On Central Processes in the

Temporal Control of Movement

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Abstract

Various applied problems in the areas of manual control and biosystems analysis involve the desire to noninvasively monitor cognitive processes during task performance. This thesis addresses the general problem that the usual methods and assumptions of biosystems analysis may not strictly apply where psychological functioning exhibits a controlling influence on behavior. An experimental situation is proposed as such a case, in which the percept of duration seems to have controlling influence on the timing of a fingertap.

The major themes in theories of motor behavior are surveyed and their reference to higher processes examined. A useful taxonomy of mental processes is outlined to clarify the nature of the processes involved.

The notion of representation is basic to the characterization of mental processes. Prevalent views on the relationship of representation and behavior are assessed, leading to a formalization of representation in which a probability measure on the representational structure can be understood as a model of belief or subjective expectation. The model of representation is constructive and satisfies the taxonomy of mental representation.

An information measure and channel analogy of central processes is formed using the probability measure. The channel analogy leads to a definition of representational event. The representational event is used to formalize the idea of a subjectively constant clocktime interval from which first-order predictions of central process effects on periodic behaviors of subjectively constant rate derive.

A fingertapping experiment was undertaken to verify these predictions. Two levels of cognitive influences on finger tapping were discernible. The conformance of the data to the predictions suggests that the developments of this

thesis could be useful to biosystems and human factors analyses of cognitive level phenomena underlying behavior.

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1. Introduction

> "Behavior is organized, but the organization of behavior is merely derivative; the structure of behavior stands to mental structure as an effect stands to its cause."
>
> J. A. Fodor 1983

This thesis addresses the general problem that the usual methods and assumptions of biosystems analysis, when applied to situations where psychological functioning exhibits a controlling influence on behavior, may not strictly apply. An experimental situation where the percept of duration seems to have a controlling influence on the timing of a fingertap is proposed as an example of such a case.

Feedback control theory and the system analysis approach have been fruitfully applied to motor systems and many other physiological processes. Grodin (1963, pg. 196) illustrates the rationale of systems theory to the physiologist in the following statement:

> "On the one hand he is ultimately interested in the overall system performance, but on the other he wants to understand this performance in terms of the contributions of individual components."

That is, the utility of control systems theory to the study of physiological systems involves the (reasonable) desire to determine the actual anatomy corresponding to the formal system elements deduced from the functional input-output structure of the system. In fact, the principal test, perhaps the criterion, of the usefulness of functional system identification to the biologist is the ability to establish the functional properties of system components such that they may be experimentally located in the organism. A noted researcher makes the point as regards clinical application (Robinson 1975, pg. 337):

> "The application of control systems analysis has helped us enormously in defining the functional operation of the oculomotor subsystems and the nature of the signal processing that must go on in each. These models must, perforce, ride roughshod over the details of anatomical connections and electrophysiology. At the other extreme are the studies which examine the details of connections but can say very little about function. Neither are of much help to the neuro-ophthalmologist whose job it is to interpret disorders on the basis of both structure and function. Obviously the two must be tied together."

And another (Fender 1964b, pg. 24) in reference to biologists generally states,

> "The investigator measures the responses of a functional unit to some simple and well-defined stimulus, and the relation between stimulus and response reveals the presence of certain classes of elements in the unit. It thus provides a clue to the makeup of the system, and with this clue the biologist can now look for the elements of the mechanism and describe its operation with more confidence."

I will refer to the desire to ascertain the system components underlying functional system behavior as the problem of the *locus of control*. That the determination of the locus of control corresponds to the primary efforts of the physiologist is clear. It is apparent in behavioral neurobiology and psychophysiology where the achievements of single cell neurobiological studies in understanding the functioning brain are certainly of the form of determination of the locus of control (e.g., Hubel and Wiesel 1962). The related objectives of source localization using the EEG (see Eriksen 1984) revolve around the desire for noninvasive monitoring of brain function during actual task performance that doesn't require prior knowledge of where to put a single electrode and that can resolve the coordinate location of brain activity during the task. The rationale is clear enough -- to identify the function of the active brain structures. Much has been, and more can undoubtedly be, revealed about brain functioning in this way.

In the periphery, the spinal level feedback control of limb movement and the functional involvement of anatomical features of the muscles tendons, etc., constitutes the perfect application of servo theory in a biological system insofar as the determination of the locus of control is concerned. Also, ascending and descending motor pathways are functionally obvious from the controller perspective. The oculomotor system has yielded nicely in many instances to system analysis and the determination of the locus of control (e.g. Fender 1964a, b).

Of course, the nonlinearities which are ubiquitous in physiological systems may preclude the ability of the systems physiologist to provide a transfer function for a system (e.g., see St-Cyr and Fender 1969), although different kinds of analysis can be carried out to achieve a system characterization to any order of approximation (e.g., white noise methods to determine the kernels in the time series characterization of the system being analyzed). The goals of these methods are not different, however, than the linear methods. Marmarelis and Marmarelis (1978, pg. 4) plainly state the intents of the systems physiologist,

> "The natural question, following the functional identification question, and a logical sequel to it, is 'How does the system do this? That is, how are the various components of the system interconnected and how do they interact to produce the observed responses?' Obviously, this question concerns the structure of the system and we, therefore, term it the *structural identification* of the system. In practice, it is usually carried out by performing anatomy . . ."

Consider the following experiment: A subject is seated in front of a video monitor and views a stimulus symbol (e.g., a numeral or the name of a number) each time he taps his finger. A fingertap presents a stimulus which remains present until the next tap. The subject causes the stimuli to be presented at a subjectively constant rate while attempting to remember the stimuli presented and assess their relative frequencies. At some point the experimenter manipulates the ensemble of stimuli either by altering the relative frequencies or by adding stimulus members to the stimulus ensemble.

The experimental results suggest that the subjectively constant fingertap rate objectively varies after the manipulation, say, in one case slowing a small amount and in the other increasing a little. It is this situation and data that I investigate in this thesis.

How can the temporal control of the fingertapping be characterized in this situation? The subject is not required to reproduce any particular clocktime rate. We only consider the objective variations in a subjectively constant rate; we are evidently observing the properties of the subsystem which acts to produce the desired output (in this case, something like constant time intervals between taps) in reaction to an actuating signal (which is, in this case, not obvious), commonly called the *controller*. The rate is produced by the finger, but at the outset it seems difficult to see how the proprioceptive cues from the finger movement could account for the data (as is required by *peripheral* theories of motor control discussed later) when no kinesthetic experimental manipulation was made. Evidently, the situation demands that the *percept* of a time interval be used to control the finger. In this task no other measure of rate is available, except for kinesthetic cues which were evidently not used because the tap rate indeed varied systematically with the stimulus ensemble manipulation. Since the tap rate varied, it also appears that that percept is a function of the stimulus conditions in relation to the task the subject is engaged in. It is unlikely that the causal property of the stimulus ensemble manipulations on the tap rate is inherently manifested in the stimuli alone outside the context of the task. That is, it is not likely that the numeral "1" can, in itself, have any unique behavioral effect but it is probable that the relationships existing between "1" and the other stimuli in the context of the task will have behavioral effects. The usual cybernetic model (a reflexive physical stimulus input-output system model involving feedback to control the output) of limb movement doesn't have much to provide by way of explaining this experiment. We are, however, clearly viewing a situation properly subsumed under the heading of the control of movement.

The problem then handed us by our experiment is, in the first instance, a functional identification. That will involve the determination of the relevant stimulus parameters, response parameters, and the consequent functional transformation from one to the other. The second part is structural identification, traditionally presumed to consist of "performing anatomy." The problem is not so obvious in either aspect. What *are* the stimuli in this task? It will be argued that we are forced to accept the conclusion that the operational description must involve the semantic content (i.e., an interpretation) of the stimuli. The second parameterization, that of the responses, must be changes in tap rate. How is one to get such a functional transformation? And if the perceptually causal properties of the stimuli are task (i.e., situation) dependent, how can one ever expect to "perform anatomy" for structural identification of the locus of control? A structural identification is proposed that is quite different than "performing anatomy."

So, it seems that one is left with the question, "Is it possible to subject this experiment to *any* scientific system analysis?", the answer to which may be a matter of taste. The analysis will certainly be different than what is usually considered to constitute a system description. On the other hand, it seems a clear case of control needing explanation.

In the case of the fingertapping experiment, it would undoubtedly be possible to get, to some first approximation, a system block diagram with a block labelled something like "higher centers comparator" (e.g., see Bahill and McDonald 1983, Robinson 1975, Dallos and Jones 1963) into which we might force some *ad hoc* parameter estimation model of "internal representation" to fit the data. But that would certainly be begging the question of how the percept of constant duration might possibly influence a movement. It will be argued that a particular physiologically isolable component corresponding to that percept is not reasonably expected (e.g., across tasks, experience, sensory modalities, etc.) because it is, in part, *central* in the taxonomy of mental processes proposed by Fodor (1983).

The choice of the percept of duration to illustrate the topic of this thesis is not essential in that *any* percept *should* do. However most percepts, especially visual perception, overwhelmingly appear to us as simple transductions of distal stimuli (e.g., see Gibson 1950) and, because of the strength of that appearance, obscure the issues I will raise. It is initially appealing to invoke a simple reflexive paradigm in the explanation of visual experience but more careful analysis often indicates the thought-like character of perception (Rock 1983). The percept of duration has no associated transducer and lacks the detailed immediacy of vision. Therefore, it has fewer obscurantive preconceptions to interfere with analysis and draws serious doubt to the generality of the notion of reflexive theories of perception. In the absence of those theories, one must resort to representational theories, but that is jumping ahead of things.

The above involvement of the percept of duration with the control of movement timing is a case example of higher levels of brain function affecting intentional behavior in a nontrivial way. The virtues of the particular case include that movement timing is immediately accessible to quantitative measurement, and that the percept of duration is physiologically enigmatic and therefore interesting in its own right.

The thesis will begin with a brief survey of the motor control literature. Other than reaction time (RT) studies, little has been written about timing of movements, interest having been focused on position accuracy or tracking accuracy (however, see Rosenbaum 1983). But time coordinates are inherent to motor activity and deserve investigation (see Bernstein 1967). A notable exception is found in studies that examine the integration and control of very rapid motor sequences.

The theories surveyed fall into two general categories. The first involves feedback systems utilizing proprioceptive information available from the controlled limb (see Sherrington 1906). These are the so-called ***peripheral*** theories (e.g., Adams 1976). Various empirical considerations lead researchers to consider other systems where a movement is planned or "programmed" centrally and then executed in a feedforward manner. These are called ***central*** theories (e.g. Lashley 1917, Schmidt 1976). It happens that, as the evidence against the exclusivity of proprioceptive feedback in movement control builds up, appeals to more and more cognitive-like processes emerge. The requirement for internal representation and psychological quantities and functions (e.g., memory and inference) becomes well established (see Wilberg 1983, Rosenbaum 1983). As these higher level functions and properties are invoked theoretically, there is generally no corresponding sophistication in the assumptions underlying structural analysis (whether actually undertaken or merely anticipated; see Robinson 1975, Bahill and MacDonald 1983). "Central processes" is a term that comes to mean nearly anything not included in a spinal reflex loop and nearly always carries a connotation of a feedforward system (see Schmidt 1982). Compact neural subsystems are usually conceived of as underlying these processes. "Peripheral processes" comes to connote a feedback system involving low-level proprioceptive information and reflexive, non-cognitive functioning (see Adams 1976, Saltzman and Kelso 1983). Structural analysis again involves compact neural subsystems.

To clarify the taxonomy of mental functions involved, I consider cognitive processes in a three-level hierarchy: transducer systems, input systems, and central processes (Fodor 1983). Central processes are defined by two properties, isotropy and quineianism. These properties produce severe difficulties for the usual neuroreductionistic assumptions underlying structural analysis. In particular, compact neural locality underlying observed function is likely lost. Many instances of higher levels of mental functioning implicated in the control of movement are arguably central in this sense, e.g., goals and knowledge of results, inferential processes and, I assert, aspects of the percept of duration.

Basic to the idea of input systems and central processes is the notion of symbolic mental representation. This issue is the source of much debate (e.g., see Ashby 1960, Searle 1980, Fodor 1981, Freeman 1983) and positions taken with respect to it define essential foundations of the doctrines of the various disciplines involved in the study of behavior -- the neuroscience movement, behavioral psychology, cognitive psychology, cognitive science, artificial intelligence, and systems physiology. The issue of mental representation is surveyed and a position taken that leads to postulated central process effects on the control of movement timing. An information channel analogy of central processes is introduced.

The literature on the percept of duration is surveyed and placed in the context of control of movement and central processes. An experiment is undertaken for which the central processes postulate, together with the channel analogy, yields quantitative prediction of central process effects on movement timing. The prediction is supported by the data. The results of the experiment also indicate that an effect due to input systems is present. Thus, two levels of cognitive functioning are shown to influence movement timing.

The possibility of structural analysis is discussed with implications for source localization techniques using EEG. Human factors and human performance issues are considered. Finally, the percept of duration is discussed and implications to percepts generally are assessed.

2. Control of Movement

This chapter will overview the major themes in theories of motor behavior and review the reasons for involving higher psychological processes. The appeal to higher processes is most clearly appreciated in an account contrasting the motivations for peripheral and central theories of the control of movement.

2.1 Peripheral theories of motor control

The peripheral theories of control of movement are, generally speaking, theories that apply the techniques and concepts of feedback control theory to the analysis of the motor system. The seminal presentation of the cybernetic perspective was, after all, subtitled "Control in Animal and Machine" (Wiener 1948). In the area of biosystem analysis of motor systems, the work of Fender (1964a) with the oculomotor system, Lorente De No (1933) or Ito (1973) with the vestibulo-ocular reflex are examples of the fruitful application of these methods. In each case, the analysis of function from the perspective of feedback and control has led to a more detailed understanding of the physiology of the associated system. The well-known muscle spindles, tendon organs, and associated alpha-gamma spinal reflex are the archetype of the complete description of motor subsystems conceivable by the system engineering approach. Feedback in the form of proprioceptive information (Sherrington 1906) allows regulation of limb position and activity.

Outside the motor control domain, but still within the domain of neural systems, feedback models form the theory of addiction, with the analysis of the nicotine action at the synapse being the classical case (see Dole 1970). Beyond that the cybernetic view has been essential to the comprehension of virtually every bodily regulation function (e.g., see Grodin 1963). The systems theory approach leads to the homogeneous treatment of a variety of systems and is therefore a very pleasing and parsimonious analysis of physiological systems of considerable complexity. The principal strength of the approach is to predict structure from function and performance as well as the converse. The duality is captured in the engineer's terms "identification" and "design" or "synthesis." The major utility of these methods is thus related to the issues of function and locus of control discussed in the introduction. In the vocabulary of experimental psychology, one implicitly is defining tissue competence with the remaining task at hand being determination of its locus and structure.

This approach has been particularly successful in the peripheral motor systems (the term being unfortunately confusing in this context). It is useful to consider examples of system identification and see what they provide us.

At the simplest level, the stretch reflex illustrates a feedback system in motor systems. Figs. 1 and 2 show the structures and connections involved. The muscle spindle is a bag of intrafusal muscle fibers imbedded in the extrafusal (voluntary) muscle that moves the limb. When the muscle is stretched, the muscle spindle fibers are also stretched. The muscle spindle is innervated by the (efferent) γ-motoneuron and group Ia afferent fiber endings. Stretching the intrafusal fibers causes generator potentials in the Ia afferent endings, sending a neural signal to α-motoneurons and spinal inter-neurons synapsed to α-motoneurons. The interneurons (in this case) are inhibitory. The α-motoneurons have endplates directly on the extrafusal muscle and cause contraction of the muscle when they fire. The inhibited α-motoneuron is attached to the opposing muscle group and the uninhibited α-motoneuron is attached to the flexor muscle. The Ia afferent signal thus generates α-motoneuron activity in the flexor pathway and suppresses activity to the opposing muscle. The familiar "knee jerk" reflex results from stretching the intrafusal fibers with a light tap.

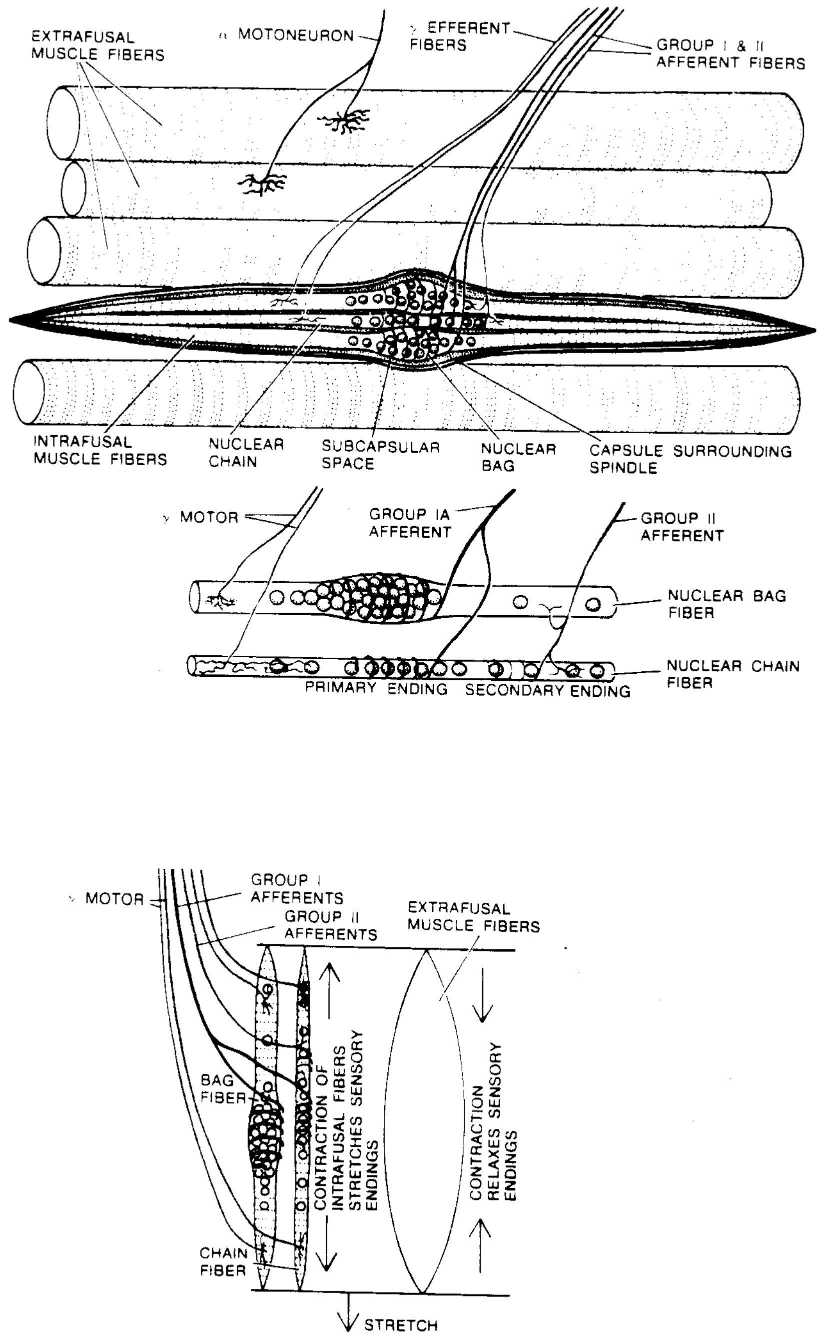
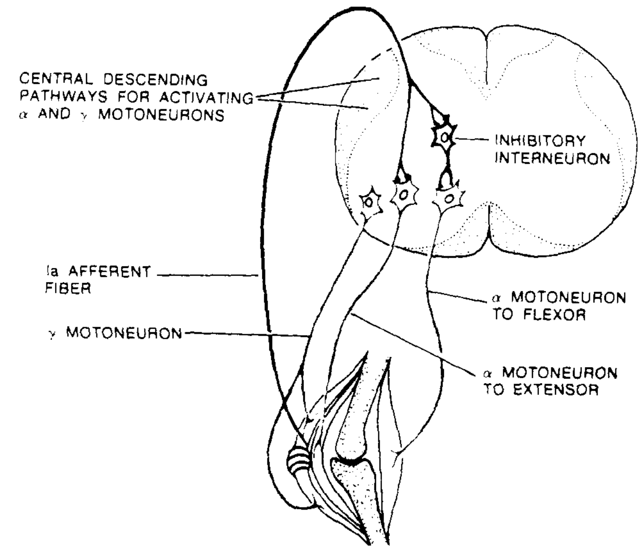


Fig. 1 **MAMMALIAN MUSCLE SPINDLE**



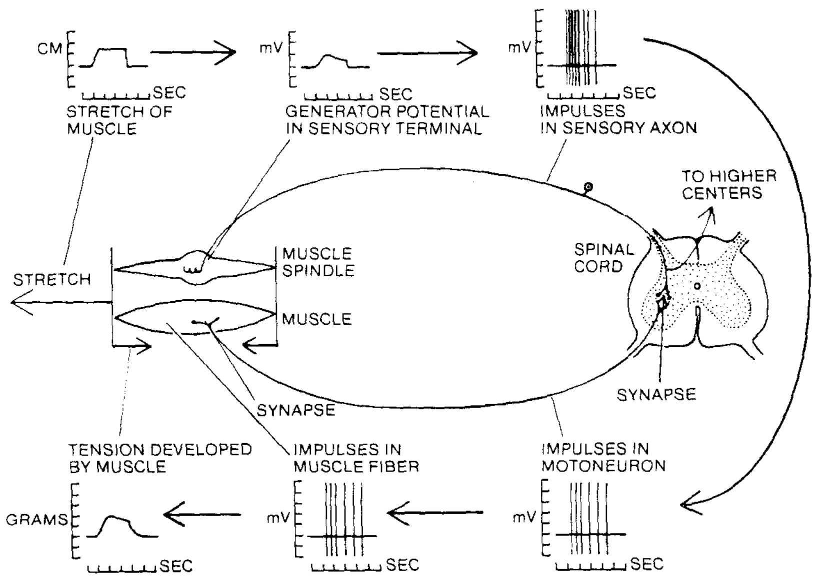


Fig. 2 **CONNECTIONS FOR STRETCH REFLEX.**

Golgi tendon organs respond to tension in the tendons attaching the muscles to the bone. They also have an afferent pathway (Ib) to the α-motoneuron, but the pathway is inhibitory, tending to decrease flexion as tension increases.

The reflex just described is a case of automatic control of limb positioning that is ipsilateral (limited to the side of the body stimulated), monosynaptic (the Ia afferent synapses directly to the α-motoneuron) and intrasegmental (each reflex pathway is limited to one spinal segment). The components and activity are all structurally compact, functionally obvious, and largely aplastic.

Both the α-motoneuron and γ-motoneuron are also influenced by efferent neural pathways descending from the central nervous system (CNS). The gain of the reflex can be manipulated by exciting the γ-motoneuron which will cause a contraction of the intrafusal fibers and thus cause the generator potential to occur in the Ia pathway endings with less stretching of the voluntary muscles than normal. The possibility of excitation of the γ-motoneuron from CNS obviously allows intentional, centrally mediated control of limb movement.

There is also evidence for afferent Ia fiber information reaching CNS. Historically, Ia information was thought not to reach sensory cortex. For example, Gelfan and Carter (1967) found, with the wrist locally anesthetized, that when muscles were passively stretched patients could make make no report of (an apparent change in) hand position consistent with the stretch. Ia information was present, but did not influence conscious perception of limb position.

Until Goodwin *et al.* (1972), such was dogma concerning Ia signals. They had a blindfolded subject attempt to "track" with one arm passive movements of the other, which had rapid vibration applied to the biceps tendon at the elbow. The vibration is known to distort Ia afferent signals.

Subjects attempting to match the perceived position of the vibrated limb displayed up to 40° misalignment. Vibration to the triceps tendon reversed the direction of error. Goodwin *et al.* interpreted the data to infer that Ia afferent activity affects the ability to consciously perceive limb position and therefore that information must reach cortex and conscious levels.

This kind of data is taken, by peripheral theorists (e.g., Adams 1971, Kelso and Stelmach 1976), to be a strong indicator of the importance of proprioceptive information at all levels of motor control. But how would that information be used at higher (i.e., cognitive) levels? The "dynamical account" of Kelso and his coworkers (e.g., Kelso and Stelmach 1976, Kelso *et al.* 1981, Saltzman and Kelso 1983) is an attempt to incorporate that issue, among others, in a theory of motor control. They postulate the existence of neural dynamical representations of motor systems in the brain. These representations are argued to be of the form of dynamical models, that is, ". . . dynamics is the language of motor memory and control." (Saltzman and Kelso 1983, pg 31). They argue that the neural models underlying motor behavior are functionally specific, special purpose processes (see Kelso *et al.* 1981). The postulated functional specificity of these processes ensures their place in the taxonomy of Fodor (1983) which will be discussed later. An important thing to notice is that the functional specificity of the postulated internal models is the most conservative appeal to mental representation possible.

In the context of closed loop theories, what central neural representation allows is at least a passing attempt to encompass the utilization of proprioceptive information, the ability to improve and to acquire new motor skills, the ability to remember motor movements and the automatic regulation of motor movements (see Adams 1976, Schmidt 1982). A very general schematic of the closed loop theories is found in Fig. 3.

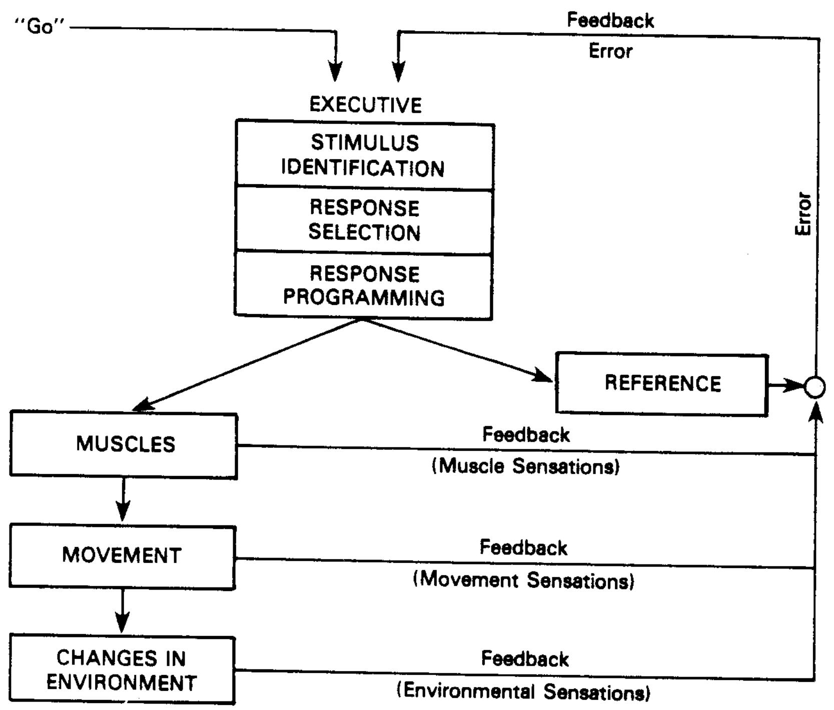


Fig. 3 General schematic of closed loop theories of movement.

In the oculomotor system rather detailed feedback system descriptions have been worked out (e.g., Fender and Nye 1961, Young and Stark 1963, Fender 1964a, Robinson 1975, Bahill and McDonald 1983) for the control of eye movements. Fig. 4 is a schematic of a possible model of an eye movement control system for object tracking. The displacement of an image from some point within the fovea produces an error signal generating positional feedback for tracking tasks. This feedback information is extracted only from displacements within the foveal region. The retinal image motion also generates velocity signals. Retina and cortex both have roles to play in the control system. (Additionally, if the head is displaced, a vestibulo-ocular reflex involving the cerebellum adjusts eye direction to the target (Ito 1973). The model of Fig. 4 does not involve the vestibulo-ocular reflex). Information extracted by the retina is used by the cortex. The retinal image position displacement and velocity information is used by the cortex to generate and adjust the command signal sequences to the ocular muscles and thereby perform the tracking task.

If predictive mechanisms are not postulated, the model produces phase lag unrepresentative of actual human eye movement data. Essentially humans can, with practice, track band limited sinusoidal and square wave trajectories with zero phase lag (Dallos and Jones 1963, Fender 1964a, Robinson 1975, Bahill and McDonald 1983). Thus predictive mechanisms are commonly postulated to exist in the control system. Their locus is usually assumed to be cortical, presumably using the global intelligence of the brain to analyze and predict target motion. Some suggestive empirical information indicates the cortical predictor. Light doses of barbiturate interfere with prediction and increase phase lag (Fender 1964b). Other evidence is that where target brightness is reduced below recognition threshold, tracking continues but prediction is lost. That indicates that, only when the stimulus is registered at levels sufficient for recognition, can

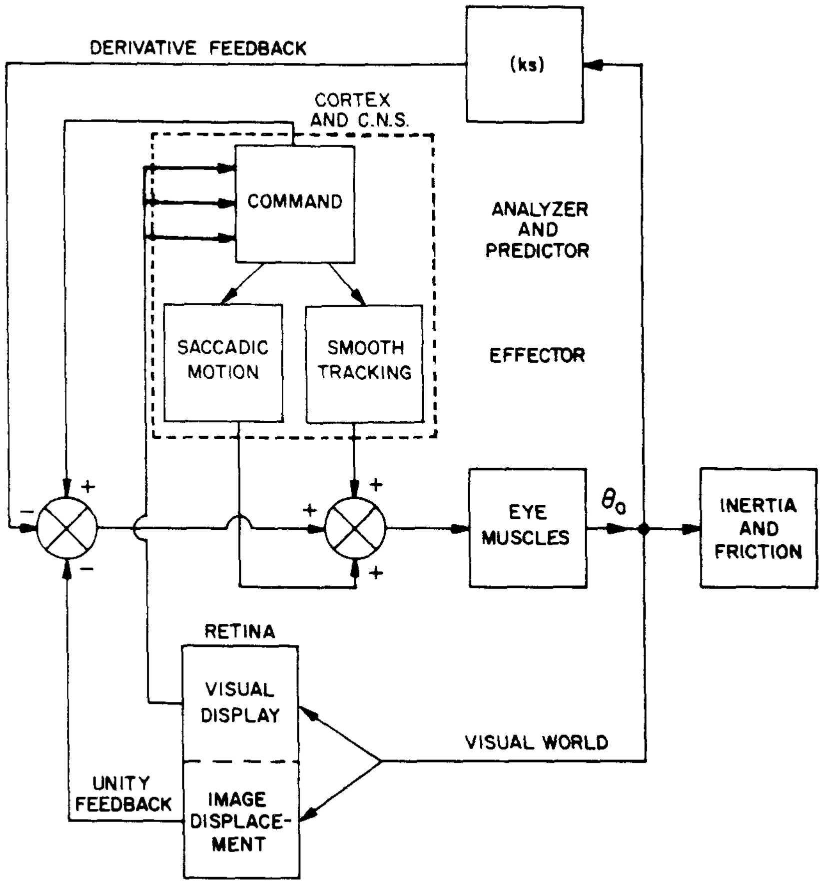


Fig. 4 Possible model of the eye movement control system.

prediction be made (Fender 1964a).

There is indirect evidence of even more cortical involvement. When locations on the retina are stroboscopically stimulated in sequence, the well-known "apparent motion" effect occurs: one perceives a single object in motion, not successive objects flashing. The neon signs in Las Vegas often depend on the illusion for their effect. Even human infants perceive motion from stroboscopic input (Tauber and Koffler 1966). Apparent motion is perceived as smooth and would undoubtably produce smooth tracking eye motions. This effect, on the basis of the above, could be due to simple retinal encoding and not be due to cortical involvement. However, under some conditions, successively stimulated retinal positions with similar luminance and temporal stimulus parameter values do *not* produce apparent motion. In particular, if the luminance points are fixed in space and are seen to be rapidly covered and uncovered by an opaque square, the illusion does not occur (Rock 1983, Chapter 7). There are many other conditions under which the illusion is lost. The conclusion implied here is that motion information is often the result of mental operations and not always a proprioceptive code. If that is true, position prediction would, *de facto*, often require mental operations.

St-Cyr and Fender (1969) argue that the predictor mechanisms derived by methods attempting to account for the zero phase lag of tracking suffer from the defect of being based on a linear system model. Since the oculomotor system is not linear at all, a transfer function cannot be derived and therefore a minimum phase lag cannot be calculated. Perhaps then it is the case that the appeals to higher levels are uncalled for and with the proper model no such levels are theoretically necessary. St-Cyr and Fender (1969) point out that the eye movement never anticipates the target. What is proposed is an information model where simple time delays are obtained by a function of the rate of information transfer between input target motion and eye tracking responses. That is, time delays are just how long it takes to process the visual position information. What is suggested is that the input time series of the retinal image positions is segmented into fixed duration "symbols." These "symbols" depend on the class of target motion and, presumably, certain other knowledge other than visual. The time delay is the time required to process one symbol of input information.

I would point out that the rejection of the intelligent brain predictor is here replaced with a system of symbolic representation that requires symbol manipulation (i.e., computation) and therefore, no less than the predictor, requires the assumption of an intelligent brain. For my purposes it suffices to say that in either case mental levels of representation are required of these closed loop systems. Whether they allow linear analysis or not is not relevant to that issue.

A third variation of system models of oculomotor control more explicitly involve central representation with the use of notion of "corollary discharge" (the term due to Sperry 1950) (a.k.a. "efference copy") in the characterization of tracking mechanisms. The efference copy is a concept whereby the output signal from motorcortex or midbrain nuclei to the spinal pathway is fed back to the appropriate cortical centers which presumably "know" about the dynamics of the neural-muscle system involved such that the output signal can be interpreted in terms of the movement that will result when that signal reaches the motoneurons. Notice a "mismatch" could create corrective signals prior to the receipt of actual proprioceptive kinesthetic information from the limb or body part.

As an example of such a model, Robinson (1975) rejects the retinotopic (exclusively proprioceptive) oculomotor tracking model and introduces one where all tracked object movements are made relative to the location in spatial coordinates of the ***body*** (not retinal) orientation. The assumption is that efference copy corrects the retinal error and recreates a ***central*** (Robinson's term) percept of the relative spatial position of the target. The central percept is sensitive to information from other modalities (say audition), knowledge of position of self (perhaps involving the vestibular system), and situational knowledge not necessarily related to retinotopic stimulation that can be tacit or explicit. This clearly involves cortex and very high cognitive functioning, as well as minimizing direct reflexive influences of proprioceptive signals.

Notice that what is being discussed in all three types of models is what centrally is happening that influences the signals in the descending pathways. That is, in the case of the limb control systems, where the "go" signal comes from,how the γ-motoneuron comes to be centrally stimulated, how intentional movements occur. The appeal to internal representation always arises, but being methodologically intractable, is always trivialized ("go"), parameterized as a predictive filter or a time delay, or simply ignored altogether.

An appeal to symbolic processes is tantamount to more central or cognitive processes. Here a dilemma begins to appear. The appeal to higher processes obscures the unifying virtues of the systems analysis approach.

A psychophysical function, being a function that takes values with physical parameters and returns values with perceptual parameters (e.g., Fechner's law $\frac{\Delta I}{I} = K$ relates changes in luminance to a percept of constant brightness), gains reductionistic power when the perceptual parameters are transformed by yet another function, let us call it substrate functionality, that corresponds to the underlying physiology and relates the physiological process to the psychophysical function (e.g., visual receptor organs for which the spike frequency increases as the luminance ratio $\frac{\Delta I}{I}$). The system responses are thereby reflexively tied to system stimulus inputs. That such a situation is amenable to systems analysis is obvious. I would only make the point that the kind of reduction described is the objective (in principle, at least) of biosystems analysis generally; indeed it is, in the strongest sense, a statement of the problem of the locus of control. Thus, as biosystems analysis is applied to the nervous system, the basic idea is that the transfer function is a psychophysical function. It is from that fact that the reductionistic appeal of neural systems modeling derives.

The dilemma is that as symbolic representation is admitted to the system along with the associated abilities of symbolic manipulation and interpretation, the reflexive linkage of system outputs to system inputs becomes more and more tenuous (see Pylyshyn 1984). For example, the *relations* between symbolic elements of representations of stimulus conditions may have no physical stimulus correlate and yet influence system response. Responses from representational systems depend as much on internal relevancies as they do on stimulus inputs: contextual relations include those of the symbol system. As the input-output structure blurs, so too does the explanatory power of the psychophysical function and, thus, the strength of the systems approach. Abiding the admonition of Morgan (1894), never to explain a psychological fact by a higher mechanism if it can be explained by a lower mechanism, will put the problem off as long as possible but it will probably not eliminate its eventual arrival since students of perception have recently begun to make a strong case for the theoretical need for representational, cognitive-like aspects of perception (Hochberg 1968, 1981, Rock 1983). In the case of motor systems, the preeminence of the proprioceptive information in motor control fades in the face of symbolic representation. Thus a straightforward systems analysis of the motor systems becomes a more remote possibility.

2.2 Central theories of motor control

The dilemma is rather clearly exposed in experimental psychology. Psychologists have been concerned with motor skill acquisition (learning), motor memory, and the effect of task demands on motor performance and reaction time. The "black box" nature of the data available to behavioral researchers and the nature of the theoretical analysis undertaken in systems science appear remarkably compatible. Thus, the area of experimental psychology devoted to motor control has been particularly influenced by the cybernetic perspective.

Sherrington (1906) proposed that motor memory consisted of persistent neural states induced by proprioceptive signals. These so-called "proprioceptive traces" have been the basis of accounts of motor skill acquisition (learning) and motor memory by peripheral theorists (esp. see Adams 1976). These theories have been challenged in the psychological literature by the so-called "central theories" of motor control (e.g., see Schmidt 1976). I will focus on two issues (there are many others) that delimit the generality of peripheral theories of motor movements that require only proprioceptive signals for the feedback control systems descriptions as well as for adaptation and learning. The first issue is the ability to acquire motor skill without kinesthetic proprioceptive feedback and the second is the control of very rapid movements. A third point will be raised regarding uniqueness of neural locus that will be more thoroughly addressed in the next chapter.

Occasionally information of the seemingly most mundane sort bears on a problem in an illuminating way. Lashley (1951) reported that subjects told to merely imagine performing a tracking task subsequently performed the task more accurately than subjects who did not previously imagine performing the task. So, by all the usual performance measures of degrees of learning, the imagining subjects ***learned*** some degree of the motor task without any proprioceptive signals and therefore with no possibility of a proprioceptive trace. Taub (1968) surveyed a program of research in which a deafferented monkey was placed in an apparatus with a rubber bulb taped to the deafferented hand. A shock was delivered to the afferented cheek after a tone was presented. The monkey could avoid the shock by pressing the bulb (which he could not feel) after the tone, or halt a shock by pressing the bulb after the shock began. With practice the animals learned to avoid being shocked. Clearly, no proprioceptive feedback from the hand was involved in the acquisition of that motor skill. To reiterate, skilled movements were learned and repetitively executed (remembered) without a proprioceptive trace.

Lashley (1917) demonstrated that a totally deafferented (by a war wound) human leg could be positioned accurately. These observations and data strongly indicate that central, apparently feedforward, systems are responsible for at least some control of movements.

The most obvious place to look for counter-indicators of proprioceptive feedback is where movements are executed so rapidly that kinesthetic feedback signals could not possibly account for control. Schmidt (1982) reports that Muhammad Ali's left jab was executed in about 40 msec. According to Schmidt's estimates, peripheral information required to detect errors and correct the movement on the average would require 150-200 msec to transit the loop. This figure is in keeping with the eye position error delay of Dallos and Jones (1963) and St-Cyr and Fender (1969) of about 210 msec. Therefore, it is hard to imagine error correction in these rapid movements arising from proprioceptive information.

There is a classical anecdote of a pianist performing a passage with finger movements occurring approximately every $\frac{1}{16}$ sec. When a key was suddenly locked in position, the fingering pattern continued uninterrupted. Thus, proprioceptive feedback from the finger did not affect the subsequent pattern. The proprioceptive trace theories would have motor memories as ordered effects of proprioceptive feedback and cannot account for the lack of disruption (Lashley 1951).

Calvin (1983) points out that to throw a stone at a rabbit-sized object at four meters requires that the hand release its grip on the stone in a time window of approximately five msec at the top of the throwing motion. For a target at eight meters, the hand must coordinate release in a time window eight times smaller, .7 msec. The throwing motion extends over a few hundred msec and, in that context, one sees the extreme precision required of the system sequencing motor movements. There is simply no nonpredictive way for feedback systems as slow as the neural variety to achieve adequate control over a .7 msec movement timing window.

Some direct evidence exists for preplanned feedforward sequences that are uncontrolled once triggered. Henry and Harrison (1961) had a subject push a button, then pull a string as fast as possible. They then gave the subject a stop signal at different time intervals after the start signal (see Fig. 5). Where the stop signal occurred at or after 190 msec, 24 msec before movement began, the movement would be executed without any modification. Clearly, this appears to be an example of a preplanned motor sequence that is unaffected by proprioceptive feedback once the plan is set. It should be emphasized that the motor program would have to locate the hand in space, time the gripping motion, etc. The task was utterly non-trivial and yet an apparently feedforward

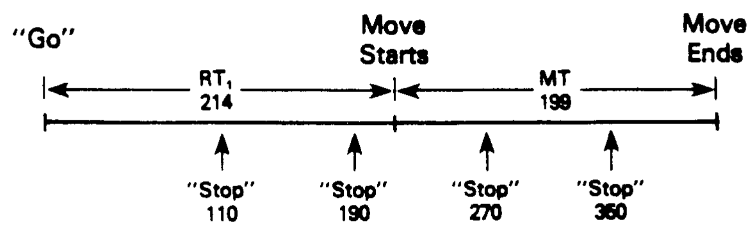

"Go"
Move Starts
Move Ends
RT₁ 214
MT 199
"Stop" 110
"Stop" 190
"Stop" 270
"Stop" 360

Fig. 5

system controls the movement. The experiment does not conclusively demonstrate that no feedback of any kind was used in the task. Central correction of fine movement with efference copy may be a possibility.

Fender (personal communication) and Becker and Jürgens (1975) report midflight correction of saccadic eye movements. Hou (1978) presents a feedforward model of saccadic movement programming from which it would be expected that data more like those of Henry and Harrison cited above would be observed. The evidence of Becker and Jürgens indicates that a saccade can be stopped in flight by new information but prolonging the saccade may occur only if information arrives before the onset of the saccade. Robinson (1975) offers that the saccade programming sequence is made with respect to a central representation of body and target position. Error signals are generated by efference copy mismatch to that central representation. Thus, the generated saccade program can be altered when the output consequences are in conflict with the subject's goal of tracking the target.

It is important to notice that the intentional goal of the subject to track the target is an essential component of the central representation with respect to which the error signals are postulated. The intentions, goals and strategies for feedforward tracking movements derive from diverse information and abilities available to a person involving a kinematical model of the target, a dynamical model of himself and the ability to incorporate knowledge derived from *results* rather than from the movement itself (e.g., the throw was too low and to the right) to correct those models. The big point is that the character of the so-called central theories are inherently representational.

The evidence presented here, being tied to the issue of speed, is often used to argue for highly localized, special purpose neural systems of a most uncognitive sort (e.g., Calvin 1983). Indeed, specialization and "hard wiring" are the most obvious way to get a fast subsystem, and that is usually implied to mean lower than cortical and not cognitive. It is worth mentioning, however, that, in the first place, these subsystems are known to be fast executing but that in itself says nothing in general about their ontogeny and, in the second place, the fact that they are representational says nothing in itself about their ontogeny either -- except for one important point. This is the second big point and is that these systems are post transducer symbolic systems. There is no inherent contradiction in that statement and in the specificity implied by speediness. In fact, what is indicated is the need for a clearer taxonomy of processes that are cognition-like. The tacit cognitive-is-cortical-is-conscious predispositions of nearly everyone involved in neural function-structure analysis are misleading (e.g., cognitive = slow) and should be replaced with a meaningful taxonomy of mental functioning. Luckily, an excellent candidate has been recently proposed by Fodor (1983) and it will be the subject of the next chapter.

There is the need to expand the system models of motor control to involve representational levels as is witnessed by above review. However, the implication of the need in rapid movement control does not imply that systems to control rapid movements are the extent of the representational domain or that feedforward systems are the extent of the systems involved. Midflight saccade modification argues against the latter; the former, in addition to being not logically implied, will be contradicted by the experiment of Chapter 7.

If representational systems and states are allowed, the fundamental problem is how evidence interacts with representations and how a representation state evolves to a less ignorant condition. This is certain to be a middle ground position between feedback systems and motor program perspectives on motor control.

2.3 Summary of evidence for peripheral and central control of movement

The peripheral theories have been seriously challenged by rapid movement control problems, and the whole notion of the proprioceptive trace as the exclusive medium of motor information has been questioned. Goals and knowledge of results, surely obtained from knowledge structures outside the motor system, do influence motor performance (Schmidt 1982). However, the simplest types of the associationistic view of complete equivalence of forms of learning and memory do not hold up experimentally (Garcia 1981) and therefore the tempting assertion of that type of equivalence (Seligman 1970) to account for the diverse influences on motor control by expanding the generality of the actor proprioceptive cues is discredited.

Central theories, on the other hand, have tended to commit to notions of motor programs (Henry and Rogers 1960) that don't encompass feedback processes that surely do exist centrally (Robinson 1975) and require question-begging parameters of the form of kinetic features of the movement (Kelso *et al* 1981, Bahill and McDonald 1983). It is simply not clear how central motor program accounts are disposed to alone provide general insight into the underlying parameterization that gives rise to the kinetic observables. Moreover, it is unclear that rapid movements should occupy a solitary position in the domain of centrally mediated motor behavior but they have received nearly all of the attention of the central theorists.

What is clear is that involvement of representational processes is essential for analysis of motor control. However, "central," as used in this literature and in this chapter is vague and not very useful. A meaningful taxonomy of mental processes is required.

3. Central Processes

This chapter will present a useful taxonomy of representational mental function.

3.1 Central Processes as a functional distinction

"Central" processes have generally been assigned vaguely to ontogenetically higher brain centers in the motor control literature (e.g., Dallos and Jones 1963). That is, researchers who invoke central processes in their theories have done so out of the desire to involve various extramotor aspects of movement such as memory, relative position or contextual constraints (e.g., Robinson 1975) and have thus often involved cognitive-like systems in their accounts. "Central" has however been variously used and often refers to midbrain or cerebellum and is thus structurally extremely imprecise. No precision in presumed functional properties of central processes is thus possible. They are a grab bag of hypothetical and often structure-defying properties. The saccadic system discussed previously is largely a midbrain and cerebellar system and was chosen as an example in part because of that fact. To assign "more centrality" (implying levels of function that are cognitive or closer to consciousness) to a higher (that is, newer ontogenetically) structure is appealing but insufficient criterion. In one direction, we see the saccadic system as a counterexample (being structurally at a relatively low level, yet involved in the high level task of prediction) and in the other, the function of striate cortex (ontogenetically a newer structure) appears to be a rather low-level retinotopic mapping far removed from conscious visual experience. Thus, one should not accept a purely structural hierarchy of centrality based on brain anatomy, but should take "central" as a functional process distinction. We would certainly expect some anatomical correspondence, at least in particular cases, to functional criteria; however, these are properly empirical findings and not theoretical axioms. Functional criteria inherently have the problem of multiplicities of equivalent implementations that are structurally diverse.

I, of course, am not implying that brain physiology does not underlie central processes. I am saying that I expect that compact, unique and localizable tissue competence may be far from relevant in analysis of central processes, where the term "central" will basically come to mean a process by which diverse cognitive activity is meaningfully related.

I take cognition to be fundamentally computational (in the sense that cognitive systems manipulate represented symbols) and that mental processes are cognitive where they are computational. At this point in the discussion, computation can be defined as transformations of representations. These transformations are taken to involve semantical constructs such as implication, logical consequence and confirmation. By definition, these constructs apply to structures to which propositional content, semantic evaluation, can be ascribed. Thus I take mental representation to be propositional in character and I largely subscribe to the "propositional attitude" view of cognitive state (see Fodor 1981). At this point I offer no argument other than the lack of obvious alternatives. Representation will be discussed at some length in the next chapter. Now, computation -- *any* particular computation, can be executed with a potentially infinite number of functional architectures. So long as any functional architecture is formally equivalent to a virtual-machine capable of effecting the computation in question it, too, can perform the computation. Thus, the previously mentioned issue of functional equivalence is an issue for anatomical localization of cognitive function.

Cognitive issues have arisen and will remain at the front of discussion for some time before we rendezvous with motor control once again.

3.2 Fodor's functional hierarchy of mental processes

A taxonomy of mental function can be outlined in three levels: transducers, input systems and central processes. Fodor's (1983) exposition is encompassing and elegant. This section presents the concepts and only the minimal motivation. The basic idea is that, for cognition, explanatory principles derive from three levels: biological, functional, and intentional.

Transduction

The operation of transducer mechanisms is the most primitive and reflexive aspect of mental functioning. That is not to say that they are not order-imposing, i.e., grammatical, but that they constitute the reflexive, domain constrained contact between the environment and cognitive mechanism. These systems are fundamentally physical processes and their behavior is adequately explained (in the sense that all relevant regularities are established) in purely physical, chemical, and biological terms. A transducer is defined by Pylyshyn (1984) to be ***part*** of the functional architecture of the brain. Transducers are reflexively bound to physical stimuli; thus, transducer system description must require physical quantities for inputs. The values of physical parameters of transducer output are completely defined by the physical properties of the input stimulation.

These statements are obvious to nearly everyone, but the reason for being explicit is that the cognitive importance of transducers is a boundary I want to strictly delimit. If one supposes sufficiently rich functional properties to exist in transducer reflexes, then the whole of perception very nearly gets pushed down to the receptor level. Gibson (1950, 1966, 1979) argues the structure of cognition-like processes is largely the structure of the stimulus environment. Then cognitive processes are largely simplified by assigning complexity to the transducer -- they detect the relevant relations; cognition need not construct them. A quote from Pylyshyn (1984) reflects my attitude toward this programme:

> "Unless what counts as transduction is constrained in a principled manner, the simplification of the problems of perception gained by postulating certain transducers has, as Bertrand Russell is reputed to have said once, all the virtues of theft over honest toil."

What must be admitted is that the obligatory output of transducer systems transforms stimulus energy into a form relevant for processing. Thus, the structure of the obligatory response of the transducer must correspond to stimulus relevancies in some sense. Any mechanism exhibiting behavior that covaries with environmental changes can be thought of as producing output that represents the environmental changes. That is what requires transducers to be considered in a taxonomy of mental processes. The nature of transducer outputs is taken to be lawfully determined by energy impinging at the receptor surface of the transducer system and that is what constrains transducers to the lowest level in the taxonomy. They exhibit mental functioning in the sense of functional architecture, opaquely transforming the environment into a symbolic code. But their function is data-driven and nonsymbolic in the sense that symbol manipulation is not an aspect of transducer function.

Input Systems

If regularities of a behavior can be subsumed under descriptions referring only to physical or biological principles, then those descriptions should be accepted as explanatory. If, however, it is the behavior of a computer one refers to, then purely physical principles will not suffice. Certain regularities cannot be accounted for at a purely physical level. The example is simple and obvious. Equivalent virtual-machines have precisely equal computational domains. The physical descriptions of the various functional architectures on which a particular virtual-machine is implemented are not by necessity equivalent. Similarly, functionally identical programs need not execute such that the physical descriptions of the computations are identical. The natural regularities exist at a symbolic level; they are functional in character. Thus, in addition to explanatory principles of a physical kind, there are necessary explanatory principles of a functional kind. It is at this level that input systems reside.

If we assert that functional equivalence is an essential mental explanatory principle, we advert to symbol manipulating systems, that is, fundamentally computational processes. Computational processes are inherently syntactical; a system that delivers information to a computation is responsible for its appropriateness and format as well as its accuracy (a.k.a. "garbage in, garbage out"). Taken the other way around, the domain of an input system must be restricted to relevant transducer system outputs. This restriction is opaque to the input system. That is, these systems are limited away from intentional levels. Input systems are special purpose (domain-limited) inference engines taking (the premise) proximal stimulus codes as input and returning (the conclusion) solutions of distal arrangements upon which central processes operate.

The primary distinguishing feature of input systems is domain specificity. Fodor calls the input systems "informationally encapsulated." An example refers to the eye movement control system and the efference copy hypothesis in tracking. If you push on your eyeball with your finger, you *see* apparent movement. There was no corollary discharge from the (hypothetical) efferent oculo-motor pathway to the (also hypothetical) input system(s) involved and the apparent motion is testimony that representational correction was not achieved. Since other information was certainly available about your finger pushing on your eyeball, but did not serve to influence the apparent motion, one must conclude that that information was not encoded in a form lying within the domain of the input processor(s) responsible for presenting proximal oculo-motor information to thought.

On the other hand, presumably on the basis of the other information present and the other associated input processors, you don't *believe* the motion exists in the fixated object. The fixation of belief is a process to be attributed at intentional, central levels and involves many input systems.

Before going on to intentional, that is, central, processes two important asides are necessary. In addition to sensory perception, there is another way that humans present information about the world to thought. Language is a vehicle clearly capable of presenting information to intentional and belief ascribing processes. Thus, Fodor takes input systems to encompass the perceptual systems plus language. that will have interesting consequences later in this thesis.

The second aside is that the special purpose, informationally encapsulated features of input systems serve to diminish the possibility of empirical chaos that unconstrained functional equivalence obtains. Perceptual systems should (teleologically) be *fast* and speed is best achieved with "hard-wired," compact, special purpose devices. It is plausible then that many cognitive functions will correspondingly be available for rather standard structural analysis. On the darker side of things, some aspects of survivability and adaptability demand the converse: distributed and plastic neural instantiations. Thus tradeoffs and redundancy are to be expected for these systems.

Central Processes

In the above example of apparent motion, it was clear that the input system was not capable of altering the percept to match the belief. That indicates that the state of the input system is not influenced by, and cannot ascribe, belief. Belief is an aspect of the meaning of things, and the apparent detachment of input processes from semantic evaluation motivates the last functional distinction to be drawn in this taxonomy.

Teleological intuition can be provided. The easiest way is to observe that unexpected stimuli can be presented to central processes only if the input processor is insensitive to belief. There is obvious survival value associated with presenting surprises to thought. If we do ascribe belief and some systems are patently unable to supply that function, then some other systems must exist to supply it.

The belief that pushing your eye and not that movement in the world caused the visual image to jump around must derive from a variety of sources of information, that is, from many input systems. Taken together this suggests that the criteria for the third level are intentionality and the *lack* of informational encapsulation. Encapsulation is likely a more-or-less state of affairs with some systems "more central" than others, but the neurological implication is clear. Absence of a clear locus of control is likely since central systems will be profligate in their input requirements from diverse input systems. The sufficient determiners of a central process are discussed in the next section.

3.2.1 Isotropy and Quineian conditions

Confirmation in science derives from empirical inference. Experimental evidence is, by nature, nondemonstrative and closely related to observational evidence generally. The explicit methods of scientific inference are taken by Fodor as an analogy of the largely unknown psychological inferential processes.

The fixation of beliefs in science is isotropic and quineian. Isotropy means that the facts relevant to the confirmation of an hypothesis might be drawn from *any* previously established empirical or demonstrative result. The corpus of scientific knowledge, in principle, bears on every hypothesis. Another way of saying that is that the epistemic frame exerts global influences on particular confirmatory processes.

The quineian condition is closely related to isotropy and states that the degree of confirmation afforded a hypothesis by evidence is sensitive to the properties of the entire belief system.

The belief ascribing properties of mental function are attributed to central processes and these processes are, as a matter of definition, described as isotropic and quineian in analogy to scientific inference. An extreme example of a central process is analogical reasoning, the method by which these very definitions are arrived at. Another example will be argued to be related to aspects of the sense of temporal durations in Chapters 6 and 7.

3.3 The difficulty of structural assignment of function

The problem of neural locality in reference to psychological functioning is very important to structural analysis. To the degree that functional equivalence is not constrained by the before mentioned selective forces related to speed and special function, there is at least the likelihood of great inter-person variability. The very nature of the categories for higher cognitive processes makes the possibility of usual structural analysis an issue of special cases and cautious conclusions. Caution is necessary because, not only may the operational structure underlying a cognitive function be idiosyncratic between individuals, it may be nonunique within individuals; it may not be spatially compact, it may be temporally dynamic, etc. These processes are defined functionally and the consequences are not trivial. The following examples are probably straws in the wind.

The complete cerebellectomy of rabbit eliminates saccades and leaves smooth tracking intact. In macaque it does the reverse (Westheimer 1975). There is no substantial morphological difference in the cerebellum between these species and rather little functional difference in tracking and saccadic behaviors. I present this evidence only to illustrate the degree to which functional assignment to structure is, in a certain sense, arbitrary.

To illustrate the degree to which neural representation is sometimes diffuse, Lashley (1951) found that rats trained to run a maze would continue to run the maze, albeit with substantially different detailed movements and performance, with cerebral lesions until they were basically totally decerebrate.

In certain cases operative brain mechanisms likely change with practice. For rapid detailed movements evidence suggests that the novice makes extensive use of proprioceptive feedback while after extensive practice the expert does not seem to (Schmidt 1982).

But more directly to the point, consider the isotropy and quineian conditions that define central processes. To the extent that these conditions are met, the psychological function under consideration is not informationally encapsulated and is globally defined. Only something like the very questionable second doctrine of Barlow (1972) might afford structural analysis great hope for central processes. That doctrine states that the nervous system is arranged such that functions of ever greater generality are assigned to smaller and smaller populations of neurons. Uttal (1981) points out that, in fact, most neurological evidence indicates ever increasing diffusion and interconnection with distance from receptor systems in contradiction to this doctrine. In the next chapter we will see Barlow's doctrine to be an example of a flawed philosophical position concerning representation called type physicalism.

Mental representation has been discussed intuitively to this point. However, clarification is required because it is the structure of that representation that must naturally obtain mental function. The thesis to this point has attempted to establish that the nature of the structure of mental representation is (1) unclear and (2) of utilitarian interest in the analysis control of movement. The next chapter addresses mental representation.

4. Representation and Behavior

This chapter will survey some of the major views concerning mental representation. It will be argued that the form of mental representation is propositional and that the structure is modeled by a notion of formal language given by Randall (1970). Propositional knowledge in combination with evidence gives rise to the idea of epistemic probability as a model of belief and a measure of confirmation. The rationale is given in an overview of Shafer (1976). The structure induced by a formal language on its domain of discourse is in part characterized by a measure that can be viewed as an epistemic probability. A survey of Randall (1970) shows that the relevant structural properties of the formal language model of mental representation are constructive, insuring (in principle, at least) the physical instantiability of this model of mental representation. The structure is isotropic and the belief measure is quineian. Epistemic probability is argued to be a link between mental structure and intentional behavior. In subsequent chapters an informational view of confirmation processes will be cast in terms of these probabilities and behavioral predictions will be derived. It will be helpful at the outset to make an abbreviated statement of the goal that an account of mental representation reaches toward in this thesis. I want an etiology of behavior (e.g., motor behavior) that references representational structure.

4.1 Representation: Dualism, reductionism and functionalism in theories of behavior

The cybernetic approach to neural systems is traditionally bound to a proximal structural analysis that finds a loss of explanatory power where a unique function-brain locus isomorphism is not necessary. I have argued that such a situation occurs where the system function under consideration is central in the sense of Fodor's taxonomy. At the foundation of central processes is mental representation. The fundamental psychological problem is to formalize the relationship between mental state and overt acts. They must be reduced to a common currency to effect such a formalization. Two reductionist approaches have been taken. One seeks to establish the equivalence of neural and mental states, thereby in principle eliminating the problem of unlike form of mind and behavior. Another reduction proposed the notion of so-called behavioral hypotheticals' being the object of proper theories of behavior. The former is the central state identity theory and the latter is logical behaviorism. Difficulties arise when attempts to establish stimulus (or for that matter, response) parameters do not involve the notion of representation, a notion that stubbornly resists adequate physical description, because representation is primarily a functional notion independent of the particulars of physical implementation. Functionalism and propositional attitudes have recently received a great deal of attention due to their explicit reference to symbolic representation and materialism.

I have nothing to provide with an account of dualism except the motivation for reductionism, but it should be remarked that dualism in the voice of Eccles (e.g., Eccles and Popper 1977) is still around in neurobiology. I provide only four unfairly encompassing sentences on dualism. Classical considerations lead to the position of Descartes that minds and bodies are distinct in kind and that there is a principled and inviolable distinction to be made between such things as intentions and acts. The problem is how a mental state might causally influence an act, a condition even dualists are usually willing to acknowledge exists. For that matter, it is no easier to see how a physical stimulus might influence the mental attestation of its presence. No adequate resolution has been provided under the dualist premise. I leave it at that.

Logical behaviorism desires that the paradigm for scientific psychology provide the general principles of behavioral hypotheticals. These are to be functional statements with variables of the physical parameters of the stimulus returning variables with the physical parameters of the response. Behaviorists explicitly disallow internal state description beyond the (usually probabalistic) stimulus to response mapping. The story goes that these probabilities derive directly from the stimulus contingencies of the environment. They will therefore be utterly physical theories. It may be obvious that this rather vigorous response to dualism will run into trouble, but it was not at all obvious that the trouble was insurmountable and, in any case, the problems were empirical in nature, thus involving the clear need for scientific inquiry in the philosophy of mind for the first time and additionally legitimizing notions such as learning and memory as objects of experimental examination.

A slightly more detailed account of the logical behaviorist position is as follows. Any truth evaluation one might seek to make concerning a mental state is taken to be semantically equivalent to some behavioral disposition of the organism. The behavioral disposition is a state defined to exist if and only if a set of behavioral hypotheticals is satisfied.

The analysis of the disposition consists of satisfying the behavioral hypotheticals, thereby obtaining elicited behaviors. A behavioral hypothetical is a function taking variables of stimulus parameters and returning variables of response parameters. The aspect of logical behaviorism that attracted so much effort is that it seems to allow the equation of mental and dispositional properties in such a way that the tenets of materialistic monism are satisfied. That is, behavioral dispositions *are* mental causation and, furthermore, a behavioral disposition is physically defined.

Fodor (1981) makes the following excellent example. Suppose that "John took aspirin because he had a headache" is true if the following conjunction holds:

> John was disposed to produce headache behaviors and being disposed to produce headache behaviors involves satisfying the hypothetical:
>
> if there are aspirin then take them
>
> and the following stimulus conditions exist:
>
> there are some aspirin.

This constitutes the behaviorist account of the situation denoted by the sentence, "John took aspirin because he had a headache."

Of course, we must offer the objection that this situation requires an interpretation of the sentence "John was disposed to produce headache behaviors because he had a headache". That leads to "John was disposed to take aspirin because he had a headache." The dispositional approach fails in principle when one considers that in this simple case logical behaviorists could not distinguish the following sentences:

1. "John took aspirin because he had a headache."
2. "John was disposed to take aspirin because John was disposed to take aspirin."

Because the doctrine cannot allow reference to internal states, having a headache must be expressed as some dispositional state such as taking an aspirin or uttering statements about having a headache under suitable stimulus conditions such as being in the presence of aspirin or in sympathetic company. Hence the disposition to produce headache behaviors (such as injesting aspirin) is the logical behaviorist's headache. We obviously would hope to make reference to the (empirical) fact of John's headache directly and find discomfort in not having the chance. But, beyond discomfort, the above sentence equality is just wrong. The second sentence is vacuous while the first is not. *Taking* aspirin and *having* a headache are not semantically equivalent. Indeed, disposition and act are not equivalent in principle

Since disposition cannot be compared to a belief state by the behaviorist (because he can't speak of mental state at all) the putative disposition to behave is difficult to account for. If John doesn't *believe* that taking aspirin will end the headache or that having a headache requires a behavior of any sort he won't do anything; that is, John seems to need a mental state description of some kind to get him into action. Having a head requires no mental state; having an ache in it does. I have surely done some disservice through brevity or omission (e.g., Operationalism, Wittgenstein) but, on the whole, further examination does nothing to rescue the doctrine.

It is these kinds of problems that make physicalism attractive. Physical states in the form of (brain) physiology are taken to *be* mental states. Behavioral causality is easy to accommodate and we can, at least, see a chance that John's headache can be acknowledged by a physiological marker to that effect. Simplistically, that "chance" is the Central State Identity Theory. The theory does not require that a particular neurophysiological state is synonomous with John's headache, but that conditions for the brain state to exist are the same conditions for which John has a headache. The contrapositive case is required to hold also. Physicalism is the basis of the modern neuroscience movement (Barlow 1972) and, in one form or another, the basis of the cybernetic approach to neurological systems. To be explicit, we need to theoretically refer to mental causes which obtain overt effects through interactions with their kind. Behaviorism simply offers no untendentious account of mental state interaction. (The "mediated responses" of the behavioral attempt to incorporate psycholinguistics is remarkable only in its tendentiousness; see

Fodor (1964)). The assumption that mental particulars are identical to physical particulars in the form of neural particulars is the obvious first theoretical position to entertain and that is the Central State Identity Theory.

An account of physicalism has two alternative branches, type or token, which refers roughly to the generality allowed the physiologically instantiated mental state. I will give details, but this is the strategy adopted: Type physicalism will be argued to be implausible. Token physicalism admits plausibility, but is rather impoverished and profligate in its use of neural resources. In consequence, functionalism (a reduction of mental states to equivalent turing machines which ensures physical realizability without dictating the realization beyond functional description) will be proposed. Then, however, if functionalism *and* token physicalism are true (to lend a plausible neural instantiation to the assertion of physical realizability) the programme reverts to type physicalism; recall, such a situation is implausible. We thus leave the account of reductionism in a sad condition and awaiting rescue.

Token physicalism interprets the central state identity theory as a doctrine about mental particulars. It holds that some particular neurological state distinguishes that particular pin prick, that particular cat, but not the universals pain or loving all animals.

Type physicalism is an interpretation of the central state identity theory that takes it as a doctrine about mental properties as opposed to mental particulars. Type physicalism makes arguments that, for example, beauty or pain, *is* a *brain* state and furthermore, beauty or pain *cannot* be a state of any other device. Type physicalism therefore makes the claim that a *certain* neural construction is the domain of all mental properties.

Type physicalism thus precludes generalization of representational properties at a level of abstraction that would permit, for example, a computer and a human to entertain a proposition, P. P's that humans entertain are taken to be proprietary to human brains. It should be obvious that the the restriction to species is purely *ad hoc* and, with no more or less justification, might be applied to individuals. Thus, my pain does not generalize in any acceptable way to your pain. These restrictions are severe to the point of denying any characterization of knowledge at a level of analysis appropriate to establish general principles in the domain of processes of representation. But this refuses to admit to science any attempt to abstract across the differences in implementation of representational processes or to refer to what is represented.

The consequence of type physicalism is thus the rejection of representation as a natural kind. But surely that is implausible; for even where the most elementary account of behavior is considered, it is the projectable physical properties of the stimulus that influence the response. Projectable properties are physical relations expressed by a projectable predicate. These relationships can influence my behavior, your behavior and an appropriately programmed robot in similar ways without essential reference to the particular substrate encoding the predicate. Such evidence points to the propriety of a relational analysis of mental properties.

Type physicalism is incompatible with the level of abstraction required of a relational approach. One reading of token physicalism is compatible with a relational approach and, in virtue of that and its ability to causally involve behavior, admits plausibility. Another reading is not compatible with a relational approach, but can be rejected on independent grounds. I will do that first.

One construal of token physicalism is that for a brain to see that cat or feel that pain, that a *particular* cell, or small group of cells, has to be active; *a for-*

tiori, that *particular* cell has to be present in the brain. There are many reasons to reject that view, but the easiest (by virtue of having a short argument) is to consider the sheer number of things we can be cognizant of and question how so much dedicated special purpose hardware might fit inside our head. Furthermore, this account would probably have it that selection pressures dictate the hardware present. But I am cognizant of angels, unicorns, ghosts and electrons. What selective pressures could possibly have given us those pieces of hardware? The non-relational reading is implausible.

The reading that admits plausibility is that to see this cat or feel that pain is to be in an individuated neurological state. This interpretation of token physicalism does not, perforce, deny a relational construal of representation; however, it is simply silent on the issue. What one means here is important to investigate, because lack of caution loses the intended distinction drawn to type physicalism and thereby cancels out the plausibility.

To be explicit, what is needed is a way to identify mental properties in such a way that they are abstracted from their physiology. That is what a relational theory of representation means. On the other hand, what is also needed is behavioral causality and that is what physicalism provides.

Functionalism steps into this breech. What functionalism is, is a theory of representation grounded in *process*. Having a belief or a percept is a constructed relationship in this view. The processes of relationship formation is the dominant focus of the approach and the objects of relations, the *relata*, are physical tokens of symbols. Obviously this allows, in principle at least, token physicalism. In this view, a canonical form of representation, abstracted from the symbol tokens, is a canonical form of symbol manipulation.

Turing Machines, a normal form for symbol manipulation by Church's Thesis, are thus taken to be the canonical form of representational processes. What is gained is, on the surface, very powerful. Generally speaking, functional specification can be question-begging and occasionally false. Universal truth determiners might be postulated to exist in the brain to account for the ability of humans to invent mathematical proofs. That would be both false and question-begging. But the Turing Machine normal form

a. does not allow such specifications in principle,

b. assures physical realizability without specifying substrate, thus allowing the appropriate level of abstraction for representational theory

c. allows for causal interaction of symbols through their tokens and, similarly, allows for behavioral causality through the substrate properties.

So functionalism provides that mental representation is relevant, relational and causal. The first and second keep what is of value from behaviorism (a relational emphasis) and the third keeps what is useful from physicalism (causality). The hair having been thus split, everything seems okay. But things are not okay for two general reasons which lead directly to the position I will develop in the remainder of this thesis.

A quote from Pylyshyn (1984, pg. xiii) illustrates the intuition underlying much contemporary functionalism,

> "One of the central proposals that I examine is the thesis that what makes it possible for humans . . . to *act* on the basis of representations is that they *instantiate* such representations *physically* as cognitive codes and that their behavior is a causal consequence of operations carried out on these codes." (italics mine)

On the basis of that intuition, careless generalization leads one astray. Functional individuation is specification based on causes and their constrained effects. Only relations between causes and effects play a functional role. Suppose that token physicalism is true. Then it is possible that all of the behavioral *causes* and *effects* of the symbol system are expressible in physical terms specific to the instantiation. What follows is that physical specification of mental particulars performs the functional individuation. But that is precisely type physicalism -- we have lost the distinction between type and token physicalism. Something has gone awry. This is the first problem for functionalism.

In the machine analogy, one cannot propose a mental function without proposing a mechanism to execute the function; that mechanism, by virtue of being in the canonical Turing Machine form, is ensured physical realizability. One cannot, however, in the canonical form make reference to a particular physical realization. Rather, one *must* express the inputs and outputs in the canonical form and therefore, at that level of abstraction, establish causality. It must be assumed that it is from that level that relevant notions of behavioral causality are obtained and not in reference to the physical particulars of the instantiation. As I discussed in the last chapter, there is a hierarchical taxonomy that needs to be observed if sense is to be made of processes of mental representation. And, as is beginning to be implied, where functional and structural analyses are injudiciously unseparated, conflict internal to the explanation emerges. Structural analysis supplies the "how" of causality and functional analysis provides "what." "Why" involves both analyses. As regards the machine metaphor, at the point that it occurs in argument, *structural* analysis commences because it constrains how the system can operate and thereby delimits what it can, in principle, do. The structure of Turing Machines supplies the causality to symbol manipulation in the sense that *any* instantiation will *impose* the input-output structure that the Turing Machine defines. This structure is relevant at the symbol, i.e., input system, level of functional analysis.

The fact that the Turing Machine metaphor insures sufficient conditions for physical instantiation is, in itself, insufficient to causally link "cognitive codes" to "behavior." That is the second problem for functionalism.

This problem turns on the issue of intentionality in behavior. Mental states defined relationally of mental representations are expressible as propositions. That is, a state is defined to exist when such and such relationships between appropriately occurring symbols hold. Mental states of this form are commonly called propositional attitudes. Propositional attitudes have several different levels of semantic properties. Firstly, "where a relation holds," implies a truth-evaluative function. Secondly, the consideration of belief, consisting of kinds of things that can be true or false, and hence a propositional attitude, gives rise to the idea of opacity. The *context* of a propositional attitude is opaque when the inferential principles of existential generalization and substitutivity of identicals fail. For example, because Sophia believes that Cookie Monster is blue and furry does not support the inference that there is a creature to which Sophia's belief applies. And that Louisa believes Daddy knows everything does not infer that Chris Barrett knows everything. These are semantical properties over which a theory of propositional attitudes must form generalizations. The problem of intentionality derives from the semanticity of a relational view of mental representation.

How this enters into behavior can be very simply motivated. I take it as obvious that beliefs influence overt behavior -- recall John's belief in the powers of aspirin. It is equally obvious, but not as easy to argue these days, that the Turing Machine metaphor cannot get this level of causality right. In the artificial intelligence community, procedural semantics is used to make precisely the claim that the machine metaphor adequately encompasses intentionality. I will not confront their arguments but will simply give arguments illustrating that the machine metaphor does not encompass intentionality. The argument can be made by explaining the following quotation, (Fodor 1981, pg. 207).

> ". . . machines typically don't know (or care) what the programs they run are about; all they know (or care about) is how to run their programs. This may sound cryptical or even mystical. It's not. It's merely banal."

Where the machine metaphor is taken as a theory of cognition, programs as instructions to execute machine operations, and the machine operations themselves, involve intentionality to the extent that their specification applies to psychological explanation. Reduction of the program execution to elementary operations does not address intention in any relevant way. The formulae that specify the operations have a causal effect, in the sense of arguments four paragraphs back, as a syntactical property. The operations are executed on symbols that must be delivered to the operation in a sensible form -- they must be, *a priori*, semantically interpreted objects. As we have said, the machine operates on the symbols purely syntactically. The specifications of the instructions lie outside the domain of the machine executing the instructions. Another quotation makes the point I seek, (Fodor 1981, pg. 23).

> "Whereas the theory of intelligence needs an account of *mechanisms*, the theory of intensionality needs an account of *symbols*. . . . There are *two*, quite different, applications of the "computer metaphor" in cognitive theory: two quite different ways of understanding what the computer metaphor *is*. One is the idea of Turing reducibility of intelligent processes; the other (and, in my view, far more important) is the idea of mental processes as formal operations on symbols . . . the objects of propositional attitudes are symbols and that accounts for their intentionality and semanticity."

So the critical aspect of causality left untouched by the machine metaphor of intelligence is "why," which cannot be substituted for by "how" as procedural semantics in artificial intelligence proposes to do. Furthermore, the creation of mental alternatives, the intentional symbolic act, is essential to any theory of mental representation including the Turing Machine metaphor.

There are a few summary points to be made: the causality of mental representations derives from their syntactical properties, i.e., their structure.

Mental representation has semantic and syntactical attributes and the semantic properties of propositional attitudes derive from the semantic properties of their constituent representations. A mental state can be expressed as a propositional attitude which has an inherent intentionality.

I hope to have developed this section so as to make apparent that the notion of representation follows the taxonomy of mental process of the preceding section and to point out that relevant structural analysis of function depends on observing the hierarchy that the taxonomy provides.

In the next two sections of this chapter, I will provide for the development of belief as a probability measure on a representational structure. The structure will be seen to be constructive and isotropic and the measure to be quineian. It will be used to assess intentional, i.e., central process, effects on behavior in later chapters.

4.2 Probable reasoning and belief, the form of epistemic frames: Shafer

I have adopted a construal of representational mental state as being propositional in form and of mental processes as functionally hierarchical. Propositional attitudes have intentional properties and exert causal influence on behavior in virtue of their structure. Belief is a species of propositional attitude and, pretheoretically, it would seem that it has causal influence on behavior. Thus, it would be interesting to formalize belief and expose its structure. It is the goal of this section to formalize the concept of belief and the goal of the next section to demonstrate its structure.

Evidence standing in relation to some proposition tends to support or refute that proposition to some degree, thus making it reasonable to attempt to ascribe a likelihood to the proposition. One might begin to formalize this by setting the total likelihood of all propositions under consideration to unity.

Suppose that it is the additive accumulation of these likelihoods that sums to the measure of total likelihood, i.e. sums to one. The likelihood of some subset is somehow to be a portion of the likelihood of every subset that contains it. In this section, that intuition gives rise to the notion of belief functions as epistemic probabilities. These probabilities are assigned as likelihoods to propositional statements about the world. It will be seen that beliefs influence one another on combination; that is, they interact causally. Overt actions predicated by a belief condition produce effects as evidence, which also interact with belief. Belief in the form of epistemic probability forms a connection between mental state and behavior.

The foundations of probability are two-sided. On the one hand, the axiomatization of the mathematical notion of probability as a measure constitutes a rigorous mathematical foundation. On the other hand, probability has a logical foundation. Convergence of concerns for probability and logic in this regard can be seen in a number of different settings. Jeffreys (1961) points out that, for a proposition A, the statement of the probability $Pr(A)$ is a generalization of the logical assertion, $\vdash A$. More precisely, rather than $\vdash A$, one adopts the conventions of using "1" for the assertion, and the use of "0" to declare it false; one writes $P(A \mid H) = 1$ and $P(\neg A \mid H) = 0$. Generalization to incomplete proofs of $\vdash A$ is seen where 1 and 0 are replaced by fractions. There are many other examples of the logical foundations of probability. For example, the idea of "possible worlds" arises in logic and probability, for the former in the notion of models and for the latter in the construction of possible outcomes.

Consider also that the usual hypothesis-testing methodology is a formalization of nondemonstrative inference and, among other things, rests on issues of type. It is justified to speak of the probability that a probability has a particular value only if a probability of type n describes probabilities of type $n-1$ or lower:

the class-individual type distinction is required. Another example also relates to hypothesis testing. If the probability of an (observed) probability is such that the null hypothesis cannot be rejected, one has the methodological obligation inherent to nondemonstrative inference of "staying within the data" and can only say that the alternative hypothesis is rejected and never that the null hypothesis is *proved.* That convention rests on the logical idea that the conclusion may not be detached from the premise, which presages opacity. If one accepts a proposition p and then q follows, *modus ponens* allows "therefore q." But to accept *modus ponens* requires the proposition, if p and $p \Rightarrow q$, then q may be detached and asserted separately. Call this principle, r. But q has not been detached; it remains part of the longer proposition r. So if we are to use r, we need the proposition if p, and $p \Rightarrow q$ and r, then q may be detached and asserted separately, *ad nauseam.* Thus, if the null hypothesis is $p \not\Rightarrow q$, p provided by the experiment, and the idea is to provide the causal evidence to assert $p \Rightarrow q$, where the evidence is too weak, we must not assert q; where the evidence is significant we merely *reject* $p \not\Rightarrow q$ and do not assert q separately. To do otherwise enters into regress as above. It should be noticed that experimental observations are indeed often taken to "prove" hypotheses rather than rendering the complementary propositions unlikely by practitioners of disciplines where statistical design of experiments has not been seen as an overriding necessity or is relegated to precision of measurement rather than the very different issue of significance tests of hypotheses. It is at the junction of concerns of probability and logic that the formalization of belief as a probability measure on propositions, along with inference and confirmation, resides.

This section reviews the idea of belief functions as given by Shafer (1976, 1979, 1982, see also 1981, 1983). This discussion is restricted to finite sets. The results have been generalized to infinite sets in Shafer (1979). At the outset I should remark that Shafer does not appeal to the use of formal languages as I will in Section 4.3. His objections are focused on two issues regarding the way in which evidence is used in the logicalist literature in probability. (See Savage 1954 or Jeffreys 1961) for various views in the foundations of probability). His first objection revolves around the fact that he claims either the systems are hallucinatory in that they do not depend critically on evidence or they axiomatically take objective relative frequencies as a measure of belief. His second objection refers to what he calls epistemic refinements -- roughly, the grain of the set of possibilities under consideration. The objection is that the ultimately fine refinement makes no sense, which he properly claims is equivalent to defining ***a particular*** formal language to characterize the set of all possibilities. We agree on the points that evidence is essential to characterize belief, that the aleatoric notion of chance should not be generally construed as a measure of belief and that an ultimate refinement makes no epistemic sense since such a language would not have access to a metalanguage within which to be defined. I do not agree that the exclusion of formal languages follows. Randall's (1970) use of formal languages considers families of languages of increasingly refined descriptive resolution that construct the sets of possibilities over which probability judgements are made. That is the topic of the next section and I therefore, in contrast to Shafer, do imagine underlying formal languages. Indeed, I cannot see how to properly characterize propositions without eventually employing semantics. The disagreement does not impede use of his particularly clear motivation of belief measure. Many issues dealt with at the intuitive level in this section actually require considerable care to be properly understood. Most importantly, the assignment of truth conditions requires much more effort than Shafer's account provides and the notion of observation likewise requires considerable clarification. The form of belief as a probability measure on propositional contents is what Shafer provides.

What does one mean by belief? Very roughly, one might construe the meaning as follows: Given what is understood to be the context in which evidence is gathered, to what extent does this evidence support that context? Belief, relative to that context, is so measured.

It is natural to think of the evidence standing in relation to some proposition. Roughly, evidence is admitted to judgements in the form of sets of evaluated propositions allowed by an existing epistemological state. Having a belief in a proposition constitutes assigning a portion of one's belief to that proposition as indicated by evidence. It is an important aside that in the usual Baysean theory a second condition is imposed: When a portion of belief is committed to a proposition, the remainder is committed to its negation. That condition is not imposed here for reasons concerning the representation of ignorance. Shafer uses Dempster's (1967) upper and lower probabilities as a model of belief.

It is necessary to relate subsets to propositions. Consider the case that one seeks to know the true value of some quantity, e.g. the true chance that a die will come up 5. If such a true value is denoted by θ and the set of possible values by Θ, then one is interested in the propositions of the form, "The true value of θ is in T," $T \subseteq \Theta$. The set of all propositions in the domain of interest is exactly all the subsets of Θ. Shafer denotes this by 2^{Θ}, a convention that will be followed.

Θ is called the ***frame of discernment***. When a proposition corresponds to a subset of Θ, the frame ***discerns*** that proposition. The epistemological character of Θ is emphasized (Shafer 1976, pg. 36)

> "It should not be thought that the 'possibilities' that comprise Θ will be determined and meaningful independent of our knowledge. Quite to the contrary: Θ will acquire its meaning from what we know or think

> [believe] we know; the distinctions that it embodies will be embedded within the matrix of our language and its associated conceptual structures will depend on those structures for whatever accuracy and meaningfulness they possess."

It is apparent that frames of discernment may be changed utterly, refined or coarsened, with changes in the subsets corresponding to the associated changes in allowable propositions.

Translation from logical notions such as conjunction, disjunction, implication and negation translate to the set theoretic constructs of intersection, union, inclusion and complementation in the obvious way. The set theoretic form allows use of elementary mathematical tools.

The frame of discernment is used to motivate a formalization of belief. A portion of belief held in relation to one proposition is committed to any implied proposition. Then belief granted some subset is also granted every subset that contains it. Of the entire belief afforded a subset $S \subset \Theta$, only some will be allocated $s \subset S$; all the rest are committed exactly to S and no smaller subset, which suggests the following definitions.

A function $m : 2^{\Theta} \rightarrow [0,1]$ is a *basic probability assignment* when

$$m(\phi)=0 \tag{1}$$

and

$$\sum_{A \subset \Theta} m(A) = 1$$

$m(A)$ is called the *basic probability number* of A.

This definition asserts simply that one's total belief has measure one and that null belief is granted measure 0. But the above consideration of allocation of belief in subsets indicates that $m(A)$ is to be understood as the measure of belief afforded exactly to subset A and no smaller subset. To obtain the ***total belief*** committed to A, not just that committed exactly to A, one must sum over all the measures of proper subsets of A:

A function $Bel:2^{\Theta} \to [0,1]$ is a ***belief function***

if it is given by

$$Bel\ (A) = \sum_{B \subset A} m(B) \tag{2}$$

where m is a basic probability assignment.

Theorem (Shafer 1976)

Where Θ is a frame of discernment, a function $Bel:2^{\Theta} \to [0,1]$ is a belief function if:

1. $Bel(\phi)=0$

2. $Bel(\Theta)=1$

3. for every natural number n and every collection $A_1,\ldots,A_n$ of subsets of Θ

$$Bel\ (A_1 \cup \cdots \cup A_n) \geq \sum_i Bel(A_i) - \sum_{i<j} Bel(A_i \cap A_j) + -\ldots + (-1)^{n+1} Bel(A_1 \cap \ldots \cap A_n)$$

Bel is called a belief function over Θ.

Not all subsets of Θ necessarily contribute to the value of a particular belief function. Those subsets that do are defined as ***focal elements*** of the function *Bel* over Θ. The union of focal elements is called the *core* of a belief function.

More precisely, A is a focal element of *Bel* over Θ if $m(A)>0$. $\bigcup_{m(A)>0} A$ is the core of *Bel*. In reference to the allocation of belief to subsets, notice that the following is obvious:

Theorem (Shafer 1976)

If C is the core of a belief function *Bel* over Θ, then a subset $B \subset \Theta$ obtains $Bel(B)=1$ iff $C \subset B$.

Bel is an incomplete description of the degree of belief held relative to a proposition in that *Bel* says nothing about the extent to which one believes the negation of the proposition in question. The effect is that *Bel* is nonadditive.

To encompass the extent to which belief of the negation of a proposition is held, define:

$$Doubt(A) = Bel(\bar{A})$$

and

$$Plausibility(A) = Pl(A) = 1 - Doubt(A); \tag{3}$$

obviously,

$$Pl(A) = 1 - Bel(\bar{A}) \tag{4a}$$

and

$$Bel(A) = 1 - Pl(\bar{A}). \tag{4b}$$

Bel and *Pl* convey exactly the same information because one derives the other, viz.,

$$Pl(A) = 1 - Bel(\bar{A}) = \sum_{B \subset \Theta} m(B) - \sum_{B \subset \bar{A}} m(B) = \sum_{B \cap A \neq \phi} m(B). \tag{5}$$

From (5) and (2),

$$Bel(A) \leq Pl(A); \tag{6}$$

that is,

$$Bel(A) \leq 1 - Bel(\bar{A}) \Rightarrow Bel(A) + Bel(\bar{A}) \leq 1, \quad \text{nonadditivity.} \tag{7}$$

The combination of beliefs is now of obvious interest. The need for a combination and conditioning rule is clear, but since *Bel* is nonadditive, the usual algebra of chances and application of Bayes rule is not generally applicable. Shafer uses a rule of combination given by Dempster (1967) that is appropriate for upper and lower probabilities.

Given a collection of belief functions over Θ based on distinct bodies of evidence, the combination rule computes a new pooled belief function. The simplest cases involve observations that refer to a single subset. In cases where belief is allocated to multiple subsets, belief is afforded the intersection. Where disjoint subsets are allocated belief, then the beliefs are in conflict and diminish one another in combination.

Suppose m_1 is the basic probability assignment for a belief function Bel_1 over Θ and having focal elements $A_1, \cdots A_k$. The basic probability numbers can be depicted as segments of the unit interval. Suppose m_2 is similarly defined for Bel_2 and $B_1,...,B_l$ and with a similar depiction as segments of the unit interval. The combination is graphically represented in Figs. 6, 7 and 8.

The figures illustrate that the intersection of the segments corresponding to the basic probability measures have areas of measure $m_1(A_i)m_2(B_j)$.

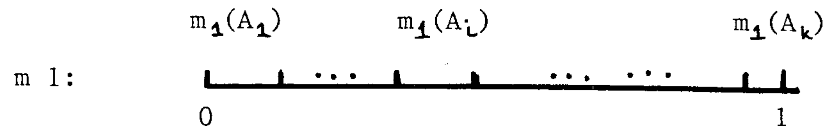


Fig. 6 Basic probability numbers, m_1 , depicted on the unit interval.

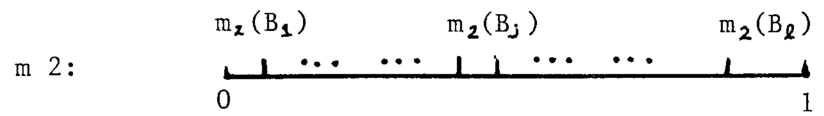


Fig. 7 Basic probability numbers, m_2 , depicted on the unit interval.

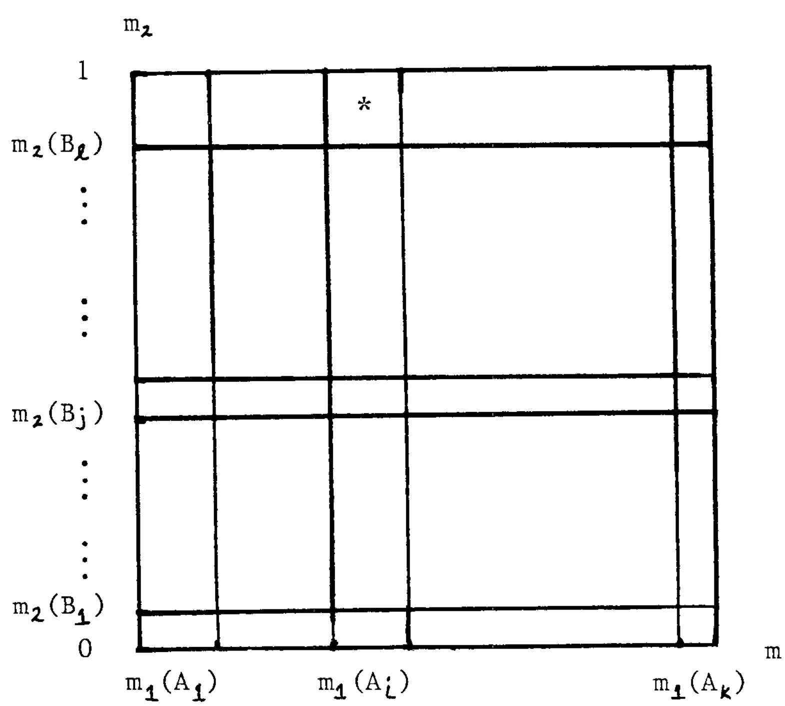


Fig. 8 The orthogonal combination of m_1 and m_2 with * illustrating the measure $m_1(A_i)m(B_\ell)$ afforded $A_i \cap B_\ell$.

$i=1,\ldots k$, $j=1,\ldots l$. These areas then correspond to the joint belief of Bel_1 and Bel_2 committed to precisely $A_i \cap B_j$ as the intersections of focal elements. Some subset $C \subset \Theta$ may have multiple intersection regions committed to it. The total probability committed to C will have measure

$$\sum_{\substack{i,j \\ A_i \cap B_j = C}} m_1(A_i) m_2(B_j). \tag{8}$$

If there exists some focal element of Bel_1 and a focal element of Bel_2 such that $A_i \cap B_j = \phi$, we would have

$$\sum_{\substack{i,j \\ A_i \cap B_j = \phi}} m_1(A_i) m_2(B_j) > 0. \tag{9}$$

To obtain the combined belief, measure of the null intersections is subtracted (the focal elements where $A_i \cap B_j = \phi$ are in conflict and diminish the combined belief),followed by normalization. The factor that will achieve the normalization such that the probability measure on the combination is unity is:

$$\left[\sum_{\substack{i,j \\ A_i \cap B_j \neq \phi}} m_1(A_i) m_2(B_j) \right]^{-1}. \tag{10}$$

It is possible to show that where

$$\sum_{\substack{i,j \\ A_i \cap B_j = \phi}} m_1(A_i) m_2(B_j) < 1,$$

the function $m : 2^{\Theta} \rightarrow [0,1]$ such that $m(\phi)=0$, $C \subset \Theta$, and $C \neq \phi$ given by

$$m(C) = \frac{\sum\limits_{\substack{i,j \\ A_i \cap B_j = C}} m_1(A_i) m_2(B_j)}{\sum\limits_{\substack{i,j \\ A_i \cap B_j \neq \phi}} m_1(A_i) m_2(B_j)} \tag{11}$$

is a basic probability assignment (Shafer 1976, theorem 3.1). Equation 11 is the discrete, finite form of Dempster's rule of combination for upper and lower probabilities (Dempster 1967).

The combination of Bel_1 and Bel_2 is called the *orthogonal sum*, and is written $Bel_1 \oplus Bel_2$. The core of the belief function $Bel_1 \oplus Bel_2$ is the intersection of the cores of Bel_1 and Bel_2. Correspondingly, if the cores are disjoint, no orthogonal sum exists: For some $C \subset \Theta$ there is the condition that $Bel_1(C)=1$ and $Bel_2(\bar{C})=1$, indicating that the propositions constitutive of the frame of discernment are in internal conflict with evidence. In such a situation Bel_1 and Bel_2 are not combinable; that is, they are incommensurable.

How new evidence should affect belief may now be described. Suppose that initial beliefs are given by Bel_1 over Θ. Suppose further that some new evidence, taken alone, yields belief Bel_2 over Θ. This combined evidence and the resulting belief is given by $Bel_1 \oplus Bel_2$.

Suppose that the new evidence has the effect of supporting a proposition (subset of Θ) unequivocally. Then, where this subset is denoted by $B \cap \Theta$, Bel_2 will be established thus:

$$Bel_2(A) = \begin{cases} 1 \text{ if } B \subset A \\ 0 \text{ if } B \subset A' \end{cases} \tag{12}$$

Bel_2 may be combined with Bel_1 by equation 13, so long as $Bel_1(\bar{B})<1$. This is clear since $Bel_2(B)=1$ and, therefore, if $Bel_1(\bar{B})=1$ equation 11 would fail to provide the basic probability assignment required to define the combined belief.

Let us define Bel_1 *conditioned* on B as $Bel_1(\bullet \mid B)$, thus denoting $Bel_1 \oplus Bel_2$. We may derive this combination as follows.

The basic probability assignment of Bel_1, Bel_2 and $Bel_1 \oplus Bel_2$ are m_1, m_2, and m. $m_2(B)=1$, thus, applying equation 11 gives the basic probability assignment, m:

$$m(A) = \frac{\sum\limits_{\substack{i \\ A_i \cap B = A}} m_1(A_i)}{\sum\limits_{\substack{i \\ A_i \cap B \neq \phi}} m_1(A_i)} = \frac{\sum\limits_{\substack{C \\ C \cap B = A}} m_1(C)}{1 - \sum\limits_{\substack{i \\ A_i \cap B = \phi}} m_1(A_i)} = \frac{\sum\limits_{\substack{C \\ C \cap B = A}} m_1(C)}{1 - Bel_1(\overline{B})} \tag{13}$$

which is used to construct $Bel_1(A \mid B)$ using equation (2):

$$Bel_1(A \mid B) = \sum_{D \subset A} m(D)$$

and by equation (13)

$$= \frac{\sum\limits_{\substack{D \\ D \subset A \\ D \neq \phi}} \sum\limits_{\substack{C \\ B \cap C = D}} m_1(C)}{1 - Bel_1(\overline{B})}$$

$$= \frac{\sum\limits_{\substack{C \\ B \cap C \subset A \\ B \cap C \neq \phi}} m_1(C)}{1 - Bel_1(\overline{B})} = \frac{\sum\limits_{\substack{C \subset A \cup \overline{B} \\ C \not\subset \overline{B}}} m_1(C)}{1 - Bel_1(\overline{B})} = \frac{Bel_1(A \cup \overline{B}) - Bel_1(\overline{B})}{1 - Bel_1(\overline{B})}. \tag{14}$$

Now, from equation (4a) the associated plausibility is formed:

$$Pl_1(A \mid B) = 1 - Bel_1(A \mid B)$$

$$= \frac{(1-Bel_1(\bar{B})\,) - (Bel_1(\bar{A} \cup \bar{B}) - Bel_1(\bar{B}))}{1-Bel_1(\bar{B})} = \frac{1-Bel_1(\overline{A \cap B})}{1-Bel_1(\bar{B})}$$

$$= \frac{Pl_1(A \cap B)}{Pl_1(B)}. \tag{15}$$

As discussed in relation to equation (5), *Bel* and *Pl* convey the same information and it is often interesting to observe the form they take. In this case, conditioning takes a familiar form in equation (15) that will allow definition of independence of beliefs.

Belief functions have been discussed to provide intuition about what can be done with and said about the notion of belief in relation to a set of propositions. Now the effects of refinements of the frame of discernment on belief will be considered .

To state what a frame of discernment is, at the intuitive level, is not difficult, although it has been postponed until now. A frame of discernment is a formal list of possibilities given as propositions over some domain of discourse. One frame is to be understood to be formed by a subset (possibly a tiny one) of the imaginable collection of distinctions one might employ to define the propositions. One can easily imagine using different subsets for different concepts or domains of discourse. One thus does not imagine belief generally to be assessed against some immutable epistemological framework.

Shafer formalized the alterations of frames of discernment where the changes result in frames that differ in degree of resolution, but do not result in incommensurable propositions. One imagines frames that are more or less descriptive of the same truth conditions.

The idea is that a frame Ω may be obtained by splitting the elements of Θ. This simple notion is formalized in the following way: For each $\theta \varepsilon \Theta$, a subset

$\omega(\{\theta\})$ is specified. $\omega(\{\theta\})$ consists of possibilities partitioned in θ. We require that the sets $\omega(\{\theta\})$ constitute a disjoint partition of Ω. That is,

$$\omega(\{\theta\}) \neq \phi \quad \text{for all } \theta \varepsilon \Theta$$

$$\omega(\{\theta\}) \cap \omega(\{\theta'\}) \text{ if } \theta \neq \theta'$$

$$\bigcup_{\theta \varepsilon \Theta} \omega(\{\theta\}) = \Omega \, .$$

The disjoint partition $\omega(\{\theta\})$ refers to any subset $A \subset \Theta$ in Ω and thus

$$\omega(A) = \bigcup_{\omega \varepsilon A} \omega(\{\theta\}). \tag{16}$$

Hence, $\omega(A)$ consists of the possibilities in Ω obtained by splitting elements of A. The mapping $\omega{:}2^{\Theta} \to 2^{\Omega}$ defines the splitting.

Shafer defines such an ω as a ***refining*** . $\omega{:}2^{\Theta} \to 2^{\Omega}$ is a refining when Θ and Ω are finite sets, the subsets $\omega(\{\theta\})$ are a disjoint partition of Ω and the sets $\omega(A)$ are defined by equation 17. The notion of refining is essential to understanding of the use of belief functions to represent the impact of evidence on frames of discernment.

The definition of coarsening is given in terms of the above. Where we have a refinement given by $\omega{:}2^{\Theta} \to 2^{\Omega}$, then Θ is called a *coarsening* of Ω.

Every proposition discerned by Θ is discerned by Ω. In particular, every proposition associated with the subset $A \subset \Theta$ is also associated with the subsets $\omega(A) \subset \Omega$. Then where one thinks of 2^{Θ} and 2^{Ω} as sets of propositions, 2^{Θ} is a subset of 2^{Ω}. The real intuitive sense of refinement is that the refined propositions express all the relations of the coarser propositions and, in addition, distinguish the domain in more detail.

We now outline the basic properties of the concept of refinement and coarsening. These properties are essential to the discussions that follow.

Shafer proves where $\omega: 2^{\Theta} \to 2^{\Omega}$ is a refining that

1. ω is one to one
2. $\omega(\phi)=\phi$
3. $\omega(\Theta)=\Omega$
4. $\omega(A \cup B)=\omega(A) \cup \omega(B)$ for all $A,B \subset \Theta$
5. $\omega(\overline{A})=\overline{\omega(A)}$ for all $A \subset \Theta$
6. $\omega(A \cap B)=\omega(A)\omega(B)$ for all $A,B \subset \Theta$
7. if $A,B \subset \Theta$ then $\omega(A) \subset \omega(B)$ iff $A \subset B$
8. if $A,B \subset \Theta$ then $\omega(A) \cap \omega(B)=\phi$ iff $A \cap B=\phi$.

$\omega: 2^{\Theta} \to 2^{\Omega}$ is not, in general, onto because of the possibility that $A \subset \Omega$ will not be discerned by Θ. Imagine a coarsening where a partition separates a more refined partition such that some of the refined propositions constituting a subset lie on one side of the coarser partition and some of the refined propositions of the same subset lie on the other side of the partition. For example, see Fig. 9.

It is clear that the subset $A \subset \Omega$ depicted in Fig. 9 is not expressible by propositions available to Θ.

A subset of Θ may be associated with any subset of Ω only by making use of inner and outer reductions. The *inner reduction*, denoted $\underline{\theta}: 2^{\Omega} \to 2^{\Theta}$, is defined as the largest subset of Θ that implies some $A \subset \Omega$. Symbolically,

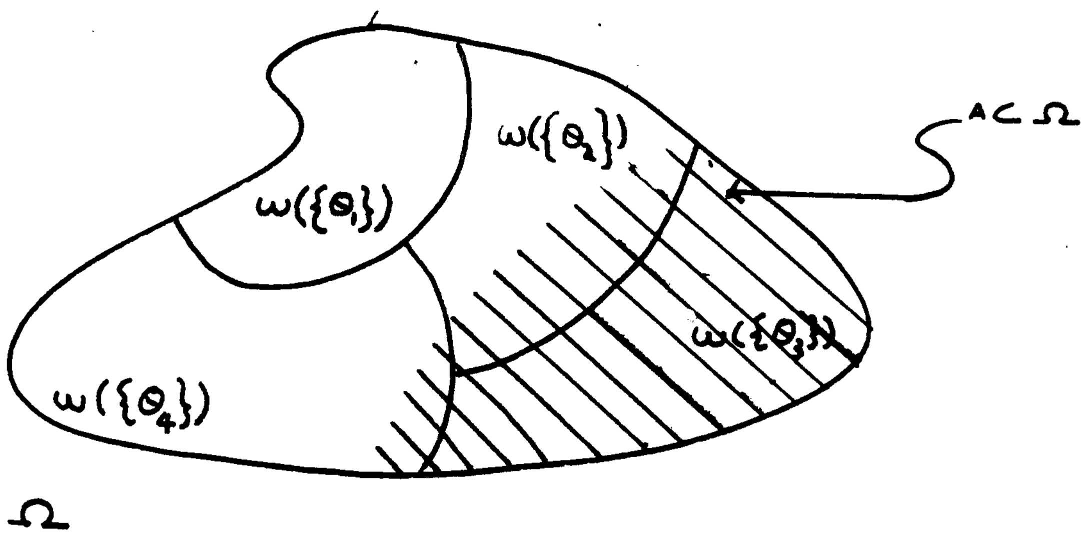


Fig. 9 A coarsening of a frame Ω by $\Theta = \{\theta \ldots \ldots \theta\}$. The shaded region is a subset of Ω.

$$\underline{\theta}(A) = \{\theta\varepsilon\Theta \mid \omega(\{\theta\})\subset A\}. \tag{17}$$

In Fig. 9, $\underline{\theta}(A) = \{\theta_3\}$. The *outer reduction*, denoted $\bar{\theta}$: $2^{\Omega}\rightarrow 2^{\Theta}$, is defined as the smallest subset of Θ that is implied *by A*. Formally,

$$\bar{\theta}(A) = \{\theta\varepsilon\Theta \mid \omega(\{\theta\})\cap A\neq\phi\}. \tag{18}$$

In general, frames of discernment are subject to indefinite refinement. Such a state of affairs amounts to being always able to imagine any set of propositions to be augmented by another set of propositions such that all of the relations in the first set hold in the second and that the second set serves only to disjointly partition the sets defined by the first. This has important consequences. Any such refinement defines a *compatible refinement*. Shafer defines the notion of a *family* of compatible frames as a family of frames gotten by refinement and coarsening only. One more definition is required to define the important properties of families of compatible frames.

Where a family $\mathbf{F}$ consists of frames $\Theta_1, \ldots \Theta_n$, every pair Θ_i, Θ_j has a *common refinement*. That refinement is given by refinings $\omega_i:2^{\Theta_i}\rightarrow 2^{\Omega_i}$ and $\omega_j:2^{\Theta_j}\rightarrow 2^{\Omega_j}$ such that $\Omega_i=\Omega_j$. The set of refinings $\omega_1,\ldots,\omega_n$ associated with $\mathbf{F}$ is denoted by $\mathbf{R}$. Shafer shows that in every $\mathbf{R}$ there exists a unique *minimal refinement*, denoted $\Theta_1\otimes\cdots\otimes\Theta_n$, that is, a coarsening of every common refinement. Common and minimal refinements are essential to the definition of consistent beliefs and independent frames which are used in the characterization of evidential support.

If Θ_1 and Θ_2 are compatible frames of discernment, two beliefs given by the functions Bel_1 and Bel_2 over Θ_1 and Θ_2 respectively, are *consistent* if they agree on all propositions contained in both Θ_1 and Θ_2. Formally, that condition may be stated as follows: $Bel_1(A_1)=Bel_2(A_2)$ whenever $A_1\subset\Theta_1$, $A_2\subset\Theta_2$, $\omega_1(A_1)=\omega_2(A_2)$

and ω_1 and ω_2 are minimum refinings. That is, $\omega_i : 2^{\Theta_i} \rightarrow 2^{\Theta_1 \otimes \Theta_2}$, where $i=1,2$. The uniqueness of the minimal refinement is used to ensure that only those subsets commonly discerned by Θ_1 and Θ_2 are compared. When given some frame Ω and the corresponding set of all subsets (propositions), denoted 2^{Ω}, we might restrict our attention to a subset of subsets (a coarsening) corresponding to a frame Θ and propositions 2^{Θ}. In that situation, we have a refinement $\omega : 2^{\Theta} \rightarrow 2^{\Omega}$ and say that a Bel_1 over Θ is consistent with a Bel_2 over Ω where

$$Bel_1(A) = Bel_2(\omega(A)\,), \;\; A \subset 2^{\Theta}$$

Bel_1 (over Θ) is called the ***restriction*** of Bel_2 (over Ω) to Θ. The restriction is denoted by $Bel_2 \mid 2^{\Theta}$. Bel_1 is a restriction of Bel_2 where 2^{Θ} is thought of as a subset of propositions of 2^{Ω} to which our beliefs are applied. It is possible to show that the basic probability function under refinement is nonincreasing. Formally, where Bel_1, Bel_2 as above have basic probability assignments m_1, m_2, the restriction $Bel_2 \mid 2^{\Theta}$ has

$$m_2(\omega(A)) \leq m_1(A)\;,\; A \subset 2^{\Theta} \tag{19}$$

(by theorem 6.8 Shafer 1976)

Compatible frames can be ***independent***. They are called independent when no proposition in one of them is nontrivially discerned by the other. The minimal refinement of the sets is used to compare the propositions. Where Θ_1 and Θ_2 are compatible and $A_1 \subset \Theta_1$, $A_2 \subset \Theta$, the proposition corresponding to A_1 implies the proposition corresponding to A_2 when $\omega_1(A_1) \subset \omega_2(A_2)$ and the refinings are given by $\omega_i : 2^{\Theta_i} \rightarrow 2^{\Theta_1 \otimes \Theta_2}$ $i=1,2$. The implication is trivial where either $A_1=\phi$, since in such case $Bel_1(A_1)=0$ and the proposition is certainly false, or if $A_2=\Theta_2$, since there $Bel_2(A_2)=1$ and the proposition is certainly true. Where the implication is trivial under the above refinings the frames are independent. Independence of frames of discernment means that establishing any proposition as true or false of one frame is unrelated to the truth or falsity of any proposition of another frame.

It is now possible to define the subclass of belief functions called support functions. The simplest case occurs where evidence univocally supports a particular subset $A \subset \Theta$, $A \neq \phi$. Such evidence is called homogeneous. The effect of the evidence on Θ is limited to providing a certain degree of support for A and, to some degree, any subset of A. No support will be afforded to any other propositions discerned by Θ. Where s is the degree of support A obtains, $0 \leq s \leq 1$, then we say that the degree of support for $B \subset \Theta$ is a function $\mathrm{S}:2^{\Theta} \rightarrow [0,1]$.

$$\mathrm{S}(B) = \begin{cases} 0 & \text{if } A \subset B \\ s & \text{if} A \subset B \neq \Theta \\ 1 & \text{if } B = \Theta \end{cases} \tag{20}$$

Clearly S is a belief function, in this case a *simple support* function. If S_1 has basic probabilities $m_1(A)=s_1$ and $m_1(\Theta)=1-s_1$ and S_2 has $m_2(A)=s_2$ and $m_2(\Theta)=1-s_2$ we may form $\mathrm{S}=\mathrm{S}_1 \oplus \mathrm{S}_2$ with basic probabilities $m(A) = 1-(1-s_1)(1-s_2)$ and $m(\Theta)=(1-s_1)(1-s_2)$. That is written as $\mathrm{S}(A)$, a simple support function focused on A, with $\mathrm{S}(A)=1-(1-s_1)(1-s_2)$.

If evidence points to different subsets A,B such that $A \cap B \neq \phi$, ***separable support functions*** are formed. This evidence is called heterogeneous. If S_1 is focused on A, $\mathrm{S}_1(A)=s_1$. Also if S_2 is focused on B, $B \cap A \neq \phi$, $\mathrm{S}_2(B)=s_2$. Support for $A \cap B$ is $s_1 s_2$. The separable support function is defined thus:

$$S(C)=\begin{cases}0 & \text{if } A\cap B\not\subset C\\ s_1s_2 & \text{if } A\cap B\subset C, A\not\subset C, B\not\subset C\\ s_1 & \text{if } A\subset C, B\not\subset C\\ s_2 & \text{if } B\subset C, A\not\subset C\\ 1-(1-s_1)(1-s_2) & \text{if } A,B\subset C, C\neq\Theta\\ 1 & \text{if } C=\Theta\end{cases} \tag{21}$$

Separability refers to the ability to assess the degree of support afforded the different subsets involved by evidence.

Where $A\cap B=\phi$, then evidence is in conflict. That means evidence assigns values to propositions discerned by Θ that are contradictory. Dempster's rule and the normalizing form of equation 10 are used to effect the combination. The effect of such combined evidence is that it mutually diminishes the support afforded either proposition alone. Graphically, the support afforded the propositions associated with A,B is depicted in Fig. 10.

In Fig. 10, conflicting evidence (lower left box) is removed and the support function normalized by $\frac{1}{1-s_1s_2}$ by Dempster's rule, giving the form of the support function of the combination:

$$S(C)=\begin{cases}0 & \textit{if } A,B\not\subset C\\ \dfrac{s_1(1-s_2)}{1-s_1s_2} & \textit{if } A\subset C, B\not\subset C\\ \dfrac{s_2(1-s_1)}{1-s_1s_2} & \textit{if } B\subset C, A\not\subset C\\ \dfrac{s_1(1-s_2)+s_2(1-s_1)}{1-s_1s_2} & \textit{if } A,B\subset C, C\neq\Theta\\ 1 & \textit{if } C=\Theta\end{cases} \tag{22}$$

The values in the combined support functions will allow the interpretation that, where A and B are contradictory, the more support afforded one, the greater the errosion of support is for the other. Thus, where contradictory

	$1 - s_2$	committed to A	uncommitted
Support S_2			
	s_2	committed to $A \cap B = \phi$	committed to B
		s_1	$1 - s_1$

Support S_1

Fig. 10

beliefs are held in relation to some evidence, it is the balance of the strengths of belief that obtains on combination. This is intuitively reasonable and a result of Dempster's rule, but obviously only a convention. It is admittedly not necessarily the "natural" convention by which beliefs are combined.

A more general class of support functions are obtained from separable support functions by coarsening the frame of discernment. Suppose there is a rather fine frame Ω and that a body of evidence can be analyzed into homogeneous subsets of the frame such that it defines a separable support function $\mathrm{S}:2^{\Omega}\rightarrow[0,1]$. We want to understand the effect of the evidence on a coarsening Θ. We will have a restriction $\mathrm{S}|2^{\Theta}$, a belief function that is not required to be a separable support function. Really we are interested in the problem the other way around: We have a frame Θ and a body of evidence not representable by a separable support function: Is there a refinement of Θ that obtains a separable support function? Indeed as regards Θ, does there exist a belief function? The existence of a (generalized) *support function* provides the answer. A support function is a belief function obtained by coarsening a frame where separable support functions exist. Formally, a belief function $Bel:2^{\Theta}\rightarrow[0,1]$ is a support function if there exists a refining $\omega:2^{\Theta}\rightarrow 2^{\Omega}$ and a separable support function $\mathrm{S}:2^{\Omega}\rightarrow[0,1]$ such that $Bel=\mathrm{S}|2^{\Theta}$.

It can be proved that the subset of belief functions that are support functions are just those where the core has a non-zero basic probability number (theorem 7.1 Shafer 1976).

A final remark on independence in relation to support functions: The prospective view of independence of frames is called *cognitive independence*. Two frames of discernment are cognitively independent where evidence that relates to one of them does not affect the degrees of support for propositions of the other one. Formally, two independent coarsenings Θ_1 and Θ_2 are cognitively independent with respect to $\mathbf{S}:2^{\Omega}\rightarrow[0,1]$, if $(\mathbf{S}\oplus\mathbf{S}_1)|2^{\Theta_2} = \mathbf{S}|2^{\Theta_2}$ for every support function $\mathbf{S}_1:2^{\Omega}\rightarrow[0,1]$ that may be restricted to Θ_1 without loss of degree of belief and is combinable with $\mathbf{S}$.

It can be shown that cognitive independence is equivalent to $Pl(A\cap B)=Pl(A)Pl(B)$ where A is discerned by Θ_1 and B is discerned by Θ_2, Θ_1 and Θ_2 being independent coarsenings of Ω. (theorem 7.9 Shafer 1976).

Independence in this sense is Baysean; consider its effect on conditioning in equation (15). Simply, cognitive independence yields the case $Pl(A|B)=Pl(A)$.

There is a final side issue and a point to be made. The representation of ignorance is understood here in a better way than usual. Most probability models treat the maximal condition of ignorance to be the case of equiprobability of alternatives. However, to hold no opinion at all is a condition of ignorance and to have support functions with high degrees of conflict when combined is to have an ignorant epistemic structure. The first case may be seen very simply. Evidentially speaking, we may ***believe*** A to some degree, but have no reason to ***disbelieve*** $\bar{A}$. This is an essential feature of nondemonstrative confirmation generally and relates to opacity. Disbelief is not by necessity equatable with lack of belief. The plausibility of A is the degree to which no counterevidence is known to exist. That is, one's belief in A may be small, but in the absence of counterevidence, the plausibility may be high. In the limit one might have unity plausibility and zero belief. By example, I ask a question that any rational parent will profess ignorance in relation to: "Will the child eat its dinner?" Under additivity $Bel(eat) + Bel(\overline{eat}) = 1$ and the highest values for both eat and $\overline{eat}$ indeed are $\frac{1}{2}$.

Let us define θ_1 as the possibility the child will eat and θ_2 as the possibility that the child will not. Then we have the set of possibilities $\Theta = \{\theta_1, \theta_2\}$. Under conditions of complete ignorance, one might adopt a ***vacuous belief*** over Θ:

$$Bel(A) = \begin{cases} 0 \text{ if } A \neq \Theta \\ 1 \text{ if } A = \Theta \end{cases} \tag{23}$$

rather than the additivity rule and equiprobability model of ignorance. After living with the child one might refine the set of possibilities. For example, will the child even sit at the table? Then one might form $\Omega = \{\alpha_1, \alpha_2, \alpha_3\}$ where α_1 denotes the possibility that the child will eat, α_2 denotes the possibility that the child will sit at the table and not eat and α_3 denotes the possibility that the child will not even sit at the table.

Under additivity,

$$Bel(\alpha_1) + Bel(\alpha_2) + Bel(\alpha_3) = 1$$

and ignorance has $\alpha_1, \alpha_2, \alpha_3 = \frac{1}{3}$. But $\{\alpha_2, \alpha_3\}$ has the same meaning as θ_2, yet $Bel(\{\alpha_2, \alpha_3\}) = \frac{2}{3}$ while $Bel(\theta_2) = \frac{1}{2}$. This is inconsistent and indicates that, in general, the representation of ignorance as disbelief leads to a poor representation of ignorance since it is contradictory under consistent refinement.

Ignorance is important to the information channel analogy of Chapter 5 and will be taken to be captured by the notions of belief and plausibility rather than belief and disbelief. In limiting cases the more restrictive usual representation can be valid.

We thus conclude the introduction to belief functions. The purpose of this rather lengthy review of Shafer's theory is to provide a sense of intuition concerning the elements of the ascription of belief, their form and inter-relations, the essentiality of the idea of a knowledge structure, its propositional character and the naturally probabilistic nature of belief. It is important to notice that what Shafer has produced is something of a calculus with a lot of magic required in attempts to apply it. For example, if we do not imagine families of formal languages of indefinite descriptiveness, as Shafer does not, how do we ever produce a proposition? What is its truth ascription if we have no available semantics? How is evidence transformed into a proposition? What is evidence? And so on. But we ***have*** been provided other things; the interplay between the degree of belief and the epistemic frame is important where many would assert that there is no theoretical need for knowledge structures. The interaction of beliefs themselves is a not-so-minor thing to confront directly. These considerations are fundamental to any formalization of a representation theory applied to behavior. It is now easier to develop the ideas of Chapters 2 and 3. Beliefs are defined as judgmental acts embodied in the comparison of evidence and the state of knowledge. The state of knowledge is refinable, with ascriptions of belief sensitive to these refinements; *a fortiori*, the judgmental act is sensitive to these refinements; hence, the judgmental act is sensitive to the epistemic condition. Beyond a link from representation and evidence to belief, prior beliefs influence the acts of judgement associated with subsequent beliefs. Then epistemic probability can be seen to have influential domain over an act, *viz.* the act of further determinations of epistemic probabilities. Actions on the part of a believer form either evidence, if they are overt, or beliefs, if they are covert, and in either case are incorporated by the theory to influence and be reciprocally influenced by the epistemic frame and epistemic probabilities.

There remains the need to *construct* the epistemic frame and epistemic probabilities to make credible the opinion that a biological system might construct them. In the next section we will review the work of Randall (1970) in which a theory of formal languages in the assessment of evidence is developed. This construction will provide method to some of the "magic" mentioned earlier. It is much more satisfactory in the sense that ontological and other philosophical positions are clearer and have definite consequences which we may observe.

4.3 The structure of epistemic frames: Randall

It has been seen that, given sets of propositions which in some sense characterize the environment, evidence can be interpreted to lend support to or erode belief of that view of the environment expressed by the epistemic frame constituted of those propositions. This section will review a formal approach to the issue of representation of knowledge and observation. The developments provide clarification and a more detailed understanding of the previous section. Epistemic frames and probabilities can be understood as formal languages, the structures they induce and a measure on those structures.

Randall (1970) developed a general definition of formal languages to characterize descriptions of observations in order to investigate scientific methodology. (Recall that the isotropic and quineian criteria for central processes came from analogy to scientific method as well). A definition of observation was produced and the interactions of bodies of observational evidence on these world descriptions was investigated. Formal languages impose structure on the universe of discourse. One may think of the structure in terms of the sets of sentences that, under semantic interpretation, induces it. This characterization intuitively corresponds to Shafer's notion of an epistemic frame of discernment which is understood as a list of propositions expressing the possibilities in the domain of interest. This section will outline Randall in order to provide essential definitions and concepts that bear on the problems of this thesis.

An *a priori* probability is defined on the structure that the formal language imposes on the universe of discourse. That prior can be employed to define a subjective probability of a observation. The subjective probabilities are belief functions; they reflect a conceptual view of the environment. Thus, the use of formal languages in the sense of Randall's work can be taken as a *construction* of the notion of frame of discernment and its relation to evidence as expressed by epistemic probabilities.

There are ontological issues that arise concerning the use of formal languages to express (1) the world and (2) mental states. Randall was exclusively concerned with (1). In this thesis I must also be concerned with (2). Randall's metaphysical assumptions correspond to the ideas that apparently underlie the epistemic frame and evidential concepts of Shafer. They are that Aristotelian logic and extensionality are valid for the universe of discourse of formal languages. That is, insofar as (1) is concerned, we assume an existing external world and that different objects of that world are sensibly differentiable. The ontology also assumes that the universe of discourse for formal languages is set-theoretic: the models of the languages will be models of axiomatic set theory.

The ontology for (2) is not addressed by these assumptions alone. Randall (properly) treats scientific theories as abstract objects and the world as existing objects. We are concerned with the physical existence of the observer (the *ascriber* of belief) as well, since he must behave, exhibit control of movement and otherwise causally interact with his environment. Since I will model the representation states by use of formal languages and since I assume that the representation state is instantiated physically (that is, physiologically) in an observer, the languages are *instantiatable* physically. That is, they too exist in the world. This assumption is a weakened form of the notion of *Dasein* in the philosophy of mind that forms a major dispute for Naturalistic and Rationalistic philosophers of psychology. The term is attributed to Heidegger and is roughly used in English as "being part of the world." The dispute is central in Pylyshyn (1984), Fodor (1980) and the notion of methodological solipsism in cognitive science. The assumption sets the requirement of constructivity for the model of mental state.

The behaviorally observable effects of mental states are assumed to derive from the fact that those states are physiologically instantiated. It is not required, however, to take the physicalist position in the philosophy of mind. We only require physical instantiability by virtue of constructivity to be ensured. Since this is a functional criterion for a normal form, such a condition does not imply unique structural, anatomical-relational correspondence as physicalism does, yet remains materialist (see Pylyshyn (1984), Fodor (1983), Uttal (1981)). It will also conform to the ontology given by Randall (and apparently Shafer) since we will be able to hold that the instantiated representation state, being part of the world, satisfies the ontology ascribed the world -- physical existence and extensionality. Hence dualism, separate ontologies for the observer and the world, is not required and neither is the rejection of mental state or acceptance of naive physicalism.

The appropriateness of a linguistic approach to cognition in the sense that we apply it in this thesis derives from the need for representations in the situation described in the introduction and the idea of propositional attitudes. I do not imply that the languages we discuss are to be considered as any spoken language like English, but are of the form of a "mentalese" or language of thought, Pylyshyn (1984), Fodor (1975). The linguistic approach to representation is extensively motivated in recent literature, e.g., Fodor (1975, 1981), Chomsky (1980), Block (1981), Field (1981), Winograd (1983), Sowa (1984), Rock (1983, Pylyshyn (1984). Criticism is available in Stalnaker (1984) but the propositional form is maintained. The notion of languages of thought is conceived where representations are viewed as truth-value bearing expressions understood as sentence analogues; i.e., representations are propositional forms. A psycholinguist might relate mentalese to spoken language, in those cases where natural utterences may be formed, by asserting they must be translations from the (original, as it were) mentalese. This is to be contrasted with the view of Carnap (1947) wherein representations are understood as sentences in the speaker's natural language, a very suspect theory. This thesis is what Fodor (1980) calls "neo-Carnapian." I think of a code (rather literally in the next chapter) of a discernible state as the truth-values borne by a set of sentence analogues and take the position that mental state is a description in the form of a proposition rather than a"picture." I subscribe to the view of Randall (1970, pg. 14),

> ". . . we are interested in language as a vehicle embodying certain structure, and we shall regard it as a formal apparatus for explicating what is going on internal to a man's understanding . . . [the characterization is] devoted primarily to . . . the structural properties of language."

Formal languages are taken to be structural models of mental states and, in some cases, mental processes.

There are two rather reciprocal views of the linguistic view of cognition. Roughly, one emphasizes the influence of language on thought; the other emphasizes the influence of thought on language. The first is exemplified by the studies of Whorf (1964) and can be appreciated by cross-cultural studies of color psychophysics and language expressiveness. For example, perhaps people from cultures with languages possessing a high degree of expressiveness in a particular frequency range (e.g., the Eskimo people having multiple names for what English speakers call "white") have correspondingly more psychophysical discrimination threshold levels in that frequency range than people from cultures with less expressive languages in that frequency range. Perhaps people with identical racial histories, but different linguistic experience would exhibit these differences. That the tacit knowledge underlying spoken language demonstrably affects the percepts of the speaker is called the Whorfian Hypothesis. The effect has more recently come to attention as the issue of the effect of programming languages on problem solving (e.g., Hamill (1985), Rich (1985)).

The second view is exemplified in studies by Berlin and Kay (1969), Heider (1972) and a summary by Brown (1976). The idea is that there should be a correspondence between the complexity of thoughts and the complexity of the sentences uttered to express them. This is called "codability" in the literature cited. The evidence for it is again often cross-cultural and, generally, goes as follows. A measure of codability in English of a color (a complex one) predicts its likelihood of recognition recall in a later task for English speakers. The measure, however, also predicts the recognition recall for monolingual speakers of Dani, a language that possesses no vocabulary for chromatic variation whatsoever. Thus the spoken English reflects the mentalese "more" than spoken Dani; a linguistic twist of fate presumably related to environmental-survival relevancies in cultural histories, but the data imply that the underlying mentalese is roughly equal for us all.

The latter view makes it plausible to explain how a polyglot "thinks in" only one of the available languages -- the transformation to speech being from mentalese to the appropriate grammar for utterances. To unite the two views, however, is an issue of nativism, one lying outside the concerns of this thesis. I simply hope to have clarified somewhat what is meant by the role of languages of thought as opposed to a study of spoken or written language. In either case cited above, the spoken language reflects, more or less accurately, the propositional conditions of thought, but the latter case specifically refers to the form of thought. It is, in any case, at least arguable that to theoretically express the idea of mental representation, formal languages are a natural and appropriate medium.

Randall delimits the minimum conditions required to define a formal language. I take those minimal conditions as convention for the use of the term "formal language." This will involve a rather broader perspective than the usual view of a formal language such as the lower predicate calculus.

A particular formal language should have a well-defined vocabulary. Sentences should be classifiable finite strings of vocabulary symbols. A process should be specifiable for the determination of the meaning of sentences. To describe a formal language, one must appeal to the use of a meta language. To ascribe meaning, one needs a model or structure to describe the possible relationships among objects in the universe of discourse of the language. The truth condition for a sentence is that the objects referred to by the sentence have the structure within a model that is required by the sentence. The above is all very typical in the usual study of model theory (e.g., Robinson (1977)). However, in model theory the models are specific to a language. Randall desired to involve a multiplicity of languages and thus wanted to characterize the idea of model independently of them. The relationship to model theory is very interesting but outside our present interests.

The notion of model corresponding to the previous ontological assumptions is based on set theory. A model of set theory is understood as a set S of objects in the universe of discourse and a binary relation ε on S satisfying the axioms of set theory of Zermelo-Frankel set theory (e.g., see Cohen (1966)). Under the assumption of the consistency of the axioms of set theory, a model is assured to exist. Many models will satisfy these conditions. For some S there is the standard or "natural" ε and, in addition, non-standard models on S. All of these models are allowed here, the natural ε being denoted $\in$ and the collection of all models of set theory denoted by $\boldsymbol{M}$ where

$$\boldsymbol{M} = \{M \mid M = \langle S, \varepsilon \rangle\}.$$

M is a model of set theory; S is fixed.

S is at least countably infinite. A given formal language will only refer to a subset of S.

A precise descriptive language used in the characterization of semantics will be called the language of set theory. It is comprised of a standard formulation of the lower predicate calculus with an identity symbol and a single binary predicate ε. The axioms of set theory are expressions within this language and the language may be augmented by a denumerable number of names corresponding to particular elements of S.

The metalanguage is taken as English with the concept of a set. Both the language of set theory and the object formal languages are defined within its domain of discourse.

Observations are also characterized in a rather broad way. Under the same ontology thus far proposed, Randall provides a definition expressed in the language of set theory. Since the existence and set-theoretic structure of the universe of observations (the world) are assumed, the language of set theory should provide for the characterization of any observation. In the present extended form of that ontology involving the observer, I require that the representation states the observer may form be similarly expressible. The observations will be cast as particular sets of objects and their functional relationship. A certain set of models will be delimited by those which possess the structure required by the observation. The coincidence of that set of models with those defined by a formal language is the focus of interest.

It should be emphasized that Shafer takes truth conditions on propositions to be intuitively self-evident and proposes no evaluative process. More careful reflection suggests, however, that one should not assume so much. Even Stalnacher's proposed alternative to the linguistic approach to the problem of representation focuses on the truth values of propositions. The truth conditions on a set of sentences requires a model; however, in general the model that satisfies the set is not unique. Upon closer scrutiny, Shafer's "intuitive content" of propositions requires the realization that one is actually partitioning the space of *models* with the set of sentences, as shall be seen. That clarification perhaps has the greatest value of Randall's efforts.

The minimal constituents and the conditions for adequacy of a formal language are (Randall):

I. Minimal constituents

i. a recursively enumerable set of symbols, T.

ii. a set of sentences $S \subseteq T^*$ where T^* denotes the set of all finite strings of T.

iii. for each model $M = \langle S, \varepsilon \rangle$ of set theory, and every sentence $\gamma \in S$, $\varphi(M,\gamma)$ is a function whose value is the truth value of the sentence γ for the model M. φ is called an interpretation (Frege, 1949), and constitutes the involvement of semantics in the language.

II. Conditions of adequacy

i. the set of sentences S is recursively enumerable over T.

ii. for any set of sentences $s \subseteq S$, the logical consequences of s are recursively enumerable over s.

These constituents and criteria embody the following basic concepts.

The possible requirement that any string of symbols be classifiable as a sentence or non-sentence in a finite number of steps is too strong. The condition imposed here is that if the string *is* a sentence, it ought to be algorithmically possible to determine that it is. The grammar of a language is the set of rules that allows a process of determination. Randall required the sentences to be recursively enumerated by the weakest possible grammar so as not to artificially encumber the theory with unnecessary structure. The weakest formal grammar is designated by Chomsky as a type 0 grammar (Chomsky 1963; see also Winograd 1983). I remark that the type 0 grammar enumerates the same functions as a Turing Machine and is therefore a constructive basis for this theory.

A sentence γ is the logical consequence of a set of sentences $s \subseteq S$; if when all of the sentences of s are true, then γ is true. Truth of a sentence is assessed by the interpretive function, φ. φ is defined for any γ and $M \in \boldsymbol{M}$. Notice that knowing γ is a logical consequence of s does not imply that it is provable from s. Provability is a property usually associated with syntactical operations governed by a set of inference rules. Randall's conditions for adequacy imply that these two concepts be equatable at the metalanguage. That is discussed in some detail by Randall (1970 pg 20, 21, 52-54).

An important feature of logical consequence is that it is independent of specific knowledge of the world. Syllogistic constructions of premise are adequate to allow logical consequence. One sees the point by Randall's illustrative use of the undecidable major premise "All men are mortal" followed by the minor premise "George is a man" allowing the conclusion "George is mortal." The conclusion is seen to be a logical consequence and independent of the ultimate verifiable truth of the premises. The important fact that comes of that, useful to us later, is that the conclusion *is* based on the *relationship* among the truth conditions. But obviously these are not, by necessity, *projectable* relationships; they may lack veridicality. In psychological terms, all the sentences are preceded by X *believes* "All men are mortal." It is extremely important to this theory that opacity is handled naturally.

Combining the notion of logical consequence and sentence enumerability obtains a substitute for the (verificationistic) requirement of finitary determination of the truth of every sentence. The weaker alternative is, given $\boldsymbol{s} \subseteq \boldsymbol{S}$ and a logical consequence γ of $\boldsymbol{s}$, we want to be able to decide that situation. There must be a constructive procedure for enumerating the logical consequences of $\boldsymbol{s}$.

Randall's definition of formal language that follows can be proven to fulfill the above conditions of adequacy. The definition is:

A language $L = \langle Syn, Sem, Rn \rangle$ is a *formal language* if:

1. $Syn = \langle T, P, L, G, \sigma \rangle$ is a *syntax* for L,

   i. T is a finite or countably infinite set of the *terminal vocabulary* of L.

   ii. P is a finite set and $P \cap T = \phi$. P is the set of non-terminal vocabulary, that is, the *parts of speech*.

   iii. L is a finite set of rewrite rules of the form $\alpha \rightarrow \beta$, $\alpha \in P$, $\beta \in T$. L is called the *lexicon*.

   iv. G is a finite set of rewrite rules of the form $\alpha \rightarrow \beta$, $\alpha, \beta \in P^*$, where P^* is the set of all finite strings of P. G is the set of grammar rules, i.e., the *grammar*.

v. $\sigma \in P$. σ is a preferred, or special, part of speech, the *sentences*.

2. *Sem* = $<C,\lambda>$ is a *semantics* for L,

i. C assigns a formula $F(x)$ of the language of set theory to each part of speech $\alpha \in P$. The formula $x = 0 \vee X = 1$ is assigned to $\sigma \in P$. $F(x)$ will often be denoted $C_\alpha(X)$. C_α is called a *semantic category*.

ii. λ assigns a formula of the language of set theory, $F(x_1, \cdots x_m, y_1, \cdots y_n)$ to each rule of grammar $R{:}\alpha_1 \cdots \alpha_n \to \beta_1 \cdots \beta_m \in G$, such that $F(x_1, \cdots x_m, y_1, \cdots y_n)$ implies

$$C_{\beta_1}(x_1) \wedge \cdots \wedge C_{\beta_m}(x_m) \wedge C_{\alpha_1}(y_1) \wedge \cdots \wedge C_{\alpha_n}(y_n).$$

λ_R is called a *semantic transformation*.

3. *Rn* = $<M,\Phi>$ is a *realization space* for L,

i. $\boldsymbol{M} = \{\mathrm{M} \mid \mathrm{M} = <\mathrm{S},\varepsilon>$ is a model of the language of set theory$\}$; S is fixed.

ii. $\boldsymbol{M}$ represents all possible configurations of the objects in the universe.

iii. $\Phi = \{\varphi \mid \varphi$ maps T into S$\}$. Φ is the set of interpretations of L.

The definition can be seen to incorporate the type 0 grammar. The interaction of semantics and syntax is most interesting. The semantic transformation, λ_R, holds that the grammatical analysis of a sentence is not independent of its meaning. The requirement 2 (ii) of the definition sets the condition that, for a formal language, the grammar is such that the strings it generates that are intermediate to a sentence must be interpretable. This captures intentionality in relation to syntactical processing as was discussed in Section 4.1.

The set of objects belonging to some semantic category is the subset of objects of the domain of discourse which share some common properties. So, there is a formula $F(x)$ of the language of set theory which characterizes some interrelational structure that holds for all objects of the subset called semantic category. The semantic categories and transformations are structural in nature, meaning that they are independent of particular objects. One might say they are "syllogistic." The semantic category for sentences, C_σ, corresponds to the development of Frege (1949), indeed, the usual Tarskian notion of semantics.

A sentence of a formal language is taken to denote its truth value. The possible denotations are:

i. $\{ 0 \}$, the sentence is false

ii. $\{ 1 \}$, the sentence is true

iii. $\{ 0, 1 \}$, the sentence is ambiguous

iv. $\{\phi\}$, the sentence is meaningless.

The details of the manner in which a sentence denotes its truth value are not necessary for expository purposes and I will only outline the notions of a parse and the interpretation of a parse. It will then be possible to consider semantic and syntactical ambiguity and the issue of meaninglessness.

Randall's notion of semantic transformation in a formal language dictates that relations exist among elements in the domain of discourse. The domain of a particular semantic transformation is just those subsets of the domain of discourse which have certain structural, that is, relational, properties. (For notational convenience I will sometimes write a sequence of subscripted variables as the variable with a bar over it, e.g., $v_1,...v_n$ as $\overline{v}$.) A semantic transformation $\lambda_R(\overline{x},\overline{y})$ for the rule of grammar $R:\overline{\alpha}\rightarrow\overline{\beta}$ is such that

$$\{<\bar{x},\bar{y}> \mid \lambda_R^M(\bar{x},\bar{y})\} \subseteq C_{\beta_1}^M \times \cdots \times C_{\beta_n}^M \times C_{\alpha_1}^M \times \cdots C_{\alpha_m}^M \}$$

for any model, M.

One sees that $\bar{x},\bar{y}, \in$ S must satisfy λ_R. The sets operated on by λ_R must belong to semantic categories specified by the parts of speech involved with the rule of grammar R. A parsing of a sentence will fail for semantic reasons if for some elements of S identified with variables $\bar{y}$ in λ_R, there is no collection of elements of S identified with variables $\bar{x}$, which satisfies the transformation.

There is the case of vacuous description such as "all the Ph.D.'s in the C. L. Barrett family," which is grammatically correct but fails to be associated with any individual. Ambiguity that is semantic in character results when more than one collection of $\bar{y}$ corresponds to a particular $\bar{x}$ as in "The daughter of Chris Barrett" (there are two).

When the abstract syntax and semantics are taken in relation to the realization space, the meaning of strings becomes concrete. Where the set of models $\boldsymbol{M}$ characterizes configurations of objects in S, Φ gives the mapping of the objects in S to words in language L. $\varphi \in \Phi$ assigns some subset(s) in S to a word $w \in T$. Because each $M \in \boldsymbol{M}$ has a distinct ε, the subsets of S to which ω is assigned varies between models. In order to simplify discussion Randall routinely fixed the interpretation φ and referred to *the* interpretation φ. I will likewise refer to φ, but emphasize that any useful account of the linguistic model being developed here must presuppose that φ varies; indeed, that L varies.

I define the parse and the interpretation of a parsed string to illustrate the mechanism of the assignment of meaning. The results which follow from them are provided without proof. Randall provides them and I seek simply to provide an adequate intuition of an expressible (that is, knowable) state in an epistemic structure.

A *parse*, p of $\lambda \in T^*$ is denoted $<\lambda,p>$ and is defined:

$$p = <\alpha_0, \alpha_1, \cdots, \alpha_n> \text{ is a parse if}$$

i. $\alpha_0 = \beta_1\beta_2 \cdots \beta_k$ where $(\beta_i \in P) \rightarrow (\gamma_i \in T) \in L$ and $\gamma = \gamma_1\gamma_2 \cdots \gamma_k$ for $i = 1, \cdots k$.

ii. for each $i = 1,...,n$ there are $\mu_{i_1}, \mu_{i_2}, \mu_{i_3}, \mu_{i_4} \in P^*$ such that

$$\alpha_i - 1 = \mu_{i_1}\mu_{i_2}\mu_{i_3}$$

$$\alpha_i = \mu_{i_1}\mu_{i_4}\mu_{i_3}$$

$$\mu_{i_4} \rightarrow \mu_{i_4} \in G.$$

An *interpretation of a parsed string*, γ, is denoted $\varphi^*_{\mathrm{M}}(\gamma,p)$ where $\varphi \in \Phi$, $\mathrm{M} \in \boldsymbol{M}$, and is given by

i. $\varphi^*_{\mathrm{M}}(\gamma,p) = \{<\varphi(\gamma_1), \varphi(\gamma_2) \cdots \varphi(\gamma_k)>\}$ if $p = <\alpha_0>$ as in condition (i) in the above definition of a parse and $\varphi(\gamma_i) \in C^{\mathrm{M}}_{\beta i}$.

ii. $<x_1,...,x_m, y_1',...,y_s', z_1,...,z_t> \in \varphi^*_{\mathrm{M}}(\gamma,p)$ where $p = <\alpha_0, \cdots, \alpha_h, \alpha_{h+1}>$

if

a. $\alpha_h = \delta_1 \cdots \delta_m\ \mu_1 \cdots \mu_r \nu_1 \cdots \nu_t$

b. $\alpha_h + 1 = \delta_1 \cdots \delta_m\ \mu_1' \cdots \mu_s' \nu_1 \cdots \nu_t$

c. $R\colon \mu_{1...s} \rightarrow \mu_{1...r} \in G$

d. $<x_1, \ldots, x_m, y_1, \ldots y_r, z_1, \ldots, z_t> \in \varphi^*_{M}(\gamma, <\alpha_0, \ldots, \alpha_h>)$

e. $<y_1, \ldots, y_r, y_1' \cdots y_s'> \in \lambda_R^M$.

iii. $\varphi_M^*(\gamma, p) = \phi$ otherwise.

It can be proven that it is possible to associate a unique formula of set theory with an interpretation $\varphi_M^*(\gamma, p)$. Indeed, if there is a parse $<\gamma, p> \rightarrow \sigma$, then there is a formula of the language of set theory, $F(\bar{x}, y)$, such that $a \in \varphi_M^*(\gamma, p)$ iff $F_M(\bar{x}, y)$ holds for $<\overline{\varphi(\gamma_i)}, a>$ where $a = 0$ under model M (written 0_M), or $a = 1$ under model M (written 1_M)

$F_M(x)$ is understood to be the interpretation of $F(x)$ given by the particular ε associated with an $M \in \mathit{M}$.

Semantic ambiguity is the condition where $\varphi_M^*(\gamma, P_i) = \{0_M, 1_M\}$ for $M \in \boldsymbol{M}$; P_i, $i \in I$. Syntactic ambiguity is the condition where $\varphi_M^*(\gamma, P_i) = \{0\}$ and $\varphi_M^*(\gamma, P_j) = \{1\}$ for $M \in \boldsymbol{M}$ and $P_i, P_j; i, j \in I$.

The case where $\varphi_M^*(\gamma, P) = \phi$ is exactly the case where, for the model $M \in \boldsymbol{M}$, the function $F(\bar{x}, y)$ does not exist. From these considerations it is possible to prove that the formal language fulfills the conditions of adequacy.

Suppose there exists a collection of models. A particular sentence will, in general, be true of some, false of some, and meaningless of others. The semantic categories serve to partition the universe of discourse in such a way that the "objects" the language discerns are not homogeneous in the sense that they do not all have the same structural properties. Objects within a semantic category do possess a common structure. There are problems associated with the range of quantifiers (quantifiers assume the elements under quantification belong to the same structural class) that are resolved by defining the range of quantification to be over those elements which extensionally define a semantic category.

When I speak of a mental state, or a knowledge structure, or an epistemic frame I mean an entity canonically identical to a formal language in the sense of the above definitions and properties. The mental state will thereby satisfy physical instantiability required under the ontological assumptions as well as have the proper form for propositional attitudes

The extra-linguistic notion of observation or evidence will now be characterized. That enables an account of the probability of an observation based on one's epistemic frame. This section will be concluded by showing that the probability (which Randall defines constructively) is a belief function. Thus, this section is taken to be a construction of belief functions where the ontological assumptions are explicit and reasonable and under which physical realizability is ensured. Moreover, the notion of representational structure and processes of the ascription of belief conforms to the taxonomy of mental processes of Chapter 3.

An observation is independent of any particular formal language. Since the world is assumed to be set-theoretic, one feels justified to define *observation* as follows (Randall):

> An observation is the condition that the relationship specified by $F(x_i, \ldots, x_n)$, a formula of the language of set theory, exists among particular objects $a_1, \ldots, a_n$. Formally, an *observation* $0 = \langle F(x_1, \ldots x_n), a_1, \ldots a_n \rangle$ where $F(x_1, \ldots, x_n)$ is a formula of set theory and $a_i \in S, i = 1 \cdots n$.

For some model, M, the a_i will have the relational structure demanded by F or not. That is, either $F_{\mathrm{M}}(x_1, \ldots, x_n)$ is true for $a_1 \cdots a_n$ or it is not. Let

$$M_O = \{\mathrm{M} \mid F_{\mathrm{M}}(x_1, \ldots, x_n) \text{ hold for } a_1, \ldots, a_n\}\ ,\ M_O \subseteq M$$

to be the set of models associated with O. The models in M_O are required to have the relationships expressed by F_M among *particular* objects $a_1, \ldots, a_n$. It is the relationship between observed objects that is set theoretically specified.

It may be that there is a "true" model which characterizes the relationships between the objects of an observation. It is not the case that that true model will, by necessity, be a member of M_O. That is, the observation may not be reflective of the world. One imagines here *possible*, as opposed to actual, observations. In psychological terms, veridicality is not required by the theory.

Every formula of set theory, F, relativized to a particular ε (that is, every F_M) that holds over a particular collection of objects in the domain of discourse constitutes an observation. Under each model, the object-event is thus a different observation. Intuitively, one imagines the class of philosophical problems carried by a situation such as: John and Mary see the same doughnut (say, the *last* doughnut) and both utter, "That is my doughnut." The sentence is the same from each, but the observations are clearly distinct. This notion of observation therefore properly addresses non-substitutivity of identicals and lack of existential generalization inherent to the semanticity of mental representation as was discussed in Section 4.1.

This notion of observation is, as has been pointed out, extra-linguistic. Randall's formalization defines observations in the metalanguage with the language of set theory used to characterize semantics in the formal languages. There is a sense in which one may roughly say that meaning, in the form of multiplicities of natural structures, exists in the world, (see Gibson 1979, Barwise and Perry 1983, Stalnaker 1984). What is indicated by Randall's formalization of observation is that the *possible* relevancies of an observation are precisely the possible structural relationships between constituents of the world including the structure of the observer. Apparently, the ideal *act* of describing an observation involves something like establishing a set of sentences of a formal language which hold over exactly the set of relational structures (models) that defines the objects of observation in the world. Randall demonstrated that establishing the coincidence of the subset of models that satisfy the observation with the subset of models under which a collection of sentences of a formal language are true gives rise to the subjective probability of the observation. This amounts to a weak form of ***projectability*** of properties in the philosophy of mind.

A set of observations, Ω, is ***consistent*** if

$$\bigcap_{O \in \Omega} M_O \neq \phi.$$

One might make a large number of consistent observations where the intersections establish a smaller and smaller subset of possible models. Indeed, it is possible to prove that a maximal set of consistent observations yields a unique model. Dempster (1966) defines a similar condition called sharpness.

Two sets of observations are related if they are in conflict or if a subset of one implies that some observations of the other are valid. Formally, sets of observations Ω_1, Ω_2 are ***related*** if either

i. Ω_1, Ω_2 are not consistent or

ii. for $\Omega_1', \Omega_2' \neq \phi$ where $\Omega_1' \subseteq \Omega_1$, $\Omega_2' \subseteq \Omega_2$ and $\bigcap_{O_1 \in \Omega_1'} M_{O_1} \subseteq$ or $\supseteq \bigcap_{O_2 \in \Omega_2'} M_{O_2}$.

Notice that the converse of this definition suggests Shafer's notion of independence of frames. Indeed, one might well conclude that the propositions referred to by Shafer are the $F(x)$'s in the definition of O and that the correspondence to an epistemic frame is captured by the coincidence of models as articulated above. The essential omission of Shafer is the multiplicity of structures by which the propositions are evaluated. The most important obfuscation Randall clarifies is the form of the observation assessable by the knowledge state -- coincidence of models which enables an account of epistemic probability

The structure imposed on the universe of discourse by a formal language as developed by Randall is expressed (Randall pg 54),

> "The fundamental idea here is that the syntax and semantics of the language, together with the interpretation φ establish a correspondence between sentences of the language and certain sets of models contained in $\boldsymbol{M}$. The result of this is a partitioning of M into disjoint subsets each of which is definable by *some collection of formulas of set theory*." (italics my own)

Some models that satisfy an observation coincide with a set of models that satisfies a collection of sentences of a formal language. From this the construction of the probability is achieved.

Sets of sentences and models are related through the definitions of element and state of a formal language. Given a set $\boldsymbol{s} \subseteq S$ of sentences of L, the *element* η determined by $\boldsymbol{s}$ is the maximal set of models such that for L and $\mathrm{M}_1, \mathrm{M}_2 \in \eta$, $\gamma \in \boldsymbol{s}$ either

1. γ is true for both $\mathrm{M}_1, \mathrm{M}_2$
2. γ is false for both $\mathrm{M}_1, \mathrm{M}_2$
3. γ is meaningless for both $\mathrm{M}_1, \mathrm{M}_2$.

Given a set s as before, the ***state*** of η determined by $\boldsymbol{s}$ is defined to be the maximal set of models such that for M_1, M_1, γ as above, either (1) or (2) holds.

The "speaker" of a formal language deals only with states, since (3) is only determinable at the metalanguage. Some of the sentences needed to describe the element may be meaningless on every model that constitutes the element. The set of sentences $\boldsymbol{s}$ defines the state η of the formal language L that produces the sentences.

The first step to the subjective probability is to define what Randall calls the observer's ***a priori*** probability. This is needed to ensure that we have measurability for the sets involved in the subjective probability. It corresponds to the basic probability measure referred to by Shafer (1976).

Randall shows that the class of subsets defined by all the models that hold over collections of objects belonging to the domain of discourse generates a σ-field. P-measurability of the subsets of the σ-field ensures the P-measurability of the defined observations. It is then shown that every state of a formal language is an element of the σ-field and is therefore P-measurable.

An ***a priori*** probability P is postulated to be a measure such that $P(\boldsymbol{M}) = 1$ and $P(\phi) = 0$. Randall proves for a formal language L that any state, η, is P-measurable. Thus the model space (observation space) is P-measurable and the partitions induced by a formal language on that space are P-measurable. Measurability is of concern in the construction of subjective probabilities. To define the subjective probability, the definition of a ***basis*** of (states of) a formal language is needed.

Two sets of models are ***indistinguishable*** if every sentence of the language either has the same set of truth values on both of them or is meaningless of one of them. That is, given L and $\eta_1, \eta_2 \in \boldsymbol{M}$, η_1 / η_2 (distinguishability sign) if there is a $\gamma \in S$ such that γ is true of η_1 and false of η_2. Otherwise, $\eta_1 // \eta_2$ (indistinguishability sign).

$\eta \subseteq \boldsymbol{M}$ is a partially specified subset of configurations having in common some structures occurring in every $\mathrm{M} \in \eta$. A sentence might distinguish between two such partially specified configurations and, if one does, we require that the configurations be considered distinguishable.

Randall gives five elementary properties of distinguishability.

1. if η_1 / η_2 and $\eta_1' \subseteq \eta_1$ then η_1' / η_2

2. if $\eta_1 // \eta_2$ and $\eta_1 \subseteq \eta_1'$ then $\eta_1' // \eta_2$

3. for any $\eta \neq \phi$, $\eta // \boldsymbol{M}$

4. for any η, η / ϕ

5. if $\eta_1 \cap \eta_2 \neq \phi$ then $\eta_1 // \eta_2$

Observe that indistinguishability is not an equivalence relation because transitivity does not hold.

Characterization of a set of independent states will use the idea of indistinguishability and the notion of minimal state that follows. Where Σ is the set of all states of L, a state $\eta \in \Sigma$ is ***minimal*** if there is no other non-empty state $\eta' \in \Sigma$ such that $\eta' \subseteq \eta$.

Given η_1, η_2 and $\boldsymbol{s} \subseteq \boldsymbol{S}$, $\boldsymbol{s}$ is said to ***agree*** on η_1, η_2 if for any $\gamma \in \boldsymbol{s}$, $\mathrm{M}_1 \in \eta_1, \mathrm{M}_2 \in \eta_2;$ $\varphi^{\dagger}_{\mathrm{M}_1}(\gamma) = \varphi^{\dagger}_{\mathrm{M}_2}(\gamma) \neq \phi$.

It can be proven that for a subset of sentences $\boldsymbol{s} \subseteq \boldsymbol{S}$ where each sentence in $\boldsymbol{s}$ is meaningful for some $\mathrm{M} \in \boldsymbol{M}$, then there is a minimal state, η, such that $\boldsymbol{s}$ agrees on $\{\mathrm{M}\}, \eta$.

A set of minimal states $\boldsymbol{B}$ of L is called a ***basis*** for L if for any $\eta_1, \eta_2 \in \boldsymbol{B}$, η_1 / η_2 and for any element E of L there is some $\eta \in \boldsymbol{B}$ such that $\eta // E$. The definition says that a set of minimal states $\boldsymbol{B}$ where any $\eta \subseteq \boldsymbol{M}$ is indistinguishable from at least one $\mathrm{B} \in \boldsymbol{B}$ is called the basis.

It can be shown that for any subset of minimal states that are pairwise distinguishable, there exists a basis such that those minimal states are a subset of the basis. L has a unique basis if and only if the set of all minimal states is pairwise distinguishable. Existence and uniqueness conditions of the basis are thus exhibited.

Three properties of the basis related to the problem of meaninglessness will be given without proof. Basis properties in relation to meaninglessness have important consequences for subjective probability.

If a set of models $\boldsymbol{M_O} \subset \boldsymbol{M}$ is such that every sentence of a formal language is meaningful for each $\mathrm{M} \in \boldsymbol{M_O}$, then for any basis $\boldsymbol{B}$ of the language, $\boldsymbol{M_O} \subseteq \bigcup_{\mathrm{B} \in \boldsymbol{B}} \mathrm{B}$. That is, any such $\{\mathrm{M}_1, \ldots, \mathrm{M}_n\}$ is a subset of the basis.

If every sentence of L is meaningful for every $\mathrm{M} \in \bigcup_{\mathrm{B} = \boldsymbol{B}} \mathrm{B}$, then $\boldsymbol{B}$ is a unique basis for L.

Finally, one may show that if every sentence of L is meaningful for each $\mathrm{M} \in \boldsymbol{M}$, then L has a unique basis $\boldsymbol{B}$ such that $\bigcup_{\mathrm{B} \in \boldsymbol{B}} \mathrm{B} = \boldsymbol{M}$.

It is now possible to define the subjective probability associated with an observation.

Given a formal language L with basis $\boldsymbol{B}$ and an observation O, the subjective expectation, $\hat{P}$, of the observation is defined,

$$\hat{P}(\boldsymbol{M_O}) = \frac{P^*\left(\bigcup_{\substack{\mathrm{B} \in \boldsymbol{B} \\ \mathrm{B} // \boldsymbol{M_O}}} \mathrm{B}\right)}{P^*\left(\bigcup_{\mathrm{B} \in \boldsymbol{B}} \mathrm{B}\right)}. \tag{24}$$

P^* is the outer measure of the *a priori* probability. The outer probability is understood in the usual way (e.g., Billingsley 1979 pg. 30). The outer measure is taken to ensure the existence of a measure.

The definition states that $\hat{P}$ is proportional to the measure of the basis elements from which the observation is indistinguishable. The structure over which the measure is defined is isotropic and the measure is quineian. Some properties of $\hat{P}$ are

1. $\hat{P}(\bigcup_{B \in \mathcal{B}} B) = 1 = \hat{P}(M)$

2. $\hat{P}(M_{O_1} \cup M_{O_2}) = \hat{P}(M_{O_1})$

3. $\hat{P}(\phi) = O$

4. in general, for $M_{O_1} \cup M_{O_2} = \phi$, $\hat{P}(M_{O_1} \cup M_{O_2}) \neq \hat{P}(M_{O_1}) + \hat{P}(M_{O_2})$.

The non-additivity of (4) is a property of indistinguishability.

I claim that $\hat{P}$ is a belief function in the sense of Shafer's theory. That claim amounts to the assertion that this section constitutes a construction of the notions of epistemic frames and the associated belief. The construction has the advantages of explicitly stating the form of the observation, the state of knowledge, and the physical instantiability of the structure of the representation.

It remains to demonstrate that $\hat{P}$ is indeed a belief function. Shafer (1976) uses the upper and lower probability measure given by Dempster (1967) as the model of belief. I will show that $\hat{P}(M_O)$ may be interpreted as a lower probability and therefore is a belief function in the sense of Shafer's theory of evidence.

To begin, I give Dempster's (1967) definition of upper and lower probabilities. Consider the spaces X and Y and a multivalued mapping Γ which takes every $x \in X$ into a subset $\Gamma x \subset Y$. Suppose that μ is a probability measure which assigns probabilities to the members of a class Υ of subsets of X. If this μ is acceptable for probability judgments about $x \in X$ and if an uncertain outcome x is known to correspond to an uncertain outcome $y \in \Gamma x$, what probability judgments can be made about that y? If Γ is single valued, μ is a unique measure over subsets of Y. Otherwise, consider upper and lower probabilities. For some $T \subset Y$ define

$$T^* = \{x \in X, \Gamma x \cap T \neq \phi\}$$

$$T_* = \{x \in X, \Gamma x \subset T, \Gamma x \neq \phi\}.$$

The domain of Γ is $Y^* = Y_*$. Define Λ to be the class of $T \subset Y$ such that T^*, $T_* \in \Upsilon$ and suppose $Y \in \Upsilon$. The upper probability of $T \in \Lambda$ is defined:

$$P_{upper} = \frac{\mu(T^*)}{\mu(Y^*)}$$

and the lower probability defined as

$$P_{lower} = \frac{\mu(T_*)}{\mu(Y^*)}.$$

P_{upper} and P_{lower} are undefined where $\mu(Y^*) = 0$

Assertion:

$\hat{P}$ is a belief function.

which is easily demonstrated. In the above definition, take

$X = \Sigma$, the set of states

Γ is a mapping taking the subset of states $\eta // M_0$ into the set of minimal states

$Y = \Sigma_{minimal}$, the set of minimal states of L.

$T = B \subset \Sigma_{minimal}$, B is the set of basis states

$\mu = P^*$, a measure on Σ.

For an observation a set of models, $\eta \subseteq \Sigma$ is associated, so we see

$$B^* = \{\eta \in \Sigma \mid \Gamma\eta \cap B \neq \phi\}$$

and

$$B_* = \{\eta \in \Sigma \mid \Gamma\eta \neq \phi, \Gamma\eta \subset B\}.$$

$\mu(\Sigma^*_{minimal})$ corresponds only to the sets that Σ is meaningfully mapped into and corresponds to summation over the basis, i.e., $\mu(\bigcup_{B \in \boldsymbol{B}} B)$. This is a generalization of Dempster's combination rule of Section 4.2.

The set of models B_* is equivalent by indistinguishability to

$$\bigcup_{\substack{B \in \boldsymbol{B} \\ \eta \subset B}} B = \bigcup_{\substack{B \in \boldsymbol{B} \\ B // \eta}} B\,;$$

thus:

$$P_{lower} = \frac{P^*(\bigcup_{\substack{B \in \boldsymbol{B} \\ B//\eta}} B)}{P^*(\bigcup_{B \in \boldsymbol{B}} B)} = \hat{P}(\eta)$$

and the assertion is demonstrated.

A quick review of where we stand after this rather long tour of epistemic probability is desirable. Much of the "magic" discussed at the end of 2.4 is gone. The observation has been defined and is understood in relation to the knowledge structure. The knowledge structure has been developed with an explicit metaphysics and constructability. One feels more comfortable with theoretical statements that require expectation in relation to propositional attitudes.

4.4 Probability and the functional relationship of representation and behavior. A summary

What I hope stands evident is that a concept of belief as epistemic probability derives from the premise of the existence of representation systems and states that are propositional in form. Beliefs are, in general, incomplete, contradictory or vacuous in relation to certain possible observations, and the structure that defines them is self referentially opaque. Beliefs are quineian, derived from isotropic structure and, *qua* epistemic probabilities, can be seen to have influential domain over an act and be reciprocally influenced by overt behaviors.

The consequences of adverting to the logical nature of probability are basic to clearer comprehension of the relation of probability, representation and behavior.

What is apparent is that the idea of probability can arguably be detached from objects and attached to the descriptions, as it were, of objects. A meaningful description can, tautologically, never be free of epistemological content and is, by kind, a propositional and representational entity. It is thus, at least, type-distinguished from the object it describes and the probability attached to it is likewise distinguished from, for example, the relative frequency of occurrence of the object.

Now, on the one hand, the act of description is representational ***behavior*** on the part of a system and, to some, enough to draw the connection to behavior. But to take the issue several steps further, Freeman (1983) shows physiological evidence that a subjective *expectation* of odor was operational behaviorally and detectable neurally. Freeman posits that the representational brain state is characterized by subjective expectation, a hypothesis tested by observation, that is, an inferential process of representation that has behavioral consequences.

The behaviorist approach to probability in relation to behavior is one that discounts all of the above considerations, takes a purely frequentist approach to the *logical* foundations of probability, and suggests one's mental representation is merely isomorphic to the behavioral relevancies of the world, in particular, the frequencies of the world. When the logical predispositions of behaviorists are removed, their statements regarding the relation of probability to behavior bear a superficial relationship to concepts in this thesis. As an example, a quote from Atkinson *et al* (1965, pg 12) is edited thus, " . . . the basic dependent variable[s] in [experimental] psychology is [are] or should be, [functions of subjective] response probability [underlying behavior]."

A language change effects an alteration of the prior distribution and is, by virtue of this essentially being symbol definition, an intentional process. One must suspect behaviors predicated upon degree of belief would systematically vary with the belief and therefore, one suspects that behaviors will systematically vary with the changes of the epistemic frame modeled by a formal language. The virtual certainty of functional equivalence of many neurophysiological states and the additional certainty of the behavioral equivalence of many distributions of subjective probabilities doubly obscures the idea that a compact, unique anatomical structure is in general to be expected to correspond to a "central process" or uniquely underlie a particular centrally mediated aspect of behavior.

In the absence of an obvious hope of neural locality (and the associated loss of an obvious structural analysis of anatomy) the structure imposed by a formal language characterizing representation might offer a causal analysis.

5. Information and Central Processes

It has been argued that the ascription of belief is central in the taxonomy of mental functioning, behaviorally causal, and probabalistically defined with respect to a representational structure. It is natural to ask how subjective expectation in contact with evidence is characterized informationally. This chapter will first survey the use of information theory in psychological investigation of cognition and motor control, and assess the information channel metaphor. A channel analogy of central processes will be proposed that is based on concepts of representation and probability developed in the last chapter. The channel analogy leads to a description of central process effects on the control of movement timing in the next chapter.

5.1 Information theory in psychology

The term "information" in psychology has been used in four ways:

1. in relation to stability and feedback processes, a Wiener-like conception of the term (e.g., Ashby 1960, Grossberg 1978a, b, or Kelso *et al.* 1981)

2. in the sense of a Shannon-type channel (e.g., Hick 1952, Mandelbrot 1952, Fitts and Seeger 1953, Bar-Hillel and Carnap 1964, Laming 1968, Dretske 1981)

3. as in information processing models formally or informally using a computation metaphor (e.g., Hunt 1966, Neisser 1967, Winston 1975, Marr 1982, Pylyshyn 1984)

4. as an intuitive and rather vague term related to a notion of finite capacity of human cognitive and perceptual functioning, such as in the terms "automa-

ticity" (e.g., Kahneman 1973, La Berge and Samuels 1974, Johnson *et al.* 1983), or "chunking" (e.g., Miller 1956, Simon 1974), or "encapsulation" (Fodor 1983).

Mathematically, the most rigorous work has been undertaken in the context of (1) which is closely related to many other areas of mathematical biology. These treatments have not received wide currency in psychological research which is probably a result of a usually rather radically reductionistic approach where such things as mental states are explicitly ignored or modeled proximally as, say, resonance in a formal neural net, (see Ashby 1960, Grossberg 1978), and without reference to cognitively explanatory vocabulary (see Fodor 1980, Pylyshyn 1984). In such accounts stimuli destabilize a system which, tending to homeostasis, behaves in such a way as to achieve stability. The details of internal state *cum* cognition are utterly irrelevant except as they relate to stability concepts. The idea of mental states resolves to a question of homeostasis of the neural substrate. There is no significant adherence to these theories in the psychological literature, apparently largely due to the preoccupation with the neural substrate of mental phenomena. However, the principle of homeostasis is ubiquitous -- the "stable state" in one guise or another is everywhere in psychology and psychophysiology. For the physicalistic theories, homeostasis unifies physical processes and mental functioning. For the cognitive science community, logical principles of simplicity at the symbolic level conjoin homeostasis at the neural instantiation of the symbol system and provide the causal influence of the (psycho)logical principles on behaviors. At any rate, the generalities of homeostasis implicitly pervade the literature despite the rather overt lack of attention that the more rigorous details of stability-type theories receive. The peripheralist theories of motor control obviously subscribe to the use of information in the sense of (1) (especially see Kelso (1981)).

With the appearance of Shannon's classical 1948 paper, a great deal of activity occurred in experimental psychology. The performance paradigms that dominated involved an ideal information channel model of human information processing in choice reaction time (RT) tasks (see Hick 1952, Hyman 1953, Crossman 1953, Fitts 1954; a comprehensive review is found in Laming 1968). The ideal channel amounts to an assumption that maximum rate is continuously achieved. The model does not reflect actual human performance data (see Laming 1968, Sen 1984), which is not really too suprising in view of the assumption. It is interesting to note that this assumption is a *coding* assumption. The setting in which the RT paradigm emerged involved some internal conflict. The notion of coding is a representational concept explicitly involving the intentional use of symbols. The title of Fitts and Seeger's (1953) paper is enough to illustrate the internal conflict of traditional behavioral analysis and information theory, "S-R compatibility: spatial characteristics of stimulus and response codes." Codes are inherently intentional symbolic representations and, as has been discussed, such notions are anathema to behavioristic and careless physicalistic theories.

Information theory was also applied to language (e.g., Mandelbrot 1952, Zipf 1949) but achieved little enduring impact on linguistics or psycholinguistics.

At any rate, by the mid-1970's Fitts law and the Hyman-Hick Law were about all that was operationally left of the information theory of human performance and remain the extent of the influence of information theory on psychology. Interesting reviews of the use of information theory in psychology are given in Pierce (1961) and Cherry (1957).

"Information processing" cognitive psychology has come to dominate behaviorism in experimental psychology (e.g., Neisser 1967) and even began to invade the domain of psychophysics and perceptual science (e.g., Bruner 1957). The advent of artificial intelligence made symbol manipulation, mental states and strategies more respectable. Early on (e.g., Hunt *et al.* 1966, Neisser 1967, Simon 1974), a view to the rigorous analysis offered by computability theory led researchers to the computer-program metaphor of cognition. The cognitive science movement has emerged with (roughly) two axioms: (a) representation is the fundamental issue of pychology and (b) algorithms are adequate theories of thinking. The romance with Turing Machine metaphor extends to the point that the more extreme versions of current cognitive doctrine, (e.g., the "strong equivalence" of programs to cognition of Pylyshyn 1984), not only axiomatically take programs as adequate theories of cognition, but postulate that programs instantiated neurally are the connection of psychology to natural science. Such doctrine is accepted even to the exclusion of such notions as percept or awareness from the domain of a naturalistic psychology. Indeed, any psychological notions that cannot be handled by a Turing Machine are excluded. The irony is that what must be ultimately rejected is pretty much the same as what was rejected by the associationistic theories of behavior (and incited their consensus desertion, see Fodor 1980, 1983). The extent of the damage due to preferring methods over questions is unclear for cognitive science. The central theories of motor control involving motor programs and cognitive intervention generally subscribe to the use of information as information processing in the sense of (3).

The fourth use of the term information inevitably overlaps the previous domains because of its imprecision and has been largely a common-sense empirical issue. When the performance of one task interferes with another, one hears discussions of "finite capacity" of, or "allocation of finite," "resources," (e.g., Posner and Boies 1971, Kahnemann 1973, Norman and Bobrow 1975).

When a task does not interfere with another one hears of tasks requiring no resources or of "automaticity" (e.g., Posner and Boies 1971, La Berge and Samuelson 1974). The capacity or resource notions are vaguely channel metaphors, where some processes presumably exceed the channel's capacity to transmit information while others presumably do not.

"Chunking" (Miller 1956, Simon 1974) is a term that must relate to coding and representation of a source of information. Thus, a channel metaphor is clearly in the background. It also suggests the philosophical issues of the simplicity hypothesis and intentionality in symbol formation: chunking has an interpretation in the context of homeostasis wherein the system seeks a representational state of minimal resource cost and maximal fidelity.

The notions of computation and information transmission are related; the mathematical treatment of information has been greatly generalized from the source-channel-receiver schema (e.g., see Vitushkin 1961, Kolmogorov 1963, Abu-Mostafa 1983). It is conceivable that the relatedness of the definition of information used by researchers subscribing to (2) and (3) would allow an interesting theoretical unity to emerge of great importance to psychology. However, even the complete mathematical unification of (2) and (3) would leave fundamental issues untouched. The overriding reason that the less than rigorous use of the term information in psychology has been prevalent is that the more rigorous treatments explicitly eliminate much of what is psychologically interesting. However, the source and receiver must "agree" on the coding scheme. As suggested above, the intentionality of the code is inherent to its specification. The numerical quantity that reflects the change in certainty after a signal is received as an information measure carries exactly the content of the underlying conception of certainty. Probability carries semanticity in the way that symbols carry their meaning in syntactical operations defined by a Turing

Machine as discussed previously.

In the literature one finds information theory of semantic content (Bar-Hillel and Carnap 1964), and an examination of the information theory resulting, in part, in the conclusion that an adequate information measure must not be a single scalar quantity, but is a relative concept requiring the measure and its reference (Kolomogorov 1963). Recently, Dretske (1981) has addressed the philosophical basis of an information theory of the transmission of knowledge. These have had little utilitarian service. In the last chapter it was shown that belief as epistemic probability was well posed. Therefore, an information measure based on belief is, plausibly, also a well-posed construct. Such a measure would appear to capture much of the common usage of the term information as well as the structure of the mathematical information theory.

When I refer to the "usual employment" of information theory in psychology I refer to the schematic of Fig. 11 (from Laming 1968, pg. 3) which clearly underlies (at least) all of the choice reaction time literature. One might notice an immediate problem with such a setup. I have argued (as does Searle 1980, Uttal 1981, Hochberg 1981, Freeman 1983, Fodor 1983, Pylyshyn 1984) that transduced sensory phenomena are not the extent of the domain of cognition. But the system of Fig. 11 is silent in the absence of a stimulus signal. One must infer that the input domain of this system is transduced sensory stimuli. Thus these schema are an embodiment of the standard S-R reflexive paradigm, or the so-called "stimulus theories" of perception (Rock 1983). So, aside from the fact that models issuing from Fig. 11 do not model the data (Laming 1968, Sen 1984), it isn't a schematic of any situation I intend to address.

To accept the setup in Fig. 11 as psychologically relevant, one must presume at the outset that there exists an unambiguous objective stimulus signal for the

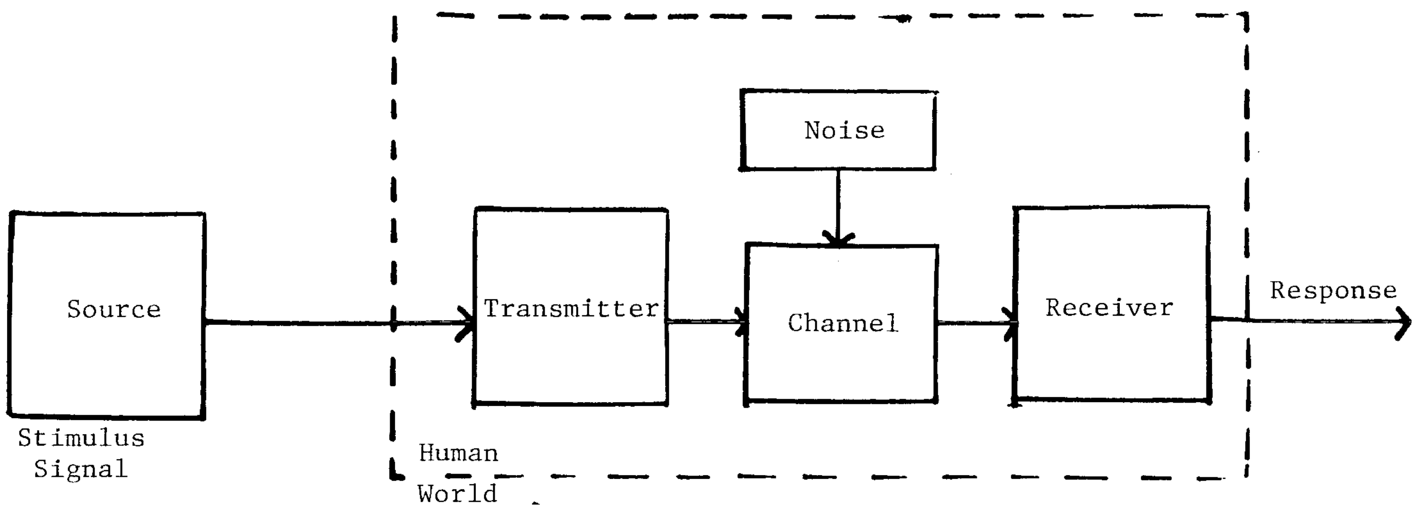


Fig. 11 Schematic of the S-R model of the ideal communication system for the RT paradigm.

system to transmit. In the previous chapter the characterization of observation precludes such a description. Roughly, the assumption holds that the *relevant* ontological facts for the theory are those, and only those, concerning the stimulus world which are known to exist unambiguously; a stimulus is completely specified by its physical parameters. This view attempts to encompass what must lie outside the domain of its ontology. In particular, with respect to a given representation state, it is the external object that is putative and the state itself which supports any assertions of existence a representational system produces. Moreover, if one considers the fingertapping experiment discussed in the introduction to this thesis, it is difficult to imagine a psychophysical account of the individual stimuli having any informational relevance to a causal description of the task. But if the issues of observation relevant to the task are *relational*, then the relevant ontological facts are not found in reflexive transduction of the stimuli, but rather in the representational structure itself which may or may not be veridical.

Notice that it makes no sense to speak of a represented object without reference to an epistemological structure. The degrees of support afforded such a structure by observations are criteria for the integrity of the structure in relation to a world *axiomatically* taken to exist. Given observational evidence, the important quantity is the degree of support afforded the observation which is a determination relative to an epistemic condition.

The ontology of Fig. 11 fails to provide adequate notions of psychological source and receiver in the same way that functionalism based on the Turing Machine metaphor fails to provide an account of mental function -- it lacks intentionality by failing to address symbol specification and violates the taxonomy of mental function.

In terms of an information channel, I am claiming that the representation might behave as a "source" while degree of support is an aspect of a "receiver." This is opposite in direction to the usual setup.

The structure imposed by a formal language induces a probability distribution. Combined with certain indistinguishability conditions, a subjective probability of an observation may be formed. Thus, the representation structure has a well-defined belief measure of a kind allowing an informational analysis of representation. In the next section the relationship of information and representation is addressed.

5.2 Information and representation

This section describes a measure of information based on epistemic probabilities. The problem in communication theory addressed by the well-known source coding theorem and a variety of other related theorems is that of a representation problem for the source. In communication theory the problem is syntactical in the sense that the messages are opaque with respect to content, interpretation being supplied by the sender and receiver. Under the condition that the theory does not involve semantic interpretation, the communication problems become ones of syntactical structure preserving transformations and certainty of syntactical structure recovery. In the design of a communication system, the sender and receiver have to "get together and agree" on the coding scheme to be employed. Under the assumption of such specifications, information measures are unambiguously defined on the codes and their ability to preserve structure and thereby transmit information is ensured.

In the application of information theory to psychological systems, getting the source and receiver together to arrange the coding scheme is more than a metaphorical inconvenience. If one ignores the fact that in communication systems interpretation is supplied by the source and receiver and, with appeals to the inessentiality of semantics to the information theory, neglects to realize that the coder, decoder ***and*** interpretation must be supplied by the psychological system, one will either misapply the information theory or will restrict analysis away from system features of psychological interest as was discussed in the last section. To include the features of the system that are of psychological interest is a large order, but one that has received at least passing interest, (e.g. Bar-Hillel and Carnap 1964, Kolmogorov 1965, Dretske 1981). I take it that the basic problem is to involve the notion of representation, in a psychologically relevant context, with a definition of information in a context appropriate for application of usual information theoretic analysis.

It was seen in the last chapter that the concept of representation as embodied by a formal language induces a p-measurable structure. There is then the clear intuition that an information measure might be formed on the representation in reference to the probability measure inherent to it. Notice that reference only to the probabilities defined by the language does, in some sense, remove the analysis from the semantic domain. It is that feature that distinguishes the measure from the semantic content measure of Bar-Hillel and Carnap 1964 (also see Cherry 1957). However, the intentional properties of the language are essential for the definition of the structure to which the belief measure is applied.

To demonstrate the information measure and provide a diagrammatic heuristic aid for discussions that follow, I will briefly review the measure of informativeness used by Randall (1970). I will refer to the diagrams used in what follows as Randall diagrams and note that they are analytically useful only in the most elementary cases. They are provided simply to make the consideration of a formal language model of representational structure accessible to the intuition.

Fig. 12(a) shows the Randall diagram for a simple hypothetical language consisting of two sentences, γ_1 and γ_2, partitioning the space of models of the universe of discourse, $\boldsymbol{M}$. Each sentence partitions $\boldsymbol{M}$ into the sets of models for which the sentence is true, false, or meaningless. The areas of the partitioned regions are interpreted to be proportional to the observer's *a priori* probability of the element that the partitioned region portrays (references to observers, utterances, descriptions, etc. are properly understood as observer-analogues, utterance-analogues, description-analogues, etc.). In Fig. 12(a) all of the elements are subjectively equally probable *a priori* and are denoted by a vector of truth values,

$$<_{\mathrm{M} \in \mathit{element}\ i} \varphi^* (\gamma_1), _{\mathrm{M} \in \mathit{element}\ i} \varphi^* (\gamma_2)>.$$

This figure is to be understood as the observer's epistemic frame of discernment.

Fig. 12(b) depicts the set of models, $\boldsymbol{M}_0$, associated with an observation. The enclosed area also corresponds to an *a priori* probability of occurance. An observer has no direct access to this probability of an observation. The representation of $\boldsymbol{M}_0$ as a closed curve and sentences by bisecting lines is an artificial distinction for graphical clarity only.

Fig. 13 shows the projection of $\boldsymbol{M}_0$ on the observer's epistemic frame of discernment. It is from this projection that the observer derives observational experience. Notice that tacit structure will exist in the observation descriptions. Indistinguishability and meaninglessness incur inarticulatable effects (from the level of the speaker of the formal language) on the utterance analogues that represent an observation.

Fig. 14 portrays the basis elements that are indistinguishable from $\boldsymbol{M}_0$. It is

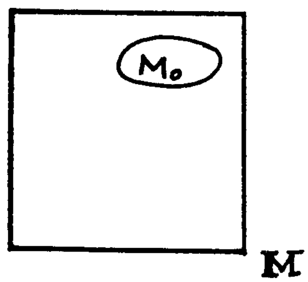


Fig. 12 (b)

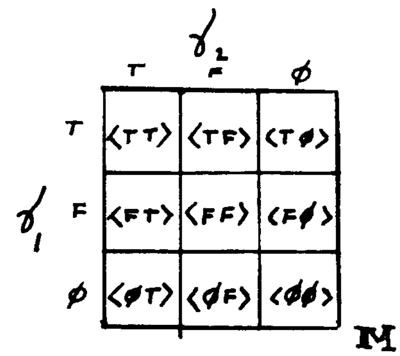


Fig. 12 (a)

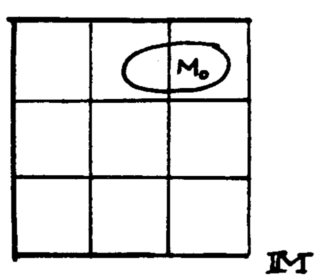


Fig. 13

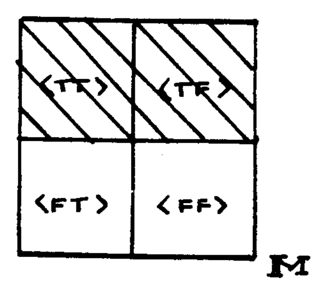


Fig. 14

this situation that diagrammatically represents the utterance-analogue constituting a description of an observation by an observer.

In this simple situation, all minimal states are basis elements and the basis is unique. Such conditions do not in general exist, but that will not be dealt with here (see Randall 1970 for discussion of the issue). From Fig. 14 it is possible to calculate $\hat{P}(\boldsymbol{M}_0)$, the epistemic probability of the observation. From the definition of Section 4.3, the formula is

$$\hat{P}(\boldsymbol{M}_0) = \frac{\sum P^*(\text{basis elements indistinguishable } \boldsymbol{M}_0)}{\sum P^*(\text{basis elements})}.$$

Here all of the elements are ***a priori*** equiprobable, so

$$\hat{P}(\boldsymbol{M}_0) = \frac{\text{number of basis elements indistinguishable } M_0}{\text{number of basis elements}}$$

which by inspection of Fig. 14 equals $\frac{1}{2}$.

Randall defines the obvious information measure using $\hat{P}(\boldsymbol{M}_0)$ as

$$I(\boldsymbol{M}_0) = -\log \hat{P}(M_0)$$

and uses it to define the information gained upon observing 0 given a previous set of observations, Ω, as:

$$I(\boldsymbol{M}_0 \mid \boldsymbol{M}_\Omega) = \log \hat{P}(\boldsymbol{M}_\Omega) - \log \hat{P}(\boldsymbol{M}_\Omega \cap \boldsymbol{M}_0).$$

Where an experiment is performed with possible outcomes $0_1, \ldots 0_n$ and $\bigcup_{i=1}^{n} 0_i = \boldsymbol{M}$ and the observations are consistent, i.e., $0_i \cap 0_j \neq \phi$ for all $i,j = 1,\ldots,n$ and $i \neq j$, then the expected gain of information is defined to be

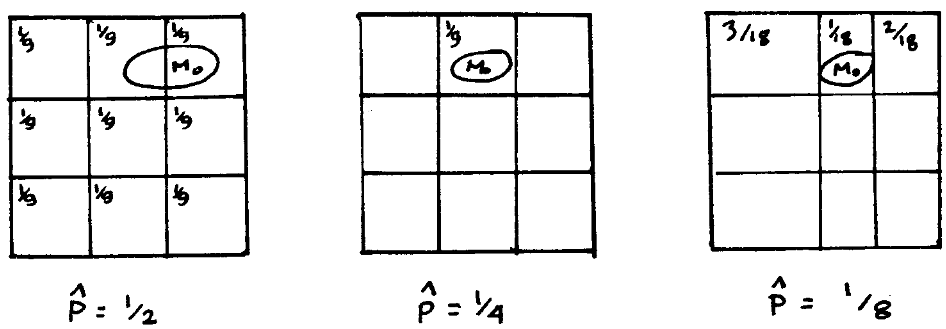
$$I(M_{experiment} \mid M_\Omega) = \sum_{i=1}^{n} \hat{P}(M_{0_i} \mid M_\Omega) \log \hat{P}(M_{0_i} \mid M_\Omega)^{-1}$$

where Ω is a set of previous observations. Using this notion of informativeness Randall explores how the formal language that characterizes a theory (and produces experimental hypotheses) can change so as to increase the expected informativeness of empirical observations.

I take it as reasonable in view of the above that an information measure can be meaningfully applied to representations of observations in the context of this thesis. With Fig. 15, I give one more brief diagrammatic example to demonstrate the differences in $\hat{P}(M_0)$ between languages that differ in the way they partition M and cover M_0. When M_0 is minimally covered by a basis element and the p-measure of the off basis elements gets small, then $\hat{P}(M_0)$ gets small. Thus, for those conditions $I(M_0)$ gets large.

The expressiveness of languages, in the sense that more expressive languages carry the expectation of proportionately greater amounts of information to be extracted from an observation, is a major issue of Randall's thesis and won't be developed here. It will suffice to say that the example is the very simplest possible consideration. Languages that are refinements of other languages are usually more expressive than the original languages. However, increasing the number of sentences occasionally can actually decrease the informativeness of a language (internal conflict can arise), raising the notion of a maximally expressive language for a given set of observations.

An important point to keep in mind in what follows is, from the observer's point of view, that one may be informed by either changes in the world relative to the epistemic frame (i.e., by observations) or by changes in the epistemic frame relative to a fixed observation. Changes in informativeness of a particu-



(a) (b) (c)

Fig. 15

lar observation or state of affairs is an essential feature of any account of central process or learning.

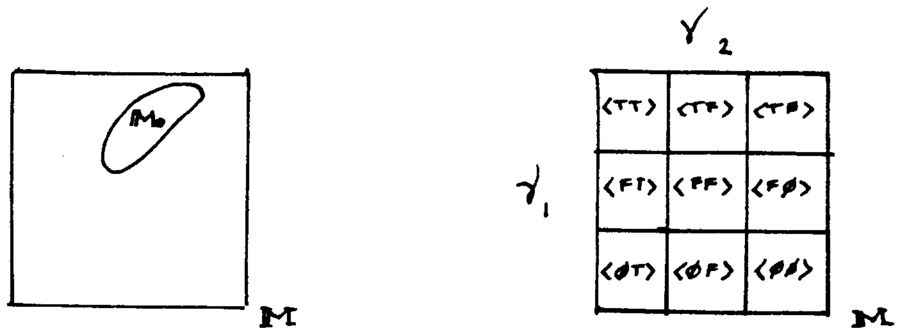
In the next section Randall diagrams and the concept of an information measure in relation to epistemic structure will be used to propose an information channel analogy of central processes. The channel analogy will be used in the next chapter to investigate aspects of the temporal control of movement.

5.3 A channel analogy of central process

A Randall diagram characterizes the elements of a formal language. The projection of observations on the partition represented in the diagram schematically portrays the interaction of evidence with the frame of discernment. The interaction is, in general, extralinguistic since the precise description of element and observation are possible only in the metalanguage. The "speaker" of a formal language deals exclusively with states; that is, can only assign belief to the truth or falsity of a sentence and not to a sentence that is meaningless. Assertions of truth or falsity regarding potentially meaningless projections of observations on the language-induced partition are limited in fidelity by the ignorance of the "speaker" implied by the indistinguishability conditions.

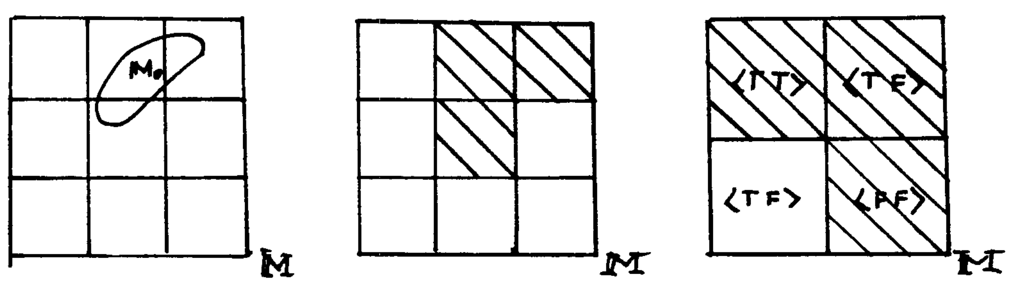
If one regards the description of an observation by a "speaker" from the perspective of the metalanguage, it is reasonable to view the utterance analog of the "speaker" as a noisy transmission of the observation. The ignorance of the speaker regarding the projection of the observation on the elements of the language is thought of as producing "noise" in utterances of the speaker concerning the observation.

Figs. 16, 17 and 18 will be used as examples to make the point. The projection of the observation on the partition in Fig. 16 is depicted in Fig. 17. If the partition is altered such that M_0 is covered by a single basis element, the



(a) (b)

Fig. 16 (a) The set of models, $\mathbb{M}_0$, associated with observation.
(b) A partition induced by a formal language $\mathbb{L}$ producing sentences γ_1, γ_2. $\mathbb{M}$ is the space of the models of set theory.



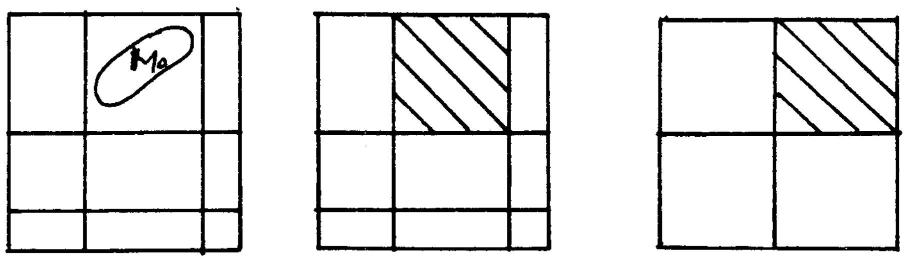
(a) (b) (c)

Fig. 17 (a) The projection of $\mathbb{M}_0$ on the partition induced by $\mathbb{L}$ on $\mathbb{M}$.
(b) The region indistinguishable to $\mathbb{L}$ viewed from the metalanguage.
(c) The speaker's perspective of the states indistinguishable from $\mathbb{M}_0$. The speaker cannot ascribe belief to elements with meaningless entries. These elements are all indistinguishable from a basis element.

situation is as in Fig. 18. It has previously been seen how calculation of $\hat{P}(\boldsymbol{M}_0)$ for L and L' as in Figs. 17 and 18 will give $\hat{P}_L(\boldsymbol{M}_0) = \frac{3}{4}$ and $\hat{P}_L{}'(M_0) < \frac{3}{4}$. Of course, then $I_{L'}(\boldsymbol{M}_0) > I_L(\boldsymbol{M}_0)$. Furthermore, it is clear that a partition minimally covering $\boldsymbol{M}_0$ and having all other basis elements as large as is mutually possible under the constraint of uniquely covering $\boldsymbol{M}_0$ will give $\min\hat{P}_{L'}(\boldsymbol{M}_0)$ and, therefore, $\max I_{L'}(\boldsymbol{M}_0)$.

If a representational system seeks to be maximally informed (by Randall's measure) by past observations and/or to have the expectation of being maximally informed by the next observation, then the language must change as observations are made. The pro- and retrospective properties of information are extralinguistic, involving as they do, characterization of observations. The prospective aspect is inductive in character, essentially being hypothesis formation. The retrospective aspect is confirmatory in character, being related to the ascription of belief. The prospective view constitutes the *creation* of expectation; the retrospective view constitutes the assignment of confidence. These are the two sides of belief, viewed from different directions in time. From the perspective of this discussion, the details of language change are inessential.

In what follows, to speak of a language or representational condition is limited to discussion of the partition induced on the model space or that partition in relation to observation. It should be recognized that the issue of language change is extremely important -- in part due to its inductive character and in part due to the fact that such change underlies a change in informativeness of objectively identical situations under observation. (For a view of the issue of change of beliefs and problems entailed see Fodor 1983, part IV, and Raphael 1971 as well as Randall 1970 and Kuhn 1962). To simplify discussion I usually assume the language to be fixed and will be explicit when I do not make that



(a) (b) (c)

Fig. 18 (a) $\mathbb{L}$ has been altered to $\mathbb{L}'$ such that $\mathbb{M}_0$ is covered by a single basis element.
(b) Viewed from metalanguage at resolution of $\mathbb{L}'$.
(c) Viewed from speaker's perspective.

(a) (b)

Fig. 19 (a) An observation covered by an element containing meaningless entries.
(b) The associated indistinguishable region from the perspective of the speaker.

assumption.

What is imagined is that the structure on M evolves in time in response to observations and according to some unknown criteria. At each instant the structure on M is called the *representational condition* and may often be thought of as the set of elements of a fixed formal language together with the *a priori* probability distribution. Involving elements, the representational condition is an extralinguistic notion.

A *representational event* is defined to be the expression of an observation by a system in a representational condition. Schematically one might imagine situations as in Figs. 17(b) and 18(b). A *minimal event* is defined to be the (perhaps partial) description of an observation by a single basis element. A minimal event is called minimal because a basis element is expressed by the "speaker" of the language after semantic evaluation of every sentence of the language and thus represents the finest grain of description possible. Since the expression of the minimal event requires every sentence to be articulated, a minimal event is isotropic (in the sense of central processes) and the associated belief is quineian. From the perspective of the speaker every observation can be thought of as a sequence (possibly a singleton sequence) of minimal events where one imagines any situation analogous to the following conventions.

In terms of minimal events, expression of an observation requires semantic evaluation of every sentence of L. Observations involving more than one element require successive interpretations of all of the sentences. Such is obvious where one understands the "name" of each element of L as a vector of truth values. This is portrayed schematically in Fig. 16(b) where the vectors of truth values can be seen to encode the elements. The sentences of L are thought of as occurring sequentially. The order of entries in the vector of truth values depends on the order of the sentence evaluation and the uniqueness of the code depends on the uniqueness of order. It is therefore imagined that the sentences are evaluated in a fixed sequence.

The conventions are directly related to the isotropy of the minimal event since a basis element is uniquely described only by all of the sentences of the language under interpretation. Every description of an observation will define a basis element or a sequence of basis elements under these conventions. That is not to say that, for every observation and every $M \in \boldsymbol{M}_0$, every M lies on the basis, but rather that every element is indistinguishable from at least one basis element to the speaker. A basis element defines a minimal representational event because it is the minimal, in the sense of being the finest grained situation in which any $\boldsymbol{M}_0$ may possibly be covered by a partition induced by L and expressed by'the speaker.

Consideration of Fig. 19 suggests, given basis states defined as in 19(b), that the question of which "source" elements of 19(a) are associated with them is a matter of probabilities and combinatorics, due to the indistinguishability conditions. Fig. 20 lists the source conditions that could give rise to 19(b), where the speaker assigns a truth value to every sentence of L and it is equally likely that a meaningless sentence is afforded a value of true or false. All of the sentences must be evaluated at least twice in this representational condition to define basis states associated with the observation.

In this situation, where one views (from the metalanguage) the projection of $\boldsymbol{M}_0$ on the partition of $\boldsymbol{M}$ by L as the source code and the partition implied by the utterance of the speaker as a received code, it is seen that an information channel is described. There is considerable equivocation associated with this channel since many source configurations can give rise to a single output.

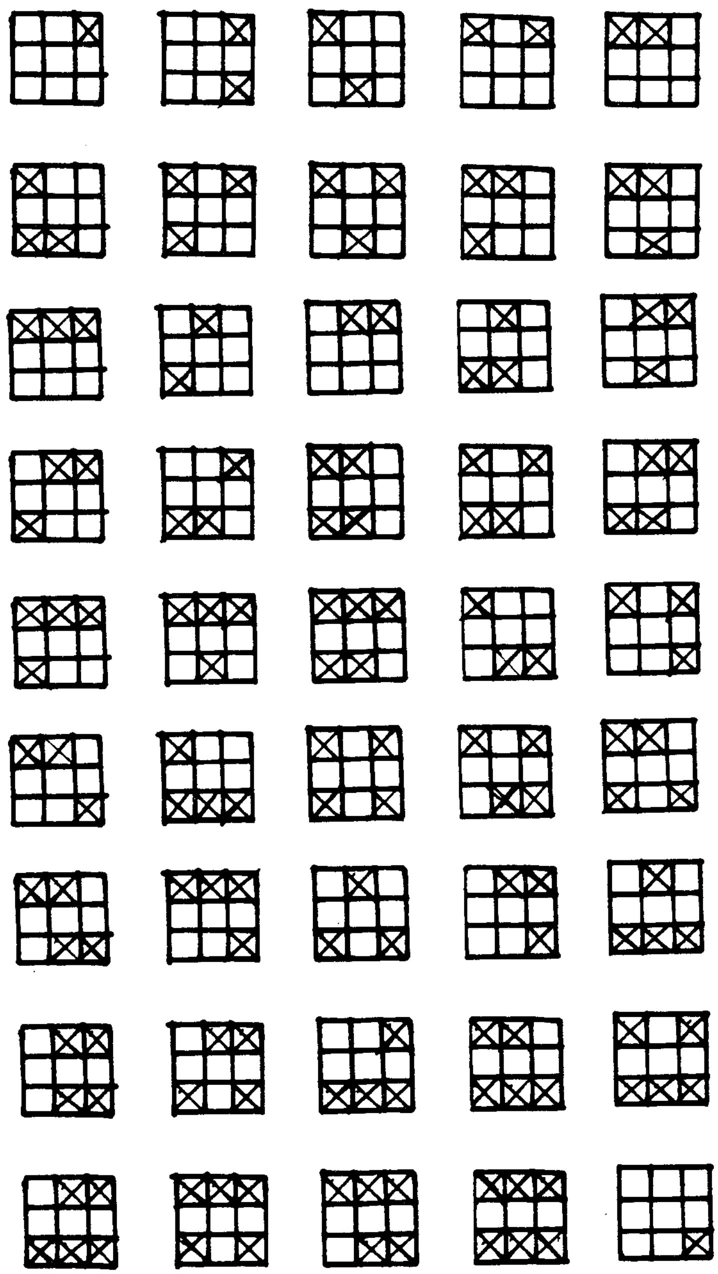


Fig. 20

Reciprocally, repeated access to the same observation will give rise to sequence of output codes (e.g., either one of the codes of the basis elements indicated in 19(b)).

To ensure the uniqueness of the codes of the elements, the order in which the sentence truth values appear in the vector must be fixed. That requirement is logically equivalent to the convention that the sentences be sequentially evaluated. Therefore, a convention of regarding the extension of the representation process in time is analogical, referring to the order requirement of the code vector entries. It is conceivable that the structure is defined in parallel and therefore without actual extension in time. But the requirement for the code vector entry order cannot be relaxed and the convention of time ordering the evaluation of the sentences is formally equivalent to the order requirement of the vector entries in the sense that any system analogous to the time ordered system can be put in the normal form of the time ordered system. Later it will become clear that the time scale of the information process is, in some sense, arbitrary. The above convention, a figmentary time scale, will not introduce a spurious time base to the later developments.

It is now necessary to put the above in a psychologically and biologically relevant context. The functional taxonomy of central processes given in an earlier chapter will be essential to this discussion.

The situation depicted in Fig. 16(a) can be taken roughly as transducer output: a set of possible structures underlying proximal stimuli that are projectable onto world states, rather in the sense of J. J. Gibson's "layout", or relevant structural invariances, of a distal stimulus (Gibson 1950, 1966, 1979). I do not have much detail to provide an account of transducer functioning except to identify it in the channel analogy. The Fig. 16(a) is, then, diagramatic representation of a "signal" in information system-type terms. It is important to notice that this signal is already internal to, say, a person and consists of possible structures (for a subset of terminal symbols or words corresponding to objects in the domain) that may or may not possess objective veridicality.

Figs. 17(a), (b) depict the interaction of the observation, and the knowledge structure embodied as a formal language. Fig. 17(c) characterizes the "channel output" or "speaker's utterance" which may be taken as an assertion that an arrangement of things in the world that the signal refers to exists. The assertion has the form of a description with a belief measure on it.

The "channel" or "speaker" here relates the process of coding and transmitting (or of parsing and processing, as one's preferred metaphor requires) and corresponds to the notion of input system in Fodor's taxonomy. The output of this system is the stuff on which belief is measured and in relation to whiich information transmission is defined. The structure and outputs of the input system are the operanda of central processes in the taxonomy.

It is important to notice two things here. Input processes do not ***translate*** the transduced stimulus for the central process. Translation is ideally an information preserving transformation; information processes are clearly not such since they impose tacit structure on the observation (see Fodor 1983). The second thing to notice is that Fodor suggests that sensory transduction is not the sole domain of input processes. For example, one can be ***told*** to expect to smell peaches, be conditioned to expect to smell peaches, or simply smell peaches and hold the expectation that peaches will be smelled. It is also obvious that mentation occurs in the absence of sensory transduction. For example, consider sleep and dreaming. Koukkou and Lehmann (1983) assert that " . . . dreams are the recall of cognition/mentation which occurred during sleep."

There is evidence that sensory information is not processed during sleep (which may indeed be the psychological *definition* of sleep), e.g., Simon and Emmons (1956), Emmons and Simon (1956). (However see Beh and Barratt (1965), Wienberg (1966), and Berger (1970) where either operant conditioning or incorporation of verbal material in dream contexts may have occurred). Fodor asserts the taxonomy requires that the input systems include the perceptual systems and, additionally, the language systems. It is self-evident that the present characterization captures that feature of the taxonomy since it is obvious that, if a formal language might depict anything, it could depict a language. That it is natural for the characterization to include perceptual systems is actually the more intriguing implication (see Rock 1983).

The essential thing to realize is that the *percept* must be something like an *assertion* that certain features and relations exist *in the object* and is *not* of the kind "picture". The distinction seems often to be lost in neuroreductionistic theory and practice where what are essentially pictures (e.g., conformal mappings from object projections on retinae to striate cortex) are apparently often understood as the neural equivalent of percept (see Uttal 1981, Freeman 1983, Rock 1983, Pylyshyn 1984).

Finally, it is necessary at the outset to provide certain caveats. The constructivity requirement of mental representation modeled as a formal language should not be interpretatively taken to literal extremes. The tendency, for example, in the Fourier theories of vision is to propose "channels" and subsequently propose certain anatomically distinct neurological features that actually constitute these channels or, simply, to make the *a priori* assumption that neuroanatomically distinct features underlie the psychophysical phenomena by necessity. There is a great deal wrong with that (e.g., see Uttal 1981) in principle and detail. I do not intend to imply the necessity of anatomical locality of the channels I propose; even the plausibility is not absolute. It would be very suprising if unique anatomical correspondence could be established for central processes. Indeed it is a topic of this thesis that such is exceedingly implausible. One does not abandon materialism with these qualifications, but it is essential to confront the implications of isotropy, functional equivalence, the content opacity of codes and the evolution of epistemic frames without the more banal axioms of naive neuroreductionism involving the central state identity hypothesis.

Preliminaries aside, the channel analogy is rather elementary. Fig. 21 exhibits a general schematic of the proposed channel. Central processes should be interpreted to act on the elements they enclose in the figure; the diagram is *not* intended to imply that central processes *include* input systems, etc. To characterize the channel, I will consider minimal events and proceed by example.

By the above conventions, the channel can be treated as a discrete memoryless channel where the source and channel coders are formally separated and the channel coder is considered to be a block encoder. Fig. 22 illustrates the channel to be discussed. It is formally necessary simply to consider the mapping of the integers enumerating the elements, which comprise unique source states, onto the truth vector code words, as the encoding. By the data processing theorem it is then enough to analyze the section of Fig. 22 extending between the channel coder and decoder and include the prior distribution of the source. Fig. 22 characterizes the *instantaneous* representational situation of the system. In general, a sequence of such channels will constitute a model of central processes. The properties of the channel are to be understood as instantaneous properties of the modeled central process.

In Fig. 22 the representational condition is depicted as the projection of M_0

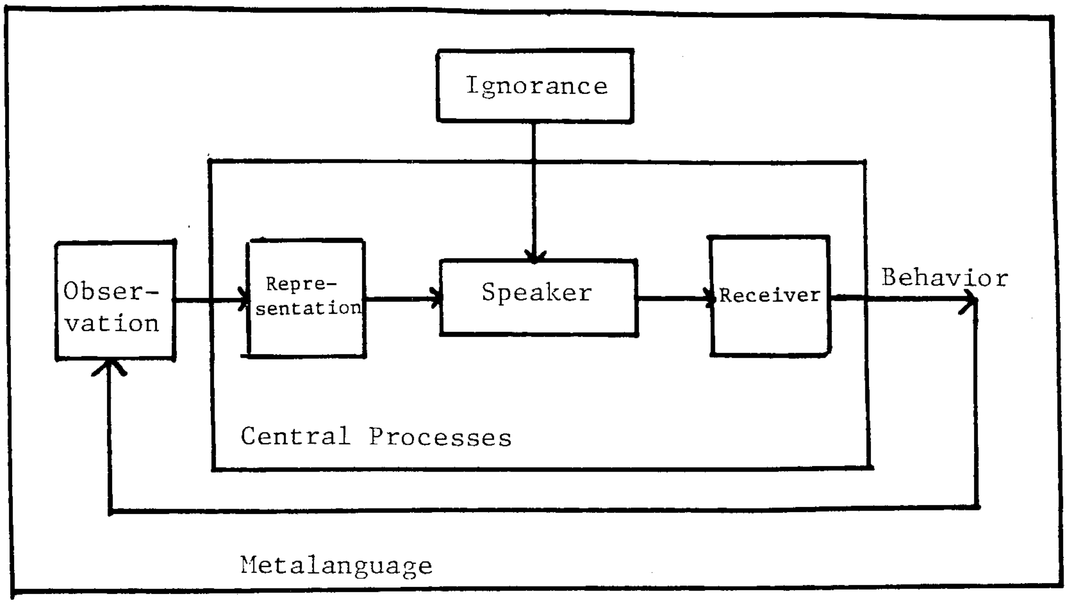


Fig. 21

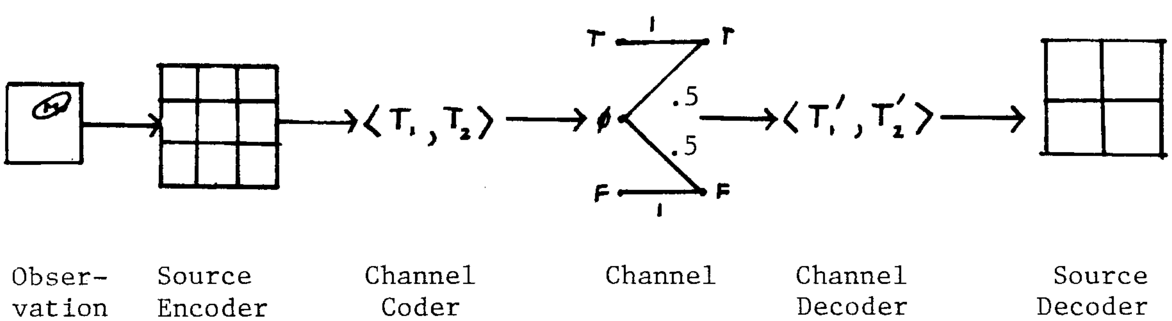


Fig. 22 Channel characterization of a representational event for a system in a representational condition characterized by $\mathbb{L}$ consisting of two sentences.

on the partition of $\boldsymbol{M}$ induced by a formal language consisting of two sentences. The channel model captures the isotropy condition because every sentence of the language is required to be evaluated with respect to the models of $\boldsymbol{M}_0$ covered by the partition. Where a sentence is meaningless, the speaker assigns true or false equiprobably. The resulting utterance code, i.e., channel output, will always be indistinguishable from some basis element. If the observation is evaluated many times, the channel output will be a set of basis elements corresponding to those basis elements indistinguishable from $\boldsymbol{M}_0$.

The ignorance of the speaker regarding the source is manifested in the inability to distinguish two source codes differing only by meaningless entries in the code vector, e.g., <T ϕ> is indistinguishable from <T T> and <T F>. The fidelity of the source code depends on the language and is opaque to the speaker.

Where $\boldsymbol{M}_0$ is not covered by an element with meaningless entries in the channel input code vector, many evaluations of the observation will result in channel output consisting of a set of codes of basis elements identical to the codes of basis elements covering $\boldsymbol{M}_0$ and in relative frequencies defined by the prior distribution. Every set of basis elements constituting a sequence of channel block outputs corresponds to a sequence of minimal events. The length of the output sequence of minimal events given a fixed observation is a measure of the representational "stability" of the system. That is, for a fixed observation, the number of events required to describe it is a measure of "stability" of the representation. For an observation covered entirely by a single basis element, the length of the minimal event sequence is one. When such an observation is made repeatedly, the system is defined to be representationally "stable" because the output state does not change.

The intuition of this definition of stability is that each basis element received reflects a representational state of the system. For a fixed observation a stable system does not change states. The degree to which a system is unstable is precisely the degree to which, for a fixed system input (observation), the system changes states.

Suppose that by M_0 one means the common model set of a number of consistent observations. If the language tends to change such that M_0 is covered by a single basis element, the system depicted by the language tends to stability.

It is reasonably clear that the expected value of the stability measure, that is, the expected length of the event sequence that defines the observation, is, in the generality required for conceivable formal languages, a rather involved stochastic process. It seems nonetheless to be a purely mathematical question. What is interesting, psychologically speaking, is that, if a system tends to stability, the fidelity of the source code is improved. The fact that the representational condition has no access to the actual world and therefore no apparent way to directly assess the source code fidelity makes this aspect of the above definition of stability, a condition measurable purely by properties of the speaker's description of an object, remarkable.

To illustrate the channel model and motivate the derivation of the representational event rate, an example will be given. Gallegher states (1968, pg 71), "A transmission channel is specified in terms of the set of inputs . . . .the set of outputs . . . and for each input, the probability measure on the output events conditional on that input." In this example these will be provided in reference to Fig. 23. In the figure, a language generating two sentences induces a partition on M with a prior distribution that assigns unequal probabilities to the elements of the language. The elements are denoted e_1 to e_9 and the prior probability associated with element e_i is denoted p_i. The probabilities associated with the

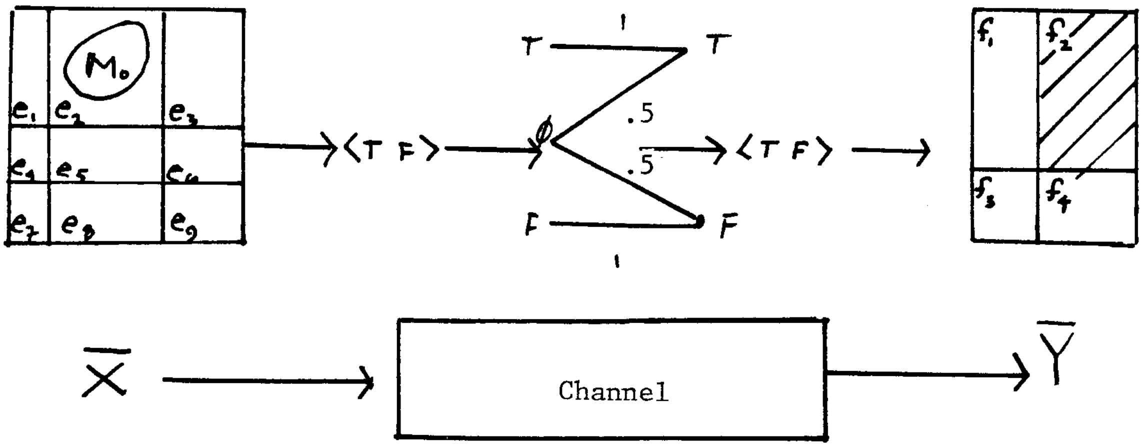


Fig. 23

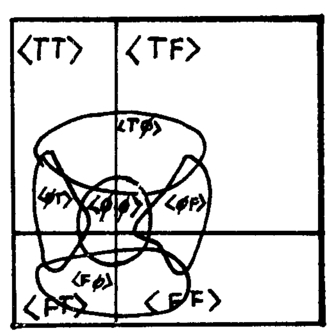


Fig. 24 Receiver indistinguishability conditions for Fig. 23.

received elements, $\boldsymbol{f}_i$, are denoted $\boldsymbol{p}'_i$.

Indistinguishability conditions of the receiver with respect to the source are depicted in Fig. 24. The figure shows how the probabilities of indistinguishable elements are distributed over the basis. Using the channel transition probabilites (see Fig. 23), the marginal distribution of Y gives

$$p'(f_2) = P(Y = f_2) = \sum_x p(Y = f_2 | X = x) p(X = x)$$

$$= p(Y = f_2 | X = e_1) \cdot p(X = e_1) + \ldots + p(Y = f_2 | X = e_9) \cdot p(X = e_9)$$

$$= p(Y = f_2 | X = e_2) \cdot p(X = e_2) + p(Y = f_2 | X = e_3) \cdot p(X = e_3)$$

$$+ p(Y = f_2 | X = e_8) \cdot p(X = e_8)$$

$$+ p(Y = f_2 | X = e_9) \cdot p(X = e_9)$$

$$= p(X = e_2) + .5p(X = e_3) + .5p(X = e_8) + .25p(X = e_9)$$

It is easy to verify that $\hat{P}(f_2)$ will give the same value, as will Dempster's Rule. The marginal of Y is a belief measure properly thought of as a measure on the speaker's utterance, where the indistinguishability conditions due to meaninglessness as depicted in Fig. 24 are taken into consideration.

The marginal of X is the prior distribution induced by the formal language. Since the input and output distributions are defined, the entropies

$$H(X) = \sum_x p(X = x) \log p(X = x)^{-1}$$

and

$$H(X|Y) = \sum_{y} p(Y=y)H(X \mid Y=y)$$

are defined. Therefore, the mutual information,

$$I(X;Y) = H(X) - H(X|Y)$$

is defined. The equivocation is the loss due to indistinguishability and therefore is a measure of the speaker's ignorance due to the meaninglessness conditions of the epistemic frame. It has been assumed here that the basis is unique, which is not, in general, true. For cases of multiple bases, it is assumed that one of them is adopted for each minimal event.

The mutual information defined with the language induced prior distribution and indistinguishability conditions is the information transmitted by the speaker's assertions concerning the object represented by the source: the speaker is a noisy channel. In psychological terms, the relationship between the representational condition and a representational event is communication in the presence of ignorance and characterizes an input system.

Suppose the source consists of M elements. The channel block codes these elements with code length equal to the number of sentences, denoted n, generated by the formal language, L. The rate of the block code is

$$R = \frac{\log M}{n} \tag{25}$$

in bits per source symbol. This rate is achieved only where the M elements are equally probable.

A channel with transition probabilities as in Fig. 22 can achieve channel capacity, C = 1 bit/unit time (see Gallegher 1968, pg. 94).

By the source-channel coding theorem (see McEliece 1977) the average system rate in symbols/unit time is

$$\bar{r} \leq \frac{C}{R}. \tag{26}$$

Define τ^{-1} to be the representational event rate with units minimal event/unit time. Define ρ^{-1} to be the number of minimal events per source symbol. From equation 26 and these definitions it follows that

$$\tau\bar{r} = \rho, \tag{27}$$

and since, for a single codeword transmission, each source symbol is indistinguishable from one basis state, each of which comprises a minimal event, $\rho^{-1} = 1$. Thus one obtains

$$\tau\frac{C}{R} \geq \tau\bar{r} = 1.$$

By equation 25 and the channel capacity,

$$\frac{C}{R} \leq \frac{1}{\left\lceil \frac{\log M}{n} \right\rceil} \leq \frac{1}{\left\lceil \frac{H(X)}{n} \right\rceil} = \frac{n}{H(X)}.$$

Thus,

$$\tau^{-1} \leq \frac{n}{H(X)}. \tag{28}$$

Define the maximal event rate for minimal events to be the measure of representational system rate,

$$\hat{\tau}^{-1} = \frac{n}{H(X)}. \tag{29}$$

This is intended to be an *instantaneous* rate because the formal language defining the representational condition is fixed in the derivation. It is correct to write equation 29 explicitly as a function of time to capture that the representational condition is not in general fixed and that the event rate therefore varies,

$$\hat{\tau}^{-1}(t) = \frac{n(t)}{H(X(t))}. \tag{30}$$

One is left with the problem that the time scale is unknown (and, in general, likely unknowable). That problem will be addressed in the next chapter where representational event rate is used to predict behavioral consequences of central processes on the temporal control of movement.

An evolutive principle is postulated to motivate language change using the channel analogy. Various equivalent interpretations of the principle will be given. The principle is written:

$$I(X;Y) \rightarrow \max(I(X;Y) \mid H(X \mid Y) \rightarrow 0) .$$

That is, the equivocation tends to minimum and the source entropy tends to maximum under constraints imposed by observations.

This is a stability-seeking principle for the representational system. On the one hand, as $H(X \mid Y) \rightarrow 0$ (recall we are discussing minimal events and sequences of minimal events) the elements that are not on the basis shrink (i.e., their *a-priori* probabilities get smaller) and the set of models, M_0, tend to be covered by a single basis element. Thus, repeated observations tend to be described by fewer basis elements, i.e., shorter event sequences. In consequence, the expected number of state changes per observation becomes smaller. Since these representational events are assumed to be physically instantiated events, the statement is that the physical system state tends to a condition where it does not change much for a repeated observation or successive consistent observations.

On the other hand, as $H(X)$ grows the elements tend to equal size (i.e., tend to a condition of equal probability). The effect is that, given the previous tendency, that the event sequence will tend to be as short as *possible*. That is apparent if one considers that, if M_0 is uniquely covered and all elements are mutually as small as possible, the *a priori* likelihood of changing states with the next observation is as small as possible. One might say, given the effect of observation histories on $H(X|Y)$, that this aspect of the evolutive principle causes the *a priori* probabilites to tend to a least biasing value for future observations. Inspection of equation 30 shows that as $H(X)$ increases, the event rate decreases for fixed n. Thus, the principle states that the event rate tends to be as small as possible, given observation history.

It has recently come to my attention that the above principle is, at least in form, given by Christensen (1981) as the "entropy minimax". Therefore, I will call it that although I cannot compare this application to applications elsewhere. The following quoted table (Christensen (1981), pg. 298) is found in a section on the examination of variable definition and analysis in statistical modeling.

Entropy Minimax Reasoning - Christensen (1981)	
Entropy Maximization: $H(X)$	Entropy Minimization: $H(X \mid Y)$
What is being varied is the set of numerical values of outcome probabilities for each given condition.	What is being varied is the set of feature space boundaries defining the conditions to which the outcome probabilities apply.
The objective is to minimize bias, i.e., assertion of information in excess of that actually available.	The objective is to maximize information extraction from the independent variable data.
This is achieved by maximizing the entropy of the individual outcome frequencies, subject to given constraints and data.	This is achieved by minimizing the conditional outcome entropy (subject, of course, to minimization of bias under each condition).
Intuitively: This fixes the outcome probabilites at values which contain the least bias by maximizing the information expected to be learned by further observation.	Intuitively: This organizes (partitions) the independent variable data so as to extract from it a maximum of information about the dependent variable by minimizing the information expected to be learned by further observation.

The above seems to correspond with the considerations of this section.

If the *a priori* distribution on the partition induced by the formal language is such that the off-basis elements have small p-measure then (considering the channel and the transition probabilities) for an input code vector entry, $p(\phi)$ gets small. That is the effect of $H(X \mid Y) \to 0$. If, in addition, the p-measure of the elements tend to equality (i.e., $H(X) \to \max H(X)$) then $p(T) \to p(F) \to \frac{1}{2}$ for the input code vector entries. Notice $p(\phi) = 0$, $p(T) = p(F) = \frac{1}{2}$ are the conditions on the input code vector entry probabilites that achieve channel capacity. Thus the minimax drives the channel toward capacity.

Lastly the minimax can be seen to be ignorance diminishing where absolute ignorance is defined in the case where plausibility is unity and belief is zero (recall the vacuous belief function, equation 23). This interpretation comes from the fact that, as the observation comes to be uniquely covered by a basis state and as the elements with meaningless entries in their code vectors get small in the sense of their ***a priori*** probabilites, Dempster's upper and lower probabilites converge. Then there will be, in the limit, no increase in plausibility without a corresponding increase in belief, which constitutes an ignorance diminishing situation in view of the definition of ignorance above.

This channel model of representational processes captures the ideas of the propositional form of representation of Chapter 4, the taxonomy of mental processes of Chapter 3 and allows the formalization of the notion of representational event. In the next chapter the representational event and event rate will be used in relation to movement timing.

6. Control of Movement Timing as an Example of Central Process

In this chapter, the representational event rate defined by the information channel analogy will be used to propose an experimentally testable influence of central processes on motor control. It is postulated that the percept of constant duration influences the intentional production of a subjectively constant rhythmic movement. The percept of duration is considered to be a phenomenon associated with sequences of mental events in a self-stocked representational system without access to an internal time standard. The event sequence construal of time percept and the postulate of a central process effect on movement timing are formalized and, together with the results of the last chapter, movement timing predictions are obtained. The next chapter will experimentally investigate the predictions.

6.1 Timing processes

Motor movements have essential time coordinates and the basis of timing in the production and control of a movement has many components. The dynamics of the limb-muscle structure and resonance properties of the controlling neural network have observable consequences for the timing of periodic movements. Where neural representations of dynamic properties control feedforward motor signals, the represented movement analog embodies periodic processes that are encoded in the efferent signal. Also, the intention to produce a rhythmic movement of constant period can serve, presumably by involving the percept of constant duration or rate, to control the timing of a movement.

The intervention of knowledge and intentions on the timing of motor activity obviously greatly complicates the attempt to understand motor control; however, the fact of that intervention has important applied implications. As a single example (Christensen and Talbot 1984), a cognitive level, workload-related, perceptual "time compression" effect has been attributed to motor sequence abnormalities that degrade spacecraft mission effectiveness by interfering with the manual control of such things as docking. Extensive training apparently does not eliminate this effect, indicating that a clearer understanding of cognition, time perception and motor control have practical utility.

6.1.1 The sense of time

The percept of time is voluminously present in the biological and psychological literature. It is a percept that is often not veridical and that possesses no associated receptor organ. Its analysis has been full of controversy and confusion and suffers greatly from arguments entailing the same words on both sides but having denotata at incommensurable levels of analysis.

The most common object of investigation is the search for a clock, a relatively stable internal oscillator, in relation to which the percept is to be derived . The full range of such rhythms has been discussed as clocks underlying the time sense. I will not review these for two reasons. They are available in many reviews of literature; three excellent reviews from very different perspectives are given in White (1963), Holubár (1969), Ornstein (1969). Secondly, I believe that this construal of the percept of duration or rate is flawed in principle and therefore I see no reason to encumber discussion with details. I will address that implausibility in the next section.

The two approaches that I do take seriously are (1) that the percept of duration is derivative of a succession of mental events; roughly, that more or fewer mental events in a unit clocktime interval will, respectively, lengthen or foreshorten the subjective experience of that objectively constant interval, and (2) that the number of mental events associated with a time interval derives from the complexity of the representation of observations occurring in that interval. Together these imply that the time experience is a cognition-related phenomenon.

Evidence for the first can be gathered from the work of White[1] and his co-workers (Cheatham and White 1952, White and Cheatham 1953, White 1963, Lichtenstein *et al* 1963). They adopted a paradigm in which a small spot of light was flashed at different rates and at different retinal eccentricities. The subject would either report the number of flashes seen in a time interval or attempt to match the flicker rate, thus obtaining a measure of subjectively perceived rate.

They investigated the working hypothesis that a centrally mediated psychological unit of duration, a psychological moment, existed. During this moment successive stimulus order is assumed to be lost. The empirical equivalent is the "indifference interval" -- a time interval during which successively presented stimuli are perceived to occur simultaneously (see Fraisse 1966). Sequences of these moments are hypothesized to underlie the percept of duration attributed to a time interval. Notice that it is neither necessary or sufficient, but is admittedly *optional*, to presume these moments are themselves produced at a fixed rate for this construal of time perception.

It was observed that the apparent flash rate of an objectively constant flicker decreased with increasing displacement from the fovea. It was also shown that retinal time aliasing could not account for the effect since the retinal response as measured by the electroretinogram (ERG) could follow the stimulus at rates up to five times higher (125 flashes per sec) than those actually presented. Thus the effect was due to higher level processing.

A paradox appeared to exist when the flash count derived rates and matched frequencies were juxtaposed. The apparent number of stimuli seen at all

[1] I wish to thank Dr. Carroll White for introducing me to, indeed providing me with, this literature.

eccentricities was approximately constant (a count rate of approximately six flashes per second at all eccentricities) but the perceived rate varied radically, being in a ratio of approximately 5:1 slower at a 70° displacement than at the fovea. This paradox can be resolved only if one is willing to accept the notion that the perceived rate is equal to the perceived number of flashes over the perceived duration and not the actual time extent of the stimulus presentation. That is, a variable psychological moment seems unavoidable. Lichtenstein *et al* were willing to postulate many such relatively fixed "time scales." I am going to suggest a more general interpretation and allow a truly variable time scale. Although a minimal moment duration may exist, that is not an essential feature of my approach.

Other evidence indicating that flicker rate influences the sense of time is found in Holubár (1969). A galvanic skin response (GSR) was classically conditioned by pairing a tone and an electrical shock. The shock occurred from three to thirty seconds after the tone and after many trials; when the shock was omitted, a measurable change in the GSR occurred at precisely the time coordinate of the expected shock. When full field flicker of nine to twenty-five flashes per second occurred between the conditioned and unconditioned stimuli, the onset of the GSR occurred earlier than when flicker was not presented. It would be of interest to see if these shifts varied with eccentricity of stimulation from the fovea, but unfortunately, that experiment was not done.

That the expectation of shock some seconds after a tone is time-shifted by visual stimulation strongly implies that intermodal influences on the sense of time exist. This is evidence for the centrality (in the taxonomy of Chapter 3) of the percept of time due to the representational isotropy that expectation derived from many senses seems to imply. That is, the *same* sense of duration that is altered by the flicker is apparently operational in the anticipatory response conditioned by a tone and a shock.

The span of the indifference interval has been an illusive measurement. White (1963) reports, where the interval is defined to be the variability in fingertapping rate in a flash rate matching task, that with increasing retinal eccentricity of the stimulus, the tapping rate decreases but variability is approximately constant. This definition would thus seem to be in conflict with the conclusion of different subjective time bases used in explanation of the subjective number/rate paradox. However, the possibility of predictive subsystems may account for the constancy.

There are fingertap data in rate matching tasks that support a variable timebase hypothesis where rate variability is taken to give the width of the indifference interval. Michon (1966) showed that tapping variability increased in a rate-matching task with increased complexity of a secondary task. The variability also is inversely related to the rate of tapping. In the first example it is clear that increased informational demands exist on the subject in cooccurrence with changes in the putative psychological moment. In the second case it is likely that inreased saliency of proprioceptive information and ceiling effects on achievable tap rates decrease the influence of central factors.

To summarize briefly the state of play, there is reason to take as a working hypothesis that sequences of perceptual-mental events which are, in part, characterized by a psychologically constant but objectively varying time scale, underlie the percept of clocktime intervals. From what such variability might derive is the next question to address.

Ornstein (1969) in a series of experiments demonstrated that increasing the complexity of stimuli presented in a fixed time interval caused the percept of the duration of the interval to be correspondingly lengthened. He accounts for this effect by use of a "storage-size" metaphor. According to this hypothesis, as the storage size (in, say, bytes) required to encode an input increases, the processing of the input requires more computation; that is, processing involves more mental events.

Since the events constitute the only representation of succession in the system (i.e., no homunculus to watch a clock is postulated) to the extent that more events occur in a fixed time interval, one would predict the experience of that interval to be lengthened.

Notice that it is the complexity of the ***representation*** of the input that is operational here. The same stimulus may be differently represented and obtain different temporal effects. Indeed, taking that perspective, one may incorporate the previously mentioned data and theory of White into the storage-size metaphor. The stimuli applied at the various retinal eccentricities were identical, but the complexity of the transducer's functional architectures delivering the encoded form of the stimuli to input processes was clearly not identical. Retinal interconnectivity and, therefore, complexity is much higher at the fovea than elsewhere on the retina and decreases as a function of eccentricity. In the absence of any projectable structure in the stimulus (a point flash is essentially without structure) it might be assumed by the storage-size hypothesis that complexity of the code imposed reflexively by the functional architecture of the transducers would be detectable in the time percept. Notice that increasing complexity would objectively foreshorten a self-referentially constant interval and thus would be expected to objectively increase a subjectively constant rate. That is precisely what is seen to occur. The perceived rate is inversely proportional to the displacement of the stimulus from the retina.

I therefore adopt a view of a subjectively constant time scale that objectively varies as a function of the information processing demands that the stimulus encoding places on the representational system. I envision a self-clocked, event driven system without reference to a clock standard. All levels of the processes of mental representation are expected to be involved in the behavioral observables associated with tasks that require production of movements at subjectively constant rates or judgements of intervals of subjectively constant duration.

6.1.2 Implausibility of an objective clock standard

The position to be taken here is that there is no relevant construal of "clocks" in a representational system that accounts for the percept of time. I will outline the argument and then state it more carefully. By a clock device I mean any dedicated objective time standard. If one entertains the idea that a clock device would supply a proximal analysis of the percept, then one must postulate a system to observe it. But this extra system must either possess the percept of time or count, that is to say perceive, a sequence of outputs from the clock without a percept of duration. The first consideration is obviously vacuous. In the latter case the extra system has been relegated to performance of an accumulative function of monitoring sequences of system states, the clock state being included in the overall system state description. But one obtains a state sequence where any representation state change occurs and not by necessity with inclusion of a clock in the representational state description. By Occams razor, a state sequence construal of time percept should not by necessity involve a clock. This can be more carefully argued as follows: By clock, I mean an oscillatory subsystem that provides a representational system with an objective time base or standard. If we allow such a clock to underlie the time percept we are obligated to provide a system to sense, observe it. There are three possibilities:

1. simply imbue the observing system with the ability to perceive time
2. postulate a system that accumulates clock states, thereby defining a sequence
3. assume that the representational system is coupled with the oscillatory system such that the representational state is redefined in time intervals defined by the period of the oscillator cycle.

These arguments can all be dismissed as follows:

It is obvious that (1) vacuously pushes the level of analysis to an arbitrary level of no apparent limit. Neither necessity nor sufficiency can be met for explanation or description of the percept.

Considering (2), note that a definition of sequence can be obtained without appeals to an objective time base. The representational system may be self-clocked if any conceivable encoding of state change captures the notion of sequence. Therefore, the necessity of the clock is not established. However, sufficiency remains plausible. Hence, sufficiency must be dismissed empirically or by establishing the equivalence of (2) with a setup that may be dismissed in principle. I will take the latter course, and will characterize (2) as a case of (3), then dismiss (3) as circular, thereby providing the counter example to disprove sufficiency. Necessity and sufficiency will thus be unfulfilled and (2) will have been found to be theoretically inessential.

If we take the setup envisioned in (3) and restrict it to be such that each oscillator cycle is input to the state description, then we reduce (3) to (2); that is, (2) is a degenerate case of (3). Then we immediately see that the argument for the lack of necessity of (2) is operational with regard to (3). The argument which follows denies sufficiency to (3) and by the above observation, also denies it to (2). Notice that (3) corresponds to the usual view of the functionality of, for example, circadian cycles providing a state sequence time standard. Taking that perspective and without loss of generality, I note that support for such a view is argued to exist when time variations in the cycles may be correlated with time variations in observable periodic behaviors and stimulus conditions. Variations in the stimulus environment are implied to operate through the circadian time base on whatever representational states are responsible for the integration in time of the observable behaviors. I point out, however, that any stimulus effect on the system producing the behaviors must operate through the representational states that characterize the stimulus environment to the behaving system. Therefore, changes in the circadian cycle time produced by the stimulus environment must be effected through the representation states characterizing the stimulus environment. But then the putative time base of the representational system, an objective witness of duration independent of representation, is dependent on the state of the representational system. This is a circularity and, therefore, (3) may be dismissed as insufficient in principle.

I conclude that such things as oscillatory systems, insofar as they exist and vary with respect to stimulus contingencies, must at most be taken to be representational of the character of certain aspects of the stimulus contingencies and do not constitute an instantiated explicit representation of objective time. I seek a characterization in which duration as a theoretical construct arises as an implicit function of representation. Such a characterization is more general and allows greater theoretical parsimony. Of course, oscillatory systems exist that play essential roles in neural and neuromuscular functions and underlie cognition as well as motor control. I seek only to generalize the notion of representation to encompass duration and rate and thereby to provide theoretical unity and to avoid circularity.

6.2 A central process view of the percept of duration and system timing

I will take the view that what is needed for an account of a time percept is a self-clocked representational system, that is, a system that enters into states, and it is the sequence of states alone that constitutes the representation of succession in the system. I will call the situation where a state changes an "event" or a "representational event." The system states under consideration are representation states and are taken to be the expression of the experience of observational evidence. It is postulated that the sequence of states is the basis of the experience of duration; thus each event is a self-referential unit interval, and the relativistic measurement of the percept of clocktime is understood as related to the rate of change of representational events. I will now formalize the event sequence construal of the time percept to make clear that it accounts for the sense of time.

Since, self-referentially, the rate of the system state sequence is states/event, the self-referential rate is always unity. I define a cumulative event function, Ψ, taking the subjective duration parameter τ as in Fig. 25. The self-referential rate is unity; the figure illustrates the tautology that the cumulative event function, $\Psi(\tau)$, increases as the number of events.

Fig. 26 shows that the accumulation of events is nondecreasing as a function of time. This must be so or the system could not be a causal system as will be seen in reference to the next figure. If the subjectively constant duration parameter is seen as a function of time, notice that it is the scaling function that sets the slope of the cumulative event function, as a function of time, to unity. Fig. 27 shows a scaling of the ordinate that obtains unit slope for the cumulative event function. Call the scaling function, $\tau(t)$, and remark that scaling has the form, $\{(t_i, t_j]\}$, the set of half open time intervals such that

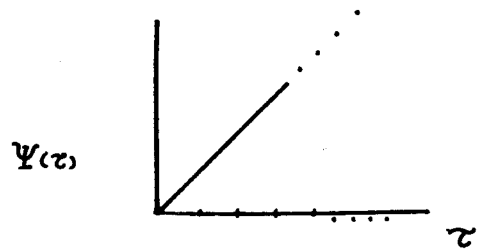


Fig. 25

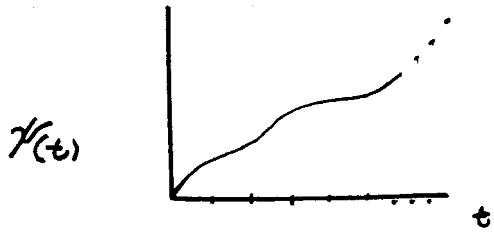


Fig. 26

Fig. 27

$$\int_{t_i}^{t_j} \frac{d\eta}{\tau(\eta)} = 1 . \tag{31}$$

It is intuitively clear that the above is the form required if one notices that were the event duration constant in time, the number of events in time t would be given by t/τ. Obviously, we want the subjectively constant event duration to objectively vary continuously, hence, the cumulative event function is given by:

$$\psi(t) = \int_c^t \frac{d\eta}{\tau(\eta)} . \tag{32}$$

The meaning of the scaling function is now clear, the time interval in which an event is enumerated.

Notice that two time intervals are perceptually equal, where $(t_0, t_1], (t_2, t_3]$ are such that

$$\int_{t_0}^{t_1} \frac{d\eta}{\tau(\eta)} = \int_{t_2}^{t_3} \frac{d\eta}{\tau(\eta)} \tag{33}$$

and that the percept of an objectively constant clocktime rate is given by,

$$\frac{\partial\psi}{\partial t} = \tau(t)^{-1} . \tag{34}$$

This is not constant in general and is related to the representation state of the system. Reciprocally, the percept of constancy may be associated with an objectively varying rate. Notice that if the cumulative event function decreased, the time order of the scale would be lost and the system would thus not be a causal system.

The central process view that the representational condition influences motor control directly is captured in the case of the temporal control of movement if we create a condition where the production of a periodic movement has subjectively constant rate. The central process view postulates that:

$$\omega(t) = k\,\frac{\partial\psi}{\partial t} = k\,\tau(t)^{-1}. \tag{35}$$

That is, the objective frequency, $\omega(t)$, is proportional to the subjective event frequency, $\tau(t)^{-1}$.

The test of such a postulate must revolve around changes induced in the objective frequency of the motor production by changes in the stimulus environment which can be functionally related to the scaling, $\tau(t)$. That is to say that the scaling, $\tau(t)$, is the behaviorally relevant clocktime artifact of the representational process.

The method of the assessment of the role of central processes in the temporal control of movement is the characterization of the parameter, τ, and the experimental investigation of the above centralist postulate.

6.2.1 The sense of time, representation and the channel analogy

To incorporate the information channel analogy and involve central processes in equation 35 I take the definition of the maximal rate for minimal events of equation 30 to be the measure of event rate. That is, I let $\tau(t)^{-1} = \hat{\tau}(t)^{-1}$. The central processes postulate becomes:

$$\omega(t) = k\,\hat{\tau}(t)^{-1} \tag{36}$$

and refers to the rate of minimal events describing an observation. If the system is close to stability, the event rate is small as compared to systems farther from stability. That is, a system closer to stability has relatively fewer events/unit time and, therefore, a fixed clocktime interval will seem relatively shorter.

Recall that the time scale of $\hat{\tau}(t)^{-1}$ is arbitrary or even figmentary in the channel analogy. Therefore, notice that there is no behavioral access to the constant in equation 35 and no knowledge of the objective event time scale, except possibly a lower bound, the fineness of the time scale determined by the physical properties of the functional architecture. It is in this sense that a minimum indifference interval would have an impact on event driven systems.

6.2.2 Movement timing predictions

There is behavioral access only to $\omega(t)$ or $\Delta\omega(t)$. Therefore, experimental predictions must refer to them. Using what is known about $\hat{\tau}(t)^{-1}$ makes it possible to get first-order proportional change predictions of central process effects on a periodic behavior of subjectively constant rate.

Where M is the number of source states and $\hat{\tau}(t)^{-1}$ is defined by equation 30,

$$\min\hat{\tau}(t)^{-1} = \frac{n}{\log M} \leq \hat{\tau}(t)^{-1} \leq \frac{n}{H(x)} = \max\hat{\tau}(t)^{-1} \,. \tag{37}$$

Thus,

$$\min\Delta\hat{\tau}(t)^{-1} = \min\hat{\tau}_0^{-1} - \max\hat{\tau}_1^{-1}$$

$$\max\Delta\hat{\tau}(t)^{-1} = \max\hat{\tau}_1^{-1} - \min\hat{\tau}_0^{-1} \,.$$

Since, by equation 37 it follows that

$$\frac{\Delta\omega(t)}{\omega_0} = \frac{\Delta\hat{\tau}(t)^{-1}}{\hat{\tau}_0^{-1}} \tag{38}$$

and the connection to observable data is made. Notice that the time scale of $\hat{\tau}(t)^{-1}$ is irrelevant here. Thus, the time ordering convention in the channel analogy does not affect the prediction. The minima and maxima above yield

$$\frac{\min\Delta\hat{\tau}(t)^{-1}}{\max\hat{\tau}_0^{-1}} \leq \frac{\Delta\hat{\tau}(t)^{-1}}{\hat{\tau}_0^{-1}} = \frac{\Delta\omega(t)}{\omega_0} \leq \frac{\max\Delta\hat{\tau}(t)^{-1}}{\min\hat{\tau}_0^{-1}}. \tag{39}$$

It is this prediction that will be tested to assess the ability of the channel analogy to predict functional effects of central processes on behavior that are caused in virtue of the underlying representational structure. This will be taken to be a test case of the assertion that structural analysis is a causal analysis of system behavior that is sensitive to the level of analysis. As regards psychological process, especially central processes, recognition of that fact is essential.

7. Experiment

In this chapter an experiment is undertaken to verify the predictions of Chapter 6.

7.1 Overview of the fingertapping experiment

The plausibility of the causal influence of central processes on the control of movement timing has been discussed. The information channel analogy produces estimates of the effects that the putative central processes would have, thus inviting experimental investigation. Five increasingly general issues that require examination in the context of an empirical verification of the channel analogy are:

1. Does the information channel analogy predict actual limb movement timing data?
2. Are central processes involved in motor activity in cases other than very rapid movements and feedforward models?
3. Is the input system/central process distinction of Fodor's taxonomy of mental processes substantiated by examination of the control of movement timing?
4. Is the percept of duration associated with more than one level of mental functioning operationally involved in the temporal control of movement? Reciprocally, can movement timing data be interpreted in terms of the percept of duration?
5. Most generally, what is the status of the propositional view of representation and percept and what are the implications for functional or structural analysis of underlying mechanisms of perception, thought and behavior in the context of the control of movement timing?

The goal of the present experiment was to propose a paradigm with which the above issues might be made available to systematic empirical inquiry. The experiment was roughly outlined in the introduction and served to motivate discussion to this point. Some preliminaries are required concerning the experiment.

The subject tapped a finger to present a visual stimulus on a video display terminal which remained present until the next tap. The subject was instructed (see Appendix) to display the stimuli at a constant rate and try to remember what stimuli were seen, together with their relative frequencies of occurrence. The stimuli consisted of single digit numbers presented to one experimental group as numerals and to the other as the written names of the numbers or numerals. See the methods section for details of the construction of the ensembles of stimuli.

The stimulus ensemble was altered after eighty (80) presentations in one of three ways:

1. the relative frequencies of occurrence were unchanged, i.e., a control condition
2. the relative frequencies were altered such that all stimuli occurred equally often
3. a new stimulus member was added.

Eighty more presentations of the altered ensemble then occurred. The subject was uninformed of the change. The relative frequencies of occurrence in all premanipulation sequences were fixed locally over blocks of eight presentations and over the entire eighty presentation premanipulation sequence as $\frac{3}{8}$, $\frac{1}{4}$, $\frac{1}{4}$, $\frac{1}{8}$. After manipulation the frequencies were unchanged in the control, set to $\frac{1}{4}$, $\frac{1}{4}$, $\frac{1}{4}$, $\frac{1}{4}$ in condition 2 and set to $\frac{1}{4}$, $\frac{1}{4}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{8}$ in condition 3. The instructions to the subjects were intended to bias the subjects in such a way as not to attend to the difference between the name of a number and its associated numeral. The experimenter also attempted to bias the subject's tap rate to be approximately once per second, although absolute accuracy of tap rate as compared to one tap per second was not of interest.

The relations of interest to the subject in the context of this experiment were the relative frequencies of occurrence of a small collection of numbers. To the extent that the instructions were successful in inducing this bias, the structural relevancies of the associated representation state corresponded to relative frequencies. Moreover, to the extent that the small population of stimuli does not impose resource limitations (Norman and Bobrow 1975) on the subject's memory and to the extent that the actual relative frequencies of occurrence are discernible to the subject, the structural relationships between the stimuli will constitute description of the representational condition. I do not suppose that the representational condition *consists* of these relative frequencies, but rather that to the extent that the above caveats hold, in this particular situation, the representational condition *corresponds* to the relative frequencies. This important distinction deserves a brief emphasis. Just as Jeffreys (1961) points out that probabilities are not in themselves relative frequencies but may in certain circumstances correspond to them, and as Pylyshyn (1984) illustrates degeneracy in the notion of the representation of time as state descriptions of gear positions in a mechanical clock -- that is, such *is* the representation of time *to the clock* -- I draw attention to the fact that a collapse of levels of representational description sometimes occurs. I do not intend -- indeed it is antithetical to this thesis -- to imply the radically empiricist notion that the relative frequencies of stimuli constitute, in general, adequate subject state descriptions.

There are two remaining preliminaries concerning the instructions and task. No particular rate was specified by the experimenter to be matched. However, very rapid fingertapping produces fatigue effects uninteresting to the issues at hand and very slow tapping begins to involve long-term memory and different aspects of perception of long intervals (see Ornstein 1969). Therefore, to bias the subject to reasonable, rather slow but wholly arbitrary rates, the monitor cursor flashed once per second during the instruction period. The subject's attention was not drawn to that fact but immediately prior to commencing the task the experimenter demonstratively tapped his finger at a rate matching the cursor (without mentioning the cursor) and suggested such a rate was desirable to prevent fatigue. The cursor was not present during the experiment. The second thing is that after the 24th presentation and the last (160th) presentation, the subject was asked four questions:

1. Did the stimuli seem to change in any way during the experiment?
2. Which stimuli seemed equally frequent? (name two)
3. Which stimulus seemed most frequent? (name one)
4. Which stimulus seemed least frequent? (name one).

All questions were forced choice, (2) through (4) required numeral responses. The questions were intended to provide detection and recognition data and also to further focus the subject on the intended relevancies of the task, thereby increasing the plausibility that the representational condition corresponded to the relationships manipulated during the course of the experiment.

To the extent that the representational condition induced by the instructions, secondary task and stimulus sequence histories corresponds to objective relative frequencies of stimulus occurrence, the possibility of behaviorally testable predictions using $\hat{\tau}^{-1}$ arises. Preliminaries aside, it is necessary to place the experiment in the context of the development of previous chapters which underlie the predictions.

Where $S = \{a_1,...,a_n\}$ is the set of numbers (that is, the *meaning* of the presented stimuli) in the ensemble and $n = \text{card}S = M$, the number of source symbols, I take it, for this task, that the relevant propositions entertained upon presentation of a_i are logically equivalent to

$$\begin{cases} a_i \in S \\ |a_i| < |a_j| \; i = 1, 2, \cdots i-1, i+1, ...n \end{cases}$$

$|a_i|$ is to be read, "the relative frequency of occurrence of a_i". These comprise

n truth evaluatable statements (propositions) for each stimulus presentation. These are understood to form n-vectors of truth values uniquely specifying (i.e., encoding) a state occurring with some subjective prior probability, $p(a_i)$ and associated with belief measure $\hat{P}(a_i)$. The prior should correspond to the objective relative frequencies in this task after a number of presentations since there are no other structural relevancies present. So it is expected, after a lot of presentations, that an entropy measure on the representational condition would be approximated by the entropy of the stimulus ensemble which is determined by the relative frequencies of occurrence of a_i's in the stimulus sequence. That is, it is assumed that the entropy of the representational condition $\equiv H(R) \simeq H(S) = \sum_{i=1}^{h} p(a_i) \log p(a_i)$ and $\hat{\tau}^{-1}(t) \simeq \frac{n}{H(S)}$. Where I take $\hat{\tau}^{-1}(t_0)$ as an estimate of the event rate associated with the premanipulation sequence and $\Delta\hat{\tau}^{-1}(t)$ as an estimate of the change in event rate associated with the postmanipulation sequence, recall the prediction is

$$\frac{\min \Delta\hat{\tau}^{-1}(t)}{\max \hat{\tau}^{-1}(t_0)} \leq \frac{\Delta\hat{\tau}^{-1}(t)}{\hat{\tau}^{-1}(t_0)} = \frac{\Delta\omega(t)}{\omega(t_0)} \leq \frac{\max \Delta\hat{\tau}^{-1}(t)}{\min \hat{\tau}^{-1}(t_0)}.$$

We can assign values to the predictions using the entropies of the various conditions (the base of the logarithms is arbitrary, but base 2 is used):

$$H(S_{\text{premanipulation}}) = -\frac{3}{8}\log\frac{3}{8} - \frac{1}{2}\log\frac{1}{4} - \frac{1}{8}\log\frac{1}{8} = 1.906$$

$$\log M_{\text{premanipulation}} = \log n = \log 4 = 2$$

$$\log M_{\text{postmanipulation}}^{\text{condition 2}} = \log n = \log 4 = 2$$

$$\log M_{\text{postmanipulation}}^{\text{condition 3}} = \log n = \log 5 = 2.322$$

$$H(S_{\text{postmanipulation}}^{\text{condition 2}}) = -\log\frac{1}{4} = 2$$

$$H(S_{\text{postmanipulation}}^{\text{condition 3}}) = -3 \cdot \frac{1}{4}\log\frac{1}{4} - 2 \cdot \frac{1}{8}\log\frac{1}{8} = 2.25$$

yielding predictions for condition 2:

$$\frac{(\frac{4}{2} - \frac{4}{1.906})}{\frac{4}{1.906}} \leq \frac{\Delta\omega(t)}{\omega(t_0)} \leq (\frac{4}{2} - \frac{4}{2})$$

$$= -.047 \leq \frac{\Delta\omega(t)}{\omega(t_0)} \leq 0$$

and condition 3:

$$\frac{(\frac{5}{2.322} - \frac{4}{1.906})}{\frac{4}{1.906}} \leq \frac{\Delta\omega(t)}{\omega(t_0)} \leq \frac{(\frac{5}{2.25} - \frac{4}{2})}{\frac{4}{2}}$$

$$= .026 \leq \frac{\Delta\omega(t)}{\omega(t_0)} \leq .111 \ .$$

Obviously, no change is predicted for the control, condition 1.

Observe that the central processes timing postulate predicts, to a rough first-order estimate, that the tap rate will slow a few percent in condition 2 and speed up a few percent in condition 3. Although rough predictions, they are opposite in sign and rather small and should they correspond to reliable regularities in the data, the presence of a central process effect would be rather convincing.

Given the above, the first four issues presented at the beginning of this chapter may be outlined in the context of this paradigm. The first issue requires simply that the above predictions be verified experimentally. The second issue follows from elaboration of the first. Since the time between taps will likely be an order of magnitude greater than the kinesthetic proprioceptive signal delay to the central nervous system ($0(10^3)$ msec vs. approximately 10^2 msec (Schmidt 1982)), the confirmation of the central process predictions would imply that kinesthetic proprioceptive information was not used in the task. The temporal regulation could not be assigned to peripheral (e.g., spinal) structures if the visually presented and semantically interpreted stimuli are shown to have behaviorally causal effects. Thus centrality would be indicated. If demonstrable central effects on the control of timing of slow movements exist, any obvious (teleological) support for a purely feedforward view of central processes is lost. Moreover, the effects predicted in the present experiment would be due precisely to the "open-loop" character of the experimental situation. The experiment described is open-loop in the sense that the only considered comparator reference value, a percept of a subjectively constant interval, is manipulated by the stimuli intended to be controlled. Hence, the reference error is always zero -- open-loop. One opens loops on ***feedback*** control systems. The necessity of equating central motor processes with feedforward systems would be discredited by verification of the central process predictions above.

The term "central process" was used in the last paragraph in the unspecific way of Chapter 2. In fact, the above characterization of the representation of the task is central in the taxonomy of Chapter 3. Isotropy is met in the sense that for each stimulus presentation all of the tacit and explicit knowledge derived from instructions and the stimulus sequence is relevant to the valuation of the n propositions asserted to define each state of the representational condition. The characterization is quineian in that any conceivable measure of degree of confirmation of belief (defined as an epistemic probability) afforded some state would involve, and be sensitive to changes in, the formal language inducing the belief. That is, any conceivable measure of confirmation would be sensitive to the global properties of the representational condition. The isotropic and quineian conditions are the requisites of central process in the taxonomy. If this characterization leads the prediction, centrality in the specific sense of Fodor's taxonomy is implied.

The third issue is implied by the previous paragraph and also involves the numeral/name-numeral factor in the experiment (henceforth called the semantic factor). Central processes "exploit the information that input systems provide" (Fodor 1983, pg. 103). If the central process prediction is substantiated in both the name-numeral and numeral conditions, then the activity of the input system(s) in question must at least involve the lexical processing of the stimuli. It must be that the meaning of the name or numeral is the object of interest to the controlling process if the prediction holds, because if the executive process operated on the uninterpreted symbols, conditions 2 and 3 in the name/numeral condition would have undifferentiated expectations. That may be seen by example as follows. The premanipulation stimulus ensemble might be represented thus:

ONE	TWO	THREE	FOUR
ONE	2	3	
1			

Condition 2 would be, in that instance:

ONE	TWO	THREE	FOUR
1	2	3	4

and condition 3 would be something like:

ONE	TWO	THREE	FOUR	
1	2	3		5

At the symbolic level, the magnitude of change in the ensemble in the name/numeral situation is equal for both ensemble manipulation conditions 2 and 3 in the sense that a "ONE" was replaced with a symbol not previously a member of the ensemble in either case: here, a "4" or a "5". Thus, at the uninterpreted symbol level of analysis, no difference in effect between ensemble manipulation conditions 2 and 3 would be expected in the name/numeral situation. On the other hand, where the number associated with the numeral or name is denoted [1], [2], [3], [4], [5], if it is the meaning of the symbols that is the output of the input system and the operanda of the central processes involved in the above predictions, then the name/numeral and numeral situations are undifferentiated with respect to the central process and ensemble manipulation conditions 2 and 3 in the name/numeral condition *are* differentiated with respect to the central process, thus:

condition 2 has [1] x 2, [2] x 2, [3] x 2, [4] x 2 and

condition 3 has [1] x 2, [2] x 2, [3] x 2, [4] x 1, [5] x 1.

In such a situation the predictions given previously are the same for both semantic factor levels. Thus, if the prediction is borne out for both levels of the semantic factor, there would be no statistical interaction of semantic X ensemble manipulation factors. It would imply a central process operating on output from an input system involved in the control of movement timing; that is, the process underlying the percept of duration operational with respect to the proportional change of tap rate would be implied to be central in the sense of Fodor's taxonomy. The prediction, being a proportional change, is silent on the possibility of a main effect of the semantic factor. The lexical processing, being an aspect of syntactical analysis, is by definition an operation of an input system in the taxonomy, but to assert the input system only from definition would be admittedly tendentious.

The fourth issue will serve to further address input systems as well as time experience in the control of movement. The central effect predictions come from a notion of event sequence that implicitly involves semantic evaluation. The syntactical level, however, requires only symbolic manipulation and does not possess what Pylyshyn (1980, 1984) calls "cognitive penetrability." That it does not is used extensively by Fodor (1983) in combination with the data of Samuel (1981) to argue that inter-lexical relations can phenomenologically cause input systems to appear to involve or produce expectation (i.e., belief) while in fact only post lexical decisions influence expectation (see Miller 1956, Bruner 1957). Such decisions are argued to be functions of belief ascribing central processes. At the lexical level, the word "ONE" requires more processing to get to [1] than "1" does. At *least* there is a transducer output storage requirement for the encoding of three symbols ("chunking" (Miller 1956, Simon 1974) must be post lexical according to cognitive penetrability) rather than one. Such would be not at all obvious if "automaticity" of letter encoding were true (e.g., Posner and Boies 1971, Kahnemann 1973, La Berge and Samuels 1974). Automaticity is a hypothesis in cognitive psychology relating to processing (presumably of highly practiced familiar stimuli) defined to be unconscious, involuntary and requiring no attention (see Posner 1978). But automaticity is apparently not true (see Johnson *et al.* 1983) for letter encoding; letter encoding probably requires attentional resources. Then the central processes, operating on [1] or [ONE], etc., operate on the same content in both semantic factor levels; however, the syntactical processes, specifically the lexical processing, does not. The transducer output storage resources required are greater for the name/numeral level than the numeral level. The attentional resources required are greater for the name/numeral level as well. That is very important to the percept of duration. Ornstein (1969) showed that a storage-size metaphor predicted the percept of the duration of an interval: as the storage requirement for the processing of stimuli occurring in a fixed time interval increased, the subjective experience of the length of the interval was protracted. Storage requirements increase with the complexity of the image in general for information extraction processes (e.g., see Abu-Mostafa 1983) and increasing stimulus complexity (measured by the methods of Atteneave 1957) elongated the estimate of temporal intervals in Ornstein (1969). Since the processing of the more complex object "ONE" likewise must involve greater storage requirements than "1," one might expect to replicate Ornstein's finding of interval elongation of the name/numeral level relative to the numeral level due to lexical processing.[2] Since this effect should be seen, if it occurs, across all three ensemble manipulation conditions, the subjective interval elongation should be expected to produce a main effect of the semantic factor on syntactical consideration of the stimuli. The perceived interval elongation would cause a subject to tap his finger at a time *earlier* in a clocktime interval than he otherwise would for a subjectively constant tap interval. Therefore, one would expect that the tap rate would have different baselines for the different semantic levels due to syntactical considerations for the particular stimuli in this experiment. In particular, Ornstein's effect should cause the name/numeral baseline rate to be faster than the numeral level baseline rate. It should be faster in *all three ensemble manipulation conditions*. Notice that such an effect would clearly be discriminated from the central processes effect which should have opposite sign in conditions 2 and 3.

The distinction is especially clear when one realizes that, at the central processes level, the amount of information increases in both condition 2 and condition 3. Ornstein's effect, if it applied to that level, would predict timing changes of the same sign for both conditions. If the opposite sign effect occurs, the storage size metaphor does not apply to central processes. If the baseline shift occurs, central process proportional change predictions being silent on such a shift, the storage metaphor applies to the lexical -- input systems -- level. At the lexical level, meaning plays no role in processing. If lexical level processes

[2] I am indebted to Dr. Charles Hamilton for drawing my attention to this issue.

can affect time perception, the effect must be uninfluenced by meaning. Fraisse (1966) demonstrated, in the context of the perceptual moment view of the percept of time discussed earlier in this manuscript, that meaning does not influence the indifference interval for successively presented stimuli. Thus, the indifference interval view of event sequence underlying the percept of duration must be relegated to transducer output and/or input systems. Lichtenstein *et al.* (1963) convincingly demonstrate that the effect of the numerosity experiment (White 1963), where perceived elapsed time of an objectively fixed time interval containing very closely spaced flashes of light is elongated as the locus of retinal stimulation is displaced from the fovea, requires the employment of psychological (that is, representational) elements to explain the data. If the processing level is symbolic, but not affected by meaning it must be syntactical in character. The effect on the percept of duration is well established in the paradigm of White (1963). In combination, White's paradigm and Ornstein's storage hypothesis strongly support the notion that psychological mechanisms taxonomologically lower than central processes influence the percept of duration. The occurrence of a baseline shift in this experiment would support such a view.

Thus two levels of cognitive activity could possibly be distinguished by the present experiment as influential in the control of movement timing -- input systems and central processes. To be clear, if the basic data are time between taps, and (a) the semantic factor has a main effect such that the baseline tap interval is shorter in the name/numeral semantic factor level and (b) there is no statistical manipulation X semantic factor interaction and (c) the quantitative predictions are borne out using the direct statistic, then the following conclusions would be indicated with respect to the fourth issue:

1. The percept of duration has executive domain over the control of movement timing.
2. The percept of duration is influenced by at least two levels of cognitive processes, input systems and central processes.
3. Movement timing data in this paradigm expose differential effects of these levels of cognitive processes on the percept of duration.
4. Ornstein's storage-size metaphor for time experience is not a general model in that it does not predict the central processes effect.

The importance to this thesis of these four issues is the following. Where the above effects are observed, an instance is demonstrated where psychological elements and systems can be argued to be theoretically essential for causal functional description of an overt behavior. The levels of the psychological processes involved can be argued to include central processes in the taxonomy of Fodor (substantiated in the experiment) with consequent serious problems for any usual neuroreductionistic structural analysis of neural mechanism underlying function. This begins to touch on the fifth issue, the discussion of which is reserved for the general discussion of the next chapter.

7.2 Method, subjects, apparatus and stimuli, design and procedure

Subjects. Twenty-four graduate and undergraduate students at the California Institute of Technology participated as subjects in the experiment without compensation of any kind. Each subject participated individually in a single experimental session lasting about one hour. The use of human subjects in this experiment met the standards of and was approved by the Human Subjects Committee at the California Institute of Technology.

Apparatus and Stimuli. Timing of intervals was carried out by a Mountain View millisecond clock card installed in an Apple II microcomputer used to control the experiment and collect the data. Clock reads were triggered by a finger tap signal and were accurate within $\pm$ 1 msec. The finger tap signal was generated upon interruption of a light path between a photo diode and a phototransistor by the right index finger. The signal was sent to a John Bell interface device that caused the clock read. A metal plate on one side of the device served to stop the finger at the bottom of a tap motion. The finger motion was otherwise unencumbered. The right index finger was splinted and wrapped in gauze with moderate pressure such that motion was allowed only at the large knuckle and that tactile and temperature sensitivity of the finger were reduced. The right arm was supported such that the wrist was straight while the right hand grasped an adjustable grip that aligned the index finger perpendicular to the light path. Taped white noise was played through headphones at a volume adjusted by the subject to mask the sound of the finger tap and yet not produce discomfort.

The subject was seated in an adjustable chair with headsupport (Ritter dental chair) such that the normal forward direction of gaze coincided with the center of an anti-glare, green display video monitor. The stimuli were approximately $\frac{1}{2}$inch x $\frac{1}{2}$inch and were viewed at a distance of two feet. The brightness of the display was adjusted to the comfort of the subject and the background illumination was normal fluorescent room lighting (see Fig. 28).

The stimuli consisted of four or five randomly chosen unique single digit numbers forming an ensemble with relative frequencies of occurrence thus:

Condition 0: $\frac{3}{8}, \frac{1}{4}, \frac{1}{4}, \frac{1}{8}$

Condition 1: $\frac{3}{8}, \frac{1}{4}, \frac{1}{4}, \frac{1}{8}$

Condition 2: $\frac{1}{4}, \frac{1}{4}, \frac{1}{4}, \frac{1}{4}$

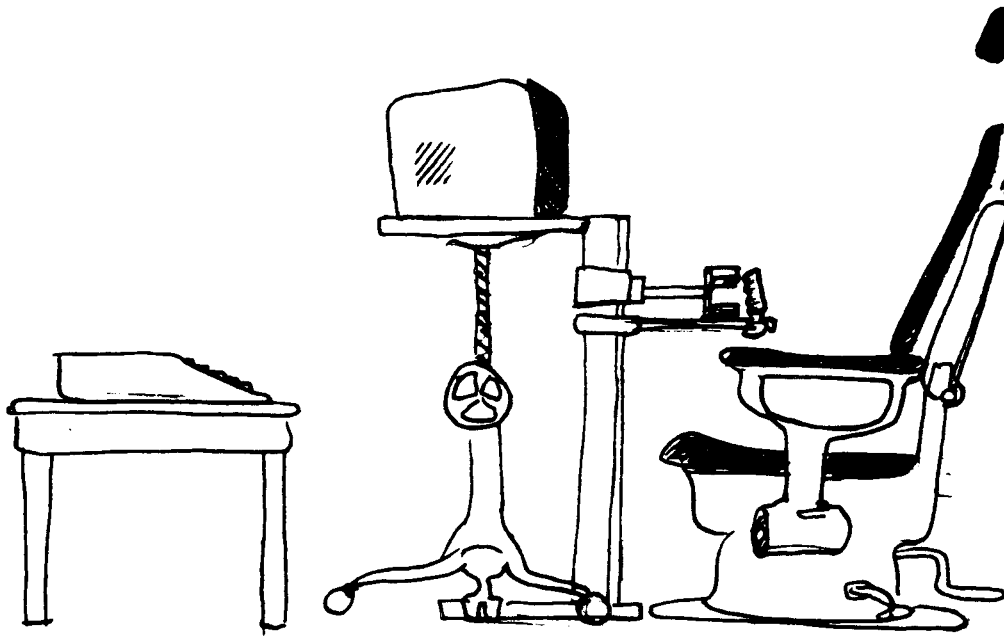


Fig. 28

Condition 3: $\frac{1}{4}, \frac{1}{4}, \frac{1}{4}, \frac{1}{8}, \frac{1}{8}$

as will now be explained. Four unique randomly chosen numbers were randomly assigned to an ensemble of eight numbers as in the following example:

1	2	3	4
1	2	3	
1			

This ensemble was then randomly permuted, producing a sequence of eight stimuli with relative frequencies as in condition 0. Ten unique sequences were produced and concatenated, forming an eighty stimulus sequence with relative frequencies given by condition 0. Hence, the long sequence did not contain the possible long unrepresentative subsequences that could occur in a purely random eighty stimulus sequence generated with overall relative frequencies of occurrence determined by condition 0. Thus the statistics held locally as well as globally in the sequence. The eighty stimulus sequence generated from condition 0 constituted the *premanipulation sequence*.

The experimental manipulations involved similarly creating a sequence composed of the same four random numbers (except condition 3, in which one new number was added to the ensemble) according to the relative frequencies given above for conditions 1, 2, 3. Condition 1 was a control condition (no change) and conditions 2 and 3 were experimental conditions. The entire stimulus sequence was 160 stimuli long in all experimental conditions.

Half of the subjects saw stimuli composed of the numerals that represented the numbers in the sequence. The other half of the subjects saw stimuli that were either the numerals or the written names of the numbers. The above example of the premanipulation ensemble for the name-numeral group would be:

ONE	TWO	THREE	FOUR
ONE	2	3	
1			

The post manipulation ensemble for the control, condition 1, would be the same. For the other conditions, 2 and 3, the post manipulation ensembles would be:

Condition 2:	ONE	TWO	THREE	FOUR	
	1	2	3	4	
Condition 3:	ONE	TWO	THREE	FOUR	
	1	2	3		5

With each fingertap a stimulus was presented and remained presented until the next fingertap. The subject was not informed of the ensemble change by the experimenter. A unique, random 160 stimulus sequence was generated for each subject in each experimental condition.

Design and Procedure. The experiment had three factors, one within subjects, producing a 3 (within subjects) X 2 X 6, (ensemble manipulation X semantic factor X order of manipulation) design. Each subject was assigned to one level of the semantic factor, (i.e., either name/numeral or numeral), performed the task in all three manipulation conditions, and was assigned to one order of the manipulation conditions. For example, a subject might have been assigned to the numeral condition, performing the task three times: condition 3 first, condition 1 second, and condition 0 third. Since there are three levels of the manipulation factor, there are 3! orders within each level of the semantic factor. Two subjects were assigned to each order. Consequently, there were twelve subjects in each level of the semantic factor and four subjects in each order across semantic factors. The within subjects factor was thus completely and multiply counterbalanced.

The subjects were instructed (see Appendix) to try to remember the stimuli and judge their relative frequencies while attempting to present them at an absolutely constant rate. As was discussed in the overview to the experiment, the experimenter attempted to bias the subject to tapping rates of approximately 1 hz. After the twenty-fourth tap and after the last tap questions were presented on the monitor and the subject's verbal answers were entered by the experimenter into the computer. The questions were presented in the previous section of this chapter.

Each 160 stimulus sequence and questions required approximately five minutes. The sequences were separated by five minutes while data were processed and sent to disk to ensure the finger and arm were not fatigued and the subject was alert. Instructions and setup required approximately fifteen minutes and debriefing required about ten minutes. Thus the subject was engaged for approximately one hour, actually tapping for approximately fifteen minutes of that time.

7.3 Results and Discussion

To establish that the experimental manipulations were detectable to the subjects, the answers to the question, "Did the stimuli appear to change in any way during the experiment? (Yes or No)" at the end of the experiment were used to estimate if detection of the manipulation was at or above threshold. Threshold is defined as in Swets (1964) to be

$$p\,(\text{manipulation perceived} \mid \text{manipulation occurred}) = .5$$

and $p\,(\text{manipulation perceived} \mid \text{manipulation did not occur}) = 0$. Fig. 29 shows that manipulation level 2 was at threshold and manipulation level 3 was perfectly judged, indicating that the ensemble manipulations were perceptually

NUMERAL

	Ensemble Manipulation	2		3	
		YES	NO	YES	NO
ACTUAL	Signal	0.75	0.25	1.0	0.0
	No Signal	0.5	0.5	0.5	0.5
NORMALIZED	Signal	0.5	0.5	1.0	0.0
	No Signal	0.0	1.0	0.0	1.0

NAME/NUMERAL

	Ensemble Manipulation	2		3	
		YES	NO	YES	NO
ACTUAL	Signal	0.75	0.25	1.0	0.0
	No Signal	0.5	0.5	0.5	0.5
NORMALIZED	Signal	0.5	0.5	1.0	0.0
	No Signal	0.0	1.0	1.0	1.0

DETECTION DATA

Threshold is defined as $P_{norm}\,(Y\,|\,S) = .5$ and $P_{norm}\,(Y\,|\,NS) = 0$. The normalization is to chance responding and is given by:

$$P_{norm}\,(Y\,|\,\cdot) \equiv \frac{P(Y\,|\,\cdot) - P(Y\,|\,NS)}{1 - P(Y\,|\,NS)}$$

Fig. 29

detectable. The control condition was used to estimate the actual p(perceived | did not occur).

To determine if the recognition threshold for the manipulations was attained, a similar estimate was formed using the ratio of correct answers to total number of answers to questions asked concerning frequencies (see overview) of occurrence. Again p(manipulation recognized | manipulation occurred) was estimated from the control condition. In the case of recognition, Fig. 30 shows threshold was exceeded in all cases except in manipulation 2 of the name/numeral factor. As rough indicator of recognition the estimate indicates that the ensemble manipulations were salient, with the exception of the above-mentioned condition.

What is argued from that detectability and recognizability is that the instructions and task indeed did tend to bias the subject's representation to conform to the structural relationships objectively constituting the task. That is, relative frequencies and stimulus members were represented in a way largely conforming to the assumptions of the numerical predictions.

To test the quantitative predictions of the central processes postulate and the information channel analogy, the tap interval data (in msec) must be transformed. The prediction is a proportional change (hence a unitless number) of tap frequency, not duration. The assumptions underlying the predictions were that, in the premanipulation sequence, after many presentations, $\hat{\tau}^{-1}$ estimated the event rate, on the average, where it was calculated from stimulus ensemble statistics. The tap data used in the analysis extended from the 40th tap to the 120th tap. The assumption is that $\hat{\tau}^{-1}$ in the last half of the premanipulation sequence was close to the estimate and shifted in the predicted way in the postmanipulation sequence. The tap intervals from the 40th to the

NUMERAL

	Ensemble Manipulation	2		3	
		YES	NO	YES	NO
ACTUAL	Signal	0.688	0.312	0.688	0.312
	No Signal	0.187	0.813	0.187	0.813
NORMALIZED	Signal	0.616	0.384	0.616	0.384
	No Signal	0.0	1.0	0.0	1.0

NAME/NUMERAL

	Ensemble Manipulation	2		3	
		YES	NO	YES	NO
ACTUAL	Signal	0.563	0.437	0.708	0.292
	No Signal	0.167	0.833	0.167	0.833
NORMALIZED	Signal	0.396	0.604	0.541	0.439
	No Signal	0.0	1.0	0.0	1.0

RECOGNITION DATA

Threshold is defined as $P_{norm}(Y|S) = .5$ and $P_{norm}(Y|NS) = 0$. The normalization is to chance responding and is given by:

$$P_{norm}(Y|\cdot) \equiv \frac{P(Y|\cdot) - P(Y|NS)}{1 - P(Y|NS)}$$

Fig. 30

120th taps were transformed to a statistic that has the form of the prediction as its mean. Henceforth, it will be called the *direct statistic* and is given thus:

where

$$x_{ijk} = \left(\frac{i^{th}\text{premanipulation tap interval (msec) for subject j in condition k}}{1000} \right)^{-1}$$

and

$$y_{ijk} = \left(\frac{i^{th}\text{postmanipulation tap interval for subject j in condition k}}{1000} \right)^{-1}.$$

I define

$$\hat{y}_{ijk} = \frac{y_{ijk}}{\bar{x}_{jk}}.$$

The direct statistic, v_{ij}, is defined,

$$v_{ijk} = -(1 - \hat{y}_{ijk}) = \frac{y_{ijk} - \bar{x}_{jk}}{\bar{x}_{jk}},$$

so

$$E(v_{ijk}) = \sum_{i=1}^{40} \frac{v_{ijk}}{40} = -\left(1 - \frac{\bar{y}_{jk}}{\bar{x}_{jk}}\right) = \frac{\bar{y}_{jk} - \bar{x}_{jk}}{\bar{x}_{jk}} \text{and } \mathrm{var}(v_{ijk}) = \frac{\mathrm{var}(y_{ijk})}{\bar{x}_{jk}}.$$

The assumptions have the form:

$$\hat{\tau}^{-1}_{\substack{\text{premanipulation sequence} \\ \text{subject } j,\text{ condition } k}} \simeq \bar{x}_{jk} \text{ and } \hat{\tau}^{-1}_{\substack{\text{postmanipulation sequence} \\ \text{subject } j,\text{ condition } k}} \simeq \bar{y}_{jk}.$$

Thus the central processes postulate prediction is that,

$$\frac{\Delta\omega_{jk}}{\omega_{jk}} \simeq \frac{\bar{y}_{jk} - \bar{x}_{jk}}{\bar{x}_{jk}} = E(v_{ijk}^{\text{post}}).$$

Notice that $E(v_{ijk}^{\text{pre}}) = 0$, normalizing all conditions and subjects. The test of the prediction was undertaken by transforming the data into the direct statistic and performing a 3 X 2 X 6 (manipulation X semantic X order) analysis of covariance. The v_{ijk}^{pre} were covariates to the v_{ijk}^{post} dependent variable and the manipulation was a within subjects factor. Fig. 31 illustrates the main effects of manipulation. The direct statistic data, averaged across blocks of eight taps and across subjects, are plotted in Fig. 32. The analysis yields no effect by semantic factor, $F(1, 22) = 1.33, p > .25$.

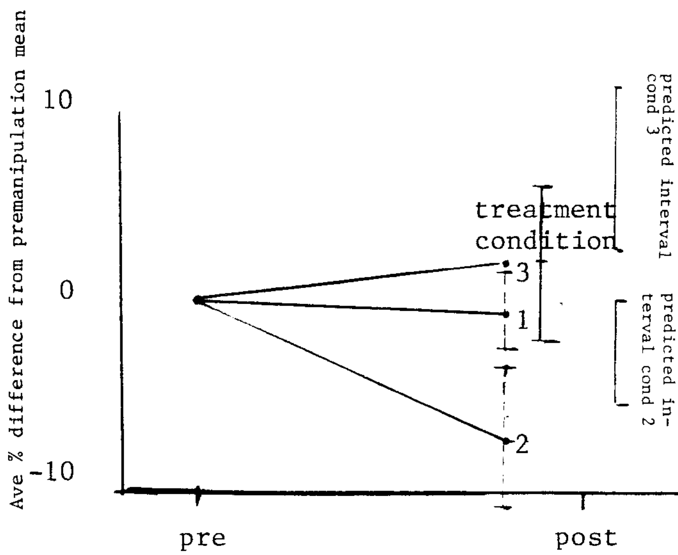
A significant effect of ensemble manipulation was seen, $F(2, 43) = 16.57, p < .001$, with contrasts yielding no difference between the pairs of groups (1,4), (2,5), and (3,6), all p's > .2. The means of the direct statistic (taken over subjects, within manipulation level and within semantic level) are given in the table below. All effects were in the predicted direction and within a standard deviation of the predicted range.

Semantic factor	Ensemble manipulation	Observed proportional change	Predicted
	1	-.005, s = .028	0
numeral	2	-.076, s = .036	-.047 - 0
	3	.02, s = .042	.026 - .111
name/	1	.001, s = .032	0
numeral	2	-.046, s = .048	-.047 - 0
	3	.057, s = .050	.026 - .111

Order effects were not significant, $F(5, 12) = 1.17, p > .25$ nor was the semantic factor X ensemble manipulation interaction significant, $F(2, 43) = .9, p > .25$. Taken together these results strongly suggest that a central processes effect on control of movement timing has been demonstrated using the channel analogy predictions. Issue one of the overview has thus been addressed and requires no further comment.

The second issue largely follows from the first and the detection/recognition results. The requisite conditions for an interpretation of the representation of the experimental task as central are argued to exist from the latter and support

Numeral condition
of semantic factor



Name/numeral
condition of
semantic factor

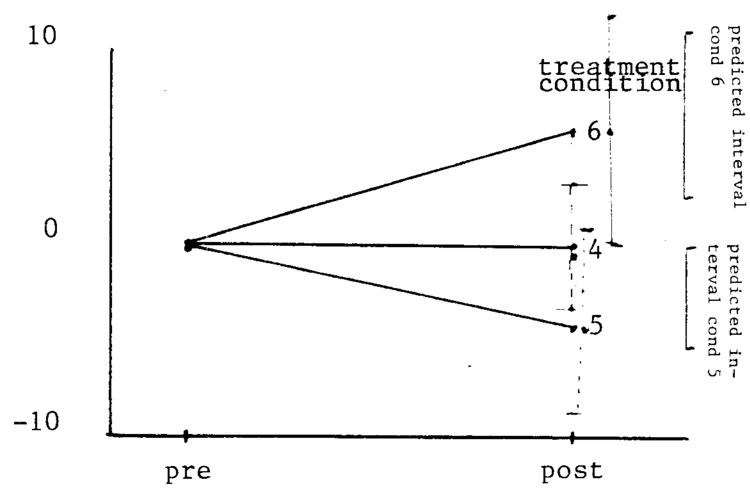


Fig. 31 Illustration of the treatment effect using the direct statistic. There is no effect of the semantic factor and no interaction of the semantic and treatment factors. The effect of treatments is significant. This is taken to illustrate the central process effect.

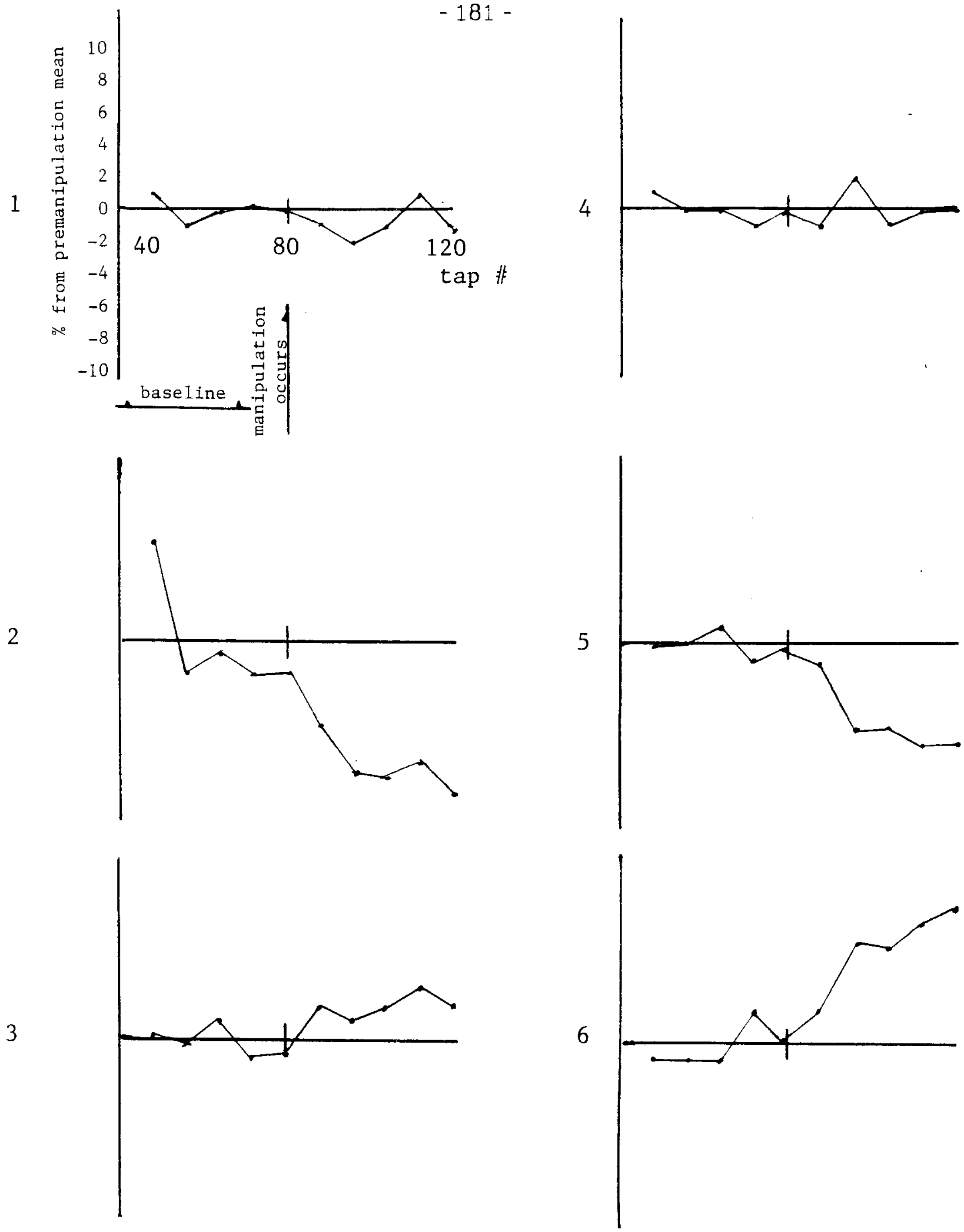


Fig. 32 Plotted points are averaged over blocks of eight taps and across subjects. Each point is thus an average of 96 data points -- twelve subjects x eight data points/subject.

Conditions 1, 2, 3	Conditions 4, 5, 6
Stimuli are numerals	Stimuli are names of numbers and numerals

Cond 1, 4: no change in stimulus population; control
Cond 2, 5: relative frequency of stimuli changed to equiprobable
Cond 3, 6: new stimulus element added

for the central processes effect is drawn from the confirmation of the former. It should be emphasized that the predictions are coarse enough to be considered principally sign and order of magnitude predictive. The most important thing to notice here is that analysis of the direct statistic unequivocally yields the correct sign for the predictions for both levels of the semantic factor. Post lexical processing is strongly implied, really almost demanded, by that fact as previously discussed. Combined with the rather good correspondence of prediction to data, even quantitatively, I feel that the case for the details of the assumed structure of the representation and epistemic probability are strongly supported. A detailed process analysis and associated predictions would be required to more convincingly establish the structural assumptions of the representation by dealing specifically with the shapes of the curves the direct statistic yields over the course of the task. That would involve considerable refinement of the determination of the value assigned to $\hat{\tau}^{-1}$ during the course of the experiment.

The third and fourth issues cannot be addressed by the direct statistic because it normalizes the baseline rate and, as was discussed, a baseline rate shift is predicted by the Ornstein effect. The central process postulate is silent on a baseline shift. The intertap interval (msec) is used as the basic datum to test the shift and subjected to the same analysis of covariance as the direct statistic. If the central effect and the input system effect were present, one would see (a) a main effect by the semantic factor, the means of all three ensemble manipulations being shifted down, (b) a main effect by the ensemble manipulation factor with condition 2 having a longer postmanipulation tap interval than condition 3 and (c) no interaction of semantic and manipulation factors. Figs. 33, 34 and 35 portray the tap interval data obtained. The results of the analysis gave an insignificant effect of order, $F(5, 12) = 1.03$, $p > .25$, and

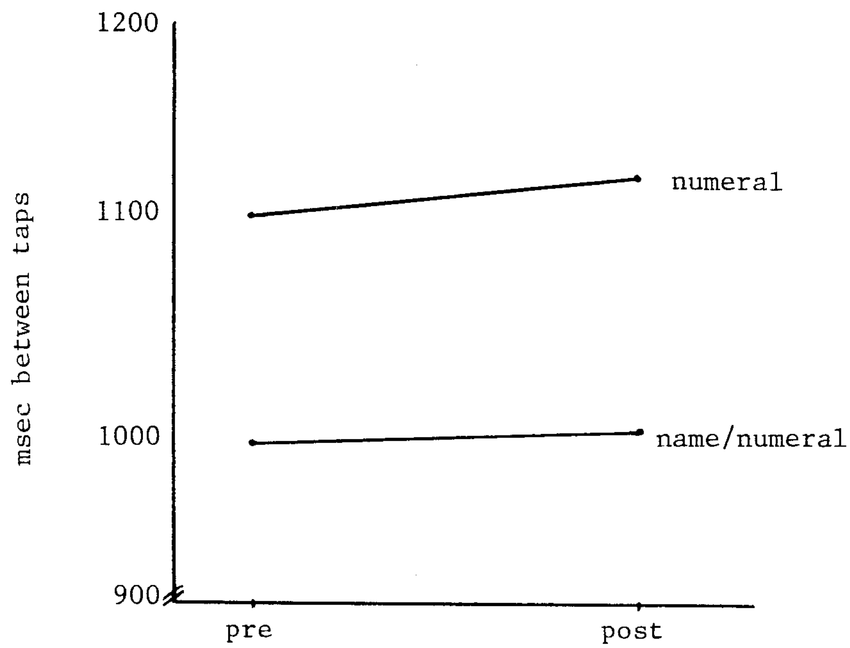


Fig. 33 Illustration of downward baseline shift use averages of pre and post tap interval data. Ave. shift = 127 msec

Numeral condition
of semantic factor

Name/numeral
condition

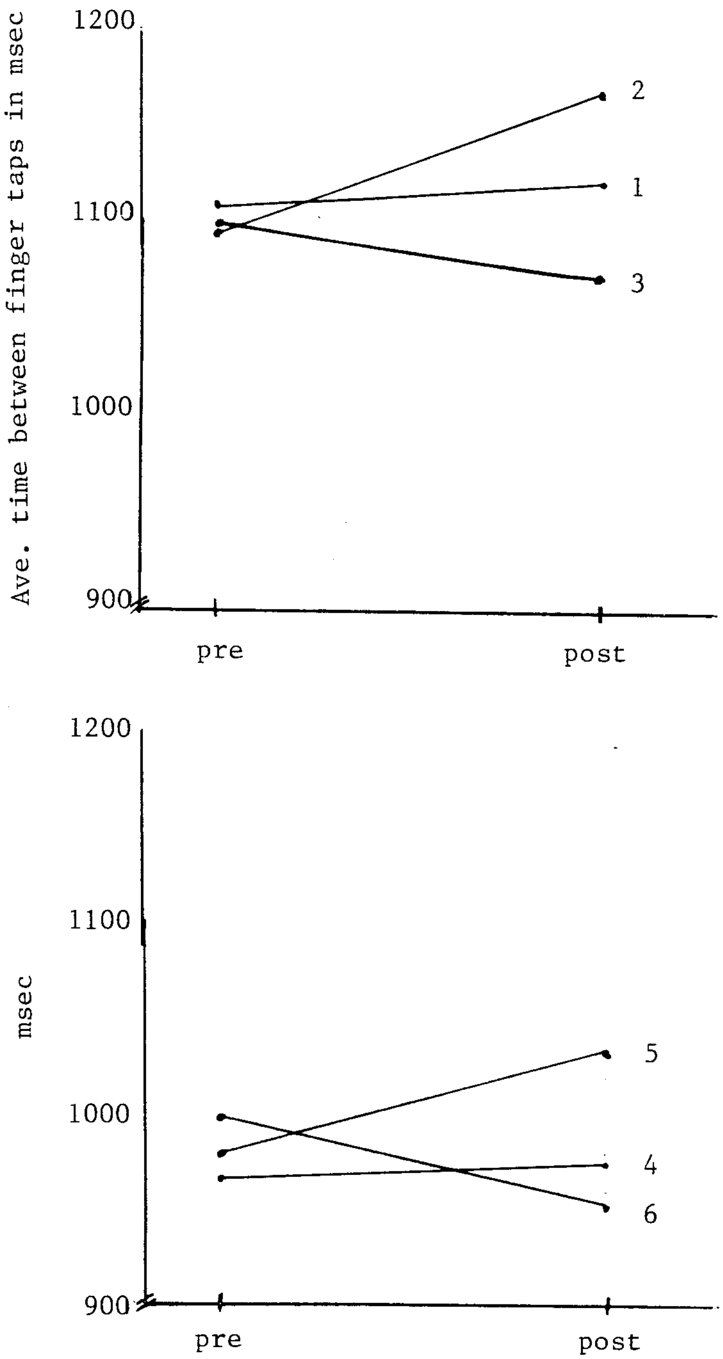


Fig. 34 Illustration of the baseline shift downward in the name/numeral condition using raw data. The effect of the semantic condition is significant as is the effect of treatments. The interaction is not significant. This is taken to illustrate the input processes effect and verify the central process effect.

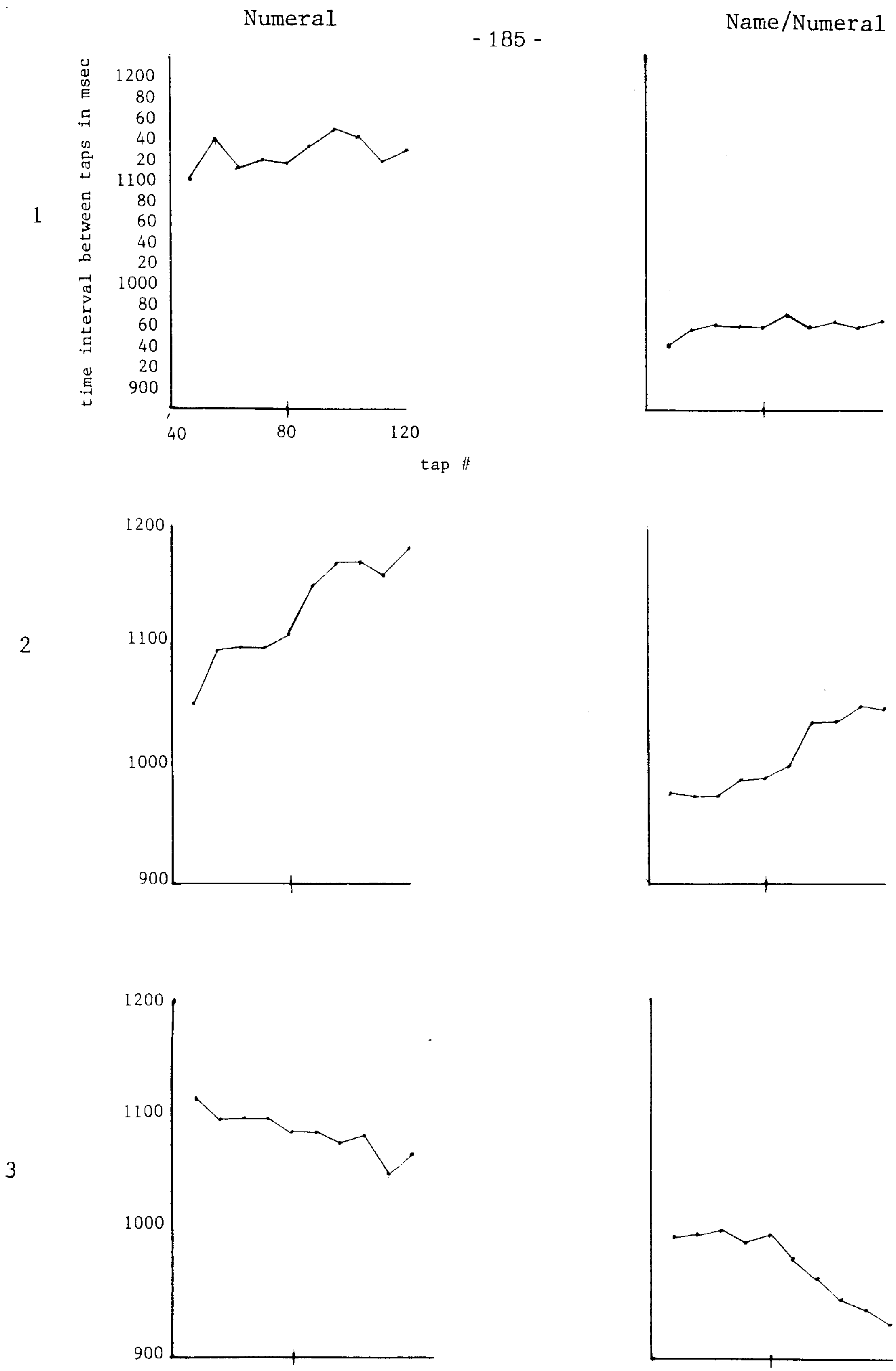


Fig. 35 Raw finger tap data plotted: msec between taps

significant effects of semantic and ensemble manipulation factors, with respective Fs, $F(1, 22) = 6.11, p < .04$ and $F(2, 43) = 25.34, p < .001$.

Hence these data indicate the presence of two post-transducer processes (i.e., cognitive processes) involved in the control of movement timing. The magnitudes of the central processes effect cannot be predicted with these data, but the support of direct statistic predictions and the main effect of ensemble manipulation using tap interval data support the contention of a central processes effect. That effect is apparently superimposed on the Ornstein effect, which has been argued to be a post-transducer level effect and which appears by these data not to be central. Thus, the taxonomy of mental processes proposed by Fodor is supported by this experiment.

In this task the percept of constant duration is assumed to be used as a comparator value influencing the clock time interval between finger taps. The percept of duration is argued to be a cognitive phenomenon displaying (at least) input system and central processes effects. The task is composed of a rather slow (@ 1 hz) rhythmic movement. Therefore, representational, cognitive level intervention in motor tasks is not restricted to feedforward systems teleologically necessary for controlling rapid movements.

It should be remarked that the differences in the inter-tap interval between successive taps were rarely greater than 10 msec and never observed to be greater than about 70 msec. It was not unusual in the premanipulation sequence to see tap intervals varying only within the ± 1 msec accuracy of the clock for several taps in a sequence. The main effects occurred as the intertap interval changed in 5 or 10 msec increments over several taps. That seems in accordance with the view that multiple processes are at work in maintaining the tapping rate. Undoubtedly the peripheral proprioceptive information that would arise from sudden changes in rate would invoke purely noncognitive corrective motor feedback. Occasionally, a subject would produce an unintentional and spurious tap that was immediately recognized. The regular rate was always immediately restored. Such clearly indicates peripheral system effects.

The cognitive effects seen in this experiment exert higher level control of the kind postulated in the oculomotor system by Robinson (1975) or Bahill and McDonald(1983), or in limb systems by Kelso *et al.* (1981). These effects are, in the above studies, always related to an internal representation of system dynamics producing expectations of a form that affect error signals to the peripheral system, for example, a belief concerning one's head position relative to a target stimulus position affects the saccade executed to fixate the target. This effect is independent of the more mechanical vestibular inputs that provide low-level information of that kind. The operational form of the belief is taken to be manifested in a biasing value added to a system error signal (see the discussion in Robinson (1975)).

In summary, the results of the experiment imply that cognitive processes involved with the percept of duration influence movement timing. Central process and input system effects can be distinguished in the data. The nature of these effects corresponds to those attributed to "higher levels" in existing systems analysis of a variety of motor systems.

Central processes inherently involve what Fodor calls isotropy and quineian conditions. He (Fodor 1983) argues those conditions cause serious difficulty for what I have termed the determination of the locus of control. It does seem, as far as central processes are concerned, that a straightforward structural analysis of neural function is unlikely to prove fruitful: since arguably central processes have functional relevance to the control of movement timing, these difficulties would appear to apply directly to systems analysis of the motor system. The magnitude of the central processes effect is predicted by a model based on probabilities which derive from an epistemic structure. Therefore, the global knowledge state of the cognitive system seems to demonstrably influence movement timing.

8. Conclusion

The motivation to undertake this thesis was purely applied. Much EEG research derives from the desire to develop a method by which to monitor the cognitive load of humans engaged in such activities as flying high performance aircraft or space vehicles. This is a natural outgrowth of the systems analysis of manually controlled systems and human factors engineering, and is one of two areas in which the problem of a hierarchy of psychological processes has recently become central to important technological issues. The other general area is program specification and the software life cycle problem. Indeed, I am inclined to believe that, if one takes the Turing Machine metaphor seriously in relation to canonical input system descriptions, an appropriate metaphor for central processes is program specification and the life cycle problem. In any case, what motivated this research was a practical need to attempt to delineate what could be said about EEG source localization in relation to cognitive processes and, thereby, for example, cognitive load.

Recently a perceptual time compression effect that appears to be related to cognitive processes has been implicated in mission-degrading astronaut performance (Christensen and Talbot 1984). Michon (1966) demonstrated that increased cognitive demands caused increased variability of fingertapping frequency in a rate matching task. It is well known that changes in cognitive load affect the percept of time (see Ornstein 1969). Both overloading and depriving the senses have effects.

Since global task demands affect both the temporal aspects of perception and timing aspects of motor performance, it was natural to focus on that issue. At the point that it became apparent that an adequate interpretation of representational processes was lacking, the research turned in the direction that this thesis recounts.

Cognitive influences on motor behavior have commonly been assumed to exist and have an important role to play in the functional descriptions of motor systems. The experiment in the last chapter demonstrates that two levels of cognitive processes can be distinguished in the timing of a slow rhythmic movement. These levels of process have very different prospects for neurological structural analysis and make it clear that when one seeks to undertake structural analysis of a mental function it is necessary to address the appropriate level of causality.

Central processes have their causal effect in virtue of their formal structure and independently of neural instantiation. They are properly thought of as constraints of specification on input systems. Input systems are functionally defined and obtain behavioral consequences due to their instantiation in neural structures. The causal relationship that exists between specification and neural implementation is type-distinguished from the causal relationship that exists between the neural activity and the behavioral consequences of that activity. The appropriate structural analyses are therefore also type-distinguished.

There is good reason to believe that cognitive processes, up to the input system level (these should really be called input-output systems), often can be neurally localized and that they therefore offer good prospects for rather standard structural analysis. Functional equivalence of structures tempers the optimism a bit, but many structures are likely "hardwired." Central processes do not seem to offer the prospect of locality.

There are some hopeful signs for neural correlates of central processes, however. The well-known P300 component of the human evoked potential has been determined to be strongly correlated with the subjective expectation of an observation (Sutton, Zubin and John 1965, Sutton, Tuetting, Zubin and John 1967, Horst, Johnson and Donchin 1980). It has been demonstrated that the P300 is not specific to the sensory modality, the stimuli or task contigencies and particulars. That is, the quineian nature of belief is borne out by the characteristics of this epiphenomenon.

Eriksen (1984) found that the equivalent dipole solution associated with the P300 achieved a best fit with a single dipole located in the center of the head. It would be absurd to conclude that the "belief cell assembly" resides, say, in the pons. This dipole, even if associated with actual local dipole-like neural activity in a structure located in the center of the head, is best viewed as a statistical object associated with the ascription of belief. As a physiological indicator of that process it has a valuable function, but as a basis for proximal structural analysis of central processes it makes little sense.

I am therefore arguing that where behavioral analysis provides evidence for central process involvement, that something like the "biocybernetic channel" of Donchin (1979) is the appropriate level of analysis for the neurophysiological correlates of that aspect of the behavior. Localization of the associated input processes may allow more usual neurophysiological analysis.

The exact nature of a structural analysis of input processes is not entirely clear, but Freeman (1983) points to a very compelling interpretation. He demonstrated with olfaction in rabbit that (in a classical conditioning paradigm) the EEG mapping on the olfactory bulb did not change if an unexpected, unreinforced odor was presented or if an expected and reinforced odor occurred. The pattern did change if an unexpected odor was reinforced or if an expected odor was not reinforced. The EEG pattern that stabilized on the bulb during conditioning persisted between trials. Notice that it is unlikely that the animal *smelled* the expected odorant between trials. This is not a case of reflexive, stimulus-bound neural representation, for, if it were, the particular EEG pattern on the bulb would (a) be unique to and occur only upon the presentation of each distinguishable odorant and (b) would always co-occur with the percept of that particular smell. Neither condition is met. However, there is overwhelming reason to think that the olfactory bulb is associated with the percept of odor. A reassessment of the idea of neural representation seems called for. This particular neural correlate appears to be associated with the expectation of odor, as in a hypothesis to be tested, derived from a situation not exhaustively expressible in terms of the olfactory stimulus parameters. The expectation must involve the conditioned and unconditioned stimuli and task relevancies. It thus involves intermodal and isotropic relationships: this is a correlate of belief. It is unlikely that the olfactory bulb is the *site* of the central process, but it is strongly indicated that central processes may be investigated by use of measurements obtained on the olfactory bulb. The details of the brain functioning that leads to the EEG patterns above described would constitute a structural analysis of input and transducer level processes.

The equivalent dipole source localization techniques seem very well suited to such studies. Since there is a reasonable expectation of neural locality for many input systems and since their number and locations during mental activity are not known, the ability to use the field distribution on the head to resolve loci and dynamics could conceivably be very useful. The same equivalent dipole solutions can be treated as statistical objects in relation to whatever component of the behavior is previously determined to be centrally mediated. The separation of behavioral effects according to the levels of processes underlying them would allow more appropriate interpretations of the equivalent dipole solutions and thereby enhance their use as a tool to monitor processes underlying performance of tasks. In particular, I am proposing that, for behavior displaying identifiable central process- and input system-related phenomena, such as the fingertapping task in this thesis, a dipole model provides unlike information to different levels of analysis. In combination, the analyses will be much more useful than either a purely correlational analysis or a purely tissue locus analysis. The ability to focus appropriate interpretations of the dipole on appropriate components of the behavior will improve the use of EEG in relation to cognitive functioning and thereby enhance its applicability to engineering problems.

Because it has been appreciated for some time (e.g., Fender 1985, in press, Donchin 1979) that cognitive level functioning brought special interpretive difficulties to the EEG, much more effort, especially with source localization, has been expended on sensory systems. It is generally assumed that the perceptual apparatus is more accessible to explanation than is cognition. In the experiment in this thesis, the time percept was evidently not independent of cognitive processes. If Rock (1983) is close to correct with the thesis that much perception is thought like, the possibility exists that the perceptual systems should be carefully assessed before undertaking serious electrophysiological analysis of the functional architecture involved.

Zajonc (1980), shows evidence that like-dislike judgements yield psychophysical recognition thresholds that are *lower* than the detection thresholds for the same visual stimuli. It is widely known that the recognition threshold for "DATE" is lower than the recognition threshold for "EDTA." One wonders if that is also true for chemists.

Pritchard (1961) found that, when the visual receptors were fatigued by stablizing a meaningful image on the retina, the percept of the image degraded in such a way that its meaningfulness was preserved. For example, "BEER" might progressively degenerate to "BEEP," "BE" etc. before vanishing. That certainly implies very high level processes.

Finally, there are (in rabbit) descending pathways from visual areas to the retina. It has been found that transection of the optic nerve causes changes in the ERG, suggesting central influences on retinal function (Fender, personal communication).

In total, it seems likely that a hierarchy of perceptual processes is involved that is similar to the hierarchy of mental processes and similar care should be taken in their electrophysiological analysis.

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Appendix

Experiment Instructions

The task to which you are asked to apply yourself has *two* aspects of *equal importance*.

1. make the stimuli appear on the monitor at a *constant rate*
2. determine *what stimuli* you have seen and their *relative frequency*.

Explanations follow:

(1) As stated above, one aspect of the task is for you to attempt to cause the visual stimuli to appear on the screen at a *constant rate*

--A stimulus will be presented each time that you tap your finger in the device on your right. The experimenter will fully explain its function to you and answer any questions that you might have concerning it. The constancy of the stimulus presentation rate is obviously determined by the constancy of your fingertap rate.

--The stimulus will remain visible until you tap your finger again, whereupon it will be replaced by another stimulus. Occasionally the stimulus before and after the tap will be the same. The sequence is prearranged and may contain successive repetitions. Do not allow such repetitions to distract your attempt to produce an absolutely constant rate. The sensing device is very reliable and will not "miss" a fingertap. So, upon encountering a repetition, do not wonder if the system failed to detect your tap and for that reason did not change the stimulus.

(2) The second aspect of the task involves an attempt to remember the entire population of stimuli presented to you (it is small) and the relative frequencies of the stimuli. The determination of relative frequencies will probably feel like a best guess -- that's okay. So try to keep track of what stimuli you see and try to judge which is most frequent, which are equally frequent, and which is least frequent. As the experiment progresses, continue to update your judgements. Don't decide you know *for sure* everything about the stimulus population until the experimenter tells you the experiment is over. Don't try to count in this sequence; it is pretty *long!*

At some point in the first half of the stimulus sequence, the program will temporarily stop the presentation of stimuli and inform you that you will be asked some questions about the stimuli. You will then answer some questions. The program will inform you when the sequence will be continued. When the BEGIN statement appears midscreen, fingertaps will commence the sequence of stimuli as before.

At the end of the stimlus sequence, more questions will be asked. You will be informed of the end of the experiment.

(Now, some words on the stimuli.) They will all be numerals or written names of numbers. Notice that the numeral "64" represents a quantity; in fact, the word "sixty four" represents exactly the same quantity. Therefore, in the sense of the quantity (the *number)* they represent, "sixty four" and "64" are entirely equivalent names. In this experiment, there is no reason to distinguish between these names, either for purposes of determining relative frequency or for purposes of remembering the stimulus population.

For example, if asked which ***numbers*** occurred after having seen "64" and "sixty-seven" the following responses will be considered entirely identical:

a. "I saw 64 and 67"

b. "I saw sixty-four and sixty-seven"

c. "I saw sixty-four and 67"

d. "I saw 64 and sixty-seven"

As a further example, if "64" occurs once, "sixty-four" occurs once and "sixty-seven" occurs once, for the purposes of this experiment, the ***number*** 64 occurs twice and the ***number*** 67 occurs once, so 64 is more frequent than 67.

The questions concerning the stimulus population and relative frequencies *always refer to **number***, and never refer to the names of the number. As you see the stimuli and as you answer the questions ***think** only of the number the stimulus represents*.

Finally, TRUST ME (really). There is nothing "tricky" about the stimuli, the task, or the questions -- no hidden motives, etc. So, please simply try to perform the task and don't try to second-guess the experimenter. It's easy to second-guess this experimenter, anyway.

Finally, finally, summary notes:

note 1 please ***don't*** vocalize the stimuli as they appear

note 2 your finger is wrapped to reduce tactile information

note 3 the headphones are intended to reduce auditory information

Remember: Do two things at once; they are equally important.

1. make the stimuli appear at a *constant* rate by tapping your finger.
2. determine what stimuli you have seen and judge their relative frequency. Don't assume your determination or judgement is final until the experimenter tells you the experiment is over.

Thank you for your participation in this experiment. You'll be debriefed at the end of the session.

Begin when you are ready.