

Biolinguistics

Exploring the Biology of Language

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CAMBRIDGE
UNIVERSITY PRESS

PUBLISHED BY THE PRESS SYNDICATE OF THE UNIVERSITY OF CAMBRIDGE
The Pitt Building, Trumpington Street, Cambridge, United Kingdom

CAMBRIDGE UNIVERSITY PRESS

The Edinburgh Building, Cambridge CB2 2RU, UK <http://www.cup.cam.ac.uk>
40 West 20th Street, New York, NY 10011-4211, USA <http://www.cup.org>
10 Stamford Road, Oakleigh, Melbourne 3166, Australia

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First published 2000

Printed in the United Kingdom at the University Press, Cambridge

Typeface Monotype Plantin 10/12pt. *System* QuarkXPress® [SE]

A catalogue record for this book is available from the British Library

Library of Congress cataloguing in publication data

Jenkins, Lyle.

Biolinguistics: exploring the biology of language / Lyle Jenkins.

p. cm.

ISBN 0 521 65233 2 (hardback)

1. Biolinguistics. 2. Grammar, comparative and general.

I. Title.

P132.J46 1999

401-dc21 98-45694 CIP

ISBN 0 521 65233 2 hardback

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1 The unification problem

Chomsky has commented as follows on the futility of attempting “the study of everything”:¹

In this connection, it is perhaps worthwhile to recall some further truisms; in rational inquiry, in the natural sciences or elsewhere, there is no such subject as “the study of everything.” Thus it is no part of physics to determine exactly how a particular body moves under the influence of every particle or force in the universe, with possible human intervention, and so on. This is not a topic. Rather, in rational inquiry we idealize to selected domains in such a way (we hope) as to permit us to discover crucial features of the world. (Chomsky, 1992:102)

The physicist David Ruelle, one of the founders of the field of nonlinear dynamics and chaos theory, writes in a similar vein: “Typically, if you are a physicist, you will not try to understand everything at the same time. Rather, you will look at different *pieces of reality* one by one. You will *idealize* a given piece of reality, and try to describe it by a mathematical theory” (Ruelle, 1991:11).

In biolinguistics, one such “piece of reality” that one might seek to describe is the relations and interpretations that hold between full phrases like *Jones* and “silent subjects,” as in this example described by Chomsky:

To illustrate in a slightly more subtle case, consider the sentence “Jones was too angry to run the meeting.” Who is understood to be running the meeting? There are two interpretations: The “silent subject” of “run” can be taken to be Jones, so that the meaning is that Jones wouldn’t run the meeting because of his anger; in this case we say that the silent subject is “controlled” by *Jones*. Or it can be taken to be unspecified in reference, so that the meaning is that (say) we couldn’t run the meeting because of Jones’s anger (compare “the crowd was too angry to run the meeting”). Suppose that we replace “the meeting” by a question phrase, so we now have: “which meeting was Jones too angry to run?” Now the ambiguity is resolved; Jones refused to run the meeting (compare “which meeting was the crowd too angry to run,” interpreted counter-intuitively to mean that the crowd was supposed to run the meeting, unlike “which meeting was the crowd too angry for us to run?” – which has no “silent subject” that requires interpretation). (Chomsky, 1994a:24)

¹ He notes that “the study of everything” he is rejecting here has nothing to do with “the theory of everything” (TOE) that the physicists are seeking (Chomsky, 1992:128).

For the physicist the “piece of reality” might be the boiling and freezing of water, as in the following example from Ruelle:

One puzzling natural phenomenon is the boiling of water, and the freezing of water is no less mysterious. If we take a liter of water and lower the temperature, it is not unreasonable that it should become more and more viscous. We may guess that at low enough temperature it will be so viscous, so stiff, as to appear quite solid. This guess about the solidification of water is wrong. As we cool water we see that at a certain temperature it changes to ice in a completely abrupt manner. Similarly, if we heat water it will boil at a certain temperature, i.e., it will undergo a discontinuous change from liquid to water vapor. The freezing and boiling of water are familiar examples of *phase transitions*. (Ruelle, 1991:122–23)

The first thing that has to be understood about both the example involving the English language data and the example involving the freezing and boiling of water is that there are problems here that need explanation at all! As Chomsky notes about the English example cited (and others he discusses along with these),

The reasons are well-understood in such cases as these. The crucial point is that all of this is known without experience and involves computational processes and principles that are quite inaccessible to consciousness, applying to a wide range of phenomena in typologically diverse languages. Even the relevant phenomena had escaped attention until recently, probably because the facts are known “intuitively,” as part of our nature, without experience. Serious inquiry begins when we are willing to be surprised by simple phenomena of nature, such as the fact that an apple falls from a tree, or a phrase means what it does. If we are satisfied with the “explanation” that things fall to their natural place or that our knowledge of form and meaning results from experience or perhaps natural selection, then we can be sure that the very phenomena will remain hidden from view, let alone any understanding of what lies behind them. (p. 25)

The same considerations apply for the freezing and boiling of water, as Ruelle emphasizes, “These phenomena are in fact so familiar that we may miss the fact that they are very strange indeed, and require an explanation. Perhaps one could say that a physicist is a person who does *not* consider it obvious that water should freeze or boil when its temperature is lowered or raised” (p. 123). In many cases the “piece of reality” we have chosen to investigate will turn out to be too complex to analyze as it stands:

So, here is a problem for theoretical physicists: prove that as you raise or lower the temperature of water you have phase transitions to water vapor or to ice. Now, that’s a tall order! We are far from having such a proof. In fact, there is not a single type of atom or molecule for which we can mathematically prove that it should crystallize at low temperature. These problems are just too hard for us. (p. 123–24)

In such cases it will be necessary to try to idealize it in various ways:

If you are a physicist, you won't find it unusual to be confronted with a problem much too difficult for you to solve . . . There are ways out, of course, but they require that your relation to reality be altered in one way or the other. Either you consider a mathematical problem analogous to the one you cannot handle, but easier, and forget about close contact with physical reality. Or you stick with physical reality but idealize it differently (often at the cost of forgetting about mathematical rigor or logical consistency). Both approaches have been used to try to understand phase transitions, and both approaches have been very fruitful. On the one hand it is possible to study systems "on a lattice" where the atoms instead of moving freely can be present only at some discrete sites. For such systems one has good mathematical proofs that certain phase transitions occur. Or one can inject new ideas into the idealization of reality, like Wilson's ideas of *scaling*, and obtain a rich harvest of new results. Still, the situation is not quite satisfactory. We should like a general conceptual understanding of why there are phase transitions, and this, for the moment, escapes us. (p. 124)

These examples suggest that, as we approach the study of the mind/brain, we should not let the intuitive familiarity of the linguistic data lull us into thinking that the problems will become any less hard: "Scientists know how hard it is to understand simple phenomena like the boiling or freezing of water, and they are not too astonished to find that many questions related to the human mind (or the functioning of the brain) are for the time being beyond our understanding" (Ruelle, 1991:11).

In a review of work on the visual system, David Hubel comments on the surprisingly long time it has taken to verify a hypothesis about brain cells that, although of great interest and importance, still makes up only a tiny corner of what Ruelle terms the "many questions related to . . . the functioning of the brain . . . [that are] beyond our understanding":

Thirty-five years ago Wiesel and I would have been incredulous had anyone suggested that only now would our scheme for explaining simple cells be vindicated or disproved. At this rate we may expect to have a verdict on a similar proposal we made for complex cells by 2031. (Hubel, 1996:197)

Apart from the unfeasibility of the "study of everything" Chomsky has also distinguished problems, which "appear to be within the reach of approaches and concepts that are moderately well understood," from mysteries, that "remain as obscure to us today as when they were originally formulated" (Chomsky, 1975b:137). Short recounts the experience of the molecular biologist Lubert Stryer:

Lubert Stryer (Stanford Medical School) told of a conversation in 1969 with Henri Peyre, then professor of French at Yale. Unimpressed by Stryer's account of how he intended to determine the molecular basis of vertebrate vision, Peyre remarked that the truly interesting question was the molecular basis of remorse. (Short, 1994:583)

However, “truly interesting” does not necessarily translate into “easily amenable to scientific investigation.” It is not obvious whether the study of a topic like remorse falls into the “problem” or “mystery” category. One can at least imagine approaches to the problem, like studying serial killers with controls to try to define a behavioral phenotype, and then look for polymorphisms (variations) or mutants.² But given the state of both molecular biology and psychology in 1969 (and probably now), Stryer was probably wiser to attack the unification problem for vertebrate vision rather than that for remorse.

Some may be disappointed to see the grand questions like, “what is the relationship between language and thought?” reformulated as a series of less romantic questions like, “what are the constraints on the distribution of pronouns, Case-marked noun phrases, etc., in English?” There have been similar misgivings in biology about general questions like: what is life?, as P. B. and J. S. Medawar have pointed out (Medawar and Medawar, 1978:7). And, they note, Keats denounced Newton “for destroying all the beauty of the rainbow by reducing it to its prismatic colours . . .” (p. 166). Although the question concerning the logical form of the reciprocal pronoun *each other* and “silent subjects” may seem less romantic than broader questions concerning language and thought at first glance, in the long run, we hope to be able to piece together a more satisfactory answer to the latter question by breaking the problem down into smaller, more tractable problems. Jacob has elegantly stated this as follows:

Science proceeds differently. It operates by detailed experimentation with nature and thus appears less ambitious, at least at first glance. It does not aim at reaching at once a complete and definitive explanation of the whole universe, its beginning, and its present form. Instead, it looks for partial and provisional answers about those phenomena that can be isolated and well defined. Actually, the beginning of modern science can be dated from the time when such general questions as, “How was the universe created? What is matter made of? What is the essence of life?” were replaced by such limited questions as “How does a stone fall? How does water flow in a tube? How does blood circulate in vessels?” This substitution had an amazing result. While asking general questions led to limited answers, asking limited questions turned out to provide more and more general answers. (Jacob, 1977:1161–62)

THE ROLE OF TECHNICAL TERMS IN UNIFICATION

Chomsky has devoted many essays to elucidating and teasing apart the uses of such terms and expressions as “language,” “the English language,”

² A polymorphism is the presence in a population of two or more relatively common alleles of a gene.

“know English,” “the word X refers to Y,” etc., as they occur in the literature of linguistics, philosophy of mind, and the cognitive sciences. When we embed a term like “language” into a theory, it becomes a technical term and takes on the meaning assigned to it within that theory. It need not have any more connection to the commonsense use of the word than the sub-atomic particle “quark” has to the “quark” of James Joyce. We could spell “language” backwards, but for convenience we retain the original word for the theory, just as words like “mass” and “energy” are retained in physics. What linguists do is to define the term “I-language” to denote the biological object under study, but to continue to use the term “language” (instead of “I-language”) where the context leaves no possibility of confusion. Similarly, in exactly the same way, we use the term “biolinguistics” for roughly the study of the five questions posed in the Introduction, but where the context is clear, we often use the shorter form “linguistics.”

This kind of analysis has been a crucial step in the unification process in every natural science, and is always carried out, either implicitly or explicitly. For example, Max Planck dedicates a long essay, “The Unity of the Physical Universe,” to the topic of unification and technical terms in his area of expertise, thermodynamics. He is concerned with exorcising the anthropomorphic element in the usage of the pivotal term *entropy*. Planck notes that many physical concepts and whole branches of physics arose out of human needs and from the sense perceptions. For example, the concept of *force* “without doubt referred to human force, corresponding to the use of men or beasts to work the first and oldest machines – the lever, the pulley, and the screw.” Heat was characterized by the sense of warmth, etc. Energy involved the idea of “useful work” and attempts to build a “perpetual motion machine.” Progress in physics was finally made by “emancipating” physics from its anthropomorphous nature: “we may say briefly that the feature of the whole development of theoretical physics, up to the present, is the unification of its systems which has been obtained by a certain elimination of the anthropomorphous elements, particularly the specific sense-perceptions” (Planck, 1993:4).

Much of the rest of the essay involves the “emancipation” of the Second Law of Thermodynamics from such notions as “human ability,” “human agency,” “ability to carry out certain experiments,” “limits to human knowledge,” etc. He considers a number of alternative definitions of this law in terms of irreversible processes and, finally, entropy. He argues that the statistical formulation of Boltzmann is the best one – “anthropomorphism is eliminated.” But we pay a price for this “step towards unification”; viz., in using statistical methods, we are “denied the complete answer to all questions relating to details of operations in physics”; e.g., about individual elements. Once again, physics isn’t the “study of

everything,” in Chomsky’s terms. And just as Newton destroyed Keats’ rainbow, Planck admits “that the picture of the future appears colourless and drab when compared with the glorious colouring of the original picture, tinted with the manifold needs of human life, and to which all the senses contributed their part.” But what we gain in turn is the “*unity* of the picture.”

Planck goes on to make the following prescient observations:

As fundamental in mechanics we need principally the conceptions of space, time, and motion, and it may be denoted by matter or condition. The same fundamentals are equally necessary to electro-dynamics. A slightly more generalized view of mechanics might thus allow it to include electro-dynamics, and, in fact, there are many indications that these two divisions, which are already encroaching upon one another, will be joined in one single general scheme of dynamics.

In a short time Einstein was to embark on this project, by scrutinizing the classical (technical and commonsense) notions of space and time in much the same way that the founders of thermodynamics had scrutinized the technical and commonsense notions of energy and work.³

“REALITY” IN LINGUISTICS AND NATURAL SCIENCE

Weinberg provides a useful characterization of “real” for natural science:⁴ “Wave functions are real for the same reason that quarks and symmetries are – because it is useful to include them in our theories” (Weinberg, 1992:79). This should be an adequate rule-of-thumb for any study of the natural world, including biolinguistics, the study of the biological object (I-)language, and we assume it here. But often linguists are held to a higher standard by philosophers, cognitive scientists, and even by many other linguists. After having presented evidence and arguments for some linguistic concept or principle to show that it is “useful to include them in our theories,” linguists are then asked to jump through some more philosophical hoops, to show that the concept or principle has the alleged property of “psychological reality,” “neurological reality,” or even “mental reality.” We discuss an example of this in a review of Chomsky’s *Rules and Representations* by Colin McGinn (McGinn, 1981).

McGinn lists several “Chomskian theses” (p. 288), of which the following two are considered here:

³ The date of Planck’s essay is not provided, but it appears to predate Einstein’s work on special relativity.

⁴ Weinberg is actually speaking through the figure of “Scrooge” in a discussion with “Tiny Tim” about the meaning of quantum mechanics, including the interpretation of the EPR paradox (see below). Weinberg notes: “I have some sympathy with both sides in this debate, though rather more with the realist Scrooge than with the positivist Tiny Tim.”

- (i) a grammar characterizes an internal structure of representations and computational principles
- (ii) this structure belongs to the *mind* of the speaker

McGinn accepts thesis (i), but, after presenting some discussion, concludes about thesis (ii): “So I do not think that Chomsky has yet demonstrated his right to the claim that generative grammars have properly *mental* reality” (p. 290).

However, Chomsky has not claimed that generative grammars have any “mental reality” at all, above and beyond the reality that any scientific model of the universe has:

The grammar of a language . . . has a claim to that “higher degree of reality” that the physicist ascribes to his mathematical models of the universe. At an appropriate level of abstraction, we hope to find deep explanatory principles underlying the generation of sentences by grammars. The discovery of such principles, and that alone, will justify the idealizations adopted and indicate that we have captured an important element of the real structure of the organism. (Chomsky, 1980c:223)

Chomsky explicitly states that he uses “mental” in much the same way as “chemical,” “optical,” or “electrical,” are used:

Take the term “mind,” or as a preliminary, “mental.” Consider how we use such terms as “chemical,” “optical,” or “electrical.” Certain phenomena, events, processes, and states are called “chemical” (etc.), but no metaphysical divide is suggested by that usage. These are just various aspects of the world that we select as a focus of attention for the purposes of inquiry and exposition. I will understand the term “mental” in much the same way, with something like its traditional coverage, but without metaphysical import and with no suggestion that it would make any sense to try to identify the true criterion or mark of the mental. (Chomsky, 1994b:181)

“Mind” is meant to be understood in a similar fashion: “By ‘mind,’ I mean the mental aspects of the world, with no concern for defining the notion more closely and no expectation that we will find some interesting kind of unity or boundaries, any more than elsewhere; no one cares to sharpen the boundaries of ‘the chemical.’”

But if “mental” and “mind” are understood in this way, then the thesis McGinn is attributing to Chomsky comes down to something like: I-language, or more generally, the language faculty, is a component of mind/brain. Stated this generally, there is no point in elevating it to a “Chomskian thesis.” It could just as well be called a “Cartesian thesis,” for example. In fact, the notion that the language faculty is a component of the mind/brain is not a particularly controversial thesis and is tacitly assumed in much work on language. When Damasio et al. did PET (positron emission tomography) scans on subjects to try to determine where

lexical categories or semantic concepts related to persons, animals, tools, etc., were stored, they didn't waste brain scans on the kidney or the big toe, to rule out the possibility of the language faculty being there, nor did they justify leaving out these controls, since, rightly or wrongly, their audience assumes this on the basis of a lot of other evidence (Damasio et al., 1996); for commentary, see Caramazza, 1996.

So McGinn presumably has a much different kind of thesis in mind – and he even uses italics with the word *mental*. But now we are dealing with a technical term, *mental*, and we are stuck until McGinn tells us what it means. Just as we would be stuck in physics if we weren't told what technical terms like “work,” “energy,” or “entropy” meant. Note that we aren't helped out by the fact that McGinn's *mental* is spelled the same way that Chomsky spells “mental.” It could just as well be spelled MENTAL or mEntAl, or with totally different symbols, to emphasize that it is a technical term. In order to evaluate the thesis, two things are needed: (1) some characterization of “*mental*” and “*mental* reality” and (2) we need to be told why we should care about it; i.e., how it is useful for or, alternatively, why it causes problems for, biolinguistic theory.

Later on we will show that Einstein, in arguing for the idea of “objective reality,” carried out exactly the two steps just discussed. He (and his colleagues, Podolsky and Rosen) provided the following:

- (1) a characterization of “objective reality”
- (2) a problem that they thought it caused for quantum mechanics

The characterization given was in terms of assigning exact values to properties, like position and momentum, of particles (like electrons). The problem proposed was in the form of a thought experiment that resulted in a purported dilemma for quantum mechanics, the so-called EPR (Einstein–Podolsky–Rosen) paradox. Einstein felt that this thought experiment showed that the theory of quantum mechanics was “incomplete.” Later on, after Einstein's death, when the EPR experiment could be performed, it was shown that the reasonable definition of “objective reality” given by EPR was ruled out by the experiment, the results of which were exactly predicted by quantum mechanics.

Einstein and his colleagues worked hard to characterize the technical notion of “objective reality” and to show why they thought it led to an impasse for the current theory of physics. We are nowhere near this situation for the analogous notion of “mental reality.” First of all we don't even know the meaning of the technical term *mental*, as it is intended by McGinn. Until we know the meaning of this term, we can't evaluate the thesis that language has “mental reality.” Nor can we know whether it helps (or counts against) biolinguistic theory. Chomsky notes that technical terms have both of the following properties – you can't have intuitions

about them and you can't do thought experiments with them – you need to be told what they mean.⁵ It is up to McGinn, or whoever else wants to, to defend the thesis that language has this mysterious property. But the work has to be done by somebody, just as Einstein and colleagues did the work in physics to argue for “objective reality.”⁶ In any case, this (non-)thesis is not Chomsky's to defend.

McGinn gives us a few hints about what he has in mind. He holds that thesis (ii); viz., that (linguistic) “structure belongs to the mind of the speaker” raises “some difficult issues, to which Chomsky does not seem sufficiently sensitive.” One problem, he claims, is that thesis (ii) is not entailed by thesis (i), the thesis that grammar is an internal structure, or else “far too much would be mental – computers, retinae, and digestive systems.” McGinn concludes: “What is wanted is some criterion for when a system of representation and computation is genuinely part of the mind” (1981:290).

But recall that thesis (ii) is not Chomsky's thesis to begin with. So if anyone tries to derive thesis (ii) from thesis (i) and ends up not being able to distinguish linguistics from the digestive system, it is their problem to come up with a criterion that can. As for Chomsky, as noted earlier, he makes “no suggestion that it would make any sense to try to identify the true criterion or mark of the mental.” Moreover, McGinn goes on to claim that a “philosophical (or indeed common-sense) account of the boundaries of the mind needs to respect distinctions insignificant to the cognitive psychologist” (p. 290). As we have already noted, we don't have

⁵ That is to say, you can't do a thought experiment to determine what the term means. That must be told to us. In the case of the EPR paradox, of course, you *can* do a thought experiment because EPR have told us what “objective reality” is.

⁶ As the physicist Wolfgang Pauli might have said of the thesis on “mental reality,” “it is not even wrong” (Zee, 1986:35). In this connection, it is interesting to note Pauli's opinions on Einstein's proposal about “objective reality” (see further discussion below):

As O. Stern said recently, one should no more rack one's brain about the problem of whether something one cannot know anything about exists all the same, than about the ancient question of how many angels are able to sit on the point of a needle. But it seems to me that Einstein's questions are ultimately always of this kind. (from the *Born–Einstein Letters*, cited in Mermin, 1990:81)

It appears that Pauli is saying of objective reality here too that “it is not even wrong.” However, this was before John Bell presented his analysis of the EPR paradox, in which he showed it was possible to decide experimentally the question of Einstein's “objective reality” one way or the other:

Bell thereby demonstrated that, Pauli to the contrary notwithstanding, there were circumstances under which one *could* settle the question of whether “something one cannot know anything about exists all the same,” and that if quantum mechanics was quantitatively correct in its predictions, the answer was, contrary to Einstein's conviction, that it does not. (Mermin, 1990:124)

a philosophical account of the boundaries of the mind, since we haven't been told the meaning of the technical term *mind*, and hence have no way to know what the boundaries of it is. And even if we are told, there is no more reason for the meaning of the technical term *mind* to reflect commonsense intuitions than there is for thermodynamic theory to make the technical term *entropy* reflect commonsense intuitions about work and energy, as Planck pointed out.

Furthermore, the question immediately arises of why one would need a "philosophical account of the boundaries of the mind" or a "criterion for when X is genuinely part of the mind" any more than one needs a philosophical account of the boundaries of the "mechanical" or the "optical." Especially given the fact that one of the great successes of unification in physics took place by ignoring the boundaries between the "mechanical" and the "optical." Sir William Rowan Hamilton once puzzled over a curious asymmetry between the mechanical and the optical:

If one compares Newtonian dynamics with classical optics, it appears that the dynamics describes only half a picture as compared to the optics; whereas the latter appeared in two different forms, the Newtonian corpuscular form and the Huygens wave form, the latter [*sic*] had no wave aspect at all. To one like Hamilton, who passionately believed in the unity of nature, this was a flaw in Newtonian physics that had to be eliminated, and he took the first step in this direction by extending the action concept to include the propagation of light. (Motz and Weaver, 1989:112)

That is, Hamilton noticed a curious gap:

	particles	waves
optics	geometrical optics	physical optics
mechanics	Newtonian theory	??
dynamics		

To unite mechanics with optics Hamilton took Fermat's principle of least time from optics and generalized it to a principle of least action. Hamilton's rationale is that he "passionately believed in the unity of nature." But he could have also dismissed the entire matter by claiming that the strange gap was the result of some unknown "criterion" that established the "boundary" between "mechanical reality" and "optical reality." Instead he chose to ignore any putative boundary between the mechanical and the optical and achieved an important unification of two domains thought to be separate from one another. Looking back at the table above, we notice the implication that one might expect physical entities in the mechanical domain to exhibit both particle-like and wave-like

behavior. This was subsequently theoretically predicted to be the case by Louis de Broglie in 1923 and demonstrated by Davisson and Germer, who discovered electron diffraction. It was one of the great insights of Erwin Schrödinger to then simply take over the ready-made formalisms of Hamilton just discussed to formulate his famous “wave” equation in quantum mechanics, one case of which is given here (in the “Hamiltonian” form) (Lines, 1994:268):

$$H\phi = E\phi$$

Nor are such unification problems of solely historical interest. In a current survey of string theory, the physicist Edward Witten writes of the “bad news” for string theory:

Perhaps what is most glaringly unsatisfactory is this: crudely speaking there is wave-particle duality in physics, but in reality everything comes from the description by waves, which are then quantized to give particles. Thus a massless classical particle follows a lightlike geodesic (a sort of shortest path in curved spacetime), while the wave description of such particles involves the Einstein, Maxwell or Yang–Mills equations, which are certainly much closer to the fundamental concepts of physics. Unfortunately, in string theory so far, one has generalized only the less fundamental point of view. (Witten, 1996:26)

Witten depicts the situation in the following (modified) diagram:

The “magic square” of string theory (Witten, 1996:28)

	particles	waves
ordinary physics	classical particle (world-line)	$\int \sqrt{g} R d^4x$ (Einstein–Hilbert action)
string theory	string (world-tube)	??

In other words, just as Hamilton set out to generalize the corpuscular aspect of Newtonian particles to a wave description, one of the tasks that Witten sees for string theory is to generalize to the more “fundamental point of view” of waves (p. 28). We have another typical unification problem, with no talk of a “criterion” for the physical or the “philosophical boundaries” of the world of strings.

Concluding, we also see no reason for biolinguistics to postulate a “psychological,” “neurological,” or “mental” reality, or find a criterion for, or delineate the boundaries of these “realities.” There exists a voluminous philosophical literature (Quine, Putnam, Davidson, etc.), much of it critical towards the biolinguistic approach to language, since it does not admit the existence of these alleged “realities.” Chomsky has reviewed much of this literature, concluding that it represents a deep-seated

“methodological dualism”: “the view that we must abandon scientific rationality when we study humans ‘above the neck’ (metaphorically speaking), becoming mystics in this unique domain, imposing arbitrary stipulations and a priori demands of a sort that would never be contemplated in the sciences, or in other ways departing from normal canons of inquiry” (Chomsky, 1994b:182).

EVIDENCE IN BIOLINGUISTICS

Since unification often involves linking “seemingly diverse objects” (Davis and Hersh, 1981:198) (and below), any and all evidence is a candidate for a theory of bilingualistics. Whether this evidence for our theory is compelling or noncompelling will depend on the depth of explanation that does or does not result. Chomsky has been insistent on these points:

Approaching the topic as in the sciences, we will look for all sorts of evidence. For example, evidence from Japanese will be used (and commonly is) for the study of English; quite rationally, on the well-supported empirical assumption that the languages are modifications of the same initial state. Similarly, evidence can be found from studies of language acquisition and perception, aphasia, sign language, electrical activity of the brain, and who knows what else. (Chomsky, 1994b:205)

Let’s give another example. When linguists study English phonetics,⁷ let’s say the distinction between the *r* and *l* sounds, they will also draw on evidence from other languages such as Japanese. It has been noted that although adult Japanese speakers do not recognize the distinction between *r* and *l*, it can be shown that Japanese infants make the relevant distinction before a certain age. The idea here is that this distinction is available in universal phonetics in all the languages of the world, but that if the distinction is not utilized in the sound system of a particular language, as in Japanese, the distinction atrophies during the course of language development. Again, this is by no means an outlandish idea, if a system of universal phonetics is given by the genetically determined initial state. If we are interested in studying that initial state, that “grows” into English, we will use data obtained from other languages, including Japanese.

Turning to syntax,⁸ Bobaljik reviews evidence that certain syntactic distinctions observed in English adult speakers are made by children in languages where the distinction is not found in the speech of adult speak-

⁷ Phonetics is part of the study of the sound aspect of language, closely related to the articulatory and perceptual properties of speech.

⁸ Syntax is the study of sentence structure, the organization of words and phrases into sentences.

ers (Swedish) (Bobaljik, 1995:330). Thus English speakers make syntactic distinctions between main verbs (*eat*, etc.) on the one hand, and modals (*will*, *can*, etc.) as well as auxiliaries like *have* and *be* on the other; e.g., *Can John come?* versus **Came John?* (Chomsky, 1957; Jenkins, 1972).

Bobaljik cites work by Håkansson suggesting that Swedish children make the distinction during a certain stage of language learning, after which it is lost (Håkansson, 1989). Thus we have a syntactic parallel to the phonetic *r-l* example just discussed. Moreover, Bobaljik reviews work by Hackl on German-speaking aphasics, in which the subjects exhibit a distinction between main verbs and modal verbs/auxiliaries, even though the distinction is lacking in the speech of adult German speakers (Hackl, 1995). What is being suggested is that this syntactic distinction is part of UG and can be indirectly observed in languages where the distinction is not overtly made in adult speech (and hence is not available to the language learner). Again, assuming we are interested in the initial state of the language faculty, it is quite legitimate to draw on evidence from Swedish, from language acquisition, from aphasia, or, as Chomsky says, from “who knows what else.”

However, the literature of philosophy of mind and cognitive science is full of pronouncements about the privileged status of one kind of evidence over another. For example, paralleling the “psychological reality,” that we discussed and rejected earlier, we learn that there is “psychological evidence” apart from, and superior to, purely “linguistic evidence.” McGinn makes claims in this vein: “What seems to me true is that grammar can legitimately be taken as a psychological theory of competence, but it requires empirical underpinning from considerations external to simply characterizing (however, illuminatingly) grammaticality for the language in question” (McGinn, 1981:289).

The giveaway that we are dealing with stipulation here is the phrase “however, illuminatingly.” It wouldn’t be sufficient to McGinn if our grammatical theory provided all kinds of interesting grammatical explanations of the behavior of children learning language, of aphasics, of the deaf, of people with learning disorders, of language change and typology, etc. That would only be illuminating “linguistic evidence,” but not “psychological evidence.” We haven’t gotten our evidence by “psychological methods,” whatever those might be – say, by strapping electrodes to somebody’s head. Nor are we told whether this other kind of evidence has to be illuminating, good, bad, or indifferent.

Biolinguistics, like Mendelian genetics, posits abstract epi(genetic) properties of the internal mechanisms of organisms. Mendelian genetics says that there are abstract “factors” and abstract principles like

Segregation and Independent Assortment that explain observed facts of inheritance. Biolinguistics says that there are abstract linguistic principles encoded in the genome that guide the growth of I-languages, permitting some parametric variation during epigenesis. Again, observed facts about knowledge, acquisition, and use of language are explained along with some facts in areas like language change and typology. It makes no more sense to say that (bio)linguistics is not “psychologically real” than it does to say that Mendelian genetics is not “physiologically real.”

McGinn notes that one *could* choose not to study psychology (hence biology) at all: “it seems that a linguist could set himself the goal of devising a grammar capable of generating all and only the grammatical strings of some language and not commit himself on the matter of psychology” (McGinn, 1981:289). Similarly, a lepidopterist could set himself the goal of classifying all the different patterns of spots on butterfly wings and “not commit himself on the matter of biology.” Or the linguist could collect short stories that contain only words without the letter R. In fact, the description just given by McGinn might even be taken to be reminiscent of the study of “Platonist linguistics” (Chomsky, 1986).

Let us take another case from the cognitive sciences. In an interview with the *Journal of Cognitive Neuroscience*, the cognitive psychologist Steven Pinker is asked (by Michael Gazzaniga):

MG: ...Before going into details, how does your MIT view differ from other MIT views?

SP: Obviously, some of the key ideas in the book come from Chomsky – that there is an innate neural system dedicated to language; that his system uses a discrete combinatorial code, or grammar, to map between sound and meaning; that this code manipulates data structures that are dedicated to language and not reducible to perception, articulation, or concepts. But there are also some differences in style and substance. Chomsky’s arguments for the innateness of language are based on technical analyses of word and sentence structure, together with some perfunctory remarks on universality and acquisition. I think converging evidence is crucial, and try to summarize the facts on children’s development, cross-linguistic surveys, genetic language disorders, and so on.

In that sense the book is more in the tradition of George Miller and Eric Lenneberg than Chomsky... (Pinker, 1997a; originally printed in Pinker, 1994b:92)

Here Pinker introduces the idea of “converging evidence,” things like “children’s development, cross-linguistic surveys, genetic language disorders, and so on.” This is contrasted with Chomsky’s “technical analyses of word and sentence structure, together with some perfunctory remarks on universality and acquisition.” Although Chomsky has developed his ideas

on “universality and acquisition” in a voluminous output over the last forty years, for Pinker these amount to “perfunctory remarks,” since they don’t meet his criterion of “converging evidence,” a very odd notion that we will now examine more closely. From Pinker’s viewpoint, whatever the latter evidence is, it is not “converging evidence” and puts Chomsky in a different “tradition” from Pinker, Miller, and Lenneberg.

The issues under discussion have been framed in the wrong way here. It is totally irrelevant in the larger biolinguistics picture, whether Professor X’s views at MIT do or don’t overlap with Professor W, Y, and Z’s views – what ultimately matters is what the nature of the biological object language is. And as for deciding what evidence we use to discover this nature there is only one tradition worth belonging to – the tradition of rational scientific inquiry. Chomsky calls this “methodological naturalism,” which “investigates mental aspects of the world as we do any others, seeking to construct intelligible explanatory theories, with the hope of eventual integration with the ‘core’ natural sciences” (Chomsky, 1994b:182). This is to be contrasted with the “methodological dualism,” discussed earlier, “the view that we must abandon scientific rationality when we study humans ‘above the neck’ (metaphorically speaking).”

Skinner’s and Quine’s views, for example, would fall into the tradition of methodological dualism, as would the view that insists that evidence be “psychologically real,” for the reasons given earlier. The views expressed above by Pinker on “converging evidence” place him in this tradition as well (see also chapter 5). For Pinker, the reams of evidence put forth “on universality and acquisition” over the past forty years, most recently as the principles-and-parameters theory are not “converging evidence,” but are, at best, “perfunctory remarks.” However, what is “converging evidence” from one point of view may be “diverging evidence” from another point of view – evidence does not come labeled as “convergent” or “divergent.” What could account for the peculiar idea that the study of principles and parameters in UG is not “converging evidence” for Pinker, as “children’s development?” is.

Part of the explanation might be the implicit assumption that the generative grammarian is not doing experiments, whereas the developmental psychologist is. For example, Chomsky proposed that one might attribute to an inborn UG the knowledge that adult English speakers have that the sentence “is the man who is hungry tall,” is acceptable, while the sentence “*is the man who hungry is tall” is not.⁹ Now compare Pinker’s discussion of this:

⁹ These example sentences and Chomsky’s account of them (one case of “the argument from poverty of the stimulus”) are discussed in more detail in chapter 3.

Chomsky's claim was tested in an experiment with three-, four-, and five-year-olds at a daycare center by the psycholinguists Stephen Crain and Mineharu Nakayama. One of the experimenters controlled a doll of Jabba the Hutt, of Star Wars fame. The other coaxed the child to ask a set of questions, by saying, for example, "Ask Jabba if the boy is unhappy is watching Mickey Mouse." Jabba would inspect a picture and answer yes or no, but it was really the child who was being tested, not Jabba. The children cheerfully provided the appropriate questions, and, as Chomsky would have predicted, not a single one of them came up with an ungrammatical string like *Is the boy who unhappy is watching Mickey Mouse?* (Pinker, 1994b:42)

The tacit assumption here by Pinker seems to be that Chomsky is making an unsupported "claim," not verified by experiment whereas the psycholinguists have taken this claim and verified it by "experiment."

But generative grammarians had already performed years of experiments to provide (tentative) verification; viz., by introspection and querying other speakers on this kind of grammatical construction and countless others, in English and across many different languages. As Chomsky notes in an interview with Kim-Renaud:

KIM-RENAUD: *What kind of evidence is used? You don't really do actual experiments?*

CHOMSKY: You actually do. For example, if I ask you as a speaker of English whether "Who do you believe John's claim that Bill saw?" is a sentence, that's an experiment. Now it happens that most of the relevant evidence for the study of psychological reality – that is, truth – of linguistic theories now comes from experiments of this kind. It would be very nice to have experimental evidence of other types, from a laboratory of neurophysiology or whatever. We don't have much of it. If it would ever come along, it'd be delightful. (Chomsky, 1988b:268–69)

So why does Pinker think that Chomsky is merely making a claim, whereas the psycholinguists are performing a (psycholinguistic) experiment? Would it satisfy Pinker if Chomsky were waving a Jabba the Hutt puppet in front of the linguistics students?:

One of the most fascinating syndromes recently came to light when the parents of a retarded girl with chatterbox syndrome in San Diego read an article about Chomsky's theories in a popular science magazine and called him at MIT, suggesting that their daughter might be of interest to him. Chomsky is a paper-and-pencil theoretician who wouldn't know Jabba the Hutt from the Cookie Monster, so he suggested that the parents bring their child to the laboratory of the psycholinguist Ursula Bellugi in La Jolla. (Pinker, 1994b:52)

The "paper-and-pencil theoretician" is performing experiments on language, as much as Pinker is with Jabba the Hutt.¹⁰

Another part of the explanation might be that Pinker can't bring

¹⁰ These kinds of experiments are well known in other areas of the cognitive sciences, where vision research has a distinguished tradition (psychophysics) which includes "paper-and-pencil theoreticians" working with visual illusions.

himself to accept arguments based on one kind of evidence, “abstract computations” (in the case of language, syntactic computations): “Though I happen to agree with many of his [Chomsky’s] arguments, I think that a conclusion about the mind is convincing only if many kinds of evidence converge on it” (Pinker, 1994b:24).

Suppose, contrary to fact, that no converging evidence at all of the kind Pinker detailed in his book – on sign language, aphasia, language disorders, etc. – had turned up yet. Would one be justified in accepting Chomsky’s arguments that the “basic design of language is innate,” to use Pinker’s words? Not according to Pinker, since “a conclusion about the mind is convincing only if many kinds of evidence converge on it,” not if all you have is Chomsky’s argument from poverty of the stimulus. We think that one would be justified – that the results from the application solely of the argument from poverty of the stimulus are strong enough to support the conclusion that the “basic design of language is innate.”

Or suppose that some of this evidence was “diverging evidence”; i.e., appeared to be at odds with the conclusion that language design is innate. Again, if the abstract arguments for innateness are strong, one could well opt to follow Eddington’s rule: “it is also a good rule not to put overmuch confidence in the observational results that are put forward *until they have been confirmed by theory*” (Eddington, 1935:211).

But if we ignore Pinker’s “converging evidence,” and even perhaps some “diverging evidence,” haven’t we abandoned the canons of rational scientific inquiry (methodological naturalism)? Not necessarily, as the following example illustrates:

Einstein’s indifference to attempts to confirm or disprove his theories has become legendary. The first to put the special theory of relativity to the test was the German physicist Walter Kaufmann, who attempted to detect changes in the mass of fast-moving electrons. Kaufmann regarded his results as a categorical disproof of the theory, but Einstein was not discouraged. Remarkably, he was equally indifferent to experimental confirmation of his work. Ilse Rosenthal-Schneider, one of his students in Berlin, tells how Einstein once interrupted a discussion of relativity to hand her a telegram that had been lying on the windowsill. It contained Sir Arthur Eddington’s report that the bending of starlight predicted by the general theory of relativity had indeed been observed during the 1919 eclipse. Surprised by Einstein’s indifference, she asked him how he would have reacted had Eddington’s expedition not borne out his theory. “Then,” said Einstein, “I would have been sorry for the dear Lord – the theory is correct.” (Sorensen, 1991:262)

Many similar illustrations can be found throughout the history of science.¹¹

¹¹ Note that we are not saying that theories of language have the explanatory depth of the theory of general relativity or the like. What is being discussed is whether the argument from poverty of the stimulus supports a specific conclusion about the mind; viz., that the “basic design of language is innate.”

Conversely, both Miller and Lenneberg are clearly in the tradition of methodological naturalism; i.e., rational scientific inquiry. Miller was one of the early pioneers, perhaps the earliest, to introduce linguistic considerations into psychology; e.g., cf. his collaborations with Chomsky (Chomsky and Miller, 1963; Miller and Chomsky, 1963). Moreover, Miller regards this early work as an “anticipation of a view that I think Noam is now calling methodological naturalism, to which I would like to subscribe in so far as I understand it” (Chomsky, 1994a:69); see also Miller, 1991. As for Lenneberg, we have already noted that he was one of the first biologists to present linguistic evidence alongside other kinds biological evidence (discussed in an appendix to Lenneberg, 1967 by Chomsky, called “The formal nature of language”).

In any case, what biolinguistics does do (as spelled out in the citation above from Chomsky) is drop all restrictions on evidence, whether to “grammaticality” or to “psychologically real” evidence, “converging evidence,” “DNA evidence,” or to any other kind of evidence:

An empirical observation does not come with a notice “I am for X,” written on its sleeve, where X is chemistry, linguistics, or whatever. No one asks whether the study of a complex molecule belongs to chemistry or biology, and no one should ask whether the study of linguistic expressions and their properties belongs to linguistics, psychology, or the brain sciences. (Chomsky, 1995a:33)

REALITY OF WAVE FUNCTIONS – “SPOOKY ACTION AT A DISTANCE”

We have accepted as a working hypothesis the formulation of Weinberg: “Wave functions are real for the same reason that quarks and symmetries are – because it is useful to include them in our theories.” We wish now to motivate and hopefully to illuminate this idea with a brief discussion of each of the theoretical entities that Weinberg mentions – wave functions, quarks, and symmetries. We begin with wave functions.

Chomsky describes the origins of modern science and the outcome of the attempts to solve the Cartesian case of the unification problem, the so-called “mind–body problem”:

Just as the mechanical philosophy appeared to be triumphant, it was demolished by Newton, who reintroduced a kind of “occult” cause and quality, much to the dismay of leading scientists of the day, and Newton himself. The Cartesian theory of mind (such as it was) was unaffected by his discoveries, but the theory of body was demonstrated to be untenable. To put it differently, Newton eliminated the problem of “the ghost in the machine” by exorcising the machine; the ghost was unaffected. (Chomsky, 1994b:189)

Newton had demolished “body,” leaving us with no coherent notion of the “physical” world:

The mind–body problem disappeared, and can be resurrected, if at all, only by producing a new notion of body (material, physical, etc.) to replace the one that was abandoned; hardly a reasonable enterprise, it would seem. Lacking that, the phrase “material” (“physical,” etc.) would simply offer a loose way of referring to what we more or less understand and hope to unify in some way. *Ibid.*

What we will do here is briefly set out one such contemporary attempt to resurrect such a physical reality; viz., Albert Einstein’s failed arguments for an “objective reality.”

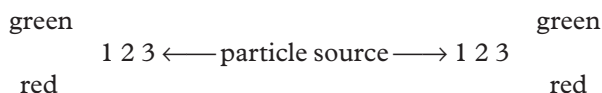
What had bothered Newton and his contemporaries was the problem of action at a distance by an “occult” force; viz., gravity. It was hard for them to accept the idea that, as Chomsky puts it, “the moon moves when you move your hand.” This notion of action at a distance was shown to be incompatible with Einstein’s relativity theory.¹² Einstein and some of his contemporaries were also deeply disturbed by another kind of action at a distance problem, seemingly also occult in nature, which Einstein himself referred to as “spukhafte Fernwirkungen” (“spooky action at a distance”; phrase from Einstein–Born correspondence). Here the problem is that “the moon is . . . not there when nobody looks”: “We often discussed his notions on objective reality. I recall that during one walk Einstein suddenly stopped, turned to me and asked whether I really believed that the moon exists only when I look at it” (Mermin, 1990:81, citing A. Pais).

In a set of interesting essays, the solid state physicist David Mermin discusses Einstein’s unsuccessful attempts to reconstruct what he called “objective reality” (Mermin, 1990). Mermin argues that Einstein was less concerned about the statistical issue (“God doesn’t play dice”) than he was about the implications of quantum theory, in particular the Heisenberg uncertainty relations, in undermining “objective reality,” where objects could be assigned properties, independent of measurement (while agreeing that there were limits to what could be measured on a quantum scale). Einstein, Podolsky, and Rosen showed that one of the implications from quantum theory was that the measurement of position or momentum of an object at one point A could have an effect on the position or momentum of another object at another point B, where there was no connection between points A and B. In order to avoid these unpleasant “spooky actions at a distance,” EPR concluded that these objects must have had position or momenta all along, independent of measurement.

Mermin illustrates the dilemma with a particle source which, when you push a button on it, sends out two particles toward two independent detectors (unconnected in any way). Each detector has a green and a red

¹² Einstein provides an accessible account of how various notions of action at a distance in classical physics gradually came to be supplanted in modern formulations of field theories (Einstein, 1996).

light, one of which flashes, depending on whether a switch on the detector is in position 1, 2, or 3:



A typical run might be 23GR. Here we have (randomly) set the switch on the left-hand detector to 2 and the switch on the right-hand detector to 3 and the lights have flashed green (on the left) and red (on the right). Over millions of runs, we note the following patterns: GG, GR, RG, and RR occur equally often; i.e., the lights flash the same color half the time. But whenever the switches have the same settings (11, 22, 33), the left and right lights flash the same color all the time, even if you throw the switches after the particles have been emitted! How can this be if there is no connection between the two detectors? (I have greatly compressed Mermin's much more lucid presentation.)

Mermin shows the only possible answer would be if each particle contained an instruction set, say GRG, which means flash green if the switch is set to 1, flash red if it is set to 2, and flash green if it is set to 3. The point of all this is the same as the one EPR were making – we must be able to assign objects (at least some) properties independent of measurement – and so we save objective reality.

But now we have a problem with the other observation made above – the lights flash the same color (GG, RR) half the time over long runs. What instruction set(s) will give us this 50 percent result? There are nine switch settings for both detectors: 11, 12, 23, etc. The instruction GRG will cause the same lights to flash the same in five of the cases: 11, 22, 33, 13, and 31. Hence the lights will flash the same five ninths (55.5 percent) of the time, instead of the predicted 50 percent. The other instructions also yield 55.5 percent, except for GGG and RRR, which yield 100 percent. This result is known as Bell's Theorem. The conclusion is that there can't be instruction sets in the sense of EPR, and we are left with "spooky actions at a distance" as our only explanation. The actual experiment (using properties like spin and polarization) was finally able to be carried out by Aspect and collaborators in the early 1980s, disproving objective reality and leaving us with "spukhafte Fernwirkungen."

Mermin (1990) presents an interesting cross-section of the attitudes toward all this:

POPPER: The general antirationalist atmosphere which has become a major menace of our time, and which to combat is the duty of every thinker who cares for the traditions of our civilization, has led to a most serious deterioration of the standards of scientific discussion. (p. 196)