and to propose that, for every properly formed identity statement, there is some 'substance sortal' (NK term or not) with respect to whose concept this principle holds.

From the epistemological standpoint of C-theory, however, characterizations associated with such substance sortals are neither universally necessary nor generally sufficient for complete identifications in concrete contexts. They are not necessary in all cases, since an entity unique in the universe, and identifiable over time, may well fail to fall under any concept other than one so broad as to be inadequate for Wiggins' purpose (such as 'heavenly body' or 'art object'). More generally, they are insufficient where they characterize more than one ident in context. Here, it is obvious that characters shared, e.g., by members of a natural kind, cannot serve to tell members of the kind apart; we may often have to fall back on positional characters or their associated values in such cases (above, p.113f). Generally, we are led to distinguish very carefully between different aspects of the structure of characterization which may be involved in considerations of identity: such as 'individuation' (short of distinctions between individuals); 'genidentity' (where more than one term picks out the same entity); spatio-temporal continuity; and unique characterization as strictly recognized or understood. C-theory yields clearly distinct formal structures for each of these; but they must be omitted here.

(3) In the case of sortals denoting artefacts (like "pots" or "clocks") the background of common theory is much more context-dependent than is the case for NK sortals. Wiggins acknowledges some of the difficulties, but his emphasis in such cases on "either a principle of <u>activity</u>, a principle of <u>functioning</u> or a principle of <u>operation</u>" suggests potential sympathy with an analysis in terms of a theory of the context (op.cit., p. 70, his emphasis: later, this is glossed to include the <u>purpose</u> an artefact is made to serve).

(4) Another area of sympathy between Wiggins' analysis and my own is an overriding concern with spatio-temporal continuity, and the notion of a unique 'life-history' as central to the concept of identity. In both accounts, this concept is taken to be primitive, prior to an analysis which can only elucidate or articulate the manner in which it is found to enter understanding. The fact that in C-theory the 'life-history' of a given ident is shown as relative not only to a particular (G)R-theory, but to the changing state-of-the-theory over time, merely

reflects the necessary concern of epistemology with the partiality and fallibility of our understanding. Again, the underlying commitment is that any true theory must determine a unique life history, unique over time, for each spatio-temporally continuant entity (ident) of its ontology. Questions about the 'truth' of theories or their statements are considered in the following Sections.

(5) A similar effect of the difference of perspective may serve to protect my account, at least partially, from Wiggins' condemnation of analyses which present time as 'cut up into slices' - which he regards as inconsistent with a primitive notion of identity involving temporal continuity. The 'R-frames' shown as constituting time-successive aspects of the structures of (G)R-theories are purely heuristic devices of the metatheoretical account, designed to reflect the way in which people seem to have to organize their understanding of changes in characterization, even where they are perceived as continuous, into successive intervals or instants. It is, in fact, totally neutral with respect to any ultimate physical or metaphysical theory of the structure of time itself: and any C-theoretical account of time measurement is likely to exhibit it as a means (similarly neutral) of organizing this understanding of succession in a mathematically regular way.

(6) Where C-theory is likely to diverge most markedly from Wiggins' account is in its handling of problems of identity associated with changes of part/whole structure over time, of which Cleopatra's Needle is only one of the

simplest. These have attracted a great deal of attention from Wiggins and many earlier philosophers, from classical Greek times onward. My analysis of such structures in terms of 'composition' makes such cases relatively easy to articulate - reflecting the ease with which most of them are handled in ordinary life - provided we allow the concept of identity to be associated with composites and their bound-idents, as well as individual idents. This has two main consequences of a novel kind. We must be prepared to extend to composites the capacity to preserve their identity through changes in structure and character, already granted freely to individuals (like caterpillars and butterflies): and, secondly, we must be prepared to accept for identification some unfamiliar aggregates (provided their composition and life history is clearly understood), without being surprised that they are not associated with any tidy pre-existing concept, or conventional covering term of language.

(7) Truth, falsity; held-truth, held-falsity; and error

I have argued that meaningfulness is a precondition for the truth, or held-truth, of a sentence; and that the meaning of a sentence, as uttered or understood in any particular context, is in general dependent on a theory of that context involving that sentence. That is to say, the truth of a sentence, at least in a concrete context, is dependent on an empirical theory of the particular context, and not only on a general theory or understanding of the language from which the sentence is drawn (as would be widely accepted).

In attempting to analyse the appropriate logical structure for the truth or held-truth of utterances, we therefore start from the position that, in the conditions of this analysis, no sentence can be true without also being held true or false in some well-formed theory: but the converse, of course, fails. Some special cases will help to elucidate the consequences of this position. I shall confine myself to the consideration of simple 'statements', as this term was explicated in Part II, Section G: that is, sentences understood by utterer or interpreter as stating some simple assignment or commitment of the relevant theory.

Plainly a statement may be held false in some theory and nevertheless be true exactly in the meaning in which it is understood in the theory: if we come to know this, the theory will be to that extent falsified. An intriguing consequence of this is that I may utter a sentence, holding it to be false, as if it were true - intending to deceive - but my utterance may nevertheless be true. I shall fail in my intention to lie; and although I have not in the ordinary sense been "speaking the truth", my hearer may hear the truth. He may safely adopt what he hears as 'held true' in his own theory of the context, and need never know that I am morally a liar. In due course I may learn of my error, and revise my theory, but will not thereby become truthful.

Equally subtle may be the case where a statement is held true in one theory, while the same words, as understood in another theory, are held false (and may be either true or false). In the example given in Section IV.4, the crew-member on the boat might shout, "The water is clear," meaning the hydrographic sample in his hand; while the skipper, thinking him to mean the channel, and that therefore what looks to him like an obstruction must be a trick of the light, may ring full speed ahead - placing the boat, unknowingly, in the hands of fate.

The subtlety of such cases increases if one of the alternative meanings of an ambiguous phrase can be thought 'better' than others, or indeed a 'standard meaning'. If someone says, "Socrates is not dead", meaning his old English sheep-dog, we might say he should make his special meaning clear, if he is not to be accused at least of speaking fancifully. But my analysis suggests that the notion of 'standard meanings' - especially of proper names - is treacherous.

A good candidate for a sentence with a fully determined standard meaning is the stated commitment, "(All) whales are mammals": it could fairly be said that anyone saying this, even with false or inadequate knowledge of what it means, would be uttering a sentence with a unique standard meaning, which in that meaning is - perhaps even 'necessarily' - true. Two points must be made here about cases of this kind. The first is that they are special cases precisely because their terms (usually scientific or technical) do point specifically to particular contexts of theory: and that no <u>general</u> account of truth can be based on special cases. The second is that not even our best theories are completely watertight, and that no clear borderline can be drawn between those theories we may hold absolutely or certainly truth-determining, and the rest. A general account of truth in concrete contexts should not, therefore, draw the line between one empirical theory and another, but between truth and held truth relative to theories in general. With this in view, we can go on to consider the appropriate logical structure, distinguishing between the different concepts concerned. For this Section only, I shall write 's' as a variable ranging over statements of a base-language L of any given empirical context, '$T(s)$' for 's is true', '$\neg(T(s))$' for 's is false', '$\theta(s)$' for 's is held true', and '$\neg(\theta(s))$' for 's is held false' in a given theory of the context. The following axioms are proposed for a theory of the logic of these expressions:

t1. $\neg(T(s) \wedge \neg T(s))$

t2. $\theta(s) \vee \neg\theta(s)$

t3. $\neg(\theta(s) \wedge \neg\theta(s))$

Corollaries: given $\bar{s}$ such that $\theta(\bar{s}) \leftrightarrow \neg\theta(s)$,

(i) $\neg\theta(\bar{s}) \leftrightarrow \theta(s) \leftrightarrow \theta(\bar{\bar{s}})$

$\neg\theta(\bar{s}) \vee \neg\theta(\bar{\bar{s}})$

$\vdash \neg\theta(\overline{s \wedge \bar{s}})$

<u>Notes</u>:

(a) The expressions $\neg(T(s))$ and $\neg(\theta(s))$ have been used, rather than distinct expressions for s false or held-false since, assuming t1 - 3, the logical relations between truth and falsity are such that this captures more simply and perspicuously what is to be understood. The broad intuition is that it is senseless to hold the same utterance at once true and false - meaning its negation is true - and that, although it is possible to have different strengths of commitment to the (held) truth (or falsity) of an utterance, it is senseless to be totally or partially committed to its partial truth, or partial falsity.

(b) Two kinds of relationship can be derived from t1 - 3 between held-truth under a given theory and truth <u>tout court</u> (in some sense, 'absolute' truth - see note (c) below). The first comes from taking t1 - 3 together and concluding that, for its truth or falsity to be in question under t1 an empirical utterance must be held-true or held-false in some well-formed theory whose logic is governed by t2,3; but that any utterance may be held true or false without being either true or false. I.e., the theory concerned may fail of a grasp of reality in such a way that some utterances properly made within it nevertheless do not make statements which are absolutely either true or false. Intuitively it seems clear that this frequently occurs in empirical theories (it has certainly done so in the past) in cases which cannot be recognized in the terms of the relevant theory. This does not say that such utterances are meaningless. Indeed, they cannot be utterances of a well-formed theory if they lack meaning; but their meaning is associated with a structure of held-truth, and not of absolute truth.

Bivalence, then (following Dummett's terminology in his (1978), pp. xix ff.), holds for empirical utterances in terms of held-truth under t2, but not in terms of absolute truth. At the same time, no third value is allowed in t1 - 3, so that the principle of 'tertium non datur' is built into the system a priori. Allowing for minor differences in notation and subtle differences in interpretation, Dummett's intuitionistic formulation of this principle emerges in corollary (iii) above. It cannot, of course, be claimed that any argument here rules out valid analysis using a range of true-to-false, or held-true-to-false, values: only that a full, subtle and well-articulated analysis of varying strengths of commitment to held-truth can be achieved without this complication, in terms of mutually inconsistent extrapolations each consistent with a master-theory (see the treatment of probability above, Section III P).

(c) The second, and more fruitful, form of relationship which can be proposed between truth and held-truth under this analysis is that, at least with respect to empirical matters, the structure of absolute truth is to be seen as an idealisation of that of held-truth - an ideal form to which all theory aspires. In this version, standard logical analyses are interpreted as theories of the logical structure of omniscience - of a total theory of some universe or other, in which all questions of truth and falsity are determinate and settled. What knowledge, exactly, is to settle these questions is left open. It is usually said, rather, that such analyses are independent of knowledge of these matters or the means by which it may be obtained. Logical theory indeed is not, and must not be, dependent on particular structures of empirical knowledge or the means by which it is obtained. But this should not, I think, mean that its relationship to knowledge and its construction can be ignored, and I can certainly not ignore it here.

On this second interpretation, then, we shall find that in the ideal, "limiting" condition which I have called omniscience, T-values and θ-values become one and the same: t1 and t3 collapse into a single axiom: bivalence and tertium non datur hold for truth as for held-truth. There can be no place for half-truths, varying strengths of commitment, or probability-values.

Meanwhile we must expect that actual empirical theories will continue from time to time to breach either t2 or t3. Breaches of t2 will show up, if at all, as cases of meaninglessness or ambiguity: utterances which fail to make a clear statement properly held either true or false under the theory as it develops. Breaches of t3 will similarly show up only as error within the theory - to be dealt with in one or other of the ways outlined above (pp. 228). We have noted that much uncertainty can be contained by systems of alternative extrapolations (note (b) above). Much that eludes our grasp can be managed by systematic neglect: a rational refusal to hold certain possible utterances either true or false. These are devices to keep t2 and t3 inviolate while construction continues.

This account is, of course, quite incomplete, leaving many important questions open, but it would be inappropriate to attempt to take the matter further here. It has seemed necessary, however, to give some indication of the way in which these topics could be approached from the standpoint of this study. It remains to say something about the structure of theories within which the logic of held-truth is thought to apply; and the status of measurement contexts in this structure.

(8) The structure and foundations of empirical theory

The central philosophical question about empirical theory concerns the rational basis for claims to recognize and understand aspects of reality. I shall use the term 'induction', following widespread usage, to describe the method on which I take all such claims to rely: accepting, at the same time, that all attempts to extract a distinctive system of 'inductive logic', alongside standard propositional logic (or the logic of 'held-truth' as just proposed), seem fated to founder for lack of what Popper calls empirical content. This critique will be further invoked and examined later. The same applies to attempts to found induction on some form of logic of probability: discussions of this subject above suggest that their failure may be due to misunderstanding of the concept of probability itself, which seems to have more to do with strengths of commitment to alternative models within a given theory, than with the inductive structures of that theory. Faced with the evident historical success of inductive method in physical science, in apparent disregard of an absence of sound logical structure, some have sought to provide philosophical justification, short of logical rigour, for the recognition of this success. My own instinctive response to this situation is to accept the success as its own rational (not moral) justification; in the sense that it is rational to suppose that the success has a rational explanation. It justifies not only the method, but the continued employment of philosophers in trying to analyse its structure.

It seems perverse, for example, to appeal to some a priori principle of regularity in the universe as a support for induction; rather, the success of induction in a particular field of inquiry is evidence for regularities in the field investigated (even if these regularities turn out to be a matter of the statistics of randomness). Some phenomena are found to exhibit regularities - of one sort or another; others appear irregular. We are ourselves amongst the more rule-governed products of the universe, and our inductive theories (especially in science) amongst our own most rule-governed products. An important question, therefore, is the one raised by Kant: to what extent are the regularities exhibited those of our own interactions with our environment? Kant effectively answered, totally: impressed by the obedience of the stars to Newton's "laws". Popper initiated a directly contrary approach, in which our theories are taken to be rational conjectures, under constant threat of contradiction from the refusal of nature to conform: contradiction we should court, since it teaches us about nature. Though his original pure doctrine of falsification by critical experiment has been much modified, the thought behind it stands: "... our theories can clash with reality; and when they do, we know that there is a reality" (Popper, 1965, p. 117). This is the principle I like to call "the authority of the unexpected". Our expectations are constructed in a framework of theories. When they are disappointed, we reject or revise our theories (if we are wise); preferring immediate recognition, taken as objective, to extrapolation under commitment.

This principle is not invalidated by the historical observation that some theories are more robust than Popper's first account suggested. They do not collapse at the first critical contradiction. Just as Kant was unduly impressed by Newton, Popper may have been influenced by the timely occurrence of Eddington's observations of 1919, decisively favouring an interpretation under Einstein's (already well-developed) theory over a Newtonian account. Newton's physics continues to inform workaday technology (including moonflights): although a prime example of a 'normal science' in Thomas Kuhn's terms, it is sustained not by the social power of its proponents, but because it obeys the logic of held-truth (set out in Section IV.7), using appropriate strategies of approximation, tolerance, and probabilistic extrapolation, and is still developing new structures. Innovation (seen by Popperians as a central interest of philosophy of science) is certainly concentrated in the more advanced applications of relativity and quantum theories; these theories have taken over only at scales where classical theory throws up contradictions. But perhaps nowhere else are we likely to find such a clear picture of different theoretical structures working side by side in what the workers in each would recognize as the same general field: each with its own inductive system, and with a "sphere of influence" determined almost entirely by the nature of the tasks in hand - and in particular their metric aspects. If this picture is less clear outside physics, it is not because other fields are more unified; but rather, on the contrary, that the fields of attention of the different theories tend to be less easily related to one another, metrically or otherwise, in terms of the elements with which they deal.

Without denying the importance, in their own terms, of the social and historical factors to which Kuhn points, our concern here is more with what Lakatos calls the "objective reconstruction" of Kuhn's concept of rival scientific "paradigms", in terms of "research programmes" (see Lakatos & Musgrave, 1970, esp. 179 n.). These last, for us, are seen as constructed upon theoretical systems, each of which is ideally coherent and consistent internally as a deductive structure. On the basis of their essays in the quoted collection, I think it is possible to suggest without undue strain that Lakatos and Kuhn would agree, with radically different emphasis, that no theoretical system which is empirical, in the sense of facing the challenge of possible inconsistency of readings with findings, can survive on social prestige alone. Least of all is this possible, I suggest, when a theory emerges from the research laboratory to face the challenge of practical use.

My own account of induction, then, is relativized to particular theoretical structures: including not only those which inform programmes of research, in any academic or technological sense, but those in regular workaday and personal human use. The relationships of one theoretical system to another are to be understood in terms of their inductive structures: showing themselves most obviously in their respective ontologies - their selections of elements of experience for attention. The degree to which such relationships can be coherently stated depends largely, indeed, on the extent to which their ontologies can be understood to overlap: the degree to which, in Feyerabend's terminology, they are 'commensurable' (a term which, perhaps significantly, connotes measurement). Given a region of common ontology, and only in such a region, does it seem possible to form a logical structure within which commitments and their consequences, associated with different theories, either enrich the system or introduce contradictions. Where (as in my analysis) ontologies include valuative, configurative and positional characters, we have seen how a rich structure of sequential extrapolation can be built leading to findings ("predictions" or "expectations") subject to later corroboration or contradiction: i.e., an inductive structure. Our principal interest at this point, especially with regard to philosophy of science, is in the logical properties of an inductive system involving a GR-theory sharing all or part of its ontology with one or more S-theories.

We can isolate, conceptually, three phases in the life-histories of inductive theoretical commitments; though these phases follow one another in logical sequence, and ideally also in time, there is often overlap or minor confusion (if it ceases to be "minor", the structure collapses - a self-determining criterion). First, the adoption of one or more new commitments (sometimes involving the rejection of old ones); second, theoretical development by the construction of findings on readings in obedience to the new totality of commitments in context, under the logic of held-truth; third, the use or testing of the new theoretical structure, during which any contradictions between findings and readings emerge. If these contradictions are sufficiently frequent or severe, the pressure is on to start the cycle again by the invention and adoption of new commitments. The search for new recognitive 'data' may well form part of the ensuing effort; and we might be tempted to distinguish this as a fourth phase, appearing at the start of end of the cycle. However, this procedure is not logically related to the other three in any distinctive way. New data (whether or not recognized as regular in some way) may or may not appear in association with any phase; and are perhaps best thought of as a general stimulus to theory construction, use or testing. Nor are new data essential to start the cycle, which (as with relativity theory) may be triggered by pure curiosity or puzzlement.

Phase (1) has often been spoken of in terms of discovery; but always involves an element of invention. The invention of a new pattern of commitment may draw attention to new aspects of recognitive material which have always been present in relevant contexts, but either neglected or misread - and to this extent are discovered in the new context. Popper cites William Harvey's observations of the blood system, which, in the light of his new approach, made key use of aspects which had previously been seen but ignored. Again, a newly invented pattern may trigger research which leads to discovery of completely new material. Untypically, the discovery of new material may, conversely, trigger the invention of a new theory to account for it. Darwin's experiences on the Beagle give a striking historical illustration: Einstein's invention of relativity theory representing the opposite case. I have spoken of the adoption of 'one or more new commitments' rather than 'a new theory', since in no case that we can actually expect to meet does this phase occur in a theoretical vacuum - especially if we accept the theoretical status of all recognitions in the terms of this study (Duhem was already making this point in the context of the history of physical theory in his (1914), Ch. 7: see discussion by Watkins in Lakatos et al. (1970), p. 37). Though philosophers of science have tended to concentrate on major "revolutions" associated with what may be thought of as completely new theories, even these have taken over many pre-existing commitments; I am anxious to emphasize that the process of theoretical innovation (and consequent obsolescence) operates on the same principles at all levels, down to and including day-to-day recognitive theories. Whether what emerges is called a new theory, rather than a new development within existing theory, seems a matter of historical judgment rather than meta-theoretical principle. (The notion of a theoretical 'commitment' can probably be translated in most cases into 'hypothesis'; but I have not found that the distinction or relationship between hypothesis and theory is clearly or consistently marked in the literature. I have tried to isolate the forms of particular commitments within a total theory which defines a context; allowing for possible overlaps, or not, of ontologies and commitments of sub-theories and sub-contexts within the whole; and for the case of a theory having only one commitment.)

I take the term 'induction' to describe the process as a whole: a process of continual construction, revision and reconstruction, but also of continual use in the service of understanding and practical purposes. Its logic is that of held-truth, as applied to the state-of-the-theory at the current frame in each context. The attempt to impose a specific 'inductive logic' on Phase (1), in the manner of the crude 'inductivism' attributed by Popper to the Vienna Circle (especially Carnap), is on this analysis to be rejected not only because it does not reflect the actual form of theory-construction, but because it would restrict or indeed prevent invention. Its naïve form is the tenet that (in certain unspecified cases) sufficiently repeated observation of like cases logically founds a generalisation from these particular cases to a universal law; its great weakness being that any attempt to specify which cases are relevantly alike turns out to lead to circularity. Quine and Ullian have a slightly modified version which categorizes 'induction' (described in more or less the above terms) as just one of several methods for constructing hypotheses (rather than 'laws') (1978, p. 90). They justify retaining the special term for this method on the ground that we already have good terms for other methods they propose: but we already have a good term for this one, the one they use, 'generalisation'. It has the advantage that it also covers generalisation from the single case, no less (and no more) reliable; the child burnt by one fire generalises his fear to all fires (for some reason they distinguish this as 'analogy'). But we have no other term which covers all three phases of the process as I have described it (the term 'hypothetico-deductive method' is used by Popper to cover only phases (1) and (2)). For this reason I propose to adopt 'induction' for all instances of the total process, independently of the means by which new commitments are arrived at. I shall say more shortly about why I think Quine seems not to appreciate the need to mark this more comprehensive concept, by one term or another. My main point here is to argue for a firm refusal to place any restrictions whatever on conceptual invention in phase (1): no taxonomy of methods should claim to be exhaustive. This view seems to be in the spirit of Feyerabend's call for a "tolerant attitude towards meanings" during a period in which "competing conceptual systems" are associated with possible new theoretical departures (he instances changes in the understanding of "impetus" to accommodate Newton's account of the nature of inertia: Reese (1963), p. 30 f.). The discipline which controls the process comes (in principle, if not in time) later, in phases (2) and (3): the demand for logical consistency of findings and readings in testing and use.

A more holistic view of a possible logic of induction is that considered by Hempel (1966, Ch. 5), in what may be seen as an attempt in the positivist tradition to overcome the failure of the more naïve version, mentioned above, to yield convincing analyses of historical cases. It is located, however, in our phases (2) and (3); he rejects the 'inductivist' poisition for phase (1), and describes the rational structure of the later phases as 'induction in a wider sense' (op.cit., p. 18). For this he proposes what he calls a 'nomological-deductive' analysis, for which, in a situation governed by a given set of 'laws' (which we may read either as commitments or theories), a given set of 'initial conditions' logically determines a specified 'event', or outcome. It has led to discussion as to whether it may be possible to extract hierarchical patterns amongst laws or conditions in such a way as to produce simple and comprehensible analyses for particular cases. But we must expect, in the light of the present study, that no such analysis would reveal any additional logical principle beyond that of held-truth as applied to patterns of commitment of the relevant theories; in association with 'initial conditions' taken to be a particular set of readings under these theories in a rather arbitrarily restricted context. So, although one can hardly quarrel with the intuitions underlying this approach, it seems to have little explanatory power.

Quine's view of the structure of empirical theory is also holistic - he is fond of talking of 'the whole of science', 'our beliefs ... in a body'. It is possible, however, to find in his work at least three aspects of internal structure, as seen from different perspectives. One is the account of induction, analogy, etc., just mentioned. Another, prominent in his (1953) and later, is a view of theory as a structure of sentences, classified in principle only in terms of their conceptual distance from reports of observation. The main thesis is that the 'core' of any theoretical system, the central features of its account of some aspect of empirical truth, is grossly underdetermined by its logical relationships to its accounts of observations, located at the 'periphery'. A corollary is that moves can be rather freely made which have the effect of altering the truth-values of sentences located anywhere in the system (including, critically, observation sentences) by adjustments elsewhere (Lakatos attributes a similar view to Duhem: Lakatos et al. (1970) p. 184 f.). There is, however, an admission that some kinds of sentences, especially near the periphery, are more "recalcitrant" than others: later given the form, "As dissident theorists converge towards observation sentences they converge to agreement" (Quine and Ullian, 1978, p. 28). A hint, about the kinds of restriction that might be expected to determine which sentences are likely to resist manipulation, and how, is to be found in the context of a third approach, from the perspective of ontology, already mentioned. This is the notion of a 'fully interpreted theory', whose 'internal deductive system', and the ranges of values of whose variables, are determined (1969, p. 51).

As we have seen, it is relatively easy to translate this last notion into the language of C-theory, in which a closer analysis of internal structure is possible, at least in contexts of R-theory and associated S-theory:

the first being naturally interpreted as Quine's 'periphery', the second including his 'core' and intermediate structures. In this interpretation, the ontologies of peripheral R-theories appear as parts of those of S-theories, in such a way that the same elements are clearly specified so that their structures of assignment are governed both by recognitive and substantive commitments. We must agree at once that, in such a structure, S-theoretical commitments (which may bring hypostatic elements into play) are underdetermined by R-theoretical structures. We can indeed point to R-commitments which are virtually (if not totally) independent of all S-theoretical inventive variation: the famous 'snow is white' might suggest one of them. We can also point to R-commitments which incorporate prior S-commitments, such as those involved in recognitions of length in terms of angle, or temperature in terms of length (and there are plenty outside measurement too). Such analysis may help to illuminate a slight threat of ambiguity in Quine's accounts of 'observation sentences': as to whether they are inference-free (and hence, presumably, theory-free), or subject to manipulation of their truth-values by theoretical adjustment.

We can note, further, that different S-theories will typically draw attention to different systems of R-theory; and may be expected to differ in their adoption of S-theoretical commitments governing the assignment structures of R-elements in such a way that doubt arises whether or not the R-systems of different S-theories determine recognizably (some or all of) the same elements. This is Feyerabend's problem of the possible 'incommensurability' of theories which superficially appear to account differently for the same phenomena. Our R-theories can be readily interpreted as versions of the 'observation theories' distinguished by Lakatos, within higher-level theoretical structures, partly to throw light on this problem. Taking Feyerabend's choice of term somewhat literally, it does appear that strictly specified metric assignments in the physical sciences are accepted in many cases ad independent of rival S-theoretical interpretations of the relevant recognitive structure; and hence as a sound basis for adjudications between them in these cases. There seem, however, to be nothing to prohibit cases where two theories which appear to describe broadly the same complex of phenomena, draw attention to different sets of elements (idents or characters), each necessarily (under its own rules) neglecting some of those whose assignment structures are governed by the commitments of the other. In such a case there may be no set of metric assignments to found commitments under which they can be shown either to conflict or to support one another; or to be strictly complementary (in the sense of describing the complex completely, between them). In such a case, it might be natural to call them 'incommensurable'. Indeed, in some such cases there may be one C-scale at which both theories pay attention to elements recognized as the same (or theoretically similar) about whose assignments they agree; while mutual neglect of elements is adopted at another scale. They are not "rivals", each is a partial account which may be consistent in its own terms.

It has become recognized in the Popperian tradition that falsification of a theory by a single critical experiment is not such a simple matter as at first it seemed. The 'logical symmetry' which determines that a general hypothesis (S-theoretical commitment) may be decisively falsified in a single case, but never completely verified for all cases, has remained in principle unassailable as a tenet of the tradition. But, to be decisive, a falsifying observation must be protected as strongly as possible from the fallibility which the tradition acknowledges as infecting all empirical propositions: it must be 'fortified' by a 'well-corroborated ... hypothesis' (see Lakatos et al. (1970) p. 108). Much does indeed depend on the strengths of commitment invested in rival theoretical interpretations of a concrete context; and these depend not only on the social or intellectual prestige of their proponents (where this is a factor) but also, rationally, on their inductive performance. In everyday conflicts of R-theory it may take very little to upset one interpretive commitment in favour of another. But philosophers of science - in a search for optimum conditions for the construction of certain empirical knowledge - tend to concentrate attention on well-established systems with highly-developed S-theoretical structures. These rely heavily on metric assignments governing well-controlled systems of elements, permitting the progressive incorporation of levels of S-theory with a hierarchical structure, each new level treating the previous one as 'observational' (R-theoretical): some illustrations of this process were given in Part III. Such systems become of interest for this tradition only when they have accumulated a considerable history of inductive success (in use and in research); and it is extremely rare for a single observation (such as Eddington's observation of 1919, a fortuitous date for Popper) to have more than a minor corrective effect. However, it is clear that for an observation to be decisive at all in such contexts it must take place in well-controlled and understood conditions for measurement.

At the same time, it is the absence of well-controlled conditions for measurement in most of the life sciences (the reasons for which were discussed in Section III) that militates, for better or worse, against either firm adjudications or hierarchical constructions of incorporated theory in these fields.

After nearly all of this study was written, a new philosophical account of science came to hand with which I think I may claim close kinship, so that I cannot deny myself the opportunity of pointing, however briefly and inadequately, to the more obvious similarities and differences. Van Fraassen (1980) mentions Popper only on probability, though he does seem to share some of Popper's intuitions - as in the observation that "... any scientific theory is born into a life of fierce competition ... only the successful ones survive - the ones which _in fact_ latched on to actual regularities in nature" (op.cit., p. 40). He may think the Popperian analyses of 'theory' just too vague to generate an effective account of its role in science (or elsewhere). If so, my position is analogous.

Where this study touches the fringes of topics of which van Fraassen gives a full and rigorous account, there is much evidence of common outlook. His demonstration of the context-dependence of explanation, as a role of scientific theory, chimes harmoniously with my own account, though the sense of 'context' is different. His analysis of probability in science as a structure of "_ideal (repeated) experiments_", closely related to "the theory under consideration" (191, his emphasis), and intimately involving extrapolation (160), is evidently in sympathy. Van Fraassen's own breakdown of the concept of 'theory' itself, drawn from model-theoretic semantics, differs from my own less sophisticated analysis (much of which, from his point of view, may perhaps be taken for granted); but not so as as to seem irreconcilable at any point. Importantly, its location in semantics does not prevent him from sharing a belief that exclusive concern with the language in which each theory is expressed may blind analysts to the real nature of more general relationships between our theories and the world. More deeply, we both see theory-building as an organically evolved structure, generating appropriate responses: a basis of commitment for the regulation of action (in science, further research).

A genuine difference is that van Fraassen, wishing to rebut objections that his empiricism leads to "self-defeating scepticism", appears to place what seems to me a dangerous reliance on a firm, theory-independent distinction between what is and is not 'observable', as having special relevance to questions of the truth of theoretical statements. He stops short of saying that no theoretical account, which goes beyond what (in its own terms) is observable, can be true; but only just (pp. 70 ff). Two features of C-theory may be of use in this difficulty (if such it is). The first is that the language of this theory allows us to display internal distinctions within theoretical contexts, between what is and is not taken to be observed (or in principle observable), as autonomous; without thereby being hopelessly adrift from rational control. Autonomy may be exercised by successive, fully 'objective', incorporations of S-theory: this incorporation extends, step by step, the scope of what is accepted as observed. The second is our emphasis here on 'held-truth' within a given theory: on this account, all well-formed held-truth is a candidate for truth, whether or not its subject matter is supposed to be observed, or observable.

Given the areas of agreement, it seems reasonable to hope that my account could be developed to yield precisely similar models of the kinds of structure in which recognition-from-observation takes place (in my terms, R-theories) in any context which may also include representations of 'unobservables' (-theoretical structures). That is to say, in his terms, our philosophical theories may be 'empirically equivalent' in certain applications. In my terms, however, <u>all</u> such structures are candidates for truth provided they tell a complete, unequivocal story essentially involving their recognitive elements. R-theories are not, of course, infallible, but their roots in an evolved system of perception are our best guarantee that our theory-building is part of that 'dialogue with (the rest of) nature' we all agree is the only source of empirical knowledge.

Amongst alternatives to total, stultifying scepticism is not only indiscriminate belief in any particular theory or set of theories (however categorized or restricted), but also a broad commitment to the belief that there exists a unique real system to which we belong, of which many of our best founded and constructed theories give true, partial descriptions: at the price of acknowledging that we can never tell just which parts of these theories (scientific or not) are false. Belief is not to be bestowed on grounds of the mere reasonableness of Realist explanations. Attached too firmly to our own (collective or individual) theories it can discourage development and bring on <u>rigor consensūs</u> - a fatal condition, not only in science.

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