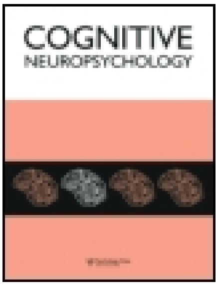

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On reducing language to biology

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On Reducing Language to Biology

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We are concerned with the relation of the psychological level of explanation to biological and formal accounts of the same phenomena. Our position with respect to the relations between different levels of explanation is that constraints operate only under very restricted circumstances. We use data from word recognition and speech perception to show that cogent accounts of the psychological processes involved require the establishment of purely psychological constructs, which are to be judged in terms of their explanatory usefulness rather than their compatibility with formal descriptions, on the one hand, and neurophysiological data on the other. Information processing models appear to be a suitable framework for expressing the psychological constructs. We examine briefly the way in which attempts have been made to account for developmental studies, results in dichotic listening, and neuropsychological work in terms of the architecture of the cortex rather than through psychological accounts of the subject's performance. While functional descriptions of the processes underlying psychological phenomena are immutably psychological, we accept that causal accounts of these phenomena may often best be given in terms of their biology.

INTRODUCTION

There are many issues raised when we ask the question, "What relevance do neurophysiological findings have for psychological models?". One issue concerns whether or not neurophysiological data impose any constraints

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whatsoever on psychological models. A guiding assumption of modern day cognitive psychology is that brain activity is related in at least some way to mental activity. Hence, one would ultimately expect to find some constraints between events in the neurophysiological substrate of the brain and psychological processes. However, to admit of constraints between the neurophysiological substrate and psychological processes is one thing; to describe the form which these constraints might take is quite another. In particular, the form of any constraints between underlying neurophysiology and psychological processes depends crucially on the way in which psychological processes are mapped onto the neurophysiological substrate. There are a variety of forms the mapping could take, including a one-to-one mapping, a many-to-one mapping, a one-to-many mapping, a many-to-many mapping, or some combination of these. Moreover, prior to the determination of any mapping function, it is first necessary to establish which elements at each level are to enter into the mapping function. In other words, what gets mapped onto what. Hence, with respect to the underlying neurophysiology, are the appropriate elements activities of specific brain locales or some more general form of activation that we might refer to as a brain state? Similarly, with respect to the psychological processes, many questions are raised as to how global or specific the elements are that enter into the mapping relationship.

Given the amount of uncertainty that surrounds the nature of the mapping between neurophysiology and psychology, it would seem wise to refrain from a premature interpretation of the way in which particular findings in neurophysiology and neuropsychology constrain possible psychological models. Unfortunately, there has been, and still is, a tendency among many neuropsychologists to treat findings regarding the neurophysiology of the brain as data that can directly confirm or refute psychological models. This tendency is most clearly present among researchers who adopt implicitly the assumption that there is a one-to-one mapping (or type-correspondence) between brain locales and psychological processes. The assumption in question is one which is adopted on grounds that it is the simplest assumption that one can make about the relationship between neurophysiology and psychological processes. However, we argue that this assumption is not supported by previous findings in the field, and that there is no justification for assuming that it will be supported in the future. Indeed, the adoption of this view that psychological processes are localised in the brain is apt to impede scientific progress by prematurely limiting the search for the appropriate elements that might ultimately enter into some mapping relationship of psychological processes onto the neurophysiological substrate.

Our paper focuses on the problems associated with assuming that psychological processes can ultimately be localised in the brain. In particular, we note that localisation does not substitute for a psychological explanation of some behaviour, nor is it apt to contribute much to attaining a psychological explanation. We explore the reasons why this state of affairs exists by considering previous research which has implicitly or explicitly adopted the localisation view, and we also examine a number of hypothetical cases where some contribution of localisation explanation might be expected. In each instance, we find shortcomings with the localisation view. Finally, we discuss ways in which research on the neurophysiological substrate may serve to inform those seeking a psychological explanation of some behaviour.

THE NATURE OF THE PROBLEM

We are concerned with a trend in the study of language that identifies the understanding of linguistic processes exclusively with the localisation of those processes in the brain. More specifically, the trend is toward strict or focal localisation of language function, in whole or in part, in the brain. The problem with this trend is that it may lend credence to the belief that the psychological modules will correspond in some simple way to modules currently proposed by anatomists. In this way brain scientists revert to the errors of the late nineteenth century. In that era, a number of quite complex models were produced that anticipated some of the facets of present day information processing models (Morton, in press). Unfortunately, these models were linked too tightly to the anatomical substrate and thus an attempt was made to achieve a precise localisation of mental centres. When patients were found who failed to show the psychological disturbance predicted from the location of their lesions, the intellectual convolutions required to rationalise these findings led to the destruction of the whole enterprise (see Head's (1926) comprehensive, though biased, review of this)[1]. If we are to avoid making the same kinds of errors, it is necessary to adopt a much more flexible approach to the relationship between psychological processes and the underlying physiology. Indeed, the nature, extent and locus of biological structures corresponding to any sort of language module will have to be determined through empirical research rather than deciding in an *a priori* fashion that the module is limited to a well-defined locus in the cortex.

Before proceeding further we wish to make a terminological distinction between, on the one hand, the use of data from brain-damaged patients and, on the other hand, the activity of mapping psychological functions into the brain. It should be clear that we consider these to be separate activities. Many writers, however, confuse these activities by labelling them with the same term. Arbib and Caplan (1979) use the term "neuro-linguistic" in both

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[1] Lichtheim (1885) was a notable exception to this trend, but his purely psychological endeavour was destroyed by association with his reductionistic contemporaries.

senses but argue as if there were only one. We will use "neuropsychological" to refer to the use of psychological data from brain-damaged patients and state explicitly when we are discussing the mapping activity.

DIFFERENT OBJECTIVES DEMAND DIFFERENT EXPLANATIONS

Let us consider what the danger is in assuming a very simple and direct mapping between brain anatomy and cognitive functions. Suppose a psychological model were specified together with a biological model as a hypothetical underpinning. Then it would be easy to be misled into believing that the psychological model was inadequate if the specific biological model that had been postulated were found to be in error. However, such a belief would be unwarranted. The validity of any psychological model is determined by its ability to account for psychological facts e.g., the sequence of processes and decisions that figure in the production of some behavioural act. To believe otherwise is to adopt the assumption that all predicates in valid psychological theories will correspond directly to predicates in valid biological theories. Note that this assumption is the strongest possible one about the relationship between biology and psychology. It is by no means a necessary assumption. As others have noted (e.g., Fodor, 1975; Osherson & Wasow, 1976; Putnam, 1973, 1980), it may turn out that the theoretical constructs that provide the most satisfactory account of some phenomenon on one level of description do not map simply onto the most useful constructs on another level of description. The lack of such a direct mapping between theoretical constructs is, in part, a consequence of the existence of different objectives for each distinct level of explanation.

From our viewpoint, the objectives of explanations of language at the linguistic, psychological and neurophysiological levels do differ.[2] Following Osherson and Wasow (1976), we conceive of the objectives of each level in the following manner. The goal of a linguistic level of explanation can be construed as describing "the class of automata (called 'grammars') that are sufficient to provide a formal characterisation of natural language". The goal of a psychological explanation for language can be taken as describing "the real-time sequence of decisions and information exchanges that figure in the production and processing of language". The goal of a neurophysiological account of language is to specify the "physical structure and processes that mediate language use". Nothing in excess of what is said in this paragraph ought to be read into our use of the terms "psychology",

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[2] The three authors have not always managed to achieve agreement with respect to the way in which some of the complex ideas underlying this paper have been simplified for the purposes of exposition. In particular, Mehler acknowledges suggestions by Fodor that the "levels" we discuss are not *a priori*, universal, sharply defined and immutable, but rather are overlapping and a function of local trends in the progress of science.

"linguistics", and "neurophysiology" throughout the paper. We view those disciplines as serving these objectives, and our arguments concerning which facts are relevant to the determination of a given type of theory are based on this assumption.

Ultimately, each of these three objectives allows the postulation of levels of explanation that will be important in understanding language. However, owing to the difference in objectives at each level, an explanation of language at one level cannot supplant an explanation at another. Furthermore, within a given level, the validity of any particular theoretical construct should be judged in accordance with its utility in meeting the objectives of explanation at that level. We read Marr (1982) to be making a similar point in his discussion of the different levels at which an information processing device must be understood, these levels being computational theory, representation and algorithm, and hardware implementation. In discussing these levels he notes that "These three levels are coupled, but only loosely. The choice of an algorithm is influenced by what it has to do and by the hardware in which it must run. But there is a wide choice available at each level, and the explication of each level involves issues that are rather independent of the other two" (Marr, 1982, p. 24). And later on the same page he notes that "since the three levels are only rather loosely related, some phenomena may be explained at only one or two of them".

The fact that explanations at the various levels serve different objectives helps to explain why there may be no simple mapping between theoretical constructs at the different levels. Fodor (1975) encapsulates this point nicely in an example that he presents. He notes that the failure to state Gresham's Law in terms of a set of appropriate predicates at the level of explanation in physics would do nothing to detract from its appropriateness as a useful and correct theory in economics. Similarly, one need not stretch the imagination very far to conceive of a possible parallel situation involving psychology and neurophysiology. For example, it is perfectly plausible that a particular psychological process might be underpinned at different times by a variety of different neurophysiological processes and yet from the perspective of the neurophysiological level of functioning there may be no coherent reason for treating these processes as members of the same natural kind. Indeed, exactly something like this happens in the case of speech production, where, owing to differences in the shapes of their oral cavities, different speakers may produce the same speech sounds using very different vocal gestures. Better yet, under different circumstances (e.g., with or without a pipe in one's mouth), the same speaker may produce the same speech sound in an entirely different manner. From the point of view of describing the underlying neurophysiology of the motor system, grouping these diverse gestures as the same kind is not apt to be the most coherent description of its underlying organisation, whereas the fact that these diverse articulatory manoeuvres have similar acoustic consequences is of considerable importance with respect to a description of language processing.

The essential point here is that, in attempting prematurely to conform to units at the level of the neurophysiological structure of the brain, researchers may not be able to attain an appropriate or parsimonious description of psychological processes. If a reduction from psychological to neurophysiological processes is forthcoming, it will only come about as a result of first determining what the necessary theoretical constructs are on each level. The considerations that determine the appropriate set of constructs at a given level of scientific description are particular to that level. Hence, constructs in a psychological explanation are chosen for their utility in providing an account of psychological processes, whereas a different set of criteria determine the selection of meaningful elements in a neurophysiological account of brain functioning.

Thus, with respect to descriptions of the processes of mental activities, there is no special status conferred on biological data. They are no more real or significant than data gathered using the methodology of psychology. Indeed, in the case of a conflict between the two, the psychological data are likely to take precedence. A similar view has been put forth by Chomsky, who noted:

> Suppose now that someone were to devise an experiment to test for the presence of a wh-clause in underlying representations—let us say, a recognition or recall experiment. Or let us really let down the bars of imagination and suppose that someone were to discover a certain pattern of electrical activity in the brain that correlated in clear cases with the presence of wh-clauses, relative clauses (finite and infinitival), and wh-questions (direct and indirect). Suppose that this pattern of electrical activity is observed when a person speaks or understands (a particular sentence). Would we now have evidence for the psychological reality of the postulated mental representations?
>
> We would now have a new kind of "evidence", but I see no merit to the contention that this new evidence bears psychological reality whereas the old evidence only relates to hypothetical constructions. The new evidence might or might not be more persuasive than the old; that would depend on its character and reliability, the degree to which the principles dealing with this evidence are tenable, intelligible, compelling, and so on. (Chomsky, 1978, pp. 208–209.)

Although Chomsky's aim here is to deflate the notion that biological data are somehow more "real" than psychological data, it is not his (nor our) intention to claim that data gathered by neurophysiological or neuropsychological methods are useless with respect to psychological theories. Indeed, as we indicate below, there are circumstances (albeit restricted) under which such data can make important contributions to the development of psychological theories.

Unfortunately, however, our fields seem to be dominated by the belief that our understanding of a given mental capacity increases in proportion to the extent that it can be localised in the cortex. Hence, although a claim that language competence is a property of the human cortex would be considered almost trivial, considerably more importance is attached to a statement that it is a property of the left hemisphere, and better yet if a specific locus in the left hemisphere is implicated. However, with respect to furthering our understanding of the psychological processes underlying language capacities, such statements are in themselves of little consequence. They would be important only to the extent that they could be shown to have different consequences for the psychological processes involved in language capacities. Much the same view was expressed by Hilary Putnam in his *Philosophy and our Mental Life* when he said:

> The conclusion I want to draw from this is that we do have the kind of autonomy that we are looking for in the mental realm. Whatever our mental functioning may be, there seems to be no serious reason to believe that it is *explainable* by our physics and chemistry. And what we are interested in is not "given that we consist of such and such particles, could someone have predicted that we would have this mental functioning?", because such a prediction is not *explanatory*, however great it may be. What we are interested in is this: can we say at this autonomous level that since we have this sort of structure, this sort of programme, it follows that we will be able to learn this, we will tend to be like that, and so on? These are the problems of mental life—the description of this autonomous level of functioning—and that is what is to be discovered. (Putnam, 1980, p. 139.)

SOME PAST CASE HISTORIES

Generally, the areas that have suffered most from believing that any phenomenon under investigation may be explained simply by pointing to its locus are: studies of lateralisation in normal subjects; studies of the effects of brain lesions; and hemispheric specialisation during language development. In what follows, we discuss instances where one can be led astray by too closely identifying psychological processes with particular locations in the brain.

Lateralisation. Since the time of Broca it has been claimed that language is a capacity largely mediated by the left hemisphere (at least for most right-handers). However, despite this widely held belief, the exact nature of the relationship that links language capacity to the left hemisphere remains obscure. Thus, some have postulated that the sole role for the left hemisphere in language processing is that it serves as a common path during the perception and production of speech (Broca, 1865). Others have argued that there are specific structures in the left hemisphere that serve various aspects of linguistic functioning (Geschwind, 1965). However, when comparisons are made of left and right hemisphere functioning, the postulated differences between the two hemispheres often take on a much more abstract character. For example, some have characterised the left hemisphere as analytical or propositional and the right hemisphere as holistic or appositional (e.g., Bever, 1980; Bogen, 1969; Harnad, 1982; Jackson, 1932). Others have claimed that the only difference between the two hemispheres is initially one of the mode of temporal analysis (Schwartz & Tallal, 1980).

The fact that there are so many diverse views regarding the functioning of the two hemispheres is at least in part attributable to a lack of understanding of processing aspects of the brain rather than just its hardware properties. In place of gaining a better understanding of each of these aspects, a more trivial approach has been substituted. Psychologists have tried to attribute every conceivable parameter to either the left or right hemisphere. The list of variables that have been tested is already large and still growing (e.g., melodic recognition; music notation; random dots; visually presented musical notes; tones of voice; intonation contours; frequency discrimination; intensity discrimination; etc.) There have been few attempts to present materials systematically in order to assess possible processing differences. Moreover, the test conditions and the measures used to assess laterality effects have varied considerably and it is by no means clear exactly what the appropriate laterality measure should be (Holmes & Marshall, 1974a,b; Marshall, Caplan & Holmes, 1975; Richardson, 1976; Satz, 1977; Studdert-Kennedy & Shankweiler, 1981). Indeed, Birkett and Wilson (1979, p. 512) conclude that laterality measures may be of some limited use only in comparing experimental results from different sources (i.e., groups, tasks and procedures) rather than in evaluating theories of cerebral asymmetry.

In some cases the methodological controls have removed apparent hemisphere effects. Thus, Safer and Leventhal (1977, p. 75) conclude "each hemisphere's specialization for tone and content can be greatly affected by the demands of the experimental task".

The main upshot of all this has been to present researchers with the task of labelling a common variable to subsume those instances in which the data suggest a processing advantage for one hemisphere over the other. Unfortunately, once such a label has been found, it can easily lead to circular reasoning such that if one assumes that the right hemisphere processes in a holistic fashion, then the discovery that a given task is completed more rapidly or efficiently by the right hemisphere is taken as sufficient evidence that the task involves holistic reasoning. No independent criteria are offered in support of this sort of attribution. Thus, Safer and Leventhal (1977, p. 81) merely assert "Recognizing expressive patterns (in speech) is consistent with the right hemisphere's specialization for holistic processing" without any conceptual justification. Bradshaw and Nettleton (1981) make a similar point.

It might be useful at this point to ask just what it might mean to make a statement of the form "x is processed in the left hemisphere", where x is some psychologically relevant characteristic. Such a statement is of interest to psychologists only to the extent that it reveals something about the way that *x* is processed; otherwise it is as empty as saying that *x* is processed by the brain. Of course, one might argue that attempts to assign the processing of some psychologically relevant characteristic to some location in the brain does indicate something about the way it is processed. For example, could data obtained from brain localisation studies be brought to bear on the issue of modularity? (See Fodor, 1983, for a complete discussion of modularity.) In other words, could data from such sources reveal whether a particular function is specific to one cognitive domain or is a general cognitive processing capacity? It is difficult to see how localisation data could be particularly informative in such a case because there is no *a priori* reason to suppose that a specific cognitive module must be narrowly localised in a particular portion of the cortex. Indeed, it is perfectly plausible that any such cognitive module would be widely dispersed throughout the cortex. But the situation is even worse than this. Suppose it were the case that all the available information processing measures (e.g., response latencies; interference effects; error analyses; developmental data and the like) pointed to the existence of some cognitive module. Moreover, assume that the existence of such a module was required to explain performance on a variety of different tasks. Finally, when the latest and most accurate measures of brain localisation are applied across these task settings, it is discovered that for half of the tasks one specific locus of the brain is active during the operation of the hypothesised cognitive module, but for the other ones an entirely different locus is active. Would we now want to conclude that there are two different cognitive modules instead of one? We think not. In general, it would be more prudent to conclude that there are two different neurophysiological modules that underlie the same cognitive module—at least until such time as the facts about cognitive functioning warranted the postulation of more than one.

These considerations apply equally to any other neuroscientific approach. We can take as a case study the work of Ojemann (1983). The technique used here is that of electrical stimulation of the brains of patients prior to operations to control epileptic seizures. We are not concerned here with the many methodological problems associated with the purely biological aspects of this work, but rather with its epistemological status. The technique involves stimulating the brain of the patient with small electrical currents at particular moments in the performance of a variety of tasks. Ojemann (1983, p. 191) states that "stimulation effects on diverse behaviors can be assessed at a given site, with the pattern of changes at a particular site providing evidence about the role of that area in language". Our only point is that the extent to which this objective can be achieved is simply a function of the degree of sophistication of the psychological models which are being employed. The "roles" that Ojemann mentions are, and can only be, psychological functions. What is being discovered is the nature of the mapping from psychological function onto the brain. The utility of such studies for the "pure" psychologist—that is, for someone who is simply interested in describing the psychological functions—lies in the ability to dissociate or doubly dissociate pairs of tasks.[3] Thus, if two tasks A and B are presented to the patient and a variety of locations on the brain are stimulated, it may be found that there is a close association between the two tasks in that locations whose stimulation affects the one are also implicated in the other. Alternatively it might be found that the tasks are distinct, or partially distinct, in this respect. Assuming that such findings can be taken literally (i.e., that there are no artifacts), then they constitute data for the psychological theorist in much the same way that data from dual task performance constitute data. The force of the data is that the psychological theory must eventually represent such overlaps. The problem is that the overlap may end up being represented at any of the available levels of description. Thus, to use Stabler's (1983) terminology, the overlap may be at the level of the computation, the algorithm, the mechanism or the hardware.

In any case the same caution should be observed with the neuroscientific evidence as with other data. Ojemann (1983, p. 195) illustrates the problem himself. Having shown that there is overlap between the sites involved in mimicking facial movements and those involved in discriminating speech sounds he claims to have identified a cortical area "that has common properties of speech perception and generation of motor output, one with the common functions described by the motor theory of speech perception". It would be an easy error to infer from this quotation that the motor theory had received independent support. However, as Ojemann points out later, the area involved may reflect some device, which is used by both perceptual and motor functions, such as a system for precise timing. Such an approach contrasts with the claim that the findings "also support the contention that a large portion of the brain related to language is fundamentally a part of the motor system". (Ojemann, 1983, p. 195) A more accurate assessment might be that the observations force us to the conclusion that what we thought to be the motor system turns out to have other functions.

Finally we should point out that while the dissociations are valuable data it is of no concern to the psychologist *where* any of these locations are to be found, or how the different kinds of locations are distributed in relation to each other. These are, of course, of concern only to those of us interested in the mapping function.

Whereas it is possible to ask questions about the nature of psychological processes using neurophysiological methods, it is all too common to find studies in which the sole end appears to be linking performance on some task

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[3] This is not an attempt to prescribe what a "psychologist" should properly be interested in; it is merely a shorthand. Of course, psychologists may wish to work on the mapping between psychological and neurophysiological levels, as they may wish to work on the relevance of psychology to sociological variables.

to a given locale in the brain. Hence, rather than systematically attempting to determine the extent of some hypothesised cognitive function by tracing out its time course, stages, and possible uses in psychological tasks, there seems to be a tendency towards cataloguing the extent to which relatively minor variables can be localised. Consider the studies involving dichotic listening procedures. Over the past 20 years there has been an epidemic of studies in which different material was presented to the two ears. Thus, there are studies involving the presentation of numbers (Kimura, 1964), environmental sounds (Curry, 1967), nonlinguistic human sounds (Carmon & Nachson, 1973; King & Kimura, 1972), melody and timbre (Spellacy, 1970), rhythm (Robinson & Solomon, 1974), intonation (Blumstein and Cooper, 1974), and various sorts of speech contrasts such as voiced vs. unvoiced plosives (Shankweiler & Studdert-Kennedy, 1967), fricatives vs. vowels (Darwin, 1971), and place and voicing of plosives (Cutting, 1972). The basic assumption underlying these studies is that if asymmetries between the two ears are found in the pattern of responding to the stimuli then the processing of the variable being examined is attributed to the hemisphere opposite the ear with the better score. This approach is well summarised by Zurif and Mendelsohn (1972, p. 329):

> Dichotic presentation has now become an established technique in the study of hemispheric differences in the normally functioning brain and has been used to find out precisely which aspects of speech perception depend upon the mechanisms of the language-dominant hemisphere.

Initial findings from these studies suggested that the processing of linguistic information was the province of the left hemisphere. The fact that a left hemisphere superiority was obtained for consonants but not for vowels fits nicely with other results obtained in speech perception studies and was cited by Liberman, Cooper, Shankweiler, and Studdert-Kennedy (1967) as converging evidence that the left hemisphere housed a special device for processing in the speech mode.[4] However, subsequent research, using the same ear superiority interpretation, suggested that there are some aspects of the speech signal that are dealt with by the right hemisphere. For example, Day, Cutting, and Copland (1971) found that syllable pitch yields a right hemisphere advantage, as does intonational contour (Blumstein & Cooper, 1974), tone of voice, and emotionality (Carmon & Nachson, 1973; Haggard & Parkinson, 1971; King & Kimura, 1972). If one were to accept the

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[4] It should be noted that Liberman *et al.* drew their conclusion regarding a specialised speech mode on the basis of information gained using a variety of different paradigms. Hence, the data obtained from dichotic listening experiments were only used to bolster arguments regarding psychological processing analysis of other results. Thus, whatever the ultimate validity of their conclusions, Liberman *et al.*, at least attempted an information analysis before postulating a link between brain structures and psychological processes.

equation of ear advantage with processing by the contralateral hemisphere, such data would imply considerable interhemispheric cooperation in speech perception.

However, the interpretation of ear advantages as indicative of specialised processing by the contralateral hemisphere has been called into question. In particular, it has been suggested that findings of hemispheric superiority can be interpreted in terms of strategy differences rather than the result of some specialised mode of processing (Bever, 1980; Kinsbourne, 1978; Morais, Note 2.). Freides (1977) concludes that "what dichotic procedures mostly measure are response strategies" (p. 251). Furthermore, Morais (1976) demonstrated that linguistic judgements do not necessarily lead to right ear advantages.[5] One might think that such results would give pause to someone who holds the view that specialised language processing mechanisms are located in the left hemisphere (and that dichotic listening experiments reveal this), but little attention seems to have been given to this problem. Yet it is difficult to imagine how one can obtain shifts in hemispheric superiority for a given stimulus dimension if a specific cortical locus is responsible for processing that dimension.

Problems like the one just described arise when investigators lose sight of the goal of explanation to which their work is properly directed. The attempts to map the processing of some psychological characteristic onto some specific location in the brain has distracted researchers from understanding the nature of the processing that takes place. As a result, investigators in this area have been concerned more with testing all kinds of stimulus-bound features in an effort to determine whether they are processed faster or more accurately when presented to the left or right ear than they have been with trying to understand the psychological processes themselves. Statements like "the left hemisphere is more important for recognition of music notation than the right" (Salis, 1980, p. 286) do not seem to advance any cause.

Brain Lesions. Another area of research in which there have been attempts to link psychological characteristics directly to brain structures is the study of brain lesions. As psychologists, our contention is that the psychological ramifications of such lesions ought to be interpreted in the context of

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[5] Evidence that is, perhaps, even more damaging to the specialised processing position comes from a closer inspection of the data from dichotic listening studies. The typical practice in such studies is to report simply the magnitude of the effects obtained. Often the data are not broken down with respect to how many subjects show a left ear advantage and how many show a right hemisphere advantage. Moreover, little attention seems to have been given to the issue of just how stable such advantages are, either within a single test session or across repeated test sessions. It is worth noting that on those occasions when it has been checked, up to 30% of the subjects failed to preserve their initial ear superiority during subsequent testing (Blumstein, Goodglass, & Tartter, 1975; Pizzamiglio, de Pascalis, & Vignatti, 1974; Shankweiler & Studdert-Kennedy, 1975).

information-processing models. The psychological account of what a lesion does must be stated in terms of types of processes whose functions have been impaired or in terms of the interruption of transmission of information between two different processes. The nature of the processes under this description does not seem to be derivable from a study of the localisation of lesions. Rather, one must first have some idea of what the processes are themselves, then the effects of the lesions can be interpreted in terms of the psychological impairments that result. Simply to localise the lesion to the nearest millimeter is not apt to help in this process. To understand the psychological impairment one need know only that someone has a lesion and then describe and interpret the behaviour as before. Data from such studies can be very informative when viewed from the perspective of the deficits in psychological processes that arise. For example, the discovery that a certain process is disrupted could be useful in determining the range of behaviours that the process participates in. The locus of the lesion is of little or no consequence in such cases, since it is perfectly plausible that lesions in a number of different loci could disrupt the same psychological process.

An instance in which an information processing approach has proven useful in understanding impairments attributable to brain lesions is found in the study of the acquired dyslexias. At the moment, up to six types of acquired dyslexia have been described and interrelated in the context of information processing models of the normal reading system (Coltheart, Patterson & Marshall, 1980; Patterson, 1981; Shallice, 1981). Almost none of this work considers the location or size of the lesions suffered by the various patients. Such considerations turn out to be irrelevant to the study of the nature of the psychological mechanisms. They are of interest, of course, when one begins to ask questions concerning the location of the processes or the effects of particular lesions. The paper by Warrington and Taylor (1978) on object agnosia is a good example of work in which both these questions are posed and answered without too much confusion between them.

Unfortunately, few studies of the consequences of brain lesions have involved a research strategy of the sort that was just described. Instead, there has been a tendency to focus on the locus of the lesion and to define psychological processes in terms of such factors. Hence, one finds statements to the effect that any processes which are found in the right hemisphere are "nonlinguistic" because the left hemisphere is the one concerned with language (e.g., Gardner & Zurif, 1975). Similarly, upon discovering that patients with right hemisphere lesions have difficulties with embedded sentences, Dennis and Whitaker (1976) conclude that the faculty for dealing with embedded sentences is not linguistic, but rather a general cognitive function. Such statements as these make the premature assumption that all language functions can be ascribed to a particular location in the cortex. That is, they assume that language modules correspond directly to physiological modules. But surely any determination as to whether a particular function is a general cognitive one depends on how it squares with facts about information processing rather than where such a function resides in the tissue of the brain.

It is not our intention to deny that there is some sort of link between a malfunctioning left hemisphere (for most right handers) and language disorders. Over a century's worth of research points to such a link (Broca, 1865; Caramazza & Berndt, 1978; Jackson, 1932; Lecours, 1982; Lenneberg, 1967; Schwartz, Saffran & Marin, 1980; Wernicke, 1874; Woods, 1980). The question is just what kind of link is it and what it can tell us about normal functioning.

Hemisphere specialisation and language development. The problems presented by trying to relate brain lesions to specific language disorders are, if anything, only magnified when one attempts to use data of this sort to provide a picture of the neurological basis for language development. Yet this is precisely the domain in which some very strong assumptions have been made regarding the identification of linguistic functions with specific brain structures. There have been several different views put forth concerning the role of the left hemisphere during the course of language acquisition. In his classic book, Lenneberg (1967) reviewed much of the existing literature and concluded that it was much less damaging to have a hemispherectomy earlier in life than late. Brown and Jaffe (1975) took much the same position and argued that language is only gradually biased towards the left hemisphere. By contrast, others have argued, on the basis of electrophysiological evidence and neuroanatomical data, that the left hemisphere is predominant for speech from birth onwards (e.g. Molfese, Freeman, & Palermo, 1975; Wada, Clark & Hamm, 1975; Witelson & Pallie, 1973). In support of this contention, there are reports from behavioural studies that children with left hemispherectomies have significant language deficits, whereas those with right hemispherectomies have only negligible deficits (e.g. Annett, 1973; Bishop, 1967; Dennis, 1980; Hecaen, 1976; Woods and Teuber, 1973). However, in a later study, Woods and Teuber (1978) found that if the lesion occurs prior to one year of age, then the consequences are worse if it occurs in the right hemisphere than in the left hemisphere. This finding is hard to reconcile with the view that the left hemisphere is dominant for language from birth onward.

It is not surprising that controversy still exists concerning the role of the left hemisphere during language acquisition, since both positions, while not without explanatory value, are plagued with difficulties. Take the view that language is only gradually biased towards the left hemisphere. The basic rationale for this view is that the whole cortex is initially equipotential in underlying language capacity but that there is a gradual shrinking of the brain base for language-like behaviour with age. However, as Kinsbourne and Hiscock (1977, p. 174) have remarked, "we know of no animal model and of no human analogue to the idea that, for a given behaviour, the brain base shrinks with increasing functional sophistication. It is a totally new creation, and until its explanatory value is clearly demonstrated, it should be treated with scepticism". Furthermore, it is clear that the full consequences of what it might mean to say that language is gradually biased towards the left hemisphere have not been properly explored with respect to any underlying model of information processing.

Similar sorts of difficulty cloud the alternative viewpoint that one can find evidence of left hemisphere specialisation for language soon after birth. For example, Dennis (1980) has summarised a series of investigations made with hemidecorticate children and concluded that there are differences in language skills depending on the side of hemidecortication. In support of her conclusion she presents data from a variety of different tasks. It is worth noting that in the vast majority of the tasks cited there are no apparent differences in the performance of left and right hemisphere decorticates. Moreover, in one of the few instances in which differences were found—on a task involving the ability to use rhyming words as retrieval cues—the differences between the two left hemidecorticates appear to be almost as large as that between the right hemidecorticate and the most successful left hemidecorticate. With a sample involving so few subjects, it is difficult to know how far one can go in generalising her findings.

Indeed, Bishop (1983) has recently raised a number of questions concerning the soundness of the experimental design and statistical techniques used by Dennis and her colleagues. Bishop points out that an early study by Dennis and Kohn (1975), which suggested differences in the way in which left and right hemidecorticates processed passive sentences, involved the inappropriate application of analyses of variance to data strongly skewed and with too few subjects per cell. Furthermore, she notes that the use of group means in presenting the results was misleading since only two left hemidecorticates scored randomly, and the other three scored well above chance level. In reviewing later studies by Dennis and her colleagues, Bishop remarks, "One might wonder whether this criticism is superfluous, since Dennis and her colleagues have marshalled further evidence for a linguistic deficit after left hemidecortication. Yet scrutiny of these later papers reveals further ambiguities of interpretation, with poor experimental design (no neurologically normal controls) and inappropriate statistical analysis." (Bishop, 1983, p. 203.) The point about neurologically normal controls deserves some discussion. Dennis presents no data from normal control subjects. Therefore, one has no way of determining whether the variability in responses from her subjects on the tasks is within the normal range attributable to individual differences or reflects real processing differences that arise as a consequence of hemidecortication. Such data regarding the performance of normal subjects are critical in light of the types of tasks which were used to assess possible deficits. It is by no means clear that tests involving similarity judgements of the most and least related words in a triad actually measure specific linguistic abilities, or general cognitive ones.

In reviewing the evidence presented by Dennis, it is hard not to be struck by the number of linguistic abilities that apparently do remain intact despite the removal of the left hemisphere. As far as the normal range of day to day experiences with language are concerned, the left hemidecorticates are apparently all but indistinguishable from normals. It is only when one applies subtle measures whose relations to language abilities are not clearly established that any deficits emerge. If there really is such a strong tie between the left hemisphere and language functioning from birth onwards, one must be amazed at the ability of the left hemidecorticates to do so well without this critical apparatus. Such an observation rarely seems to occur to partisans in arguments regarding the left hemisphere role in language, perhaps because they are too closely focused on mapping language functions to brain structures.[6]

Over and above the problems already mentioned, the attempts to relate brain lesion data to language development suffer from another malady. Language is taken to be an immutable reality irrespective of the age and degree of acquisition that the child may have attained. Such an assumption seems both unwarranted and wrong. Universal grammar may be an immutable reality (in the sense that it constitutes part of an innate endowment), but surely language is not. First, the fact that an early hemispherectomy does not result in loss of competence for language functions rules out the possibility that language competence is permanently localised in that particular area of the cortex. At best what may be localised is one or more specific modules involved in language processing, either in the acquisition stage or the stable state. Second, it is perfectly plausible that a particular area of the cortex may fulfill different roles at different ages. There is no *a priori* reason to assume cortical stability when the whole system is undergoing continuous change and restructuring. The same physical lesion at one, two, three or four years of age may result in a different functional lesion. A similar view has been expressed by Ojemann (1983, p. 195), who suggested that "the area of the cortex utilised for a particular language process may be greater for functions of greater difficulty. Over time, as one develops competence with a language, simple processes such as naming utilise smaller primary cortical areas. Perhaps the wider areas originally related to this function retain that ability as a secondary capacity that can

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[6] One might argue that attention has been given to this problem by neurologists and neurophysiologists who have employed such terms as "vicariousness of function" (Lashley, 1950), "plasticity" (Lenneberg, 1967), and "functional restoration" (Hecaen, 1972). However, the only reality to such constructs is that they name the fact that the so-called organ substrate for the function can be removed without the loss of the function itself. Since there is as yet no theory of plasticity to account for the reorganisation that takes place in the cortex after some area has been excised, the term does not refer to anything more than a fact that needs explaining. Only recently has there been some progress made and, to some extent, some hypotheses offered as to what does and does not count as a model for cortical reorganisation (cf. Brown, 1979; Goldmann, 1978). Nevertheless, little seems to be gained by referring to an even more obscure mystery to explain another that is already far from clear.

be called into play as part of the recovery process after injury to the primary area. This hypothesis suggests that the exact cortical sites related to a language process may not be stable over time even in adult life, expanding or shrinking in relation to facility with a particular function". Note that at present the available evidence is perfectly compatible with this alternative view of language organisation, but as Kinsbourne and Hiscock (1977) pointed out in the passage quoted earlier, there is little empirical support for such an idea in neurological studies with animals or humans. Rather, the sole justification for this view is that it is one way of describing what needs to be explained. The moral here is that, as psychologists, we gain nothing by shifting from the purely information processing point of view to the neurologically centred point of view. In each case what is needed is a proper description of the acquisition processes and the characteristics of language performance. At this stage, neurological underpinnings seem as unnecessary to the psychologist as they are to the theoretical linguist.

UNITS AND STRUCTURE IN PSYCHOLOGICAL PROCESSING

To attempt a mapping between the underlying neurophysiological structures and basic psychological processes, prior to determining what the key elements are that must enter into a correct description of each level, is haphazard at best. In the long run, premature assumptions about the mapping between neurophysiological substrate and specific psychological process could actually impede scientific progress. This state of affairs would result whenever an otherwise correct explanation, psychological or neurophysiological, for some behaviour is rejected because it does not fit present conceptualisations of how psychological processes are related to the neurophysiological substrate. Again, given the variety of possible mapping schemes there are between psychology and neurophysiology, the risks of making an incorrect rejection of a valid hypothesis would seem to outweigh any advantages to trying to tightly constrain psychological explanations to current neurophysiological ones. Hence, in framing a basic research strategy to understand psychological processes, prudence dictates that at the psychological level, the search for basic explanatory constructs must be guided by psychological considerations, not physiological boundaries. In this section, we review some attempts to delimit units at the psychological level that illustrate the kind of approach we are advocating.

An interesting case history involves the search for the basic units of speech processing, because it provides an example of how the search for psychological units was guided chiefly by attempts to conform to constraints from explanations in linguistics. Two constructs, the distinctive feature and the phoneme, have proven to be useful tools in organising accounts of the phonological structure of natural languages. Indeed, a great number of generalisations emerge over and above the economy that these constructs introduce into the descriptions of the phonological structures of natural languages. Impressed by the efficacy of these constructs at the linguistic level, most workers in the fields of acoustic phonetics and psycholinguistics assumed that distinctive features and phonemes also had temporal and sequential processing reality as units of processing in speech perception and production. (In other words, they assumed that since distinctive features and phonemes are important cogs in linguistic explanations, they might also be important components in psychological explanations of sentence perception and production.)[7] Although Fromkin (1971), Garrett (1982), Shattuck-Hufnagel and Klatt (1979), and others have shown that phonemes, and possibly distinctive features, are necessary constructs in accounting for certain facts about speech production, Miller and Nicely (1955) have suggested that these constructs may play a pivotal role in speech perception. However, almost from the outset, attempts to account for speech perception in terms of units such as phonemes encountered many difficulties. Spectrographic analyses of speech failed to yield an exhaustive set of common properties corresponding to individual phonemes (Liberman *et al.*, 1967). In fact, as Liberman *et al.* duly noted, attempts to isolate individual phonemes by cutting up segments of syllables ended in failure, with subjects either reporting the whole syllable or just a nonspeech noise. Nevertheless, despite these findings from researchers at Haskins Laboratories, there was a tendency to accept phonemes and distinctive features as the elementary units of psychological processing. Perhaps this view remained in vogue even after the failure to find direct correlates for them in the acoustic signal because researchers found it easier to accept the possibility that a mismatch might occur between physical (acoustic) units and psychological ones than to accept the notion that the elementary psychological units might not correspond to the elementary linguistic ones.

The notion that the elementary unit of speech processing might be something other than the phoneme began to be considered seriously after the report of an interesting finding by Savin and Bever (1970). They set up a task in which listeners had to monitor sentences for a particular auditory target and to respond as soon as the target was detected. Two types of units were chosen as targets—phonemes and syllables. Savin and Bever reasoned that in sentence processing the identification of the smallest units leads to the identification of larger ones. Thus, monitoring for the elementary units should produce faster reaction times. Hence, if phonemes were the primary units for speech perception, then the identification of a syllable would require recovery of the component phonemes. Therefore, reaction times should be

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[7] Linguists had paid some attention to the syllable, but it is likely that both psychologists and linguists operate on the assumption that the more atomic units constrain the more wholistic units and therefore should be investigated before the latter. Our argument is that all these units play a role in speech perception but that they play a different role at different periods.

faster to phoneme targets than for syllables. In fact, the results were just the opposite, leading Savin and Bever to conclude that syllables are the elementary units, with phonemes being derivatives from them by an additional step of decomposition.[8]

The notion that the syllable is a primary unit in speech processing and the phoneme only a derived one receives support from other quarters as well. In particular, studies of reading readiness point to the fact that young children often experience a great deal of difficulty decomposing words into component phonemes, whereas the comparable task for syllables is considerably easier (Rozin & Gleitman, 1977). Likewise, Liberman, Shankweiler, Fischer and Carter (1974) showed that four- and five-year-olds could tap out the number of syllabic segments in a word, but not the number of phonemic segments. Results such as these suggest that awareness of phoneme-sized units might arise largely in conjunction with the development of reading skills (Jusczyk, 1982; Mehler, 1981). Some empirical support for this notion comes from a recent investigation by Morais, Cary, Alegria, and Bertelson (1979). They tested adult illiterates on a task in which subjects had to add or delete a phoneme or a syllable in a word or non-word. The illiterates were unsuccessful on tasks involving phonemes, but they had little problem with the syllabic ones. By contrast, a comparable sample of adult subjects who had only recently learned how to read succeeded on both the syllable and the phoneme tasks. Hence, the similarities between the illiterates and the young prereaders on the tasks mentioned above bolster the view that the syllable is the primary unit for speech perception and the phoneme is a derived one arising in conjunction with literacy. Note that this does not affect the validity of using the phoneme as a primary unit in linguistic analyses or in the explanation of phenomena in speech production.

Let us next describe how another unit of processing can be established by purely psychological tasks. In this instance, the unit being studied relates to the visual processing of words. Specifically, what is the unit underlying the visual recognition of words? How abstract is it? Although a variety of different paradigms can and have been used to provide information about this issue (recognition memory, lexical decision, reading times, semantic classification), in the interest of brevity we will only focus on one of them for purposes of illustration: the facilitation of perceptual recognition. In the main part of the experiment, subjects are presented with individual test words for a brief period and the exposure time necessary for correct identification is measured. The variable of interest here is what effect the

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[8] It must be acknowledged that the Savin and Bever findings were later criticised (e.g., McNeil & Lindig, 1973; Healy & Cutting, 1976), and in turn these latter studies have also been criticised (see Mehler, 1981). This is not the place to discuss the intricacies of that debate. The main point that we want to make here is that it was the Savin and Bever findings that made researchers seriously consider that the basic units for speech perception might be something other than phonemes and distinctive features, and that data from studies of psychological processing are the most germane to the determination of what the true speech perception units are.

prior exposure of words related to the test word in various ways will have on the exposure time to identify the test word. Accordingly, prior to the test phase of the experiment there is a training phase during which some of the test words are presented together with other words related to them. The most reliable outcome of such training is that an item which was seen in the training phase is much more readily seen than any that has not previously occurred. The effect of training is thus said to "prime" the recognition of the word during the test phase. Indeed, such priming has been found to be effective for at least 45 minutes after the initial exposure to the word (e.g., Clarke & Morton, 1983).

Given this procedure it becomes possible to investigate the nature of the unit underlying the recognition of the word. This can be explored by varying the relationship of the words in the training phase to those in the test phase. We will discuss two such experiments. In the first, Murrell and Morton (1974) tested four groups of subjects, all of whom saw the same words during the test phase. One such word was *seen*. The groups differed only in the words that they saw during the training phase of the experiment. One group saw the actual test item (e.g., *seen*). A second group saw words morphologically related to the test items (e.g., *sees*). A third group saw words visually similar to the test word (e.g., *seed*). The fourth group saw words unrelated to the test item (e.g., *boat*). As expected, prior exposure to *seen* facilitated its recognition during the test phase, but, more importantly, so did prior exposures to *sees*. By contrast, the prior presentation of *seed* had no effect. Thus, the facilitation from *sees* to *seen* is not simply due to the visual similarity between them. On the basis of these findings, Murrell and Morton concluded that the morpheme is the unit of processing on tasks of this sort.

Further evidence concerning the unit of visual processing comes from a study by Osgood and Hoosain (1974). They showed that familiar nominal compounds such as *town hall*, *stock market*, and *post card* behave as units. Prior presentation of such compounds has no facilitatory effect upon the subsequent recognition of their components. Thus, the presentation of *stock market* in the training phase had no effect on the recognition of *market* in the test phase. By contrast, the prior presentation of a two word sequence like *street market* did prime the recognition of *market*. On the basis of these results Osgood and Hoosain concluded that the word, and *not* the morpheme is the basic unit in visual word recognition.

Independently considered, the data from the two studies appear contradictory (at least their interpretations do). However, when considered together these data suggest that the mental unit of processing, which we call a "logogen", is neither a word nor a free morpheme. Instead, it is something more abstract than either. The limits of the unit have not yet been completely delineated. However, more recent experiments have shown that for "strong" verbs (e.g., "bring-brought"), the past and present forms behave as separate units at this level of processing (Kempley & Morton, 1982). The question is not one of whether the processing unit called a logogen corresponds to a word or a morpheme. The experimental method establishes the items that are to be classed together. Whether or not this class corresponds to any class in any linguistic theory is irrelevant both for the psychology and for linguistics (Morton, 1982).

The purpose of this section has been to illustrate the ways that progress can be made in isolating the basic units for explanation at the psychological level. Whether the syllable (or rather the processing units corresponding to the syllable) and the logogen ultimately prove to be basic units of psychological processing is irrelevant at this juncture. If these constructs are found to be lacking in some way, it will be evidence concerning their adequacy and utility in psychological explanations that will decide this. In this regard, we believe that an information processing model will be the best framework to use in acquiring such evidence.

TOWARDS A VIABLE RELATIONSHIP BETWEEN BIOLOGY AND LANGUAGE

We have argued for the independent determination of the basic theoretical constructs at each level of analysis. Any attempted mapping between levels can only be successful to the extent that it respects the integrity of such constructs at each level. At this juncture we are a long way from complete theories at either the psychological or the neurophysiological levels of functioning. Nevertheless, we do believe that even now there are some ways, restricted though they may be, in which neurophysiological data could influence psychological theories. In this section we consider the kinds of circumstances under which one might expect such influences.

Suppose that a neurophysiologist had discovered that activities in certain parts of the auditory system are sampled every 20msec and that this is the only type of analysis of input that is ever used. Any psychological model of the acoustic analysis system which operated on any other principles would fail. However, there are two points to be considered here. The first is that it is necessary to specify in more detail the nature of the observations that would lead to these conclusions. A candidate experiment would be that a number of electrodes placed in the auditory cortex responded differentially to different stimulus frequencies, that these neurons fired only every 20msec, and that any acoustic changes within the 20msec intervals were not reflected in the electrical activities of the neurons. Furthermore, it would be required that there be sufficient sampling of activities in the auditory cortex and other areas connected to it to ensure that no other area of the brain responded selectively to perturbations within the 20msec intervals. Of course, it would also have to be true that no psychologist had performed experiments indicating that humans are sensitive to such perturbations. In this hypothetical situation, we see a case in which psychological theories are constrained by neurophysiological data. However, note the conditions under which the exception occurs. First, the psychologist who constructed the inappropriate model of the acoustic analysis system went beyond the available data without adequate tests of the model's generalisations. Second, our hypothetical example depends critically on the prior determination of a one-to-one mapping between the psychological unit and the neurophysiological one.

In general, there are conceptual difficulties involved with this sort of argument. Thus, it does not follow that because region R1 is connected to region R2 in the brain there are particular consequences for an information processing model. This is so because the things which are connected in an information processing model are processes, not regions. Until the particular processes are themselves identified, the regions have no existence. But let us suppose for the sake of argument that by some means the locations of two of the functions from an information processing model were identified. Suppose further that in the model these processes were deemed to be unconnected, but that the anatomical evidence indicated that the brain regions in which they were located were directly connected. Under these conditions, one would probably have to conclude that there was a direct relationship between these two functions that the model had overlooked. But once again there are a lot of assumptions that one must make in drawing such a conclusion. A concrete example will help to illustrate them.

Consider the logogen model of word recognition. In this model, the visual input logogen is a device responsible for categorising letter strings into units corresponding roughly to words (Morton, 1979; Morton & Patterson, 1980). Such a device must at least send an output to some interpretative device, called the Cognitive System. It might or might not also have direct access to the appropriate output code for words which is supposed to be made available by the action of the output logogen system. At present, the data are not sufficiently clear to say whether or not there is a direct connection from the visual input logogen system to the output system. Assume that the brain locations of the input and output logogen systems were discovered. Furthermore, suppose that it has been determined that sets of single fibres connect these two regions. The question is whether the latter finding can be taken as evidence that there is a direct connection between the two systems. The answer is clearly "yes". Under conditions like these an inference from one level to the level above is permissible. But note what the conditions are for such an inference. There must be one-to-one mapping from the level above to the level below. In this case this means that a function as complex as the input logogen system must be localisable, and that another as complex as the output logogen system must be localisable as well. Hence, for any particular neuron, one must be able to say whether it is a member of one set or the other. If this cannot be determined, then one cannot rightfully claim that there is any direct connection between the two regions. The only alternative is to say not that the intermediate neuron has no identifiable function, but that it has *no* function. For if the neuron were to have an unidentified function that unidentified function might correspond to a function of the Cognitive System, and thus one could not argue for the direct connection on purely anatomical grounds. To see the true magnitude of the problem here, consider that identifying the functions of complex cells in the visual system is already difficult to do even though these are cells with very simple functions. How likely, then, is a successful localisation of the logogen system?[9]

For the time being let us put our skepticism aside regarding the possibility of mapping psychological function onto specific brain locales. Given the possibility of such a mapping, in what ways could neurophysiological data affect psychological theories? Let us consider several hypothetical possibilities involving neurophysiological techniques that are greatly advanced.

The Planum Temporalis and Grammar. Suppose that someone claimed that all grammatical functions were affected by the planum temporalis, and that the planum temporalis was concerned only with grammar. Such a claim might be arrived at by noting that the planum temporalis was active during normal reading, speech comprehension, and speech output. Suppose also that activity was present in that area with the profoundly deaf when they were reading sign language, but not when lip reading or interpreting finger spelling. Further, imagine that one observed significant activity in the planum temporalis during proof-reading, copy-typing, and reading aloud. Moreover, suppose it is discovered that Morse Code operators give evidence of activity in this area only when they become experts and not when they are beginners. We would take this evidence as absolutely conclusive that grammatical function is co-extensive with the planum temporalis.

Would this discovery impose any constraints on psychological models? The most obvious candidate for a constraint would be that if a particular human activity is shown to be accompanied by the involvement of the planum temporalis, then we can conclude that the grammar is involved. Thus, if a blind person reading Braille were discovered to show activity in the planum temporalis as an expert reader and not as a beginner, it might be concluded that one aspect of proficiency in Braille reading was the involvement of grammatical functions. Similarly, it might be discovered that the planum temporalis of agrammatic aphasic patients showed normal activity when the patients were attempting (but failing) to read. In this case

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[9] Of course, it is always possible to claim that the simple cell is too low a level to observe the mapping between psychological and neurophysiological descriptions, and that one must go to a higher level of organisation to achieve this. This is a reasonable argument, but if it leads to the claim that the parietal lobe is for vision and the frontal lobe (or part of it) is for motor output, then this will not be of much use to information processing models. Similarly, arguments to the effect that logogen systems have no psychological reality can be countered by ones similar to those provided by Chomsky (1965; 1968) in discussing the reality of grammatical structures. The productivity and utility of such concepts need no defense here. If they are inadequate, they will be replaced by something of the same order of complexity in a model of the same form.

one could conclude that the patients' agrammatism was caused not by a loss of grammatical processes but rather by their disconnection from the processes which normally receive input from them.

In these two cases it seems as though we have found a constraint on processing models that arises as a result of neurophysiological data. However, it is not the neurophysiology alone that prompts these conclusions. Rather, it is the mapping between a psychological representation—the grammar—and a specific area of the brain that has been used. Moreover, once again the mapping is a tight one between substrate and function. Nevertheless, even under these conditions, the epistemological status of the data is far from clear. For example, suppose that it were now discovered that the planum temporalis was involved in recognising faces or listening to music. If the constraints on processing were real then we would have to conclude that grammatical processes were involved in face recognition and listening to music. This would lead to considerable redefinition of grammar or of the nature of what is involved in processing faces and listening to music. However, a far more likely outcome would be that the conclusion drawn from this hypothetical set of data would be one that redefined the role of the planum temporalis, such that it would be seen as responsible for a certain type of computation regardless of the nature of the data, rather than being language specific. Or alternatively, we might conclude that the planum temporalis is multifunctional which would then raise doubts about the existence of any one-to-one mapping between substrate and function. In effect, then, it would be the psychological analysis that constrained the nature of the mapping between psychology and neurophysiology.

Mechanisms of Developmental Change. One of the most vexing problems in developmental psychology is explaining apparent discontinuities in children's behavior. Thus, while many developmentalists have acknowledged the existence of discontinuities by postulating stage theories of development (e.g., Bruner, Olver, & Greenfield, 1966; Kohlberg, 1969; Piaget, 1952; Werner, 1940), none has been able to provide a satisfactory account of the mechanism of developmental change. In general, psychologists have tried to offer accounts of such discontinuities by postulating some reorganisation of basic cognitive processes. A typical account is that some activity or process is guided by relatively simple principles until it is confronted by an unwieldy amount of discrepant information. At this point, a different mode of operation is induced, and hence a discontinuity in behaviour is observed. Such accounts at the psychological level are rarely satisfactory because, among other things, they provide no explanation for why the change in behaviour is always in the right direction. That is, why does the reorganisation always seem to lead to a more advanced stage of behaviour rather than sometimes leading to a less advanced stage, or a different type of behaviour at the same level of sophistication? Given these difficulties with processing accounts for developmental change, the door would seem to be open for a neurophysiological explanation. Indeed, some such as Fodor (1975), Jusczyk and Earhard (1980), and Mehler (1981) have argued that the only way to explain stagelike changes in development may be in terms of the maturation of structures at the neurophysiological level.

How might a neurophysiological account of developmental change work? Consider the following possibility. Recent reports indicate that during the first month of life infants will often imitate the facial expressions of adults (Meltzoff, 1981; Meltzoff & Moore, 1977). So, for example, if an adult opens his mouth, protrudes his tongue, or purses his lips, an infant will imitate these gestures reliably. Other reports suggest that this behaviour stops at about three months (Maratsos, Note 1). The details are unimportant for this illustration. One need only assume that, for this example, what has to be explained is the cessation of the imitation pattern. Suppose that one could establish that a particular area, B, of the brain is always active when the infant is imitating facial gestures. Suppose further that infants with abnormal development in this area do not display such imitative behaviour. Finally, suppose that at about three months of age morphological changes in the brain were observed that were quite general, but for anatomical reasons alone were concentrated particularly in area B, and that the onset of these changes coincided with the cessation of imitation. Under such circumstances we would feel comfortable in inferring that the cause of the behaviour change was biological. A processing explanation of the change in behaviour in these circumstances would seem to be unnecessary or even impossible. On the other hand, a description of the change in terms of its consequences on the state of the psychological processes before and after the change would be necessary to ensure valid psychological models at both stages of development. It should be clear that this example differs from the previous two in that it introduces the notion of causality. Given that there is a necessary biological precondition for the development of a particular psychological process, it would seem fruitless to attempt a purely psychological account of the change. On the other hand, a purely psychological account of the process before and after the change would not only be possible, but required. In the earlier examples, we considered cases in which constraints could be found from biology to psychology only under very restrictive circumstances. These constraints depend on a tight one-to-one mapping between the constructs at the two levels. This restriction, which is necessary for between-level constraints for descriptive explanations, does not hold for causal explanations.

We can further illustrate the distinction between causal and descriptive explanations by reference to schizophrenia. Suppose that, as some people claim (Crow, 1979), a particular biochemical deficit, *D*, is a necessary precondition for schizophrenia. Let us also assume that this deficit is genetic in origin and has no other consequences with respect to behaviour. Any purely psychological account of schizophrenia would thus be deficient, for it would not be able to distinguish between those individuals who were susceptible and those who were not. It might make sense to say something like "schizophrenia is *caused* by *D*." However, under no circumstances would it make sense to say that "schizophrenia *is* a biochemical deficit." A processing account of the behavioural problems will always be required.

In some of the hypothetical cases that we have discussed, we have made the assumption that a mapping from psychological processes to specific brain locations is possible. This assumption requires that psychologists and neurophysiologists already have identified the components that must enter into the mapping. Even under these extremely favourable, and improbable, circumstances, we have seen that neurophysiological evidence is apt to have only minimal influence on psychological theories. Would the situation have been any different if, instead of a mapping of psychological processes onto specific brain locations, we substituted a mapping of psychological processes onto patterns of brain excitation (as determined by ERPs, brain-blood-flow patterns or some other measure)? Given the nature of the examples we have examined, it is unlikely that much would be gained by this shift from brain locations to patterns of excitation. Once again, the most favourable conditions under which we could expect to discover a direct linkage between psychological processes and the underlying neurophysiology would be in cases of one-to-one mapping, and this entails some advance knowledge of what the units of analysis are at each level that enter into the mapping relationship. Furthermore, the situation involving one-to-one mapping is clearly the optimal one in which we could expect to find tight constraints between neurophysiological data and psychological theories. Here, at least, it might be possible to observe some sort of consistent correlation between neurophysiological events and psychological ones. The situation with other sorts of mapping of psychological events onto neurophysiological ones (e.g., one-to-many or many-to-many) is likely to be, if anything, horrendously more complex because any potential correlation between the two types of events is apt to be more obscure and indirect, and hence, exceedingly difficult to determine.

Some Real World Examples. Fortunately, even now there are ways that neurophysiological data can be of use to psychological theories, and these do not require making assumptions to be made about a one-to-one mapping between psychological processes and brain locations. What is required for this work to succeed is that psychologists think, not only in terms of the physical organisation of the brain, but mainly in terms of its functional organisation. Thus, when confronted with an individual who has some sort of brain lesion, it behooves psychologists to think not in terms of the locus of the lesion, but in terms of what psychological processes have been disrupted and how this affects performance in a variety of different settings. Thus, the behavioural deficits that such patients display can be very effective in clarifying the relationship between different components of an information processing model. Psychologists can, and have, used neuropsychological evidence in this way (e.g., Caplan, Matthei, & Gigley, 1981; Friederici, Schoenle & Goodglass, 1981; Morton & Patterson, 1980; Posner, Pea & Volpe, 1982), as a concrete example will help to demonstrate. Patient PW (Patterson, 1978, 1979; Patterson and Marcel, 1977; Morton & Patterson, 1980) has been termed a "deep dyslexic". Morton & Patterson (1980) have analysed the functional nature of PW's deficit in terms of an expanded version of the logogen model. From an analysis of PW's deficits in reading and comprehension, it was possible to assign five distinct "lesions" on the diagram of the model shown in Figure 1.

For reasons we will not go into here, it is likely that only three of the processing deficits actually lead to deep dyslexia. Note that the deficits shown need not be disconnection deficits in the sense of Geschwind (1965). Nor are the lines on the model intended to correspond to well-defined fibre tracts. Some of them may correspond to fibre tracts, but that is a separate question. The main benefit of studying patients in this manner is the insights that arise with respect to the organisation of existing information processing models. The study of such patients has forced changes in the models, on the one hand, and provided evidence in favour of certain constructs on the other.

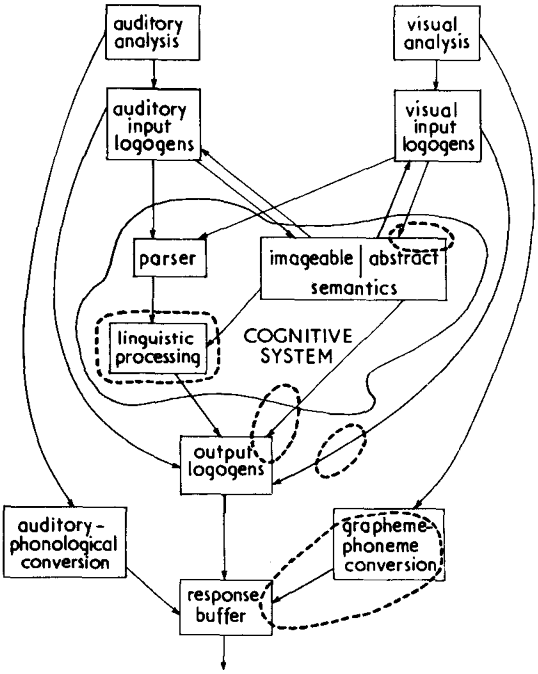

FIG. 1. An expanded version of the logogen model indicating the "functional lesions" of a patient PW (from Morton & Patterson, 1980).

Thus, the symptoms of deep dyslexic patients put beyond debate the view that written material can be understood without reference to phonological codes.

There are also ways in which purely neurophysiological data can be of use to psychological theories. To the extent that it is possible to specify the way in which information about the environment is transduced by sensory mechanisms, one may be in a better position to understand the psychologically important parameters of some environmental stimulus. A case in point comes from recent research in speech perception. In the past, the starting point for analysing the important components of the acoustic signal for the perceiver was the information available in the sound spectrogram. Hence, the claims that there are no invariant acoustic cues which might serve to identify stop consonant segments (e.g., Liberman *et al.*, 1967) were based on the fact that such cues were not present in the sound spectrogram. In effect, the physical features in the sound spectrogram were taken to be the ones that were available to the perceiver. Thus, a number of experiments were devised to determine the way in which various physical components of spectrograms were processed by human listeners. The past few years have seen a change in this attitude. There is an increasing awareness that the kind of analysis performed on the speech signal by the human auditory system may differ from the one given in the sound spectrogram. Data from investigations of the underlying neurophysiology of the auditory system (e.g., Delgutte, 1980; Kiang & Moxon, 1974; Kiang, Watanabe & Clark, 1965) have been very influential in this regard. These data provide an indication of the way in which the incoming acoustic signal is filtered and transformed by the auditory system. The type of information available in such so-called "neural spectrograms" has inspired a number of recent attempts to devise new means of analysing the speech signal (e.g., Kewley-Port, 1983, Note 3; Searle, Jacobson & Rayment, 1979; Stevens & Blumstein, 1981). These new types of analysis procedures have reopened the question as to whether or not the form that the acoustic signal takes after undergoing auditory processing reveals invariant properties that serve to identify phonetic segments.

It is important to be clear as to what types of gains have been made by auditory neurophysiological data. The data in question indicate that the kind of parsing that the auditory system does is not the one apparent in the physical representation of speech in the spectrogram. These data then eliminate a certain set of hypotheses regarding the way that speech perception takes place. What the data do not tell us is whether the salient features present in the neurospectrogram are the ones that correspond directly to the important perceptual features. The latter can be determined only through systematic psychological tests whereby certain aspects of the kind of information available in the neurospectrogram are manipulated and their consequences for perception are observed. In other words, the neurophysiological data have provided a starting point for subsequent psychological investigations, but they have by no means removed the necessity for them.

It should be apparent, then, that both neurophysiological and neuropsychological evidence can influence the construction of psychological models, provided that one views such evidence in terms of its consequences for psychological functioning. Moreover, one can say that such evidence acts as a constraint on psychological theories, but the constraint arises from what the evidence dictates about links between psychological processes, not from some assumptions about the brain locus for the processes.

Could there be other ways in which findings about the underlying neurophysiological structure would serve to constrain psychological theories? One possibility is that neurophysiological research could demonstrate that there are only a finite number of possible brain states. Such a discovery would then rule out any psychological theories that require more than the available number of brain states to encode the postulated psychological information.[10] Notice, however, that any revelations regarding the available number of brain states are not likely to be forthcoming until we have a carefully worked out theory of neurophysiological functioning. In the interim, it seems foolhardy to impose on psychological models any constraints in the form of arbitrary limitations on the number of available brain states.

CONCLUSIONS

We have tried to call attention to the difficulties that arise in accepting the most frequent interpretations that neuropsychologists have given to the relationship between the neurophysiological substrate and psychological processes. We have argued that if a mapping between psychological processing and neurophysiological structures is possible, it will only come about after the key theoretical constructs are established for each level of explanation. In the interim, there are restricted circumstances in which neurophysiological observations can play a role in the development of psychological models. But these circumstances require the consideration of such evidence, not only in terms of the physical organisation of the brain, but also in terms of its functional organisation.

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[10] We thank Jerry Fodor for calling this point to our attention. However, we make no claims about whether he would endorse the solution to the problem that we propose here.

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