THE LANGUAGE LOTTERY: PROMISES, PROMISES ...

Rudolf P. Botha

In *The Language Lottery* (henceforth "LL"), Lightfoot presents an account of how linguistics may be construed as a biological science that pursues the question "What is the genetic, internally prescribed structure of language?" (p. x). [1] He does this by trying to describe the work of Chomsky and his associates "accurately and faithfully", "revamping" their ideas only in the interests of the nonspecialist audience of linguists, biologists, ethologists, psychologists, and anthropologists that he wants to reach (p. xi). Given the richness of the content of this work and the arbitrariness of the traditional boundaries between linguistics, biology, psychology, etc., Lightfoot's attempt to make this work accessible to such a heterogeneous audience should be welcomed. And from the highly positive appraisals of LL by Marshall, Keil, and no one less than Chomsky himself on the back cover of the book, one may infer that Lightfoot has succeeded remarkably well in what he has set out to do.

LL disappoints, however. Lightfoot's audience does not get a detached account of what Chomskyan generative grammar entails. The book rather represents an exercise in academic salesmanship through which Lightfoot attempts to win at least the sympathy of his audience for this approach to linguistic inquiry. And in the process he unfortunately represents various important aspects of the Chomskyan approach rather less than "accurately". This is the point that I will argue below.

Before turning to a number of the inaccuracies in LL, let us briefly consider what, in general terms, Chomskyan generative grammar is about on Lightfoot's account. The central problem of this approach to linguistic inquiry is to give a characterization of how children master their native languages on the basis of experience (also called a "stimulus") that is claimed to be "deficient" in more than one respect (pp. x, 13, 21). To solve this problem, Chomskyan generative grammar assumes that

there is a specific genetic structure that guides language acquisition
(also called "growth") along a predetermined course under the trigger-
ing effect of the environment (or the "impoverished linguistic stimulus")
(p. 12). This genetic structure (also called "the genotypical principles
responsible for language acquisition") is characterized by means of a
theory of grammar (also called "Universal Grammar") (pp. 22, 27). The
speaker's acquired, tacit knowledge of the grammar of his or her lan-
guage --- i.e., the speaker's fully developed linguistic capacity
which constitutes part of his/her phenotype --- is characterized by
means of a particular grammar that conforms to the above-mentioned theory
of grammar (pp. 22, 27).

To determine the "genetic specifications of language structure" (or "the
genotypical principles responsible for language acquisition") the Chom-
skyan linguist uses arguments from the "deficiency" of the stimulus[2]
as his or her "basic line of reasoning", "as is usual amongst biologists"
(p. 15). Such arguments proceed from the observation that the stimulus
is not rich enough to determine certain properties of the mature sys-
tem. These properties, consequently, must stem "directly from some
genetic specification or may follow less directly, being epigenetic, due
to the mechanico-chemical constraints that arise in the genesis of the
embryo but are not actually encoded in the genes" (p. 12).

We now have the necessary background for considering five of the major
aspects of Chomskyan generative grammar that are inaccurately described
in LL.

1   *LL incorrectly characterizes "the basic line of reasoning" which
     Chomskyan linguists are supposed to use as arguments from the
     "deficiency" of the stimulus.*

Some Chomskyan linguists --- but see 2. below --- use a line of
reasoning that should rather be characterized as arguments from *ignorance*
of the stimulus. This point is clear from Lightfoot's (pp. 51ff.) at-
tempt to illustrate the so-called basic line of reasoning by "reworking"
Baker's (1978) analysis of the pronominal *one* in English, as it is used
in sentences such as (1).[3]

(1)   *You bought an old box and I bought a new one.*

The gist of Lightfoot's illustration is that the constraints of (2)
have to be attributed to "... the genetic endowment in order to explain
how children might attain what seems to be the right description of
English *one* by exposure to only limited [i.e., "deficient" --- R.P.B.]
data" (p. 56).

(2)   "a. $\bar{\bar{N}}$ consists of a Specifier and $\bar{N}$, with the order to be
specified for each particular grammar.

b. $\bar{N}$ consists obligatorily of N or $\bar{N}$ and an optional
Adj, $\bar{\bar{P}}$, or $\bar{S}$ with the order to be specified for each
particular grammar.

c.   Rules dealing with reference apply only to major cate-
gories (categories that may contain a phrase, that is,
more than a single word)."

Referring to (2) as "Hypothesis (11)", Lightfoot (p. 66) claims that

"Hypothesis (11) was based on a detailed examination of some
facts from one language, carefully distinguishing the subset
of facts to which children have access .... This is the
empirical base for the present investigation."

These are truly remarkable claims. A "careful" scrutiny of Lightfoot's
discussion for evidence that would bear out his claim about the factual
base of hypothesis (11) reveals that Lightfoot does not furnish a single
fact about the "facts" (or "primary linguistic data") to which specific
children have (had) access in acquiring the constraints in question.
What Lightfoot (p. 56) actually does, is to produce a number of untested
speculations about what these "facts" might be:

"One can *imagine* [my italics --- R.P.B.] evidence that *would*
[Lightfoot's italics --- R.P.B.] force the child to the
correct choice .... Such events *might* [my italics ---
R.P.B.] occur but they *would* [my italics --- R.P.B.] be
rare in the child's early experience ...." 4)

Clearly, Lightfoot illustrates a line of reasoning or a logic of inquiry
which is based not on factual knowledge of the deficiency of the stimulus
but rather on factual ignorance of the stimulus. In a later part (p. 90)

of his discussion, he has to admit that

> "Since dialects and styles can vary so much, very little
> is known about the precise trigger experiences of indivi-
> dual children, about which expressions a certain child
> heard and registered and with what frequency at what ages."

Lightfoot does not mention whether non-linguists, e.g. biologists,
also use arguments from the "deficiency" of the stimulus when they are
in fact ignorant about the stimulus.

2   *LL incorrectly suggests that arguments from the "deficiency"
     of the stimulus have been central to Chomsky's own recent lin-
     guistic analyses.*

On the one hand Lightfoot (p. 15) characterizes such arguments as repre-
senting "the basic line of reasoning" used by Chomskyan linguists. And
he (p. 99) assigns more weight to these arguments than to either arguments
in terms of "coverage of data" or arguments in terms of "simplicity and
elegance". On the other hand, Lightfoot more than once —— e.g., pp.
129, 212 —— refers his audience to Chomsky's *Lectures on Government
and Binding* (his "Chomsky 1981b") for a technical account of recent syn-
tactic and semantic work that instantiates the Chomskyan approach. Thus,
Lightfoot (p. 212) states that

> "... Chomsky 1981b is the best and most comprehensive discussion
> of more technical aspects, the substance of current theories
> of grammar."

Lightfoot's audience, consequently, may expect that in *Lectures on Govern-
ment and Binding* arguments from the "deficiency" of the stimulus will
represent the *basic* means by which specific general-linguistic analyses
are justified. This work, however, contains no arguments such as the one
on the basis of which Lightfoot attributes the principles of (2) to the
genotype. That is, contrary to what Lightfoot seems to suggest, Chomsky
does not motivate principles of the general linguistic theory (i.e., the
theory of grammar) in *Lectures on Government and Binding* by making speci-
fic unjustified claims about the primary data to which children are (not)

exposed in the acquisition of particular structures. Central to the
argumentation in *Lectures on Government and Binding*, are considerations
relating to predictive success, elimination of conceptual redundancy,
and depth/unifiedness of explanatory principles.

In the book under consideration, Chomsky (p. 13) does consider "the
objective of reducing the class of grammars compatible with primary lin-
guistic data" to be "a guiding principle in the study of generative
grammar". He (p. 13) does, moreover, consider "accounting for the
attainment of knowledge of grammar" to be "the fundamental empirical
problem to be faced". In motivating specific general-linguistic prin-
ciples in the book, however, Chomsky does not employ the form of argu-
ment used by Lightfoot to illustrate the "basic line of reasoning" of
generative grammar.

It is significant that for the purpose of illustrating the so-called
basic line of reasoning, Lightfoot does not use (in a suitably simpli-
fied form) one of the numerous analyses presented by Chomsky in *Lec-
tures on Government and Binding*, but rather chooses to "rework" an ana-
lysis by Baker. Equally significant is the fact that Lightfoot (p. 38)
has to admit, albeit implicitly, that one of the defining general prin-
ciples of Chomsky's linguistic theory --- that transformations have a
central role in the specification of syntactic structure -— has *not*
been motivated with the aid of arguments from the "deficiency" of the
stimulus. Rather, on Lightfoot's (p. 38) own account:

> "... criteria of simplicity and elegance, as are standard in
> scientific theorizing, suggested the need for another model,
> allowing a simpler account of the phenotype or the ability
> eventually attained by the native speaker. Chomsky argued
> that a grammar incorporating *transformational* rules [as
> opposed to a phrase structure grammar --- R.P.B.] would
> meet this requirement."

Lightfoot does not explain to his audience why arguments from the
"deficiency" of the stimulus should be considered to represent "the
basic line of reasoning" of generative grammar if one of the funda-
mental substantive principles of Chomsky's linguistic theory can be ---
and in fact has been --- motivated with reference to criteria such as
simplicity and elegance, criteria depicted by him as "standard in scien-

tific theorizing". And he does not explain why the principles of (2)
cannot be sufficiently motivated with reference to such criteria alone.

3   *LL inaccurately characterizes the view of psychological reality
     held by Chomsky since 1980.*

To substantiate this point, I will quote extensively below from LL and
from recent publications by Chomsky and Harman. These quotations should
also give readers a first-hand idea of the peculiar way in which the
content of LL has been compiled.

To begin with, consider what Lightfoot (p. 27) has to say about the
nature of the justification (not) required by the psychological (reality)
or mentalistic claims expressed by Chomskyan linguistic theories:

> "The theory of grammar is a hypothesis about the initial state
> of the mental organ, the innate capacity of the child, and a
> particular grammar conforming to this theory is a hypothesis
> about the final state, the grammar eventually attained. These
> are hypotheses about truth, about reality in the domain of
> psychology. This has led to much confusion in the literature
> and is sometimes misconstrued in terms of an independent con-
> cept of psychological reality. Some writers assume that one
> can discover various grammars that 'work' simply and elegantly
> and that one can then ask which of these grammars is psycholo-
> gically real. This presupposes the existence of psychological
> evidence, as distinct from linguistic evidence, which has some
> special status for establishing claims about psychological
> reality. There seems to be little virtue in deciding that
> data from, say, developmental stages in young children are
> inherently psychological and not linguistic, or vice versa.
> Rather we should seek simply 'the correct grammar' for a cer-
> tain person, presupposing a restrictive theory and using what-
> ever data can resolve the questions we want to answer, data
> from well-formedness judgments, ambiguity, paraphrase, language
> acquisition, historical change, pathology, and whatever else
> may be useful. When we achieve a good grammar, it will be as
> unnecessary to ask the further question of whether the account
> is psychologically real, as it would be for a physicist, having
> constructed a good theory about the internal structure of the
> sun or some other object that we cannot actually get inside,
> and having accounted for the manner in which radiation is
> emitted and other data that can be observed from the earth, to
> ask whether the theory corresponds to physical reality, to
> what is actually happening inside the star. The answer in both
> cases is that the theory proposed purports to be the best avail-
> able account. The researcher may seek to improve a theory,
> looking for new evidence and ideas, but cannot hope to achieve
> a new *type* of reality."

In this quotation, Lightfoot presents a view of psychological reality
at the basis of which lies what may be called "the nondistinctness
thesis":   in the psychological (or physical) domain truth and reality
are nondistinct.   This view of psychological reality was defended
by Chomsky in the seventies, for example, in the book and target article
which were published under the title "Rules and Representations" in
1980.

In reaction to criticisms levelled against the latter article by Harman
(1980),   Chomsky has given up the nondistinctness thesis stated above,
thereby abandoning the view of psychological reality attributed by
Lightfoot to generative grammarians.   Let us consider the essence of
Harman's (1980:21) criticisms of the nondistinctness thesis:

> "Chomsky claims that it is pointless to distinguish the ques-
> tion of psychological reality from that of truth, and he
> asserts that no similar distinction is made in the natural
> sciences.   But, given any theory we take to be true, we can
> always ask what aspects of the theory correspond to reality
> and what aspects are mere artifacts of our notation. Geography
> contains true statements locating mountains and rivers in
> terms of longitude and latitude without implying that the
> equator has the sort of physical reality the Mississippi River
> does.   Similarly, we can describe some part of the universe,
> given a choice of spatiotemporal coordinates, recognizing
> that the special role of that choice of coordinates in our
> description is an artifact of our notation.   And we might pre-
> sent a theory in axiomatic form without assigning any physical
> significance to the distinction between axioms and theorems.
>
> Sometimes we are not sure about the physical reality of some
> aspect of a theory, even given strong evidence for the truth
> of the theory.   A different sort of evidence may be needed.
> The postulation of quarks gives a structure to the prolifera-
> tion of subatomic particles, but physicists demand a different
> sort of evidence in order to establish the physical reality of
> quarks.
>
> Chomsky implicitly recognizes the point as it applies to lin-
> guistics when he acknowledges that one linguistic theory may
> be a 'notational variant' of another.   Aspects of a true
> theory not shared by its notational variants are not taken to
> have psychological reality.   The 'linguistic evidence' for a
> given linguistic theory is like the evidence that led to
> quark theory --- namely that the theory brings order to a
> given domain.   That by itself may not indicate what aspects of
> the theory correspond to reality and what aspects are arti-
> facts of notation.   We might wonder, for example, whether the
> grammatical structures of sentences have psychological reality
> or are mere artifacts of our notation, so that a notational
> variant of our theory could assign different structures to sen-
> tences."

Chomsky (1980b:45) unambiguously concedes the correctness of Harman's criticisms:

"The points that Harman makes are well taken. ... With regard to psychological reality, my main point is that no new problems of principle arise in the study of language that are not familiar in the 'hard' sciences, and that evidence does not come in two epistemological categories: 'linguistic evidence' bearing on 'good theories,' and 'psychological evidence' bearing on 'psychological reality'. Harman and I agree, I believe, on these points. As for the first, as I noted, there are serious questions about what is meant when we take a theory to be true: 'what is the status of its theoretical entities, its principles, its idealizations,' and so on. Harman points out some of these questions, quite appropriately - though, I think, as his final example shows, it is misleading to say that 'linguistic evidence' merely shows that 'the theory brings order to a given domain' in any sense that does not hold as well for a theory of click experiments and the like. He is also right to emphasize that we may ask about the physical reality of elements of a theory that we take to be true, and that psychological reality is on a par with physical reality in this respect. In this connection, he correctly points out an error in my formulation: there is a question of physical (or psychological) reality apart from truth in a certain domain, as Harman explains."

What is more, Chomsky (1980b:45-46) goes even further by pointing out a second state of affairs that illustrates the untenability of the non-distinctness thesis:

"There are interesting examples that go beyond notational variants in a narrow sense. Thus, suppose we assume the trace theory of movement rules (cf. Chomsky 1975; 1977). Consider two theories: (1) generate base structures, which are mapped to abstract S-structures including trace by transformations, with S-structures mapped to phonetic representations by the rules R_1 and to "logical form" representations (LF) by the rules R_2; (2) base-generate S-structures directly, mapping them to phonetic representation by R_1 and to LF by rules R_2 and R_3, where R_3 have the properties of the transformational movement rules (properties distinct from R_2, I believe). These two theories are not notational variants in a narrow sense, but it is not entirely clear whether they have different empirical content within the domain of "linguistic evidence," and it might be argued that on such evidence one should not attempt to choose between these theories but only to aim at a more abstract theory of which these are two specific realizations (for discussion, see Chomsky 1977, chapter 4; 1980, chapter 4; Koster 1978). Real and interesting questions of this sort arise when theories are given a fairly precise form, and Harman's comments are applicable to them."

This interchange between Chomsky and Harman appeared in a 1980 number of *The Behavioral and Brain Sciences*; Lightfoot's book was published in 1982 and contains a large number of references to works --- inclu- ding two by Chomsky --- that were published in 1981. LL, strangely, contains no reference to the Chomsky-Harman interchange, a fact which gives rise to questions about the manner in which the material presented in the book has been selected.[5]

Returning to the substance of the matter, from the quoted remarks by Chomsky (at least) three points are clear: (a) In Chomsky's view there is a question of physical or psychological reality apart from truth in a certain domain. (b) Given (a), it does make sense within Chomsky's approach to demand that claims about psychological reality be supported by additional evidence (not necessarily so-called psychological evi- dence) independent of the evidence that has been furnished for the truth of the theories in question. (c) Given (b), LL gives an erroneous account of the means required for the justification of the psychological claims expressed by Chomskyan linguistic theories.

4   *LL inaccurately construes the thrust of recent methodological criticisms of the logic of justification used by Chomskyan generative grammar.*

Independently of Harman, various other scholars --- e.g., Bresnan (1978), Botha (1980) --- have argued that the psychological claims expressed by Chomskyan linguistic theories cannot be empirical unless these claims are made responsible to evidence other than that adduced initially in support of these theories.[6] These arguments, in my view, embody some of the most serious methodological criticism of Chom- skyan generative grammar as an approach that makes ontological claims about an underlying psychological or biological reality. In LL, Light- foot has included a chapter, entitled "Reflections on methods" (pp. 86ff.) which deals with what he apparently considers to be less peripheral questions about the methodological bases of the Chomskyan approach. .This chapter does not, however, contain a single reference to the criti- cisms mentioned above. Instead, it appears as if Lightfoot (pp. 95ff.) wishes his audience to believe that the more important epistemological criticisms of the Chomskyan approach can be attributed to the critics'

adoption of unrealistically stringent Popperian standards of falsifica-
tion.   In arguing against these criticisms, Lightfoot attempts to slay
a paper dragon, in the process keeping his audience in ignorance of
the epistemological criticisms that have recently been levelled against
this approach.[7)]

5   *LL creates an inaccurate impression of the range of approaches
     that presently may be viewed as alternatives to Chomskyan gene-
     rative grammar.*

A standard mode of presenting a particular approach to an audience is
to compare this approach with alternatives in regard to content, method,
relative merit, etc.   The insightfulness of this mode of presentation
depends, among other things, on whether the alternatives selected for
such comparison represent real ones, whether the comparison is carried
out in a fair, detached manner, etc.   For the purpose of explicating
the content of Chomskyan generative grammar and of locating this approach
in the bigger domain of linguistics, Lightfoot too adopts this mode of
comparison in LL.   Thus, in the section entitled "Some Alternatives",
Lightfoot (p. 30) "... briefly mention[s] three other programs, to give
some sense of the range of alternatives and so to locate our own per-
spective somewhat".   These three alternative "programs" are American
taxonomic linguistics of the 1930s and 1940s (pp. 30-31), a second ap-
proach which "holds that the basic form of language is determined not
by genetic principles but by its communicative function" (pp. 31-32),
and a third approach which uses an evaluation metric, based on a notion
of simplicity, to account for language acquisition (pp. 32-33).   In a
later section, entitled "More on Alternative Approaches", Lightfoot
also considers Jespersen's historical grammar (pp. 204-205), Poutsma's
pedagogical grammar (pp. 204-205), and, to mention one further example,
Piaget's constructivism (pp. 205-206) as alternatives to the Chomskyan
approach.   I will not criticize Lightfoot's two selections on the basis
of the oddness of some of the "alternatives" that they include. Rather,
I would like to draw attention to a certain class of alternatives that
have been excluded from Lightfoot's two selections.

It was mentioned above that linguists such as Bresnan and Katz have
criticized the Chomskyan approach for what they take to be serious flaws

in its epistemological and ontological assumptions. Moreover, comple-
mentary to their criticisms, both Bresnan (e.g., 1978) and Katz (1981)
have developed the outlines of approaches which they present as alter-
natives to the Chomskyan approach. Bickerton (1981) represents a
further example of a linguist who has taken this course: he has not
only criticized Chomskyan generative grammar for what he considers to
be fundamental shortcomings, but he has outlined an alternative approach
to the study of the biological basis of language.

Bresnan's (1978:3) approach is based on the following assumptions:[8]

> "A realistic grammar must be not only psychologically real in
> this broad sense, but also realizable. That is, we should
> be able to define for it explicit realization mappings to
> psychological models of language use. These realizations
> should map distinct grammatical rules and units into distinct
> processing operations and informational units in such a way
> that different rule types of the grammar are associated with
> different processing functions. If distinct grammatical rules
> were not distinguished in a psychological model under some
> realization mapping, the grammatical distinctions would not
> be 'realized' in any form psychologically, and the grammar
> could not be said to represent the knowledge of the language
> user in any psychologically interesting sense."

Katz's (1981:76) alternative, by contrast, makes the following two
basic claims:

> "The first, and weaker one, is that linguistics is not a psy-
> chological science, that its theories are not about states
> of mind, mental events, or their neurological realizations,
> but about sentences and languages directly in the way that
> we ordinarily take linguistics to be about sentences and lan-
> guages. [Footnote 1 omitted --- R.P.B.]. The second, and
> stronger claim, is that sentences and language are abstract
> objects and thus linguistics [like mathematics --- R.P.B.]
> is about abstract objects."

Bickerton (1981), in turn, wishes to replace the "static, antiproces-
sual framework" (p. 104) adopted by Chomsky with a "dynamic generati-
vism" (p. 105) that claims

> "that there is an innate bioprogram that determines the form
> of human language" (p. 134).

To determine the content of this bioprogram, linguists adopting Bickerton's approach study processes of language development:

> "development in the individual [i.e., actual language acquisition --- R.P.B.], development of new languages [specifically the emergence of creoles --- R.P.B.], and original development of language" (p. 294). 9)

In LL, Lightfoot's audience will find not a single reference to Bresnan's, Katz's, or Bickerton's approach. This is both odd and unfortunate for reasons that I will give below.

The approaches of Bresnan, Katz, and Bickerton are mentioned here not because I consider them superior to Chomskyan generative grammar.[10] They are mentioned here because they exemplify a range of approaches which in a dual sense are *real* alternatives to Chomskyan generative grammar. On the one hand, though these approaches are based on a (argued) rejection of fundamental aspects of the Chomskyan approach, all of them are presented as forms of *generative* grammar. By failing to compare Chomskyan generative grammar to such other models of generative grammar, Lightfoot has denied his audience a clearer understanding of the notion "generative grammar" and of the features that make the Chomskyan approach a distinct model of generative grammar. On the other hand, as is clear from the literature, professional linguists have taken Bresnan's, Katz's, and Bickerton's approaches seriously as alternatives to Chomskyan generative grammar --- which is not to say that these linguists necessarily consider one or more of the former approaches to be superior to the latter one. By ignoring Bresnan's, Katz's, and Bickerton's approaches, Lightfoot provides his audience with inaccurate information about the actual location of Chomskyan generative grammar on the map of present-day theoretical linguistics. In sum, LL's discussion of alternatives to the Chomskyan approach is less than insightful because it ignores non-Chomskyan models of generative grammar that are taken seriously by many linguists.

To conclude: I find LL disappointing because it does not present the "accurate" description of the Chomskyan approach promised in the Preface. The kinds of inaccuracies considered above detracts from the potential usefulness of the book, especially for the nonspecialists in Lightfoot's audience.

NOTES

*I am grateful to C.-J. Bailey for useful comments on an early
version of this paper.

1. The full title of Lightfoot's book is: *The Language Lottery:
   Toward a Biology of Grammars.* It was published in 1982 by the
   MIT Press in Cambridge (Mass.) and London.

2. Chomsky (1980a: e.g., p. 34) refers to such arguments as "argu-
   ments from the poverty of the stimulus".

3. This "reworked" analysis has also been published in Hornstein and
   Lightfoot 1981:18ff.

4. For a similar formulation cf. also Hornstein and Lightfoot 1981:20.

5. Lightfoot (p. 35) refers his audience to Chomsky's book *Rules and
   Representations* for a discussion of "some alleged difficulties
   for the psychological orientation adopted here [i.e., in LL ---
   R.P.B.]". In my view, he should rather have referred this audience
   to Chomsky's target article "Rules and Representations": skirting
   peripheral issues and eliminating much of the redundancy that cha-
   racterizes the book, the article gives a much clearer exposition
   of the core of Chomsky's views. The article (in *The Behavioral and
   Brain Sciences*) is followed, moreover, by a section of "open peer
   commentary" in which more than twenty linguists, philosophers,
   psychologists, etc. comment critically on these views. And this
   commentary, in turn, is followed by a section in which Chomsky re-
   sponds, often in a most insightful way (cf., e.g., his response to
   Harman), to the major criticisms and/or misunderstandings of the
   commentators. Fodor's (1983:3ff.) recent discussion of the modu-
   larity aspect of Chomsky's mentalism should also be of some interest
   to Lightfoot's audience.

6. Proceeding from different premises and pursuing a different objec-

tive, Katz (1981:70-71) too has argued that "... it makes perfectly good sense to ask whether the best theory we can devise about a language is also a theory of some psychological reality". Katz presents, among other things, a detailed methodological critique of Chomsky's approach.

7.   Obviously, these criticisms may be incorrect but one cannot show this by pretending that they do not exist. Notice, incident- ally, that there was a time when Lightfoot (1979: 19) held a posi- tion on the epistemological status of "psychological reality claims" that appears to be related to the one defended by Harman, Bresnan, Botha, etc.:

> "... one *must* [my italics --- R.P.B.] claim that 'the correct grammar' is psychologically real, if one is to make the usual claims for explanatory adequacy ... That is, the theory of grammar *must* [my italics --- R.P.B.] be interpretable as making some predictions about a variety of domains (such as diachronic change, language acquisition or even neurological processes ...), if it is to achieve explanatory adequacy in the usual sense ..."

8.   For a recent exposition of fundamental assumptions of Bresnan's approach cf. Bresnan and Kaplan 1982.

9.   An outline of Bickerton's approach is to appear in a forthcoming target article in *The Behavioral and Brain Sciences*.

10.   Readers should not get the impression that I take Bresnan's, Katz's, and Bickerton's approaches to exhaust the list of alternatives which could have profitably been considered by Lightfoot. I have selected these three approaches simply as concrete examples of a particular range of alternatives not dealt with in LL.

BIBLIOGRAPHY

Baker, C.L.

1978   *Introduction to Generative-Transformational Syntax*.
                Englewood Cliffs, N.J.: Prentice Hall.

Bickerton, D.

1981   *Roots of Language*.   Ann Arbor: Karoma Publishers.

forth-   "The Language Bioprogram Hypothesis", *The Behavioral*
coming   *and Brain Sciences*.

Botha, R.P.

1980   "Methodological Bases of a Progressive Mentalism",
                Synthese 44:1-112.

Bresnan, J.

1978   "A Realistic Transformational Grammar".   In M. Halle,
                J. Bresnan, and G. Miller (eds.) 1978:1-59.

Bresnan, J. (ed.)

1982   *The Mental Representation of Grammatical Relations*.
                Cambridge, Mass.: MIT Press.

Bresnan, J., and R.M. Kamplan

1982   "Introduction: Grammars as Mental Representations of
                Language".   In Bresnan (ed.) 1982:xvii-lii.

Chomsky, N.

1977   *Essays on Form and Interpretation*.   New York: North
                Holland Elsevier.

1980a   *Rules and Representations*.   New York: Columbia Uni-
                versity Press.

1980b   "Rules and Representations", *The Behavioral and Brain
                Sciences* 3:1-15, 42-61.

1981   *Lectures on Government and Binding.*   Dordrecht:
              Foris.

Fodor, J.A.
  1983   *The Modularity of Mind.*   Cambridge, Mass.: MIT Press.

Halle, M., J. Bresnan, and G. Miller (eds.)
  1978   *Linguistic Theory and Psychological Reality.*   Cam-
              bridge, Mass.:   MIT Press.

Harman, G.
  1980   "Two Quibbles about Analyticity and Psychological Reality",
              The Behavioral and Brain Sciences 3:21-22.

Hornstein, N., and D. Lightfoot (eds.)
  1981   *Explanation in Linguistics.   The Logical Problem of
              Language Acquisition.*   London and New York: Longman.

Hornstein, N., and D. Lightfoot
  1981   "Introduction".   In Hornstein and Lightfoot (eds.) 1981:
              7-31.

Katz, J.J.
  1981   *Language and Other Abstract Objects.*   Oxford: Basil
              Blackwell.

Koster, J.
  1978   *Locality Principles in Syntax.*   Dordrecht: Foris.

Lightfoot, D.W.
  1979   *Principles of Diachronic Syntax.*   Cambridge: Cambridge
              University Press.

Lightfoot, D.
  1982   *The Language Lottery: Toward a Biology of Grammars.*
              Cambridge, Mass. and London: MIT Press.