The Primacy of Grammar

Nirmalangshu Mukherji

1 The Loneliness of Biolinguistics

Reflecting on the state of language research, after a decade of work in the principles-and-parameters framework, Noam Chomsky (1991b, 51) observed that

the systems found in the world will not be regarded as languages in the strict sense, but as more complex systems, much less interesting for the study of human nature and language, just as most of what we find around us in the world of ordinary experiences is unhelpful for determining the real properties of the natural world. . . . I have spoken only of language, which happens to be one of the few domains of cognitive psychology where there are rather far-reaching results. But I think it would hardly be surprising if the truth of the matter were qualitatively similar in other domains, where far less is known . . . only ancient prejudice makes this prospect appear to many to be unlikely.

I find it instructive to open the discussion with a somewhat free interpretation of these remarks. The citation has two parts. In the first, it is suggested that the study of "complex systems" "found in the world" is not likely to lead to the discovery of the "real properties of the natural world." In the second part, the citation mentions the discipline of cognitive psychology where "rather far-reaching results" have been achieved in some of its domains. The results have been "far-reaching" in the sense that something has been learned in these domains at a sufficient remove from "the world of ordinary experiences." Combining the two, it follows that, in these few domains of cognitive inquiry, research has been able to abstract away from the complexities of systems found in ordinary experience to isolate some simple systems whose properties may be viewed as real properties of nature.

The implicit reference here is to some small areas of the more established sciences such as physics, chemistry, and certain corners of molecular biology, where rather surprising and deep properties of the natural world are sometimes reached by abstracting away from common experiences

and expectations (Stainton 2006). I will have many occasions in this work to evaluate advances in the "few domains" of cognitive psychology in terms of the history and methodology of physics and other advanced sciences (also see Boeckx 2006). For now, Chomsky seems to be generally suggesting that, in these domains, something like the explanatory depth of the natural sciences is almost within reach. How did it happen?

1.1 Some Classical Issues

If "cognitive psychology" is understood broadly as a systematic study of human cognitive behavior (as contrasted to, say, motor behavior), then the study is probably as old as human inquiry itself. Extensive, and sometimes quite rigorous, studies on this aspect of human nature dominated much of philosophical thinking across cultures for centuries. These studies were not always cast in direct psychological terms—that is, in terms of the properties of the human mind. For example, language was often studied as an independent, "external" object by itself, and the character of the studies ranged from mystical reflections to more critical and often constructive suggestions on the nature of this object.

Such studies proliferated in large parts of the ancient Indian intellectual tradition. In the *Rgveda* (c. 1000 BC), for instance, the phenomenon of language is once described as a "spirit descending and embodying itself in phenomena, assuming various guises and disclosing its real nature to the sensitive soul."[1] On the other hand, much later but within the same tradition, Pāṇini (c. 450 BC) worked out the first extensive and rigorous grammatical account of Sanskrit to trigger discussion and analysis that continue today (Kiparsky 1982; Barbosa et al. 1998, 2; Dasgupta, Ford, and Singh 2000; Coward and Kunjunni Raja 2001). Although nothing like the sophistication of Paninian grammar was ever reached in other domains, vigorous discussion of conditions governing human knowledge, perception, memory, logical abilities, and the like, continued for over a millennium in eight basic schools of thought with many subschools within each. The complexity and the depth of this tradition have begun to be understood in contemporary terms only recently. Unfortunately, the context and agenda of the present book do not allow more detailed comments on this tradition.[2]

Similar variations are found in the Western tradition as well. For the mystical part of the tradition, one could cite Hegel, for whom language is "the medium through which the subjective spirit is mediated with the

being of objects.'' The critical and constructive part of the enterprise took shape since Plato and Aristotle and continued to Descartes, Leibnitz, Kant, Hume, and later thinkers such as Wilhelm von Humboldt (1836).[3] Here as well we notice the interesting unevenness between linguistic studies, say, in the Aristotelian and Cartesian–Port Royal traditions, and the rest of the studies on human cognition. While studies on language and logic grew in sophistication, it is hard to see any radical progress since, say, the Theory of Ideas proposed by Plato in the fifth century BC. Very tentatively, therefore, there seems to be a sense in which the ''few domains'' of language and related objects are such as to open themselves to focused theoretical inquiry.[4]

It is not difficult to reinterpret at least some of these studies from either tradition in naturalistic terms to suggest that they were directed at uncovering the ''real properties'' of one part of nature, namely, the human mind. For Bhartrhari (c. 450–500 AD), a philosopher of language in the Paninian tradition, speech is of the nature of the Ultimate Reality (*Sabda-Brahma*): ''Although the essence of speech is the eternal *Brahman*, its significance evolves in the manner in which the world evolves.''[5] The thought is subject to a variety of (often conflicting) interpretations. However, no familiar conception of divinity—for example, an object of worship—attaches to the concept of *Brahman*. In that sense, nothing is lost if *Brahman* is understood as a system of invariants that constrains both the evolution of the world and the significance of speech.

For the Western tradition, consider what Chomsky takes to be the central question in cognitive psychology: How do humans come to know so much from so little exposure to the environment? In different places, Chomsky calls this problem variously ''Plato's problem'' (Chomsky 1986), ''Descartes' problem'' (Chomsky 1966), or ''Russell's problem'' (Chomsky 1972b). These names suggest that, at least in the Western tradition, the general problem was raised throughout directly in psychological terms—that is, in terms of constraints on human knowledge. Nevertheless, despite the noted unevenness between linguistic and other studies, studies from *Rgveda* to Russell hardly qualify as scientific studies in any interesting sense of ''science.'' Suppose we label the most rigorous efforts in this area as ''proto-science.''

For some domains of current cognitive psychology, in contrast, what Chomsky is claiming is a lot stronger. He is claiming that studies in these domains already exhibit some of the properties of the most *advanced* corners of some of the natural sciences. So the situation is this: the general

questions currently asked in these domains are fairly classical though the form and the content of the answers have radically changed. I can think of only one way this could have happened.

Recall that only a few domains of "cognitive psychology" seemed to be intrinsically open to serious theoretical inquiry leading to proto-science; they at once await and motivate, as it were, development of new ideas and theoretical tools. Whenever new ideas and tools are directed at classical questions, interesting answers begin to appear at a certain remove from common experience only in these domains from among the assorted domains to which the general, philosophical inquiry was initially, somewhat aimlessly, directed. Assuming this, it is no wonder that the object responded to the efforts of Paninian, Aristotelian, and Port Royal grammarians, as well as to contemporary generative linguists. Also, it could have been the case that the object, the new tools, and novel ideas formed a symbiotic relationship in that these tools and ideas interestingly applied only to this object. If so, then we have some explanation of why thousands of years of philosophical investigations into the nature of the rest of human knowledge in either tradition revolved around basically the same set of ideas and problems while formal studies on language and related topics flourished. I return to these issues shortly.

1.2   Limits of Cognitive Inquiry

The picture sketched above sheds some light on what seems to me to be a major perplexity in contemporary studies on language and mind. The perplexity is this: although there are reasons, both historical and conceptual, for skepticism about the very idea of (serious) cognitive inquiry, certain approaches to language have rapidly reached the standards of the advanced sciences.

There is an old adage that a theory of language is an impossibility since the theory has to be stated in some language or other. Thus, the theory always falls short of its object. It quickly generalizes to a dim view of theories of mind as well: a theory of mind is an impossibility since the theory itself will be a product of the mind, and hence a part of the object under examination. The adage appeals to the image of eyeglasses: we can give only a partial and distorted description of the glasses when we wear them; we can take them off, but then we cannot see. This adage is distinct from classical skepticism that denies the possibility of any knowledge. The effect of the adage is restricted only to cognitive inquiry; in that

sense, it allows the possibility of knowledge of the "external world," say, the world of physics.

Moreover, the adage needs to be distinguished from the more general observation by Chomsky (1980) that, since the human science-forming capacity is itself a natural object, there ought to be limits on scientific inquiry: the constraints that govern the capacity both allow and restrict the formation of scientific theories. Thus, unsolved problems divide into two kinds: "puzzles" that the human mind can in fact solve, and "mysteries" whose solutions, perhaps even intelligible formulation, lie beyond the power of the human mind (Chomsky 1975). The scope of this suggestion is difficult to estimate. On the one hand, the suggestion seems to apply to all the sciences: Are the unsolved problems of the origins of life or of the universe puzzles or mysteries? On the other hand, it is unclear whether it applies to the entire study of inner domains. For example, Chomsky specifically thinks that what he has called "the creative aspect of language use"—our essentially unbounded ability to produce and interpret sentences appropriately in novel circumstances—is a mystery. In the limit, we could conjecture that any significant general study of the science-forming capacity itself is beyond the power of the capacity. It does not follow, as the adage requires, that the study of cognitive systems such as the visual system, structural aspects of human reasoning, the observed diversity of languages, and so on also fall beyond the capacity, as Chomsky's own work on language testifies.

The adage is also different from a more recent skeptical perspective on the history of science. According to Chomsky, lessons from the history of the natural sciences seem to suggest that "most things cannot be studied by contemporary science." On this issue, it seems to him that Galileo's intuition that humans will never completely understand even "a single effect in nature" is more plausible than Descartes' confidence that "most of the phenomena of nature could be explained in mechanical terms: the inorganic and organic world apart from humans, but also human physiology, sensation, perception, and action to a large extent." Developments in post-Cartesian science, especially Newtonian science, "not only effectively destroyed the entire materialist, physicalist conception of the universe, but also the standards of intelligibility that were based on it." Thus Chomsky (2001b) supports Alexander Koyre's remark that "we simply have to accept that the world is constituted of entities and processes that we cannot intuitively grasp." Clearly, these remarks apply to the whole of science including, as noted, the most innovative proposals

in theoretical physics. The remarks tell us about the kind of science we are likely to have at best; they do not deny that some form of science is available to humans in most domains of inquiry. The adage under discussion, on the other hand, suggests that scientific explanation may not be available for the study of "inner" domains at all, notwithstanding the character of scientific explanation already available for the "outer" domains.

Nevertheless, the adage and Chomsky's observations possibly converge around the issue of complexity. Chomsky suggests that sciences of outer domains work under severe constraints, cognitive and historical. These constraints perhaps lead to the striking unevenness in the development of science. Genuine theoretical understanding seems to be restricted to the study of simple systems even in the hard sciences such that "when you move beyond the simplest structures, it becomes very descriptive. By the time you get to big molecules, for example, you are mostly describing things" (Chomsky 2000a, 2). Thus the quality of explanation falls off rapidly as inquiry turns to more complex systems. Given that the organization of our inner domains—that is, the respects in which we wish to understand them—is vastly more complex than free electrons or isolated genes, it is not surprising that we lack scientific progress in these domains. These remarks suggest that, even when we reach some understanding of cognitive domains such as language, the understanding is likely to be restricted to small and simple parts of the domain such as grammar.

The adage fosters a lingering intuition that our ability to have a theoretical grasp of ourselves must be severely restricted somewhere: "There are inevitably going to be limits on the closure achievable by turning our procedures of understanding on themselves" (Nagel 1997, 76). It is likely that when we approach that point our theoretical tools begin to lose their edge and the enterprise simply drifts into banalities since, according to the adage, our resources of inquiry and the objects of inquiry begin to get hopelessly mixed up from that point on. Such a point could be reached in the "hard sciences" as well when they attempt to turn "inward." This may be one way of understanding the origin of the deep puzzles around the so-called measurement problem in quantum physics. The conjecture here is that, for inner domains such as reasoning and language, such points show up sooner rather than later.

Despite the intellectual appeal of the adage, it is not clear how to examine it in a theoretically interesting manner. In fact, from the point of view of the cognitive sciences, the adage may be viewed as intrinsically uninteresting. How can we tell now what an enterprise is going to look like in the

future (Fodor 2000, 11 n. 1)? Skeptical questions could have been, indeed must have been, raised at the beginning of physics. But physics progressed, through calm and stormy times, without ever directly answering them. The questions were ultimately answered indirectly by the growth of physics itself to the point that skepticism became uninteresting. However, there is no credible evidence in the history of the sciences—just the opposite in fact, as we will see briefly in the context of biology—that lessons from the history of physics generalize to other domains of inquiry. It could be that Galilean physics is an exception rather than the rule in scientific inquiry. To emphasize, Galilean physics could be an exception precisely because it could extract and focus on simple parts of nature.

In any case, the natural sciences typically focus on "outer" domains of nature, called the "external world" in the philosophical literature; the study of inner domains just does not belong to serious science. This is one source of the classical mind-body problem. The mind (the collection of inner domains) is thought to be so fundamentally different from the body (the collection of outer domains) that the forms of scientific explanation available for the latter are not supposed to obtain for the former. Chomsky has dubbed this doctrine "methodological dualism" (Chomsky 2000d, chapter 4, for extensive criticism). When we add the further assumption that the forms of explanation that apply to the outer domains are the only ones in hand, it follows that inner domains fall out of science.[6]

To find some grip on these very general issues, I will assume, as noted, that the study of inner domains is essentially concerned with what Chomsky has called "Plato's problem": How do organisms form rich cognitive structures from little exposure to the environment? I take this to be the original and fairly classical motivation for cognitive science although not everyone who currently works in the cognitive sciences shares the motivation.[7] The problem arises from what has come to be known as the "poverty-of-the-stimulus arguments," which show that there is not enough information in the environment for the rich systems constructed by organisms (Chomsky 1959; Piattelli-Palmarini 1980; Wexler 1991; Crain and Pietroski 2001; Berwick and Chomsky 2009, etc.). As Chomsky (2000a, 6) puts it, "We can check the experience available; we can look at it and see what it is. It's immediately obvious that it's just much too limited and fragmentary to do anything more than shape an already existing common form in limited fashions."

The observation applies across the board to human language, the visual system, bird songs, insect navigation, bee dances, and so on. For

example, in the visual-cliff experiment, a given pattern is broken into an upper and a lower half with a glass top extending from the "shallow" half over the "deep" half. Thus, in the absence of depth perception, the lower half looks continuous with the upper. Newly hatched chicks and one-day-old goats will stop at the upper edge of a visual cliff at the very first exposure; a goat will in fact extend its forelegs as a defensive measure when placed on the "deep" side and leap onto the "shallow" side (Kaufman 1979, 237).

For human language, which is our basic concern, it has been extensively documented that children rapidly acquire languages not only on the basis of impoverished information, but also, in many cases, seemingly without any relevant information at all (Jackendoff 1992, chapter 5). In a particularly telling case, three deaf children were able to construct a sign language secretly and for use only among themselves in the face of parental opposition. Investigations later showed that this language compared favorably with the spoken language developed by normal children of the same age (Goldin-Meadow and Feldman 1977; Gleitman and Newport 1995; Goldin-Meadow 2004). In fact, studies show that deaf and normal children make the same "mistakes" at the corresponding stage of acquisition. At a certain stage, normal children typically use *you* to refer to themselves and *I* to refer to the addressee. Amazingly, corresponding gestures in American Sign Language for *you* and *I* by deaf children show very similar reversal despite the fact that these gestures are iconic (Chiat 1986; Petitto 1987).

Studies show that twelve-hour-old babies can distinguish between linguistic and nonlinguistic acoustic inputs. Jacques Mehler and his associates showed further that four-day-old infants can distinguish between the prosodic contours of, say, Russian and French (Mehler et al. 1986). Turning to more abstract syntactic abilities, four-month-old babies are sensitive to the clause boundaries of, say, Polish and English, their native tongue. By six months, however, they lose their sensitivity to Polish clause boundaries, but retain the same for English (Karmiloff-Smith 1992, 37). In other terms, as we will see, some parameters of specific languages are fixed by then (Baker 2001). The general task of the cognitive sciences is to explain this astonishing ability in every domain in which it is displayed.

Returning to the adage and setting other inner domains aside, it is already clear that language escapes the suggested divide between what does and does not fall under science. Language not only belongs to the inner domain, it is an extremely complex system even when it is studied under the so-called top-down—rules-and-representations—approach

(Jackendoff 2002, 6); at the level of neurons and their connections, the complexity is astronomical. This is where we would least expect genuine scientific understanding. Yet, in just over four decades of research, we not only have substantive solutions to Plato's problem in this domain, the solutions have the form of the most advanced corners of science. Somehow the adage lost its skeptical power when scientific attention was directed at a specific aspect of human language. If the results of language research are to be coherently accommodated within current scientific outlook, some fundamental assumptions have to give way.

In contrast, despite immense international effort accompanied by technological development, progress in other classical cognitive domains continues to be largely elusive. According to Jerry Fodor (2000, 2), "The last forty or fifty years have demonstrated pretty clearly that there are aspects of higher mental processes into which the current armamentarium of computational models, theories, and experimental techniques offers vanishingly little insight." Even if Fodor's rather sharp remark is only partly true, it seems the adage continues to control the study of these domains.

The opening citation from Chomsky, together with its free interpretation, gives some preliminary idea of what might be happening. The basic idea, as hinted, is that linguistic inquiry could escape the adage precisely because it could address Plato's problem in an area of human cognition that has traditionally allowed inquiries to abstract away from common experience. Inquiries that are more directly involved with common experience, even implicitly, seem to fail to do so. For now, I will make some brief remarks on this point with some speculation on how our cognitive capacities might be organized with respect to our ability to study them. The rest of the work may be viewed as a gradual unfolding of these preliminary ideas.

It seems that our grammatical—not *linguistic*—capacity is such that we have no firm common beliefs about its nature and function; we just use it to form "surface" intuitions in the form of judgments of acceptability. That is, our use of this capacity does not require that we form some opinion of it; we are not congenital syntacticians. Karmiloff-Smith (1992, 31) disagrees: "Normally developing children not only become efficient users of language; they also spontaneously become little grammarians." Very young children, no doubt, make surprisingly sophisticated grammatical judgments on the basis of substantial tacit knowledge, as Karmiloff-Smith documents, but they do not know what *noun phrase* or *anaphora* means.

In this sense, the grammatical system is "opinion-encapsulated." Therefore, it is possible for the human science-forming capacity to study

these "surface" intuitions reflected in the production and interpretation of speech, and to abstract away from them relatively undisturbed by "folk syntax." In other words, grammatical competence is typically put to use without any knowledge that a grammar is in use—that is, without knowing that the user is putting something to use. When asked if the (complicated) structure *John is too intelligent to fail* is okay, a competent user can give assent without having any resource to explain why it is so.

If this is roughly correct, it explains why linguists can place their own linguistic intuitions under scientific scrutiny, thus opening up an explosion of data for language research. As we will see much later in the work (in section 4.2), even when human grammatical judgments are uncertain, we cannot remove the uncertainty by conscious effort. So, even uncertain intuitions become data for science. Interestingly, what I just said about grammaticality judgments seems to apply to perceptual judgments as well when the contexts are properly controlled and the stimulus is presented rapidly. It is generally said that, among the studies on cognitive capacities, the sciences of language and vision have made the most progress, though the sharp unevenness of progress between the two is also acknowledged.

In other domains—for example, the human conceptual system—it seems that we need to become "folk semanticists" in varying degrees to be able to use this system. This is because this system is directly involved, at varying levels of consciousness, with what beliefs we form about the world so that we can lead a life in it. Thus, we need to form fairly conscious judgments regarding which concept is related to which one, which is "higher" and which is "lower," which has a sharp boundary and which is relatively loose, and so on, in order to be able to use them in appropriate contexts.

In that sense, users of *dog* or *apple* need to be prepared to explain what they are talking about. In fact, asked to explain if the sentence mentioned above, *John is too intelligent to fail*, is okay, a user is likely to answer in terms of the meanings of *John*, *intelligent*, and *fail*, rather than whether the small clause is correctly placed. These judgments interfere, quite fatally as we will see, with our scientific ability to penetrate below them to examine the "real" structures that no doubt exist: "One cannot guess how a word functions. One has to *look at* its use and learn from that. But the difficulty is to remove the prejudice which stands in the way of doing this. It is not a *stupid* prejudice" (Wittgenstein 1953, paragraph 340).

This is not to deny that we might form some common opinion on what counts as "language" essentially in terms of this "folk semantics," an

opinion that in turn might lead to some opinion on what counts as "grammar." Schoolchildren are constantly subjected to grammatical lessons on, say, how to convert direct speech to indirect speech in terms of specific contexts of use. But, if the preceding analysis is broadly correct (and partly clear), then we know why it is possible for the biolinguist to ignore such opinion without much difficulty—or, to use such opinion itself as data—and focus on the underlying object instead.

1.3 Overview of Biolinguistics

Chomsky initiated the contemporary research on language nearly half a century ago essentially to solve Plato's problem for the domain of language, as noted. From the beginning, the research focused on language as a cognitive system in the mind/brain that solves Plato's problem for the child (Chomsky 1955a);[8] hence, the enterprise is called "biolinguistics." In the domain of language, Plato's problem took an interesting form very early in the research program: the tension between descriptive and explanatory adequacies (Chomsky 1965, chapter 1). The tension arose as follows.

When researchers attempted to give a precise description of the properties of expressions in individual languages—the condition of descriptive adequacy—they were compelled to postulate very complex mechanisms with varied grammatical constructions that mostly looked specific to the language under study. Following Plato's problem, the condition of explanatory adequacy required that the construction of grammars be based on the impoverished conditions of language acquisition. So, the very complexity of the descriptions made the languages essentially unlearnable because there just is not enough information available to the language learner for constructing those elaborate grammars.

Moreover, since children are not born with genetic dispositions to learn specific languages, the language faculty ought to allow every normal child to acquire any of the thousands of human languages with equal facility, so Plato's problem compounds. As a matter of fact, children do acquire any human language rapidly and with ease with little stimulus from the environment, as we saw. In most cultures, children acquire a number of languages before they know the names of these things; they do not even know that they have acquired *languages*. The rich descriptions of languages were thus incompatible with what children do.

Notice that the problem is somewhat different from the closely related problem of acquiring the visual system. The problem of explanatory

adequacy for language learning (alternatively, "the logical problem of language acquisition") clearly suggests that the initial human language system, the faculty of language, ought to be simple and uniform across the species. This must also be the case with the visual system, as with every cognitive system, given Plato's problem. But the visual system is not only uniform across the species like the language system; the states that it can attain, unlike the language system, are largely uniform as well, pathology aside. The shapes that occur in this line, for instance, can be copied by anyone, but they will be understood only by competent users of English. The states that the language system can attain vary wildly, as thousands of human languages and dialects testify. This led quite naturally to the principles-and-parameters framework, as we will see.

Continuing with historical remarks, the research that ensued for the domain of language began receiving some acceptance in the early 1960s, most notably at MIT, where Chomsky taught. Still, the field of biolinguistics was rather small at this stage, with only a handful of researchers at MIT and elsewhere. Chomsky reports that "it used to be possible, not so long ago, to teach [biolinguistics] from zero to current research within a term or a year or so" (Chomsky, Huybregts, and Riemsdijk 1982, 52). In just a few decades since, biolinguistics has become a major scientific enterprise across the globe. Jenkins (2000, ix) reports that, apart from research in theoretical linguistics (syntax, semantics, morphology, lexicon, phonology) covering hundreds of languages and dialects, the enterprise now actively touches on areas such as articulatory and acoustic phonetics, language acquisition, language change, specific language impairment, language perception, sign language, neurology of language, language-isolated children, Creole language, split-brain studies, linguistic savants, and electrical activity of the brain, among others.

Notwithstanding astonishing growth within a short time, biolinguistics is very far from being the acclaimed program in studies on language in general. Apart from biolinguists and some of their coresearchers in psychology and the neurosciences, researchers on language include other varieties of linguists such as sociolinguists and historical linguists, practitioners of a large and amorphous discipline called "cognitive linguistics," literary theorists, semioticians, philosophers of language, logicians and formal semanticists, communication theorists, varieties of computational linguists (including those who work on machine translation), and so on. Although some people from these disciplines do work within the broad generative enterprise, it is a safe bet that most researchers in these disci-

plines not only do not work within the biolinguistic enterprise, they are positively hostile to it.

1.3.1 Language and Biology

Some of this resistance comes from (1) the antiscientific aspects of the general intellectual culture especially when it concerns topics of "human" interest, (2) varying conceptions of language and theory of language, (3) discomfort with formal analysis, and (4) the continuing influence of traditions of linguistic research outside the generative enterprise.

Many of these strands can be traced to the widespread belief that language is not an object for the natural sciences at all. In other words, the basic source of this resistance to the enterprise is the central claim of biolinguistics that, in studying the nature and function of human language, linguists are in fact studying some biological aspect of the human brain. People working on language are generally uncomfortable with the idea that language is essentially a biological system (Koster 2009): it is a determinate, restrictive structure that grows in the mind of the child under highly specific inner constraints. This is in conflict with a conception of language shared by many that language is a "cultural" entity; it is thus flexible and moldable, not unlike the alleged malleability of social institutions, customs, and political beliefs.[9]

However, contrary to expectations, Chomsky does not defend the biological basis of linguistic theory by citing (corroborative) evidence from the brain sciences; just the opposite in fact in major respects for now. Somehow then the claim that language is a natural object—a biological system with a genetic component—is maintained independently of the advances in the biological sciences! In recent work, Chomsky has argued for this perspective by showing that, even if the current biological sciences do not provide any manifest basis for the results of linguistic research, there is no coherent alternative to the view that language is a biological system; any alternative perspective is likely to fall into one untenable version of dualism or another (Chomsky 2000d). Indeed, according to Chomsky (2005), those who explicitly resist the idea of the biological basis of language often adopt it implicitly for coherence. Even then it is not immediately clear what naturalistic basis to ascribe to language in the absence of direct support from biology.

The issue of the biological basis of the generative enterprise can be raised for different aspects of the enterprise. These aspects fall into two broad categories: observational and theoretical. First, a number of

observations on the character of linguistic and related behavior are made
to argue that the human language system must be highly constrained in-
nately. Second, linguistic theories are formulated to seek specific proper-
ties of the mind/brain that give rise to the observed phenomena. The issue
of biological basis affects these two aspects in different ways. Chomsky's
position, stated above, belongs essentially to this second aspect, although
it touches on the first as well.

The idea that organisms are highly constrained innately is taken to be a
truism in the study of organic systems: "Take the fact that people un-
dergo puberty at a certain age . . . if someone were to propose that a child
undergoes puberty because of, say, peer-pressure ('others are doing it, I'll
do it too'), people would regard that as ridiculous" (Chomsky 2000a, 7).
Still, prevalent conceptions of language require that the truism be explic-
itly demonstrated. Central to these demonstrations are the poverty-of-
stimulus arguments, noted above. In this sense, Chomsky and others have
drawn on a variety of evidence, from the way children learn languages to
brain disorders of language, to argue that the linguistic system is innately
constrained. The most plausible way of construing these innate con-
straints is to think of them as having a biological (= genetic) basis.

However, apart from telling us, in general terms, that there ought to
be a biological basis of language, poverty-of-stimulus and related argu-
ments do not supply any clue about what that basis is: "Poverty of stim-
ulus considerations tell us that some knowledge is innately represented;
they don't tell us how the knowledge is represented or processed" (Collins
2004, 506). These "arguments" are really observations that help set up a
problem—essentially, Plato's problem—that theories of language try to
solve. Note also that such arguments, including those from split-brain
studies, are typically focused on behavior, which is the output of the con-
cerned cognitive system; they are not directly focused on the biological
properties of the system. In solving the problem raised by these argu-
ments, a theory of language attempts to go below the behavior to isolate
the specific properties of the innate biological system involved here. Thus,
a more demanding concept of biological basis arises when we shift to par-
ticular proposals—theories—regarding the innate constraints. Here, as I
understand it, Chomsky's perspective is that biolinguistics stands essen-
tially on its own; biological sciences do not provide any support.

In fact, the basic problem that is currently animating linguistic research
is even more enigmatic than the problem just mentioned. The enigma
arises as follows. Suppose that the biolinguistic framework is reluctantly
admitted if only because, as noted, coherent alternatives are difficult to

conceive. Now, biological systems are standardly viewed as poor solutions to the design problems posed by nature. These are, as Chomsky (2000a, 18) puts it, "the best solution that evolution could achieve under existing circumstances, but perhaps a clumsy and messy solution." In contrast, the so-called exact sciences, such as physics and parts of chemistry, follow the Galilean intuition that nature is perfect: natural effects obtain under conditions of "least effort." Thus, the search for these conditions in nature had been a guiding theme in these sciences.

The design problem that the human linguistic system faces is the satisfaction of legibility conditions at the interfaces where language interacts with other cognitive systems. Roughly, the sensorimotor systems access representations of sound, and conceptual-intentional systems access representations of meaning. As Chomsky (2000a, 17) phrases the design problem, "To be usable, the expressions of the language faculty (at least some of them), have to be legible by the outside systems. So the sensorimotor system and the conceptual-intentional system have to be able to access, to 'read' the expressions; otherwise the systems wouldn't even know it's there."

Explorations under what is known as the Minimalist Program are beginning to substantiate the view that the system is "perfect": it solves the design problem under conditions of least effort. What look like apparent imperfections in the system, such as the existence of (semantically) uninterpretable features in the lexicon, are best explained as optimal mechanisms for meeting legibility conditions imposed by systems external to language. We will look at the phenomenon later (chapter 5). How do we accommodate these discoveries with the idea that biological systems are "clumsy and messy"?

Some years ago, Chomsky (1995b, 1–2) formulated the big puzzle that emerges as follows: "How can a system such as human language arise in the mind/brain, or for that matter, in the organic world, in which one seems not to find anything like the basic properties of human language?" Chomsky thought that the "concerns are appropriate, but their locus is misplaced; they are primarily a problem for biology and the brain sciences, which, as currently understood, do not provide any basis for what appear to be fairly well established conclusions about language."

Unless one is intrinsically excited about the prospect of discovering a new aspect of nature in whatever terms are available, especially in the "inner" domains, one is not likely to be convinced by Chomsky's diagnosis of the problem without further arguments. Given the power and prestige of the "hard sciences," it is difficult to swallow the idea that

biolinguists are right and all the life sciences, as currently understood, are wrong, or at least insufficient, in this respect.

In fact, if the enterprise is not to be viewed as just a technique for generating linguistic structures, then it is an open question how many generative linguists themselves seriously subscribe to the idea that, for example, in studying the intriguing structure *John had a book stolen* they are in fact studying the human brain.[10] In a conversation twenty-five years ago regarding the early developments in transformational grammar, Chomsky remarked that "it was just used as another descriptive device." "There are things," Chomsky continued, "that you can describe in that way more easily than in terms of constituent structure, but that is not a fundamental conceptual change, that is just like adding another tool to your bag" (Chomsky, Huybregts, and Riemsdijk 1982, 40). It will be surprising if the general attitude has changed much in the meantime, even if the "tools" have.

Chomsky's remark has an immediate echo in an intriguing period in the history of science that he has alluded to from various directions in recent years. The period at issue concerns the character of chemistry, as viewed by most of its principal practitioners before its unification with (quantum) physics. As Chomsky (2001b) puts it, "It *was* claimed, up until the 1920s by Nobel laureates, philosophers of science, and everyone else, that chemistry is just a calculating device; it can't be real. This is because it couldn't be reduced to physics." Since linguistics could not be "reduced" to biology either, it is not wholly unreasonable to view the generative enterprise as a "calculating device" by its practitioners.

As noted, Chomsky placed the "locus" of the concern on the biological sciences; others might prefer to place it on the generative enterprise itself. Could it be that the entire discipline of biolinguistics lacks foundations? Although nothing can be ruled out, finding something fundamentally wrong with the internal research of biolinguistics now requires working through this increasingly difficult discipline with its very abstract formulations and a massive body of interdisciplinary research, as noted. It is likely that, from now onward, foundational problems with the generative enterprise, if any, will be noticed within the enterprise itself, as in physics and mathematics—not from the outside.

In fact, the enterprise has already faced a number of such problems. The conflict between descriptive and explanatory adequacies, mentioned above, is one of the earlier ones. In the 1980s, the postulation of "inner" levels of representation, d- and s-structures, posed another fundamental

problem since no other system of the mind accesses them. In the current Minimalist Program, such problems include the existence of uninterpretable features: lexical features, such as structural Case, that have no semantic interpretation are found in every language; certain operations seem to require "look-ahead" information, and so on (I return to these issues in section 5.1.2). Notice that foundational problems have progressively become more theory-internal, as expected in an advancing science (see Freidin, Otero, and Zubizarreta 2008). It is not surprising that attempts to challenge the foundations of the discipline from the outside have more or less faded out in recent decades.

Outside the enterprise, a more convenient strategy is to grant that Chomsky may be right in what he is doing, but he is doing very little—so little, in fact, that we need to *re*do the whole thing, including syntax. As Peter Gärdenfors (1996, 164–165) puts it, "Semantics is primary to syntax and partly determines it. . . . This thesis is anathema to the Chomskyan tradition within linguistics." Anna Wierzbicka (1996, 7) complains that the "Chomskyan anti-semantic bias" has "led to a preoccupation with formalisms . . . in which 'meaning-free' syntax has for decades usurped the place rightfully belonging to the study of meaning." To show the extent of disapproval, she cites Nobel laureate Gerald Edelman: "The set of rules formulated under the idea that a grammar is a formal system are essentially algorithmic. In such a system, no use is made of meaning. Chomsky's so-called generative grammar . . . assumes that syntax is independent of semantics."[11] Ray Jackendoff (2002, 269) suspects that "the underlying reason for this crashing wave of rejections is the syntactocentrism of mainstream generative grammar: the assumption that the syntactic component is the sole source of generative capacity in language." Hence, he is led to suggest "a radical reformulation of linguistic theory that in some strange sense 'turns the grammar inside out'" (p. xii). Given the continuing popularity of these complaints against biolinguistics, apparently leading to a "crashing wave of rejections," some brief remarks are in order at this point. I will discuss the question of meaning in biolinguistics at length as I proceed, especially in chapters 3–7.

"Meaning," Chomsky (1957) observed in his early work, is a "catchall" term. The term evokes a variety of expectations, not all of which can be met in serious theoretical inquiry. Furthermore, there is no assurance that, when the common concept of meaning is placed under theoretical scrutiny, whatever remains of the common concept will be located in one theoretical place. It is more likely that the thick and loose ordinary

concept will be broken down into theoretically salient parts, and that the individual parts will be attached to different corners of the total theoretical plane.

Keeping these points in mind, consider Chomsky's general characterization of the computational system of language. A computational system consists of "rules that form syntactic constructions or phonological or semantic patterns of varied sorts to provide the rich expressive power of human language" (Chomsky 1980, 54ff.). Notice that this characterization includes "semantic patterns." Almost every topic in biolinguistics is directly concerned with semantics and questions of meaning. For example, treatment of grammatical phenomena such as understood Subject, antecedents of anaphors and pronouns, quantifer movement, and so on, are directly semantically motivated. In fact, the entire nonphonological part of computation (N $\rightarrow$ SEM computation) is currently viewed as geared to form an "image" SEM in a way such that the (configurational) demands placed by the systems of thought are optimally met.

Furthermore, it is most natural to view the language faculty itself as containing what may be called "I-meanings": representations encoded in the formal-semantic features of lexical items. Finally, other naturalistic things being equal, the domain of syntax may be broadened to include much of what goes by the label "formal semantics"; thus, the concept "semantic value" could cover syntactic objects internal to the mind but external to the language faculty. So much for Chomsky's "preoccupation with 'meaning-free' syntax," and his stopping "people from working on meaning" (Marvin Minsky, cited in Jenkins 2000, 52). It is hard to find any interest, then, in the objections to the biolinguistic enterprise from the outside.

Therefore, the only option is to try to make sense of linguistic research in the context of current science. And here the stumbling block, to repeat, is that there is nothing in the relevant current sciences that tells us how to make that sense. The problem, as Chomsky notes, may well lie with biology and the brain sciences, which do not provide any basis for what appear to be well-established conclusions about language. More specifically, the biological sciences may not have sufficiently advanced to respond to the questions posed to it by linguistic research. It could be, as Chomsky (2000d, 104) observes, "any complex system will appear to be a hopeless array of confusion before it comes to be understood, and its principles of organization and function discovered."

I will briefly cite two examples to suggest what is at stake here. Consider the research on nematodes. Nematodes are very simple organisms

with a few hundred neurons in all, so people have been able to chart their wiring diagrams and developmental patterns fairly accurately. Yet Chomsky (1994b) reports that an entire research group at MIT devoted to the study of the "stupid little worm," just a few years ago, could not figure out why the "worm does the things it does." More recently, citing cognitive neuroscientist Charles Gallistel, Chomsky writes that "we clearly do not understand how the nervous system computes, or even the foundations of its ability to compute, even for the small set of arithmetical and logical operations that are fundamental for any computation for insects" (Chomsky 2001b; Gallistel 1998). Commenting on Edward Wilson's optimism about a "coming solution to the brain-mind problem," Chomsky remarks that the "grounds for the general optimism" regarding "the question of emergence of mental aspects of the world" are at best "dubious."

There are serious attempts in biology itself to address the tension between the concept of perfection and what is known about biological systems. In recent years, there has been increasing application of considerations from physics (such as symmetry, least-energy requirement, and the like) to try to understand the organization and function of complex biological systems (Jenkins 2000; Leiber 2001; Piattelli-Palmarini and Uriagereka 2004; Chomsky 2005, etc.). If this approach is successful in providing an account of some of the complex physical structures and patterns found in the biological domain, then biology will also confirm the intuition about nature's drive for the beautiful which has been a guiding theme of modern science ever since its origins, as Chomsky (2001b) remarks following Ernst Haeckel. Still, even if we grant that the patterns on zebras or the icosahedral structure of viruses have interesting least-effort explanations, the chance of such explanations extending to the abstract structures of language is at best remote. I return to this point in section 7.3.1.

1.3.2   A Body of Doctrines

Pending such advances in biology, the only option is to make scientific sense of linguistic research in its own terms. In effect, I view the basic vocabulary and the constructs of linguistics—its lexical features, clause structures, island constraints, argument structures, landing sites, constraints on derivation, and so on—as theoretical devices to give an account of at least part of the organic world, namely, the human grammatical mind, and perhaps much more. More specifically, one should be allowed to draw a tree diagram and claim that it describes a state of the brain.

Returning at this point to the period in the history of chemistry mentioned above, recall that chemistry was viewed as a mere "calculating device" on the grounds that it could not be unified with physics. The gap seemed unbridgeable essentially because the chemists' matter was discrete and discontinuous, the physicist's energy was continuous (Chomsky 2001b). Under the assumption that the physicist's view of the world is "basic" at all times, it is understandable that chemistry was viewed as "unreal." However, as Chomsky has repeatedly pointed out in recent years, the gap was bridged by unifying a radically changed physics with a largely unchanged chemistry. Analogically, from what we saw about the current state of biological research on cognition and behavior, it is possible that a "radically changed" biology, perhaps on the lines sketched above, will unify with a "largely unchanged" linguistics. Since the likelihood of such biology is remote, all we have in hand is the body of linguistic research itself.

Chomsky drives the point by citing what he calls the "localist" conception of science attributed to the eighteenth-century English chemist Joseph Black: "Let chemical affinity be received as a first principle . . . till we have established such a body of doctrine as [Newton] has established concerning the laws of gravitation" (quoted in Chomsky 2000d, 166). Thus, chemical research proceeded along a different path from physics, and the gap between the disciplines widened.

In my opinion, the gap between biology and linguistic research is even wider. After all, the chemist's view of matter as discrete and discontinuous was not something unheard of in (earlier) physics. Newton himself was a "corpuscularean" about many aspects of nature in the sense that he thought that all matter in the universe is made up of the same "building blocks." So, for him, even something as ethereal as light also consists of "particles," a view confirmed from a wholly different direction two centuries later. In that sense, in adopting the chemist's view of matter, (later) physics was reconstructing a part of its own past.

Furthermore, the disciplines did get unified. It could mean either that something totally unexpected happened, or that the disciplines were "proximal" for centuries for this to happen eventually. The bare fact that this form of unification is a very rare event in science lends support to either interpretation. Still, the second option of proximality is more plausible since the first involves a miracle. In contrast, although the general idea of biolinguistics goes back to ancient times in many traditions, there is no record of "proximity" of the disciplines of linguistics and biology—just the opposite in most cases, as we have seen. In general, if

there is exactly one case of large-scale unification in the whole of science, it is natural to expect that there is a history to it, which does not often repeat itself.

Pursuing the point a bit further, it seems that although physics and chemistry became separable bodies of doctrine at some point, there was some conception of a unified picture throughout. That is, some conception of properties of more "basic" elements combining to produce both physical and chemical effects guided much research for centuries. In this connection, the issue of John Dalton's professional identity is interesting: Was Dalton a chemist or a physicist? The *Britannica Micropaedia* article on Dalton (vol. 3, 358) lists him as both a chemist and a physicist; so does the main *Macropaedia* article (vol. 5, 439). The *Macropaedia* article on the history of the physical sciences, however, lists him as a chemist only. But the section on chemistry in this article (vol. 14, 390) begins as follows:

> Eighteenth century chemistry derived from and remained involved with questions of mechanics, light, and heat as well as iatrochemistry and notions of medical therapy. . . . Like the other sciences, chemistry also took many of its problems and much of its viewpoint from [Newton's] *Opticks* and especially the "Queries" with which that work ended. Newton's suggestion of a hierarchy of clusters of unalterable particles formed by virtue of the specific attractions of its component particles led directly to comparative studies of interactions and thus to the table of affinities.

Following these remarks, the article goes on to view Joseph Priestley's work on chemical affinities as continued explorations of Newtonian queries. This is exactly what I had in mind about the "proximity" of chemistry and physics. In light of these considerations, the alleged "divergence" between physics and chemistry as separable "bodies of doctrine" could be viewed as a late-nineteenth-century construction motivated largely by the temporary decline of corpuscular theories and the rise of wave mechanics in physics. Similarly, the alleged "convergence" of the physical and chemical in the postquantum era could be a twentieth-century construction based on the revival of "corpuscular" theories in physics. Chemistry, as Chomsky emphasized, remained essentially unchanged throughout. It is at least questionable how much weight should be placed on these temporary phases to form a general conception of science as it develops over centuries.

In fact, without this background continuum, the concept of body of doctrine with its "locality" does not make clear sense, just as clusters of South Pacific land masses are "islands" in the context of the continuum of the ocean. I have no problem with the concept of body of doctrine

that separates physical, chemical, biological, geographic, and so on with respect to a general conception of science emanating from a unitary source, here Newton. Otherwise, the concept just seems to label any inquiry whatsoever (astrological, sociological, economic, etc.) and is, therefore, without empirical force.

In sharp contrast, there is neither any historical effort nor any contemporary evidence for us to be able to place studies on language somewhere in this continuum. There are two crucial points to this. First, language theory is envisaged here entirely in terms of its object, which is an abstract computational system with certain output properties with, I think, a possible range of application across related domains such as music, arithmetic, and logical reasoning: this object is called "grammar." It does not include the conceptual system, and its operations are fairly "blind" with respect to the band of information it computes on. We will see all this as we proceed. Second, the most amazing fact is that language theory is available in the Galilean style. As long as language theory was not there, we had some loose "philosophical" conception of a domain that also did not belong to the Newtonian continuum precisely because it was not a science at all. So, a very different issue opens up once the Galilean style began to apply to language, and language theory in its recent form emerged. On the one hand, the availability of the Galilean style surely signals the arrival of a science of language; on the other, this arrival has had no historical link with the only scientific continuum in hand, namely, the Newtonian one.

In fact, there is a sense in which there indeed is a "continuum" in which to place language theory, as hinted in section 1.1. The curve begins with, say Pāṇini, and continues through Aristotle, Port Royal, von Humboldt, Saussure, Turing, and so on, to lead to generative grammar. The continuum could well be called the "generative enterprise," in a wider sense. No doubt, this continuum, unlike the Newtonian continuum, is an abstract conception without direct historical-textual lineage. Its themes originated more or less independently in different textual traditions in India and Europe; the spirit of high ideas knows no boundary. With the intervention of German "orientalists," we can also think of the two traditions converging at Saussurean linguistics in the late nineteenth or early twentieth century. This enterprise flourished essentially independently of, and parallel to, the Newtonian continuum more or less throughout. The emergence of language theory shows, in afterthought, that it had always been a scientific enterprise. With two continuums in hand, various possibilities for unification arise. The present point is that such unification is

likely to be very different in structure from the ones *within* the Newtonian continuum.

Nevertheless, Black's "isolationist" conception of science still holds fairly decisively with respect to his original example: Newton's theory of gravitation. It is well known that the postulation of universal gravitation immediately raised a storm of controversy strikingly similar to contemporary controversies around Universal Grammar. Much reordering has happened in science since Newton's formulation of the theory over three hundred years ago. Newton's original conception ("action at a distance") was replaced by the concept of a gravitational field. The concept of field was extended to the phenomena of electricity and magnetism, which were subsequently unified under Maxwell's laws. More recently, there has been further unification within physics of electromagnetic forces with forces internal to the atom. As noted, physics was unified with chemistry, and chemistry with molecular biology. Through all this turbulent history, gravitation remained an enigma; the concept just could not be coherently accommodated with the rest of physics (Held 1980; Hawking and Israel 1987, for history of gravitation theory). It continues to be a problem even in the recent advances in quantum field theory, the most general unified theory in physics currently available (Cao 1997).

Roger Penrose (2001) formulates the general problem as follows. According to him, quantum theory and gravitation will be properly unified—that is, we can expect gravitational effects at the quantum scale—only in a "new physics"; the current scales of quantum theory and relativity theory are insufficient. If we take a free electron (current scale of quantum theory), we get the relevant quantum effects, but the gravitational effects are too small. If we take a cat (current scale of relativity), quantum theory produces paradoxes. So we settle for an intermediate scale, say, the scale of a speck of dust: "With a speck of dust you can start to ask the question, 'could a speck of dust be in this place and in that place at the same time?'" The point is that a speck of dust is not in the domain of either quantum theory or relativity theory.

Pushing the analogy with gravitation further, it is of much interest that, for several centuries after Newton postulated the force, the physical character of gravitation, even in the field version, remained an enigma. That is, although the properties of gravitation itself were mathematically well understood and its empirical effect widely attested, no one really knew what it means for the physical universe to contain such a force. Albert Einstein finally characterized universal gravitation in terms of other parameters of the physical universe, namely, the spatial properties of

bodies. But, as Penrose's remarks suggest, in so doing he had to construct a theory that just would not mesh with the rest of physics. In other words, although Einstein explained the "evolution" of gravitation (from bodies), the theory of evolution he needed could not be arrived at from some theory of proto-gravitation already available in existing physics.

There is a case, then, in which a body of doctrine has resisted unification with the rest of physics for over three hundred years, despite some of the most imaginative scientific reflections in human history. Note also that this problem has persisted in research essentially on "outer" domains. In contrast, the unification problem facing biolinguistics arises for an "inner" domain. Here a solution of the unification problem requires not only a radical shift in domains, but also that the purported solution works *across* "inner" and "outer" domains. It can only be ancient prejudice that the entire body of biolinguistics is often dismissed on the grounds that it does not come armed with a certificate from existing theories of organic evolution.

Biolinguistics is a body of doctrines that is likely to remain isolated, in the sense outlined, from the rest of science far into the future. To emphasize, this conclusion is based on the history of science, namely, that the problem of unification between psychological studies and biology is as unresolved today as it was two centuries ago.[12] The crucial recent dimension to this history is that psychological studies now contain a scientific theory, so there is a genuine partition in science. I will draw on this perspective a lot in what follows.

1.3.3   A Mind-Internal System

In the meantime, we can ask other questions about biolinguistics. Given its (current) isolation, a natural question is, "What is its reach?" This question can be rephrased as follows. According to Jenkins (2000, 1), the biolinguistics program was supposed to answer five basic questions: (1) What constitutes knowledge of language? (2) How is this knowledge acquired? (3) How is this knowledge put to use? (4) What are the relevant brain mechanisms? (5) How does this knowledge evolve? How many of these issues are within the reach of current biolinguistic inquiry?

In view of the state of the unification problem, it is clear that substantive answers to questions 4 and 5 are currently beyond reach. This does not mean that no answers are available, especially for 4. For example, one could simply take the constructs of linguistics to be properties of brain states as currently understood, and proceed from there; some of that could already be happening in the brain-imaging literature. To con-

sider just one case, it is suggested that a system of neurons executes what is known in linguistics as the "trace deletion hypothesis" (Grodzinsky 2000). Similar proposals are routine in physics; geodesics and potentials are viewed as located all over the universe. Yet, the difference between physics and linguistics is that the universe is what physics says it is; there is no other account of the universe once we grant that scientific understanding is limited to its intelligible theories. But the brain is not what linguistics says it is.[13] There are independent electrochemical-microbiological accounts of the brain on which these images take place. These accounts do not explain what it means for the trace deletion hypothesis to be executed there (Smith 2000). As for answers to question 5, until explanations are available via a "radically changed" biology envisaged above, what we have in hand for now, according to Chomsky (2002), are more like "fables" and "stories."

Substantive work in biolinguistics, through all its phases, has been basically concerned with question 1: "What constitutes knowledge of language?" In the generative enterprise, this question was pursued in an interesting symbiosis with question 2, the issue of acquisition of language. As noted, the enterprise was directly concerned with what is known as the "problem of explanatory adequacy": "languages must somehow be extremely simple and very much like one another; otherwise, you couldn't acquire any of them" (Chomsky 2000a, 13). Question 1, therefore, was taken to be shorthand for "What constitutes the knowledge of language such that Plato's problem is solved in this domain?" In that sense, answers to question 1 have an immediate bearing on question 2: we would want to know if the abstract conditions on acquisition, which are postulated to answer question 1, are in fact supported by, say, child data. Thus within the computational-representational framework, interesting answers to question 2 flow directly from substantive answers to question 1 (Crain and Pietroski 2001 and references). However, beyond this framework, the problem of unification affects answers to question 2 insofar as we expect the issue of acquisition to be addressed in terms of the physical mechanisms of organisms.

These remarks extend to question 3 as well. If the issue of language use concerns mechanisms, not to speak of actions, the unification problem blocks substantive answers. However, subtle questions of language use can be posed within biolinguistics itself. Recall that the design problem for the language faculty was posed in terms of language use: To be usable, the expressions of the language faculty (at least some of them) have to be legible to the outside systems. It looks as though the computational

system of the faculty of language is, in a way, sensitive to the requirements of the conceptual-intentional systems—the "thought" systems.

For example, to meet its own conditions of optimality, the computational system sometimes places a linguistic object in a location where pragmatic conditions are also satisfied. Consider the so-called passive constructions in which the Object of the main verb moves to the Subject position: *John read the book → The book was read by John*. This is a purely grammatical phenomenon that I will look at in some detail from different directions later. Now consider the following sentences (Pinker 1995a, 228): *Scientists have been studying the nature of black holes. The collapse of a dead star into a point perhaps no larger than a marble creates a black hole*. The trouble with the "active" second sentence is that there is a discontinuity of topic with the first. The first sentence has introduced the topic of black holes and the second should take off from there. This is achieved by the passive sentence: *A black hole is created by the collapse of a dead star into a point perhaps no larger than a marble*. In that sense, substantive answers to question 1 also lead to interesting answers to question 3.

Furthermore, these results, apart from suggesting a unified approach to questions 1–3, also offer a theoretically motivated division of the concept of language use. Pretheoretically, it is natural to view the question of language use thickly in terms of whatever it is that we do with language: express thoughts and feelings, talk about the world, ask questions, get someone to do something, and so on. In this picture, there is a cognitive system called "language" that we put to use in the external social setup to enable us to do these things. Therefore, question 3 ("How is this knowledge put to use?") is classically viewed as a question about, say, how we talk about the world: the problem of reference.

As we just saw, a very different set of issues emerges when we frame the question of language use in terms of satisfaction of legibility conditions at the interfaces. Here the question, roughly, is about language-external but mind-internal systems that not only immediately access but also (partly) influence the form of the representations constructed by the language faculty. In this sense, in meeting the legibility conditions, language has already been put to use! But this concept of use, restricted to mind-internal systems, need not appeal to any concept of use that involves, say, the concept of reference; in other words, the question of how language is related to the rest of the world to enable us to refer is now delinked from the question of how language relates to mind-internal systems. This is fundamental progress, and it was achieved by answering

question 1 in minimalist terms. From this perspective, the classical problem of Intentionality—how language relates to the world—basically falls out of biolinguistics.

Is there a meaningful problem of Intentionality outside of biolinguistics? Chomsky's (2000d, chapters 2 and 7) basic position is that words do not refer, people do; in fact, "I can refer to India without using any word, or any thought, that has any independent connection to it." "It is possible," Chomsky (2000d, 132) suspects, "that natural language has only syntax and pragmatics"; "there will be no provision" for what is assumed to be the "central semantic fact about language," namely, that it is used to represent the world. I return to these issues in chapter 3.

Chomsky's conclusion may be viewed as a rejection of what Jerry Fodor and Ernst Lepore have called "Old Testament semantics": "Semantic relations hold between lexical items *and the world* and *only* between lexical items and the world." According to Fodor and Lepore (1994, 155), there is no semantic level of linguistic description: "The highest level of linguistic description is, as it might be, syntax or logical form." Roughly, the claim is that the output of grammar, LF, is *not* a semantic level.[14] I will question this conception of LF in some detail in this work to argue that LF itself may be viewed as a (genuine) semantic level.

Keeping to Fodor's conceptions of syntax and semantics, much of Fodor's recent work (Fodor 1994, 1998) may be viewed as a defense of Old Testament semantics—the study of language-world connections—against any other form of semantics such as those involving conceptual roles, exemplars, prototypes, and the like (Murphy 2002): call them "New Testament semantics." If Fodor is right in his rejection of New Testament semantics, and if Chomsky is right in rejecting Old Testament semantics, no intelligible concept of semantics survives outside the internalist concept of language use proposed in biolinguistics (Bilgrami and Rovane 2005). Beyond biolinguistics, vast gaps of understanding surround studies on language and related mental aspects of the world, even when we set aside the various dimensions of the unification problem.

We now have some idea of the respects in which biolinguistics is isolated from the rest of human inquiry, including other inquiries on language. The twin facts of the isolation and the scientific character of biolinguistics raise the possibility that biolinguistics may have identified a new aspect of the world. I assume that we talk (legitimately) of an aspect of the world only in connection with a scientific theory of an advanced character with the usual features of abstract postulation, formalization, depth of

explanation, power of prediction, departure from common sense, and so on. This is what Black's notion of a ''body of doctrines'' implies, in my opinion. Chemical, optical, and electrical count as bodies of doctrines because, in each case, there is a cluster of scientific theories that give a unified account of a range of processes and events that they cover: the broader and more varied the range, the more significant the appellation ''body of doctrines.'' With the exception of rare occasions of unification, science typically proceeds under these separate heads, extending our understanding of the aspect of the world it already covers. Thus, not every advance in science results in the identification of a new aspect of the world.

It follows that, since biolinguistics is a science, it extends our understanding of some aspect of the world. However, since it is isolated from the rest of science, the aspect of the world it covers does not fall under the existing heads; therefore, biolinguistics has identified a new aspect of the world. We need to make (metaphysical) sense of the puzzling idea that the object of biolinguistics stands alone in the rest of the world.

The obvious first step to that end is to form some conception of how biolinguistics works. As noted, biolinguistics attempts to solve a specific version of Plato's problem with explicit articulation of the computational principles involved in the mind-internal aspects of language, including aspects of language use that fall in this domain. We have to see how exactly meaningful solutions to Plato's problem are reached within these restrictions.