Stellenbosch Papers in Linguistics, Vol. 8, 1982, 01-50 doi: 10.5774/8-0-112

ON HOW NOT TO ARGUE ABOUT CHOMSKYAN MENTALISM

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1 Introduction*

In "Methodological bases of a progressive mentalism" (Botha 1980; henceforth: MB), I first identify a number of methodological shortcomings of Chomskyan mentalism and then attempt to overcome these by articulating the methodological bases of an alternative form of mentalism.(1) Peter Slezak (1981:2) now proposes "to examine Botha's criticisms of Chomsky in detail with a view to demonstrating that they are without foundation and are based on the most fundamental misunderstandings".(2) It will be shown below that Slezak has failed, for two basic reasons, to give substance to this proposal. First and foremost, Slezak's discussion is self-aborting because it fails to address the main arguments offered by MB in support of its criticisms of Chomskyan mentalism. Second, the more relevant comments which Slezak makes on MB's criticisms of Chomskyan mentalism are generated by an assortment of misconceptions and misrepresentations. For these reasons, then, it will be concluded that Slezak's discussion leaves MB's criticisms of Chomskyan mentalism completely intact.

2 The unchallenged main arguments of MB

The main criticisms made in MB of Chomskyan mentalism are quite straightforward and the arguments offered in support of these criticisms are not overly complex. Nevertheless, Slezak has been able neither to correctly identify these criticisms and arguments nor to address himself to them

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in a way which is both to the point and coherent. It is therefore necessary to briefly recapitulate these arguments, showing how they are mutually interrelated.

The central criticism made in MB of Chomskyan mentalism may be represented as (1)(c) and the considerations furnished in support of it as (1)(a) and (b).

(1) (a) If a mentalistic linguistic theory makes existence claims which are nonempirical, it is methodologically defective.[3]

(b) Viewed as mentalistic theories, Chomskyan linguistic theories make nonempirical existence claims.[4]

(c) Hence, viewed as mentalistic theories, Chomskyan linguistic theories are methodologically defective.

The major premise (1)(a) requires no justification within the context of a discussion of Chomskyan mentalism: it represents a methodological canon explicitly accepted by Chomsky (1976:3, 10, 20) himself. In support of the premise (1)(b), MB offers two further arguments, (2) and (5), which are mutually independent. The first argument supporting (1)(b) may be represented as follows:

(2) (a) If an existence claim is ontologically indeterminate, it is nonempirical.

(b) Compared to astrophysical existence claims such as (B) in note 5 and neurophysiological existence claims such as (C), Chomskyan mentalistic claims such as (A) are ontologically indeterminate.[5]

(c) Hence, Chomskyan mentalistic claims are nonempirical.

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In support of the major premise (2)(a), no special justification has to be furnished: an ontologically indeterminate claim has no clear test implications. Consequently, it fails to satisfy one of the conditions on empiricalness of the conventional falsificationist framework within which Chomsky (1978:9) seems to operate. The argument furnished by MB (pp. 20ff.) in support of the minor premise (2)(b) may be represented as follows:

(3) (a) If an existence claim postulates entities that cannot be uniquely identified, it is ontologically indeterminate.(6)

(b) Chomskyan mentalistic claims postulate entities that are not uniquely identifiable.

(c) Hence, Chomskyan mentalistic claims are ontologically indeterminate.

In support of the claim embodied in the minor premise (3)(b) the following argument is presented in MB (pp. 22ff.):

(4) (a) If it is unclear what general nature and specific properties a given kind of theoretically postulated entities must have as constituents of a certain kind of reality, these entities are not uniquely identifiable.(7)

(b) The general nature and specific properties of the nonactual mental mechanisms postulated by Chomskyan mentalistic claims are unclear when these mechanisms are compared to the physical and neurological entities postulated by astrophysical and neurophysiological existence claims respectively.

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(c) Hence, in comparison to physical and neurophysiological entities, Chomsky's nonactual mental mechanisms are not uniquely identifiable.

MB (pp. 23-25, 33) substantiates the point of the minor premise (4)(b) in some detail with reference to specific physical, neurological, and candidate mental entities.

This brings us to the second main argument which provides independent support for the claim (1)(b), viz. that interpreted as mentalistic theories, Chomskyan linguistic theories make nonempirical existence claims. This argument is developed in §§4.4.2, 5.2, 5.4.3 of MB and may be represented here as follows:

(5) (a) If an existence claim is evidentially indeterminate, it is nonempirical.

(b) Chomskyan mentalistic claims are evidentially indeterminate, given the possibility of a nonmentalistic interpretation of the linguistic hypotheses that underlie these claims.

(c) Hence, Chomskyan mentalistic claims are nonempirical.

In support of the minor premise (5)(b), MB furnishes an argument which may be represented as follows:

(6) (a) If a mentalistic claim is to be evidentially determinate, it must be responsible to some evidence which is irrelevant to the validation of a nonmentalistic interpretation of the linguistic hypothesis that underlies this claim.(8)

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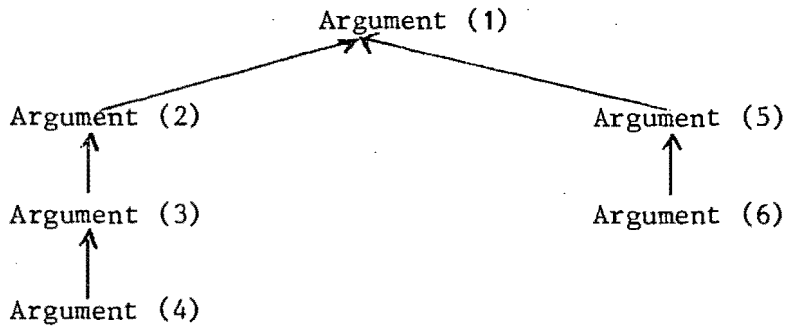
(b) Chomsky's position that it is not necessary to use external linguistic evidence, in addition to internal (intuitive) evidence, for the validation of mentalistic claims implies that there is no evidence to which these claims are responsible but which is irrelevant to a nonmentalistic interpretation of the linguistic hypotheses underlying these claims.

(c) Hence, Chomskyan mentalistic claims are not evidentially determinate.

The minor premise of this argument is substantiated in MB (pp. 42ff., 77ff.) by a fairly detailed analysis of Chomsky's (1976) position on the status of external linguistic evidence.

The mutual interrelatedness of the main arguments supporting MB's criticisms of Chomskyan mentalism may be represented schematically as follows:

(7)



Notice that the two main arguments, (2) and (5), furnished in support of the criticism that Chomskyan mentalism makes nonempirical existence claims are mutually independent.[9] This implies that to rebut this criticism both these supporting arguments must be controverted.

This brings us to Slezak's appraisal of MB's criticisms of Chomskyan mentalism. As regards arguments (5) and (6), he altogether fails to address

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them. He focusses his attention exclusively on a fragment of the third (pp. 11-35) section of MB. But arguments (5) and (6) are developed in the fourth (pp. 35-47) and fifth (pp. 62-64, 77-81) sections of the paper. This implies that Slezak's discussion leaves entirely untouched MB's point that Chomskyan mentalism is methodologically defective because it makes mentalistic claims which are nonempirical on account of their being evidentially indeterminate. This fact, of course, deals a rather severe blow to his (p. 21) "hope" of having "shown that Botha has failed to provide any valid criticisms of Chomsky's mentalism and the question of psychological reality".(10)

What, then, has Slezak to say about the independent arguments (2), (3), and (4) with regard to the other main point of MB, viz. that Chomskyan mentalism is methodologically defective because it makes mentalistic claims which are nonempirical in virtue of their being ontologically indeterminate? Again Slezak's way of appraising MB is rather extraordinary: he fails to address himself directly and explicitly to these arguments. That is, he attempts neither to criticize the logic of these arguments nor to controvert their premises in a way which is both direct and to the point. Taking into account that he has promised his readers "a careful critical scrutiny" (p. 2) of MB, examining its criticisms of Chomsky "in detail" (p. 2) this failure makes the above-quoted "hope" all the more remarkable.

Slezak, in fact, restricts his discussion entirely to MB's critical analysis of two analogies used by Chomsky (1976) to clarify and justify certain methodological aspects of his form of mentalism. Moreover, he does this without ascertaining precisely how MB's criticisms of these analogies tie in with its main arguments (1)-(6). The closest Slezak comes to dealing with MB's point about the ontological indeterminacy of

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Chomskyan mentalistic claims is when, in the discussion of the astrophysical analogy, he latches onto the way in which MB uses the expression "nonactual mechanisms" in statements such as the minor premise of argument (4). Let us now take a look at this part of Slezak's discussion.

3. Nonactual mechanisms

Slezak (1981:16) contends that it is MB's "own fabricated and confused locution" nonactual mechanism which generates its criticisms of the ontological status of Chomskyan mentalistic claims:

> On the basis of this locution Botha proceeds to find puzzles about the ontological status of 'nonactual mechanisms' and consequently he (1980:25) attributes to Chomsky claims which are 'ontologically indeterminate' and 'not refutable in principle, hence not empirical'.(11)

But this claim of Slezak's grossly distorts the relevant point of MB. As is clear from argument (3) above, MB's point is that Chomskyan mentalistic claims are considered ontologically indeterminate because they postulate entities which are not uniquely identifiable. And the point of argument (4) is that these entities are not uniquely identifiable because it is unclear what general nature and specific properties they must have as mental entities or mechanisms. Contrary to the impression misleadingly created by Slezak's use of the expressions on the basis of this locution and consequently MB's point is not that Chomskyan mentalistic claims are ontologically indeterminate because they postulate "nonactual mechanisms". A passage such as the following from MB (pp. 24-25) amply substantiates these points:

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Let us now consider Chomsky's claim (3)(a) and (b) [= (A)(a) and (b) in note 5 --- R.P.B.] as existence claims about a mental reality. The mechanisms or entities postulated by these claims include a 'mental representation' and a 'mental computation' which have such aspects as PRO, t, wh, (wh-)movement, (wh-)island, (wh-island) constraint, etc. It is not at all clear that mental entities such as these can be uniquely identified by a mentalist linguist. That is, it is unclear how a mentalist linguist, when presented with an arbitrary entity, can decide in a non-arbitrary manner whether it is or isn't an instance of one of the listed kinds of mental entities. It is simply not clear which properties these kinds of entities have as mental entities. Chomsky does not even specify what the general make-up of a real mental world would be. He fails to specify what entities or mechanisms in such a world would correspond to 'a computation' or 'a representation'. The expressions 'computation' and 'representation', as Chomsky uses them in this context, are at best metaphors, at worst completely contentless. Existence claims such as (3)(a) and (b), consequently, have to be ontologically indeterminate. It is not clear what referents linguistic concepts such as 'PRO', 't', 'wh', '(wh-)movement', '(wh-)island', '(wh-island) constraint' (can) have in a real mental world.

This passage from MB --- and others (e.g., pp. 32-33) with the same general purport --- reveals just how inaccurate the quoted claim of Slezak's is.

The untenability of this claim can also be illustrated from another angle. By using the expression on the basis of, Slezak's claim implies that, if "the locution" nonactual mechanisms were to be abandoned, the basis of MB's argument that Chomskyan mentalistic claims are ontologically indeterminate would automatically collapse. Let us therefore consider what would be the effect of deleting the expression nonactual in the argument (4). Clearly, this deletion has no effect whatsoever on the force of this argument: unique identifiability and ontological indeterminacy are obviously not defined in terms of this expression.

Though MB's use of the expression nonactual mechanisms --- more specifically of nonactual --- is unfortunate in the sense that it presented Slezak with a red herring, it is not in any way perverse, contrary to what he seems to suggest. MB uses this expression on the basis of remarks such as the following by Chomsky (1976:9):

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> Of course, there are differences; the physicist is actually postulating physical entities and processes, while we are keeping to abstract conditions that unknown mechanisms must meet. We might go on to suggest actual mechanisms, but we know that it would be pointless to do so in the present stage of our ignorance concerning the functioning of the brain.

Chomsky (1966:91) has also called the mental entities postulated by mentalistic theories "abstract structures" and it is to reflect the difference between these "abstract structures" and actual physical mechanisms that MB has used the expression "nonactual (mental) mechanisms" to denote these structures. But, as shown above, this choice of terminology is entirely irrelevant to an assessment of the force of the relevant arguments in MB.

In conjunction with the remarks quoted above, Slezak (p. 16) claims that

> At the risk of laboring the obvious, Chomsky's contrast with 'actual mechanisms' is not some mysterious "nonactual mechanisms", but rather an abstract specification of these mechanisms, actual nonmechanisms, as it were.

Slezak, however, refrains from explaining how the views expressed in this quotation would controvert the criticism of ontological indeterminacy as motivated in the arguments (3) and (4). On the one hand, these views, as far as I can see, in no way enhance the unique identifiability in a real mental world of the intended referents of theoretical concepts such as 'PRO', 't', 'wh-movement', 'wh-island', 'wh-island constraint', etc. On the other hand, if these views were to imply that the postulated mental entities do not have to meet the condition of unique identifiability, the methodological bases of Chomskyan mentalism would be all the more problematic. If the mental entities postulated by such theoretical concepts as 'PRO', 't', 'wh-movement', etc. were not made subject to this condition,

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they would differ in a fundamental respect from the physical entities postulated by concepts such as 'proton', 'alpha particle', 'positron' etc. which do have to meet the condition. This would further undermine Chomsky's attempt to justify the linguist's imputation of existence to his theoretical constructs by arguing that it is analogous to the physicist's imputation of existence to his theoretical constructs. If the mental entities postulated by the linguist do not have to meet the same conditions as the physical entities postulated by the physicist, then imputing existence to Chomskyan theoretical constructs is clearly not the same thing as imputing existence to physical theoretical constructs. In this event, the physicist's imputation of existence to theoretical constructs could not be used --- as is done by Chomsky --- to justify the linguist's imputation of existence to theoretical constructs.(12)

Slezak (p. 16) makes matters even worse for himself by claiming that MB's "confusions" on the question of the ontologic status of Chomskyan mentalistic claims can clearly be seen in its remark that Chomsky's expressions "mental computation" and "mental representation" are at best metaphors, at worst completely contentless. Before we examine Slezak's motivation for this judgment, it should be noted that MB's point about the content of the expressions "mental computation" and "mental representation", cf. the passage from MB cited on p. 8 above, is quoted out of context. In this context MB's judgment about the status of these expressions follows logically on the remarks that "Chomsky does not even specify what the general make-up of a real mental world would be. He fails to specify what entities or mechanisms in such a world would correspond to 'a computation' or 'a representation'.".

Let us now consider Slezak's (p. 17) motivation for his claim about the way in which the relevant judgment of MB would reflect MB's "confusions"

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about the ontological status of mentalistic claims. This motivation is so peculiar that it deserves to be quoted in full:

> On the contrary, however, these terms are central to the recent discussions of cognition in psychology and philosophy[5] and have been particularly useful in illuminating the character of linguistic theories. While there is certainly room for argument over the details and conception of the 'computational' approach to cognition, Chomsky's use of the terms in this context clarifies his conception of grammars by assimilating it to the conception of cognition which is now being articulated in the literature. Botha's dismissal of Chomsky on this matter without argument suggests his unfamiliarity with the important literature of the computational or information processing approach, and reinforces my point about his failure to appreciate Chomsky's sense of "keeping to abstract conditions that unknown mechanisms must meet".

Let us consider the reasons for the excessive peculiarity of this motivation.

First, Slezak, who implicitly claims superior knowledge of the relevant literature, fails to give a single reference to Chomsky's work from which it is clear (i) that Chomsky "assimilates" his conception of grammars to "the conception of cognition which is now being articulated in the literature", and (ii) how Chomsky achieves this. There is a rather obvious reason for this failure of Slezak's: particulars about such an "assimilation" are simply not to be found in the writings of Chomsky to which MB could have had access. Moreover, given Chomsky's thesis of modularity, it is highly unlikely that at this stage he envisages any nontrivial, nonterminological, way of "assimilating" his form of mentalism to other conceptions of cognition. This thesis --- which holds "the mind to be modular in structure, a system of interacting subsystems that have their own special properties"[13] --- entails for Chomsky (1976:22-23) that

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It remains an open question, and an interesting one, to determine whether there really are significant analogies between the principles of mental representation and computation that seem well motivated in the study of language, and other mental operations, in other domains. Personally, I am rather skeptical; I see no interesting analogies in other cognitive domains, but so little is known that we can really say very little.(14)

Against this background Slezak's "assimilation view" takes on the substance of a fantasy.

Second, Slezak fails to illuminate this fantasy of his, giving no indication of precisely what he takes this "assimilation" to entail. More specifically, he makes no attempt to specify precisely how the content of Chomsky's notions "mental computation", "mental representation" --- and that of more elementary notions such as "PRO", "t", "wh-movement", etc. --- is clarified by this "assimilation". Moreover, he refrains from explaining how the empirical status of mentalistic claims involving these notions is enhanced by this "assimilation". How precisely does this "assimilation" affect the empirical content of, for example, the claims involved in Chomsky's (1976) wh-explanation?

Third, Slezak's implicit suggestion that a better knowledge of the relevant literature on the computational or information processing approach will deflate MB's criticisms of Chomsky's notions "mental computation" and "mental representation" appears to be self-defeating. This literature contains studies which suggest that, for various reasons, it is either impossible or nonproductive at this stage to attempt to "assimilate" Chomskyan claims couched in terms of the expressions "computation" and "representation" to terminologically related claims made by models of cognition, models of speech production and perception, and various approaches in artificial intelligence. Colby (1980:170, 171, 172, 176), for example, stresses the point that just like "linguistics", "artificial intelligence"

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and "cognitive psychology" must face the problem of psychological reality, implying that, in attempting to do so, models of cognitive psychology and approaches to artificial intelligence have not fared any better than linguistic models. In fact, he (1980:172) seems to suggest that psychological models of cognition use a concept "mental representation" which is even less clear than the linguistic concept "ruleset":

> Just as current linguists insist on explicitness of the proposed rulesets, the psychologist-theoretician must become more crisp about his concept of mental representations.

This judgment of Colby's ties in with Butterworth's (1980:423) more general view that

> Models of psychological processes, if they don't actually abound, at least exist in reasonable numbers. However, there is almost nothing in the literature characterizing the general properties of the kinds of entities and processes that might be postulated, nor, more importantly, the general conditions on models of different sorts ...

As is clear from the quotation on p. 8 above, a similar point is made in MB in regard to the mental entities postulated by Chomsky.

As regards the notion "computation", Marr (1977:41-42) has seriously questioned the possibility that artificial intelligence may come up with a computational theory of English syntax "of the type that transformational grammar attempts to define". And Marshall (1980:138, n. 22) has cast doubts on the very sensibility of adopting an artificial intelligence approach to making a language acquisition device:

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At artificial intelligence conferences, it is apparently permissible to make a virtue of having no theory of a natural language computation. Thus Shank (1978) wrote, "Suppose every domain we worked on required yet another ad hoc solution. This might well be the case after all. What would we lose if this happened? Nothing at all. That's what artificial intelligence is all about" (p. 9). If this is the methodology of artificial intelligence, would it not be easier to revert to the traditional biological technique for making a language acquisition device?

Chomsky's language faculty is of course related to such a language acquisition device.

Even more striking, is the argument of Dresher and Hornstein (1976:322), two Chomskyan linguists, that

.... current work in AI [e.g., that of Winograd, Minsky, Shank, and Wanner and Kaplan --- R.P.B.] does not in any way address the central questions that any scientific inquiry into language ought to address. Furthermore, we will argue that most of this work, though purporting to simulate aspects of human linguistic performance is of virtually no psychological --- as opposed to technological --- interest because it is totally devoid of any principles which could serve as even a basis for a serious scientific theory of human linguistic behavior.(15)

Marshall (1980:106-107), also, comments negatively, but from a different angle, on the usefulness of what he calls "the metaphors of 'representation' and 'computation'":

The metaphors of representation and computation that were intended to solve or at least bypass the mind-body problem have not brought anatomy and action together.

Finally, consider the usefulness which Chomsky's notions "mental computation" and "mental representation" may have in embedding grammars in processing models, something Chomsky (1976:12) would very much like to do. Kean (1980:240), a follower of Chomsky's, has to assume that

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.... a theory of sentence processing, when developed, will include computational mechanisms distinct in kind from the rules of grammar [these rules represent Chomskyan "mental computations" --- R.P.B.]. What role the rules of grammar will have in a theory of processing mechanisms is an open question.

She (1980:265-266) makes it quite clear that, if computations are taken to be real elements of processing models, grammatical rules cannot be called "computations" simply by terminological decree. What is required, according to her, is "an explicit theory of the role that rules of grammar play in computation and processing". MB, of course, stresses the point that Chomsky does not have such a theory.

Studies such as those by Colby, Butterworth, Marr, Marshall, Dresher and Hornstein, Kean and others, highlight the extraordinary nature of Slezak's "assimilation" claim. Such studies show that, to substantiate his claim, Slezak will have to stop dealing in vague generalities, backed by nothing more than a couple of nonspecific and unexplicated references, and face the specifics of a representative, nonarbitrary selection of the "important literature".

This general point also applies to Slezak's (p. 14) use of a rather tired computer analogy. He seems to suggest that, had MB been able to come to grips with the nature of the relation between the program or "software" and the "hardware" of a computer, it would have been less confused about the nature of the closely analogous relation between an abstract specification of the language faculty and the physical mechanisms which may underlie this faculty. To clarify his point, he quotes the following remarks by Chomsky (1976:3):

.... linguistics is the abstract study of certain mechanisms, their growth and maturation. We may impute existence to the postulated structures at the initial, intermediate, and steady states in just the same sense as we

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impute existence to a program that we believe to be somehow represented in a computer or that we postulate to account for the mental representation of a three-dimensional object in the visual field. Evidence bearing on empirical hypotheses such as these might derive from many and varied sources. Ultimately, we hope to find evidence concerning the physical mechanisms that realize the program, and it is reasonable to expect that results obtained in the abstract study of the system and its operation should contribute significantly to this end (and in principle, conversely).

However, this computer analogy obscures rather than clarifies the nature of the relation between Chomskyan mental structures and actual physical mechanisms. The reason for this is that it is highly implausible that the relation between Chomskyan mental structures and actual physical mechanisms can be closely analogous to that between the "software" and "hardware" of a computer.

First, as explained by Moor (1978:215), the dichotomy between "software" and "hardware" in the case of computers is useful as a pragmatic distinction, but generates "myths" when assigned too much "ontological significance". This distinction derives its pragmatic nature from the fact that it is not an absolute distinction: the boundary between "software" and "hardware" is variable because programming can occur on many levels. A consequence of this is that what is "software" for one person may be "hardware" for another:

.... since programming can occur on many levels, it is useful to understand the software/hardware dichotomy as a pragmatic distinction. For a given person and computer system the software will be those programs which can be run on the computer system and which contain instructions the person can change, and the hardware will be that part of the computer system which is not software. At one extreme if at the factory a person who replaces circuits in the computer understands the activity as giving instructions, then for him a considerable portion of the computer may be software. For the systems programmer who programs the computer in machine language much of the circuitry will be hardware. For the average user who programs in an applications language, such as Fortran, Basic, or Algol, the machine language programs become hardware. For the person running an applications program an even larger portion of the computer is hardware.

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Returning to the Chomsky/Slezak computer analogy: what does this analogy claim about the nature of the relation between mental structures and actual physical mechanisms? Is it a variable relation, like the one between "software" and "hardware"? If so, what does variability imply in this context and how does it affect the empirical content and testability of specific mentalistic claims, for example those embodied in Chomsky's wh-explanation? If not, what could be the point of invoking this analogy to clarify the nature of the relation between mental structures and actual physical mechanisms?(16)

Second, Moor (1978:215) and Colby (1978:178) make it clear that to call a computer program or "software" abstract cannot imply that it is not physical. Thus Moor (1978:215) notes that

> Unfortunately, computer hardware is frequently characterised as 'the physical units making up a computer system' (Chandor [1970], p. 179). By contrast this seems to suggest that software is not part of the computer system or even worse that it is not physical. It is important to remember that computer programs can be understood on the physical level as well as the symbolic level. The programming of early digital computers was commonly done by plugging in wires and throwing switches. Some analogue computers are still programmed in this way. The resulting programs are clearly as physical and as much a part of the computer system as any other part. Today digital machines usually store a program internally to speed up the execution of the program. A program in such a form is certainly physical and part of the computer system.(17)

That is, the "software" or program of a computer has a physical realization distinct from the "hardware" it "commands". But what is claimed in this regard by the Chomsky/Slezak computer analogy about abstract mental structures? Do these structures also have a physical basis distinct from the actual physical mechanisms which are taken to correspond to the "hardware" of a computer? If so, what is the nature of this basis and how does its existence affect the empirical content of Chomskyan mentalistic claims? If not, how can it be maintained that the distinction between mental struc-

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tures and physical mechanisms is so closely analogous to the one between "software" and "hardware" that the latter may be taken to clarify the former?

The mere fact that it is possible to raise such questions about the computer analogy suggests that one cannot take too seriously Slezak's contention that one of the reasons for MB's confusion about the ontological status of mentalistic theories is its failure to come to grips with the "software"/"hardware" distinction. These questions suggest, moreover, that this analogy in its present form is more harmful than helpful. But why has Slezak, with his superior knowledge of the "important literature", failed to mention questions such as these?(18)

In summary: MB's conclusion that Chomskyan mentalistic claims are ontologically indeterminate --- and hence nonempirical --- is touched neither by Slezak's criticisms of MB's use of the expression "nonactual mechanisms" nor by his attempt to discredit MB's analysis of the Chomskyan notions "mental computation" and "mental representation" or by his implicit contention that a proper understanding of the computer analogy would have eliminated some of MB's confusions about the sense in which mentalistic theories are abstract. By implication even that part of Slezak's attack of MB which, on a charitable reading, may be considered marginally relevant has no real bite.

4 The astrophysical analogy

Slezak uses the greater part of his paper to attack MB's criticisms of the astrophysical analogy used by Chomsky in an attempt to clarify and justify certain methodological aspects of his form of mentalism.(19) Taking as a point of departure Chomsky's statement that "The analogy is modeled on an account given by Bahcall and Davis (1976)", MB argues that

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this analogy fails to serve its purpose because there are at least six basic differences between Bahcall and Davis's astrophysical methodology and Chomsky's mentalistic methodology.(20) Slezak now claims that MB "thoroughly misreads the intent of the analogy" (p. 7), that it fails "to perceive the issues in question" (p. 11) and, consequently, that it insists on "self-evident banalities" (p. 11), and the like. Strangely, however, Slezak fails to ascertain how MB's analysis of this analogy of Chomsky's ties in with the main criticisms presented and motivated in the arguments (1)-(6) of Chomskyan mentalism. Consequently, he fails to see that only one of these criticisms, that of ontological indeterminacy, is presented in the context of MB's analysis of this analogy. He also fails to see that even this point of criticism can be made independently of an analysis of this analogy: MB's notion of ontological indeterminacy does not depend for its content on the particulars of this analogy. This point of criticism is, however, all the more striking when made in the context of an analysis of the astrophysical analogy: it identifies a nontrivial <u>difference</u> between Chomskyan mentalism and astrophysics in a context in which Chomsky draws attention to alleged <u>similarities</u> between these two fields. All of this entails that, even if every single criticism by Slezak of MB's analysis of the analogy were correct, the main criticisms made in MB within the framework of the arguments (1)-(6) would retain their force. But these criticisms of Slezak's are in any case unfounded, as I shall now show.

Slezak's criticisms of MB's analysis of Chomsky's astrophysical analogy are based on his contention that MB has misinterpreted the status of this analogy: MB has missed the point that Chomsky intended the analogy to be interpreted as "a parody". Thus, Slezak (p. 6) contends that

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> .... Chomsky's analogy serves as a reductio ad absurdum or parody of accounts such as Botha's which take such questions seriously in connection with the constructions of linguistics. Thus, Chomsky (1976:4) suggests that supposing the astronomer to have developed a theory of the interior of the sun from evidence of the periphery, we confront him with the following: 'True, you have presented a theory that explains the available evidence, but how do you know that the constructions of your theory have physical reality --- in short, how do you know that your theory is true?'.

According to Slezak (pp. 7-8), MB's failure to interpret this analogy as a parody, stems from the fact that MB has missed the point that the above quoted question to the astronomer is "clearly intended as irony" by Chomsky "for the sake of highlighting his point about a general scientific realism". In support of his "parody" interpretation of the analogy and his "irony" interpretation of the question to the astronomer Slezak furnishes not a single scrap of textual evidence from Chomsky's paper. In the tradition of less respectable forms of literary analysis, he arbitrarily projects the attributes represented by "parody" and "irony" onto Chomsky's text. This, however, turns out to be a comparatively minor flaw; let us consider some substantive reasons why Slezak's "parody"/"irony" interpretation has to be rejected.

First, Slezak's "irony" interpretation of the question to the astronomer is incoherent because it does violence to another basic component of the analogy, viz. one of the ways in which the astronomer can properly react to the question, according to Chomsky. On Slezak's "irony" interpretation, the question to the astronomer expresses a lack of understanding, on the part of the challenger, of the claims of the theory and of the way in which the available evidence bears on these claims. By contrast, on MB's "non-irony" interpretation, the same question expresses the dissatisfaction of the challenger with the available evidence for the theory. On this interpretation, the challenger fully understands the claims of the theory and, moreover, sees how the available evidence bears on the theory

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but he considers the available evidence as providing insufficient justification for the existence claims of the theory. Notice now that Chomsky (1976:5) characterizes one of the ways in which the astronomer may properly react to the question as that of searching for additional evidence for the challenged theory, evidence which may be obtained by the astronomer's hitting upon "a more direct method for studying events taking place at the interior of the sun". From remarks such as the following by Chomsky (1976:5-6) this way of reacting is clearly prompted by the astronomer's having come to share the challenger's dissatisfaction with the available evidence:

Or, like the astronomer dissatisfied with study of light emissions from the periphery of the sun, we can search for more conclusive evidence... (21)

But how can Slezak's "irony"-interpretation of the question to the astronomer be reconciled with this component of Chomsky's analogy, this way of reacting to the challenge? Why would Chomsky provide for a way of reacting in terms of which additional, more conclusive evidence is sought by the astronomer if the astronomer judges --- as implied by Slezak's "irony"-interpretation --- that the original evidence is sufficient and that the challenge does no more than reflect the challenger's lack of understanding of how this evidence bears on the theory? But this way of reacting by the astronomer, while incoherent on Slezak's "irony"-interpretation, makes good sense on MB's "non-irony" interpretation of the challenging question.

Second, Slezak's (1981:7) attempt to "illuminate" his interpretation of the analogy "by means of a well-known joke which has the same logical character" turns out to have unfortunate, rather unfunny consequences for this "irony"-interpretation. But first Slezak's (p. 7) joke:

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A native upon first seeing a steam locomotive is totally mystified and asks for the explanation of its movement and power. After an elaborate account of the coal fire, the water turning to steam and driving the wheels etc., the native is asked if he now understands the behaviour of the locomotive. He replies that he understands everything perfectly, except for one small detail, namely, where they fit a horse inside. Faced with this question, clearly the only answer is to repeat the entire explanation.

This joke backfires because the "native's" question is clearly distinct from both MB's "non-irony" interpretation, and Slezak's "irony" interpretation of the question to the astronomer. Thus, the "native's" question reflects such a total lack of comprehension that it is not possible to properly react to it in either of the two ways in which, according to Chomsky, the astronomer may respond to his challenger. On the one hand, a search for additional evidence would clearly be inappropriate, ruling out a "non-irony" interpretation of the "native's" question. On the other hand, the "native's" lack of comprehension is clearly so profound that there could be no point in merely repeating the original explanation that has already been presented. This rules out an "irony" interpretation of the "native's" question. A person trying to reply to the "native's" question would have to adopt a third way of reacting, moving onto a completely different level on which he first explains to the "native" that, underlying the "horse" question, there are assumptions which are incorrect, and then explains why each of these assumptions are incorrect, etc. Suppose someone wanted to ask the astronomer a question which is analogous to the "horse" question of the "native". The former question would have to be something like: "Where do they fit a lamp inside the sun?" This question, however, paraphrases neither the "irony" nor the "non-irony" interpretation of the real question to the astronomer, which shows just how irrelevant Slezak's joke is. But why would Slezak --- who (p. 7) believes "that Botha so thoroughly misreads the intent of this analogy" --- wish to demonstrate his superior understanding of the analogy by

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dishing up an irrelevant joke?

Third, Slezak's "irony" interpretation of the question to the astronomer cannot be reconciled with the real question figuring in the astrophysical inquiry on which Chomsky models the analogy. As explained in MB (p. 14) the question which initiated Bahcall and Davis's (1976:264) inquiry springs from their dissatisfaction with the improper fit between the predictions of the theory of solar nuclear burning and the available evidence, not from somebody's misunderstanding of the claims of the theory or of the way in which the evidence bears on these claims. Thus, Slezak's "irony" interpretation has to model a question which can be interpreted, in the context in which it occurs, in a non-ironical manner only.

There are two ways not open to Slezak for attempting to evade this criticism. On the one hand, it would not do Slezak any good to repeat his (p. 8) claim that

> I believe that the details of the astronomical inquiry and its specific methodology are utterly irrelevant to the point Chomsky has wanted to make.

To depict the nature of the question challenging the astronomer as constituting an irrelevant detail, would be to destroy the very basis of Chomsky's astrophysical analogy. In what nontrivial sense could Chomsky use Bahcall and Davis's astrophysical inquiry as a model if the nature of the question which initiated and guided this inquiry were a mere irrelevant detail? Slezak makes no attempt to draw a nonarbitrary distinction between those aspects of Bahcall and Davis's methodology which are irrelevant details and those which are not.

On the other hand, it would also do Slezak no good to repeat his (p. 8)

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claim that

.... Chomsky cites only incidentally [Bahcall and Davis's (1966) study --- R.P.B.] as having provided his example for the sake of illustration.

As noted above, Chomsky (1976:4) explicitly assigns Bahcall and Davis's (1976) study the status of the model for his analogy, not that of "an example for the sake of illustration" as Slezak creatively suggests. What sense there would be in both modelling an analogy on a given study and at the same time in citing this study "only incidentally" Slezak alone will know. Moreover, even the most cursory reading cannot fail to reveal how Chomsky (1976:4ff.) works out this analogy with reference to the challenging question, the nature of the possible reactions to this question, the assessment of the significance of the additional evidence yielded by more direct investigation, etc. Likewise, the way in which Chomsky (1976:5-6) invokes this analogy in his argument against the obligatory use of external evidence can hardly be overlooked, even in a superficial reading of this paper. In short, there is nothing incidental to the way in which the astrophysical analogy figures in Chomsky's discussion, neither in the reference to Bahcall and Davis's study as the model for the analogy, nor in the way in which this analogy is worked out and used by Chomsky. Consequently Slezak's "parody" interpretation of the astrophysical analogy and his "irony" interpretation of the question to the astronomer, as well as the criticisms of MB which he bases on these interpretations, have to be rejected for the reasons given above.

Slezak's discussion of MB's analysis of the astrophysical analogy is not, however, wholly uninstructive: certain aspects of this discussion reveal that he has altogether failed to understand the general thrust of the

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criticisms made in MB of Chomskyan mentalism. Having failed to correctly identify the main arguments (1)-(6) of MB, Slezak mistakenly seems to assume that MB --- and "other similar accounts" --- criticize Chomskyan mentalism for not meeting the following conditions.

(8) To properly react to a challenge of his existence claims, the mentalist must provide, in addition to the ordinary kind of "realist" justification, "some other kind of justification" or "other grounds" for these claims.

(9) To properly react to a challenge of his existence claims, the mentalist must address himself explicitly at a general meta-scientific level to epistemological questions about the status of his theoretical constructs.

As regards (8), Slezak (p. 7) seems to adopt Chomsky's (1976:4-5) point that this condition is not part of "ordinary scientific realism", representing a requirement which physical sciences do not have to meet. But this point is entirely irrelevant for the simple reason that MB does not accept (8) as a condition for Chomskyan mentalism. In fact, MB's point is the very opposite: in regard to the validation of existence claims, Chomskyan mentalism must meet the same conditions which apply to the natural sciences taken to be paradigmatic by Chomsky. These conditions are formulated in terms of the notions "ontological determinacy" and "evidential determinacy". To adopt these conditions amounts to no more than insisting that the mentalist's imputing of existence to theoretical constructs must represent a step which has empirical consequences. Concretely, if Chomsky imputes existence to the theoretical constructs used in his wh-explanation, resulting mentalistic claims such as A(a)-(c)

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in note 5 must have falsifiable consequences which cannot be derived from a nonmentalistic interpretation of this wh-explanation.

This brings us to a crucial point: Slezak fails to understand the point --- discussed in some detail in MB (§§4.2, 5.3) --- that to propose a linguistic explanation such as Chomsky's wh-explanation does not necessarily entail interpreting the hypotheses involved in the explanation as descriptive of an underlying "mental organ" or "language faculty". From the relevant literature it is clear that there are two basic alternatives to assigning a mentalistic interpretation to linguistic hypotheses proposed to account for conventional internal linguistic data. The first alternative, as explained by Katz (1977), entails adopting a position in terms of which the hypoheses involved in the explanation are interpreted as not expressing ontological claims about any underlying reality, mental or other.(22) The second alternative, as made clear by Stockwell (1980: 354ff.), entails that hypotheses such as those involved in Chomsky's wh-explanation are viewed as descriptive of some kind of reality, not however of a Chomskyan mental reality. The alternative realities include a "cultural reality", a "social reality", a "psychological reality distinct from Chomsky's", etc.

That Slezak is unaware of the alternative modes of interpreting the ontological import of one and the same set of formal linguistic hypotheses is clear from the way in which he (p. 2) carelessly conflates the distinct notions "generative grammar" and "Chomskyan mentalistic theories":

> Rudolf Botha (1980) argues that there are fundamental considerations which cast doubt on the view that generative grammar is a form of empirical inquiry. In his lengthy study Botha (1980:3) says: 'The general tenet of the criticism is that Chomskyan mentalistic theories are both ontologically and evidentially indeterminate and hence, in terms of Chomsky's own methodological theory, nonempirical'. (23)

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Clearly, "generative grammar" and "Chomskyan mentalistic theories" are arbitrarily taken by Slezak to be one and the same, notwithstanding many arguments to the contrary in the relevant literature.(24)

That Slezak has completely missed the crucial point about the possible different interpretations of the ontological status of linguistic hypotheses such as those involved in Chomsky's wh-explanation is moreover clear from his remark (p. 10) that

> Since we automatically attribute physical reality to whatever is postulated in our best available theories, the challenge concerning their reality can only be met by repeating the evidence and the explanations, since there can be no other grounds for attributing physical reality to theoretical constructions.

This remark, read in conjunction with those following it, suggests that Slezak equates the postulation of linguistic hypotheses such as those involved in Chomsky's wh-explanation with the attribution of psychological reality to the theoretical constructs used in these hypotheses. But generative grammarians do not automatically attribute psychological reality to their linguistic theories constructed to account for the conventional kind of internal linguistic data. As explained above, they can refrain from attributing any kind of underlying reality to these theories or they can attribute a non-Chomskyan kind of reality to them. It has in fact been argued that in the early days of generative grammar, even Chomsky himself refrained from attributing psychological reality to his linguistic theories.(25)

And this brings us to the crux of the matter: given the various alternative ways of interpreting the ontological status of one and the same set of linguistic hypotheses proposed to account for internal linguistic data, the linguist who attributes a specific kind of reality to these

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hypotheses has to provide additional justification for the resulting ontological claims, if these claims are to be empirical. Additional justification in this context is not identical to some bizarre kind of extraordinary justification alluded to in (8) above. Additional justification --- as made clear in MB (§§4.4, 5.4) --- is simply justification which is appropriately independent of that provided initially for the linguistic theory. If the kind of reality attributed to the linguistic theory is Chomskyan psychological reality, the additional justification must comprise evidence which simply does not bear on nonmentalistic interpretations of this theory. If a linguistic theory in a mentalistic interpretation is not made responsible to some kind of evidence --- e.g., external linguistic evidence, as argued in MB --- which is irrelevant to the validation of other ontological interpretations of this theory, then the mentalistic claims associated with this theory are evidentially indeterminate, and hence nonempirical. This is the fundamental point about the validation of Chomskyan mentalistic theories argued in (5) by MB, a point clearly distinct from the one involved in Slezak's fictitious requirement (8) above.(26)

This brings us to (9), a second condition to which MB does not subject Chomskyan mentalism, contrary to what Slezak seems to suggest. Slezak (p. 3) misleadingly suggests that MB "demands" from Chomsky an "elaborate epistemological analysis of the notion 'empirical'" and that it takes Chomsky's failure to do so as constituting a serious defect of Chomskyan mentalism. As is clear from the arguments (1)-(6) above, MB criticizes Chomskyan mentalism for making nonempirical existence claims, NOT for Chomsky's failure to provide a metascientifically explicit characterization of his motion "empirical". Such a characterization, if accurate, would of course have made it easier to assess the epistemological status of Chomskyan mentalistic claims, but its absence is not taken by MB as an

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indication that these claims are nonempirical.

Slezak repeatedly indulges in this mixing of levels, confusing ordinary methodological considerations which (should) govern the research activities of the practising mentalist with the requirement that the mentalist must present an explicit philosophical characterization or justification of these considerations. The following remarks by Slezak (p. 9) provide a striking example of his confusion of levels:

> .... Botha claims that there is another feature of the astronomical inquiry which is 'hidden' by Chomsky's account, namely that this inquiry is not concerned with such epistemological notions as 'truth' or 'conclusive evidence'. Again, Botha mistakenly takes Chomsky to be attributing such philosophical aims to the scientist's research, whereas Chomsky's point is exactly the opposite. Thus, Botha (1980:14) informs us that 'a close study of Bahcall and Davis's paper, however, destroys the impression that their inquiry was truth-oriented'. Now, of course, in one obvious sense it must be perfectly correct to say that such scientific inquiry is 'truth-oriented' and there is no indication that Chomsky has anything in mind other than this uncontroversial sense. Botha takes Chomsky to be attributing epistemological concerns to the astronomers once again because he has misread Chomsky's intention in asking them rhetorically 'how do you know that your theory is true?'

MB (pp. 14-15), however, uses "truth-oriented" and related expressions such as "truth", "conclusive evidence", etc. to characterize the aim which Bahcall and Davis set for their inquiry: the aim of uncovering, through substantive inquiry, possible defects in the theory, not the aim of searching, by means of this inquiry, for evidence which may be used to argue for the truth of the theory. Contrary to what Slezak suggests, MB neither assumes that Chomsky attributes "philosophical aims" and "epistemological concerns" to Bahcall and Davis nor makes the extraordinary claim that they have, or have not, in fact directed their inquiry at these or any other abstract "philosophical aims" and "epistemological concerns". Again Slezak confuses methodological considerations which

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(should) guide or govern the scientist's research activities with a metascientific concern with these considerations.(27) MB's claims about Bahcall and Davis's inquiry deal with the former considerations, not with the latter concern. Slezak's confusion of levels clearly indicates his limited understanding of the nature of MB's criticisms of Chomskyan mentalism. It is this limited understanding which spurs him on to defend Chomsky against "absurd" and "banal" criticisms, nowhere to be found in MB.

5 The neurophysiological analogy

Referring to work by Grobstein and Chow (1975), Chomsky claims that linguists and psychologists who make "nativist" claims about the language faculty are doing essentially the same thing as neurophysiologists who attribute restrictive principles to the genetic program. He notes that neurophysiologists are not criticized for violating a methodological canon in making their "nativist" claims and rhetorically asks why mentalist linguists and psychologists should be criticized for doing the same thing.(28) MB (§3.3) criticizes this neurophysiological analogy of Chomsky's by noting that there are essential differences between the mentalist's and the neurophysiologist's "nativist" claims. On the one hand, the mentalist's "nativist" claims postulate entities which are not uniquely identifiable, making these claims, unlike "nativist" neurophysiological claims, ontologically indeterminate. On the other hand, the mentalist's "nativist" claims cannot be tested by means of (experimental) procedures which stand comparison with the methods --- e.g. micro-electrode sampling --- available for the testing of the neurophysiologist's "nativist" claims. Consequently, MB concludes that making mentalistic "nativist" claims is not essentially similar to making neurophysiological "nativist" claims. This implies that the legitimacy of making the former claims cannot be justified by pointing out the legitimacy of making

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latter claims, as Chomsky seems to do by posing the rhetorical question mentioned above. For this kind of justification to be acceptable, the mentalist's "nativist" claims must in all relevant respects --- notably ontological determinacy and testability --- be similar to the neurophysiologist's "nativist" claims. Specifically, making the same kind of existence claims entails accepting the same kind of epistemological responsibility for these claims. If the nature of this responsibility is decreed to be an "irrelevant detail", the analogy --- be it neurophysiological or astrophysical --- collapses.

Although Slezak (p. 18) admits that certain aspects of MB's discussion of Chomsky's neurophysiological analogy are "totally unclear", he nevertheless proceeds to argue that this discussion "amply confirms the criticisms" he has been making of MB. Predictably, it turns out that Slezak's own criticisms of MB's discussion of the neurophysiological analogy are rather off-target, making much of non-issues and making public some rather exotic beliefs.

Let us first consider an example of a non-issue treated at some length by Slezak. In a note (n. 19, p. 104), MB in passing draws attention to a rather curious aspect of Chomsky's reference to Grobstein and Chow's (1975) paper. MB's point is simple. Over the years Chomsky has attempted to deemphasize --- though not eliminate --- the role of linguistic experience in language "development". Grobstein and Chow, by contrast, have found it necessary to present, as "a new perspective", the point that genetic information is probably intrinsically inadequate to effect the proper development of the visual pathways in cats, rabbits and monkeys, and to stress the fact that visual experience significantly influences and is probably indispensable to this development. MB merely considers this marked difference in emphasis noteworthy in the context of the refe-

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rence Chomsky makes to Grobstein and Chow in clarifying and justifying his "nativism". As is clear from the arguments (1)-(6), MB makes no further use of this point, not adopting it, for example, as a basis for its criticisms of Chomskyan mentalism. Slezak (p. 20), however, construes MB's observation about this difference in emphasis as a point raised to seriously damage Chomsky's "nativism". He attempts to explain at length why this difference should not be considered harmful to Chomsky's "nativist" position, thereby making much of something which is a non-issue in MB. Surely this is a strange procedure for someone with a proper understanding of the issues at stake in MB.

We come now to that part of Slezak's discussion which, on a charitable reading, may be considered to bear vaguely on one of the points raised in MB in connection with Chomsky's neurophysiological analogy. This is the part of Slezak's discussion in which he raises an issue discussed by both Chomsky (1976:9) and MB (pp. 25-26) in relation to Chomsky's astrophysical analogy. In that context, Chomsky claims that the direct investigation of human mental mechanisms, "by intrusive experimentation", is prevented by barriers which are ethical in nature. In response to this point, MB (pp. 25-26) argues that, even if there were no such ethical barriers, such "direct investigation" is ruled out by a methodological consideration:

> Normally, experimentation has the function of putting to test claims which are so determinate in their content and so precise in their formulation that they have clear test implications. Thus, determinacy and preciseness of content are preconditions for carrying out experiments. In the absence of clear, ontologically determinate and precise claims, there is simply nothing to test, nothing to direct 'intrusive experimentation' at. Thus the fundamental barrier to 'direct investigation' is a methodological one, not an ethical one. The ethical question arises only after it has become clear that Chomsky's 'intrusive experimentation' is possible in principle.

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Slezak (p. 18) quotes the italicized remarks of this passage[29] without giving an indication of how they fit into the argument whose conclusion they represent. He (p. 18) then proceeds:

> Botha's point here seems to derive from his worries about "nonactual mechanisms" but, in any case, the simple fact is that neurological data is already increasingly being brought to bear on linguistics and conversely under the heading of the new discipline of 'neurolinguistics', precisely as we saw Chomsky suggest earlier.[6] Indeed, this is hardly even new considering the significant neurological insights gained into language in the classic studies of aphasia, alexia etc. in the nineteenth century. Chomsky's reference to ethical barriers, then, is only a way of pointing to the kinds of inquiry into neurological realizations of the abstract mechanisms postulated in linguistics.

However, these remarks by Slezak leave MB's criticisms of Chomsky's neurophysiological analogy unscathed. Slezak's improper use of the expression "nonactual mechanisms" in criticizing MB has already been dealt with above and nothing more need be said on this score. Let us therefore focus on his view of the role allegedly played by "neurological data" in the validation of "nativist" mentalistic claims. Note that, contrary to what Slezak suggests, MB does not make any claims about the way in which "neurological data" can or cannot be brought to bear "on linguistics", whatever this may be. A rather more precise claim is made in MB, viz. that unlike neurophysiologists' "nativist" claims such as those made by Grobstein and Chow, mentalists' "nativist" claims such as those embodied in Ross's wh-Island Constraint or Chomsky's deeper Subjacency Principle cannot be tested by confronting them with neurophysiological data. In response to this claim, Slezak must either argue that it is not necessary to test the specified Chomskyan "nativist" claims in this way or he must show that it is in fact possible to test these claims in this way. He pursues neither of these alternatives. Instead, he goes for a non-option, viz. to refer in the vaguest of terms to "the simple fact that

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.... neurological data is increasingly being brought to bear on linguistics" and, on top of this, to "the significant neurological insights gained into language in the classic studies of aphasia, alexia etc. in the nineteenth century"! Of course, Slezak neither specifies which "neurological data" can be brought to bear on, for example, Chomsky's Subjacency Principle nor spells out the auxiliary assumptions or bridge theory --- to use a notion of MB's --- that are required for making the "neurological data" relevant in principle to the validation of mentalistic "nativist" claims. He is maximally imprecise about how Penfield and Roberts's findings of 1959 --- to which he alludes on p. 21 --- bear on a "nativist" Chomskyan principle such as Subjacency. And, to mention only one further point, Slezak makes no attempt to explain why mentalist linguists such as Chomsky have not simply jumped at the opportunity of validating their "nativist" claims with reference to the "neurological data" and "insights" which he mentions.

MB (p. 84) argues that there are major obstacles in the way of making neurological/neurophysiological and other kinds of external linguistic evidence relevant in principle to the validation of Chomskyan "nativist" claims. Amongst other things, it would be necessary to develop a bridge theory or interpretive mechanism mediating between the former data and the latter claims. And recently various scholars have argued that no real progress has been made in the development of such theories "which can mediate between noun phrases and neurons", to use a turn of phrase of Marshall's. Marshall's (1980:106) overall assessment of the possibility of constructing such theories in the near future is not encouraging:

> They [i.e., biologists --- R.P.B.] have accumulated a vast body of knowledge concerning the gross anatomy of the parts of the central and peripheral nervous system that seem to be implicated in the acquisition and exercise of linguistic abilities. Some knowledge is even available

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about the slightly less gross physiology of the relevant brain areas. Nor does primary responsibility rest with the students of developmental psycholinguistics. They too have amassed alarming amounts of data on the progression from the birth cry to the multiply embedded relative clause. The problem is rather that no one knows how to relate these two domains of inquiry to each other.

On the possibility of couching such interpretive theories specifically in physiological terms, Marshall (1980:125) makes the following remark:

> There is at the moment no discipline that could be called the physiology of language and, to the best of my knowledge, no one has ever considered how theories within such a discipline could be constructed or evaluated. That is, we have no principled ideas about how language is coded by the brain.(30)

But Slezak claims that ".... in any case, the simple fact is that neurological data is already increasingly being brought to bear on linguistics ....". Appraisals such as those by Marshall and Colby show just how remarkable this claim of Slezak's is when construed as part of an attempted rebuttal of MB's criticisms of Chomsky's neurophysiological analogy. We seem to have yet another case where a contention of Slezak's fails to derive credibility from the "important literature".(31)

6 Conclusion

Introducing his paper, Slezak (p. 2) proposes "to examine Botha's criticisms in detail with a view to demonstrating that they are without foundation and are based on the most fundamental misunderstandings". Concluding his paper, Slezak (p. 21) expresses the hope that he has shown "that the conceptions on which these criticisms rest are so seriously flawed as to make it unprofitable to attempt to unravel the rest of

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his analysis". These formulations, by all standards, represent rather strong rhetoric. But, as the preceding paragraphs have shown, Slezak's discussion sadly lacks the relevant and accurate analyses needed to give substance to his rhetoric.(32)

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NOTES

*I would like to thank Marina Savini and Thereza Botha for suggestions which led to improvements in the formulation of this paper.

1. Chomsky's (1976) answers to questions such as the following are taken in MB (p. 6) to jointly constitute "Chomskyan mentalism":

   (a) What are the objects in the real world which mentalistic (linguistic) theories --- grammars as well as general theories --- are about?

   (b) What are the aims that these theories pursue in regard to the objects in question?

   (c) What is the epistemological status --- empirical or non-empirical --- which the claims expressed by mentalistic theories are supposed to have?

   (d) What is the evidence and the logic required for the validation --- i.e., confirmation of refutation --- of these mentalistic theories?

2. When referring to Slezak's paper, I will henceforth omit the year of publication and specify the relevant page number only.

3. The hypotheses embodied in Chomsky's (1976) wh-explanation are proposed to account for conventional, internal linguistic data such as grammaticality judgments about sentences. Proposed as such, these hypotheses are not inherently mentalistic, as is explained in MB (pp. 4-5, 36-37). These hypotheses jointly acquire the status of a mentalistic theory if a linguist --- such as Chomsky --- imputes existence to or attributes psychological reality to the theoretical constructs used in them. As a (Chom-

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skyan) mentalistic theory --- or interpreted mentalistically --- these hypotheses purport to characterize in some sense a particular state of a "mental organ", Chomsky's "language faculty". We return in §4 below to the different ways in which one and the same set of linguistic hypotheses may be interpreted ontologically.

4. In MB, as in other similar discussions, the expressions "existence claims" and "ontological claims" are used as synonyms.

5. MB uses "Chomskyan existence claims", "Chomskyan ontologic claims" and "Chomskyan mentalistic claims" as synonyms. As typical examples of Chomskyan mentalistic claims, astrophysical existence claims, and neurophysiological existence claims MB uses (A), (B) and (C) respectively.

(A) (a) $[_S$ which for PRO to play sonatas on t $]$ exists as a component part of a mental representation underlying the question What sonatas are violins easy to play on?

(b) wh-movement exists as a component part of the mental computations by means of which the question What sonatas are violins easy to play on? is derived.

(c) The wh-island constraint is a genetically determined, that is, innate, mental mechanism --- an aspect of the initial state of the language faculty.

(B) (a) The sun's heat is produced by thermonuclear reactions that fuse light elements into heavier ones, thus converting mass into energy.

(b) The basic solar process is the fusion of four protons to form an alpha particle, two positrons (e^+), and two neutrinos (ν); that is $4p \longrightarrow \alpha + 2e^+ + 2\nu$.

(C) The possible orientation specificities for individual neurons are genetically determined, that is, innate.

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Whereas the claims of (A) are derived from Chomsky 1976, those of (B) and (C) come from Bahcall and Davis 1976 and Grobstein and Chow 1975 respectively.

6. In the context of statements such as these, MB, like other similar discussions, uses "entities" and "mechanisms" interchangeably as synonyms.

7. MB (pp. 21-22) takes some care to explain that the expression uniquely identifiable is synonymous neither with "directly observable" nor with "the existence of which can be demonstrated or proved in a logical/mathematical sense".

8. Evidential indeterminacy is taken in MB (p. 45) to be distinct from "mere" underdeterminedness by evidence. To say that a scientific hypotheses is underdetermined by the evidence is simply to say that the evidence bearing on it cannot, in principle, prove it to be true. To say that a hypothesis is evidentially indeterminate is to say that it is not possible to bring any evidence of the appropriate kind to bear on it.

9. These arguments have in fact been presented elsewhere in the form of a separate, independent paper, viz. Botha 1979.

10. For an earlier, independent argument to the effect that the evidential base of Chomsky's mentalistic theories is inadequate for validating its claims about psychological reality cf. Pylyshyn 1971:551; 1973:44ff.

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11. The italics are mine.

12. Slezak (e.g. pp. 6, 9) suggests that the linguist's and the physicist's imputation of existence to theoretical constructs manifests the same "ordinary scientific realism". Unfortunately he does not explain, with reference to this "ordinary scientific realism", what sense it would make for an astrophysicist to impute existence to physical concepts by claiming that the entities postulated by them represent "abstract conditions that unknown mechanisms must meet". Although the notion "ordinary scientific realism" is pivotal in Slezak's discussion, he uses it without giving an explicit and nonarbitrary characterization of its content. Cf. also note 26 below.

13. For this characterization cf. Chomsky 1980:90. In this publication Chomsky (e.g. pp. 28, 40-47, 59-61, 64, 89-90, 180) deals, in some detail, with the significance of this thesis and with its consequences.

14. Chomsky (1976:23) also airs this skepticism when he says that "There seems little reason to suppose, for the moment, that there are general principles of cognitive structure, or even of human cognition, expressible at some higher level, from which the particular properties of particular 'mental organs' such as the language faculty can be deduced, or even that there are illuminating analogies among these various systems". This view is also repreated in the passages referred to in note 13.

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15. Dresher and Hornstein's (1976:396-397) conclusion should also make disappointing reading for Slezak. For some of the general problems encountered in attempting to "assimilate" --- in some specific, nonterminological sense --- Chomskyan grammars to artificial intelligence approaches cf. also Valian 1979:17-19. Should Slezak attempt to take evasive action by claiming that he does not include artificial intelligence in "the computational or information processing approach", he would achieve no more than underlining the obscurity of this expression and of his 'locution' "'artificial intelligence' and information processing approaches to cognition" (p. 15).

16. With reference to work being done in cognitive simulation, Moor (1978:216-217) presents a concrete illustration of the kind of problem that may arise when the "software"/"hardware" distinction "is understood and taken to have more ontological significance than it has".

17. Colby (1978:178) formulates the point as follows: "In computer terms, the symbolic program commands the hardware, but the program has a physical (spatio-temporal) location in memory as contents of addresses".

18. The list of items representing "important literature" which appear to have escaped Slezak's attention may be readily extended. For example, in appraising the computer analogy with reference to the "mind–body problem", Bunge (1980:14, 62) furnishes a variety of reasons as to why "in the case of brains the hardware–software dichotomy makes no sense ..." (p. 63). Bunge also cites other "important literature" --- e.g. Young 1971:130 --- in which it is argued that the computer analogy in fact constitutes a disanalogy.

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19. This analogy is outlined as follows in MB (pp. 12-13):

> The essence of Chomsky's (1976:4ff.) astrophysical analogy may be reduced to four main points. First, like the physicist endeavouring to determine the nature of the thermonuclear reactions that take place in the interior of the sun, the (mentalist) linguist too investigates hidden mechanisms, viz. the apparatus of the language faculty. Second, like the physicist, the linguist constructs his hypotheses on the basis of indirect data about these hidden mechanisms. In the case of the physicist, these indirect data relate to light emitted at the outermost layers of the sun; in the case of the linguist, they are derived from linguistic behavior. Third, if doubts are raised about the existence of the hidden mechanisms postulated by either the physicist or the linguist, he can react in one of two ways. He can repeat the original evidence and show once more how this evidence is explained by the hypotheses postulating the hidden mechanisms. Or, he can look for a more direct manner of investigating the hidden mechanisms in question. In the case of the physicist, the more direct manner of investigation takes on the form of the experimental study of neutrinos released by the thermonuclear reactions in the solar interior. Fourth, in neither the physicist's nor the linguist's case can the evidence yielded by the more direct investigation really meet a challenge about the existence of the postulated hidden mechanisms. This evidence has no privileged status and cannot conclusively show that these mechanisms really exist.

20. These differences concern the nature of the question which initiated the inquiry, the nature of the epistemological aim pursued in the inquiry, the nature of the logic required for validating the existence claims, the assessment of the weight of additional evidence for these claims, the reliability/firmness of the evidence, and the ontological import of the existence claims.

21. The italics are mine.

22. For more details about one such a position, viz. the "Platonist Position", cf. Katz 1977:562ff.; MB:37, 62-63.

23. The italics are mine. Slezak's claim about what Botha (1980) argues is truly astonishing if one takes into consideration the fact that MB (p. 27) explicitly states that ".... the conclusion drawn above is

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that mentalistic claims such as (3)(a) and (b) are nonempirical as existence claims about a real mental world. The conclusion is NOT that a nonmentalistic interpretation of linguistic theories such as the one involved in the wh-explanation is nonempirical as well". The italics and capitals occur in MB already; (3)(a) and (b) denote the mentalistic claims presented as (A)(a) and (b) respectively in note 5 above. The essence of the quoted statement is repeated in MB on pp. 31, 46-47, 61, and in other similar passages.

24. Slezak also seems to be unaware of another distinction, viz. the one between "nonnativist" mentalistic claims associated with Chomskyan grammars and "nativist" mentalistic claims associated with Chomsky's general theory of language. This is clear from the way in which he (p. 3) equates "linguistic grammars" with "linguistic theories" in the following remarks: "Botha is concerned with the mentalistic claims made by Chomsky for linguistic grammars, that is, with his attribution of psychological reality to the constructs of linguistic theories". But, contrary to this claim by Slezak, MB is concerned both with "nonnativist" mentalistic claims associated with Chomskyan grammars and "nativist" mentalistic claims associated with Chomsky's general theory of language. The quoted remarks of Slezak also suggest that he has not come to grips with Chomsky's (1976:3) distinction between "the initial state of the mind" --- of which the general theory of language is claimed to present a partial characterization --- and "the steady state" --- of which particular grammars are claimed to present a partial characterization. This distinction of Chomsky's is also outlined in some detail in MB (pp. 6-8).

25. Cf. Steinberg 1975 for an analysis of the way in which Chomsky's

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mentalism originated and developed.

26. Stockwell (1980:356-358) has independently presented an argument for a related point. (Cf. also note 10 above.) Perhaps Slezak has missed MB's fundamental point because of the way in which he operates with the notion "a general scientific realism", a notion which also appears to underlie his use of such expressions as "a quite general scientific realism" (p. 4), "the standard realist view" (p. 6), "ordinary scientific realism" (p. 9) etc. The obscureness of the content of this notion allows Slezak to make arbitrary pronouncements on what is and what is not part of "a general scientific realism".

27. This confusion of levels is also manifested in Slezak's (pp. 8-9) misinterpretation of MB's use of the expression "ontological question". Whereas MB uses the expression "ontological question" to denote a challenge requiring a scientist to provide additional substantive motivation for his existence claims, Slezak mistakenly takes it to denote some abstract metascientific question to which the scientist apparently has to respond in terms of a philosophical discourse.

28. (A)(c) and (C) in note 5 above represent examples of "nativist" linguistic and "nativist" neurophysiological claims respectively.

29. These remarks are not italicized in MB; the italics are used here for the sake of convenient reference.

30. Colby (1978:177) puts the general problem in a nutshell: "The conceptual distance between symbolic rules and neurons is so great that it is difficult to propose how knowledge about one might contribute

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to knowledge about the other. There is no question of the relevance of neural sciences to the understanding of human behavior. That the central nervous system is involved in the production of overt and computational behavior is a less than absorbing thesis". The implication is clear: mentalist linguists require devices which are non-existent at present, viz. theories which mediate in a precise way between specific theoretical linguistic constructs on the one hand, and specific neurological entities on the other.

31. Slezak gives another clear indication that he has failed to appreciate the implications of Chomsky's (1976) use of the astrophysical and neurophysiological analogies for the purpose of clarifying and justifying the methodological bases of mentalistic theories, when he (pp. 13-14) states that "... we may say that competence models are to be seen as 'functionalist' accounts in Fodor's (1964, 1968) sense. This means that they are abstracted from the actual details of the material realization or physical embodiment of the process". Chomsky (1976), however, does not attempt to justify the imputation of existence to (theoretical constructs of) mentalistic theories by arguing that they may be interpreted in a "functionalist" way. In (Chomsky 1976), this justification must come from the alleged parallelism between these mentalistic theories on the one hand and the relevant astrophysical and neurophysiological theories on the other hand. And the latter theories clearly do not receive a "functionalist" ontological interpretation. Thus, one cannot assign a "functionalist" interpretation to mentalistic theories and simultaneously use the analogies in question to clarify and justify the ontologic status of these theories. Of course, if one were to abandon the astrophysical and neurophysiological analogies such an "account" becomes a logical possibility.

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But then the point stressed repeatedly in MB is that Chomsky's imputation of existence to theoretical linguistic constructs would have to be judged "improper" and "illegitimate" to the extent that it derives its justification from the undermined astrophysical and neurophysiological analogies.

32. In this reply to Slezak, I have defended MB as an analysis of a particular aspect of Chomsky's "On the biological basis of language capacities" (1976). MB was published informally in 1979 as Nr. 3 of Stellenbosch Papers in Linguistics and clearly does not represent an analysis of Chomsky's more comprehensive Rules and representations (1980) of which the former paper constitutes one chapter. For this reason I have refrained (almost completely) from referring to Rules and representations in this reply to Slezak. For the same reason I have referred neither to criticisms of Rules and representations --- e.g. those published in the "Open Peer Commentary" in The Behavioral and Brain Sciences (1980, 3:15-42) --- nor to Chomsky's response to these criticisms. As a matter of fact, a number of the main points argued for in MB may receive a sharper formulation against the background of the interchanges between Chomsky and his critics (specifically Cummins and Harnish, Harman, John Morton, Rosenthal, and Stich) in the latter journal. For example, Chomsky's (p. 45) concession that Harman (p. 27) "... correctly points out an error in my [i.e., Chomsky's --- R.P.B.] formulation: there is a question of physical (or psychological) reality apart from truth in a certain domain ..." gives rise to the possibility of constructing an even stronger argument for MB's point that Chomskyan mentalistic theories are evidentially indeterminate.

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