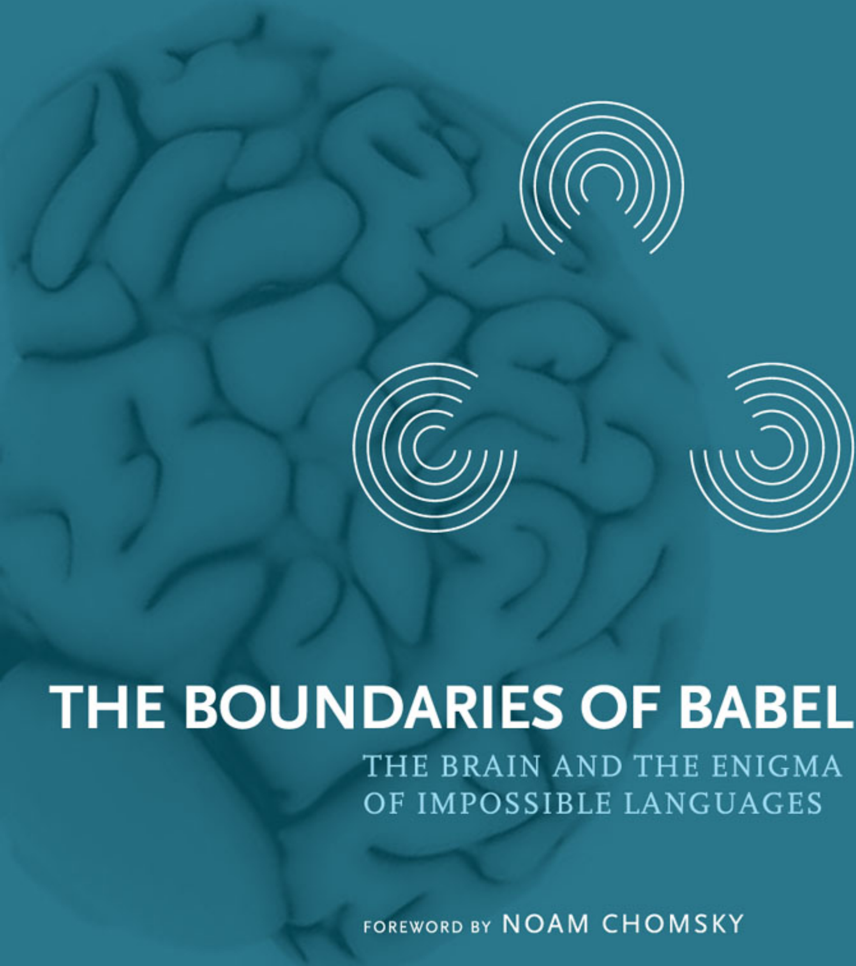
ANDREA MORO



THE BOUNDARIES OF BABEL

THE BRAIN AND THE ENIGMA OF IMPOSSIBLE LANGUAGES

FOREWORD BY NOAM CHOMSKY

The Boundaries of Babel

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The Boundaries of Babel

The Brain and the Enigma of Impossible Languages

Andrea Moro

Translated from Italian by Ivano Caponigro and Daniel B. Kane

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This book is dedicated to all the beautiful things that come to an end because they prove to me that I am looking for one that does not.

Dumtaxat rerum magnarum parva potest res exemplare dare et vestigia notitiai

So far as it goes, a small thing may give an analogy of great things, and show the tracks of knowledge.
—Lucretius, *On Nature*

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Foreword

The modern study of language within a biological context began to take shape in the 1950s. In 1967, a now classic work—Eric Lenneberg's *Biological Foundations of Language*—laid a substantial basis for the emerging discipline. Many inquiries, international conferences, seminars, and other studies followed. Nevertheless, until fairly recently the "biolinguistic perspective," as it soon came to be called, remained to a large extent an ideal and a framework for posing problems and pursuing inquiry into them. That proved highly productive, but the core of the discipline remained difficult to explore. The development of imaging technology has offered promise to enrich these inquiries in novel ways, but designing experimental work that will link neural processes to basic properties of language has proved to be a daunting challenge, not surprisingly.

Andrea Moro has gained a unique position in formulating and implementing constructive approaches to these difficult and demanding tasks. He is able to address them with a deep understanding of modern linguistics, a field to which he has made major contributions of his own, and mastery of the relevant technology and its potential. His new book is a lucid introduction to these exciting areas, superbly informed and imaginatively presented, with intriguing implications well beyond biolinguistics: for the cognitive sciences generally as well as for philosophy of language and mind. What is more, it should be accessible to the interested general reader, and is sure to be of great interest to professional researchers in all the related disciplines, a rare achievement in itself.

Noam Chomsky
January 2008
Cambridge, Massachusetts

Translators' note

We would like to thank the author for his close collaboration and precise editorial input throughout the entire translation process. We are also grateful to Adam Albright, Eric Bakoviæ, Grant Goodall, and Sharon Rose for their help with various linguistic issues.

Ivano Caponigro
Daniel B. Kane

Preface

This is not a neuroscience or linguistics textbook. Many good textbooks on those subjects are available.[1] This book is the history of an encounter of two cultures: linguistics and the neurosciences (or, more precisely, the cognitive neurosciences).[2] It is also the attempt to expose a "hidden" revolution in contemporary science: *the discovery that the number of possible grammars is not infinite and that their number is biologically limited.* I say "hidden" because, despite the fact that concepts as difficult and revolutionary as natural selection and black holes have made their way into the public discourse, little has been said about this radical change in the way we look at language, a change that is no less surprising than the discovery of black holes. It requires a rethinking not just of the fundamentals of linguistics and the neurosciences but also of our view of the human mind.

Together we will move forward in search of the boundaries of Babel,[3] the neurobiological constraints on the apparent chaotic variation of

1. For a general introduction to linguistics, see Akmajian et al. (1995); Haegeman (1997); Roberts (1997); Chierchia and McConnell-Ginet (2000); Fromkin (2001); Carnie (2006); Fromkin, Rodman, and Hyams (2007). For an overview of neuropsychology see Denes and Pizzamiglio (1999) and Kandel, Schwartz, and Jessel (2000); for a general introduction to linguistic issues and interactions with other sciences see Chomsky (1988, 2004); Jackendoff (1993); and Pinker (1994). For a critical and detailed survey of the notion of possible language across different theoretical perspectives see Newmeyer (2005).

2. I use the label "cognitive neurosciences" and not "cognitive neuroscience," because I'm referring more to the heterogeneous group of methodologies than to a specific field. This is in accordance with Marconi's (2001) distinctions between "cognitive science" and "cognitive sciences."

3. Babel is the original name of the city of Babylon cited in the Bible and in the Koran. It has different etymologies: it can either be derived from Akkadian Bab-Ilu (the gate of God) as a translation from Sumerian Ka-dingir; the connection

human languages. I will try to give the essential elements so that readers who are not experts in linguistics or the neurosciences can grasp this revolution. In order to do this, I will give a short summary of some of the fundamental results from linguistics research in the last fifty years. I will also describe two recent neuroimaging experiments that I was fortunate enough to take part in. Finally, I will present a line of language research where the impact of our biological structure is crucial.

The book is organized into three chapters. In each part I present only the essentials—a collection of samples—for linguistics and the neurosciences. In chapter 1, "Hidden Texture," I start with a methodological discussion, and then I introduce some fundamental aspects of human languages. In the second part, "Language in the Brain," these fundamental aspects of language will be used to understand two brain experiments. The presentation of the experiments is preceded by a brief sketch of the two fundamental neuroimaging techniques they make use of: positron emission tomography (PET) and functional magnetic resonance imaging (fMRI). This section is crucial for understanding the limits and potentials of these new neuroimaging techniques. We will see how, if a sound theoretical framework is lacking, techniques and machines cannot provide interesting data.

Chapter 3, "The Form of Grammar," is speculative: I discuss some general consequences of a peculiarity of human languages at the intersection of biology and linguistics that is emerging as one of the dominant themes of contemporary research: the connection between the linear nature of the linguistic code and grammatical rules. The book's structure allows readers with expertise in linguistics to skip the first chapter and go directly to the second chapter. Readers who are mostly interested in linguistics issues can jump from the first chapter to the third chapter without going into the issues of neurobiology covered in the second chapter.

It is, of course, up to the reader to decide whether this journey between grammar and brain in search of "boundaries of Babel" is convincing. My minimum goal is to convey the same amazement and curiosity that I felt when I first considered the following simple question: Why aren't all the grammars that we can conceive realized?

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with the history of the fall of the tower in chapter 11 of the Genesis and the spread of different languages after the original substantial unity has yielded to a false etymology tracing it back to the Hebrew verb *balal* (confound). By coincidence, it is often pronounced in English as the word *babble*, also referring to linguistic action, like irrelevant chatter or murmuring.

Acknowledgments

Translating this book has changed its style, its content, sometimes even its scope and goals: in fact, it is a new book. Still, behind these pages there are all the people who allowed me to have the ideas that I had, bad or good. Noam Chomsky and Richard Kayne, first of all, who gave me the *raison d'être* of my work, and more profoundly, of some crucial choices I made in my life; and then the virtual community that has provided me the ideal environment to pursue these ideas all through these years: Edoardo Boncinelli, Stefano Cappa, Gennaro Chierchia, Caterina Donati, Giorgio Graffi, Giuseppe Longobardi, Jacques Mehler, Daniela Perani, Orin Percus, Massimo Piattelli-Palmarini, Luigi Rizzi, Alessandra Tomaselli and Marco Tettamanti. I am also very grateful to Rita Levi-Montalcini for the unexpected and generous encouragement that she gave me. Special thanks to Cristina Musso for her enthusiastic support in carrying out the experiments we made in Germany under the coordination of Cornelius Weiller. I am also indebted to Ivano Caponigro and Daniel B. Kane for their effort in translating this book and, I am sure, in forcing me to make a better one: better, not perfect; for a perfect one there are not enough languages to translate it into. Some friends of mine helped me to improve it with their careful reading and many stimulating ideas: Umberto Manfredini, Franco Bottoni, Marco Vigevani, Tom Stone, and my father, Angelo, who doesn't need to read this new version of the book, for the enigma of Babel has surely been solved where he is now.

The Boundaries of Babel

Prologue

In 1811, Monsieur Leborgne arrived at the Bicêtre Hospital in Paris. He was a twenty-one-year-old man who exhibited an unusual linguistic problem: Whenever he was asked a question, he would always answer by saying one syllable twice, *tan tan*, in conjunction with quite varied intonation and expressive gestures. For this reason the patient was nicknamed Tan-Tan, or sometimes simply Tan. He spent the remainder of his life hospitalized. Through the years his condition deteriorated, until, eventually, the limbs on his right side were paralyzed. On April 12, 1861, he was transferred to the surgery ward to be treated for gangrene. It was then that Pierre Paul Broca, a doctor who worked at the hospital, met him.

A little more than one week earlier, on April 4, Broca had participated in a meeting of the Anthropological Society, which he himself had founded two years earlier. During this meeting, another doctor, Ernest Auburtin, presented some interesting studies about the possibility of pinpointing the location of language in the human brain. Auburtin was attempting to support the hypothesis that the brain did not work as a homogenous mass, at least when it came to higher functions such as language. At that time, this hypothesis was supported by a minority of the Society's members that included Auburtin's father-in-law, Jean-Baptiste Bouillaud, who had been a student of the founder of phrenology, Franz Joseph Gall. Phrenology was the study of the psychological characteristics of an individual based on the external shape of the cranium. Phrenology never yielded acceptable scientific results, but it had left a strong impression on Bouillaud, who became convinced that the ability to speak was located in a specific area of the brain, the frontal lobes. Although Aubertin did not bring any conclusive evidence in favor of the hypothesis that specific areas of the brain are dedicated to specific functions, his efforts helped to keep this hypothesis alive.

It did not take long for Broca to realize that the case he had stumbled upon constituted strong evidence to decide the scientific controversy. Although exactly how much Tan-Tan understood could not be determined with absolute certainty, he clearly was able to understand almost everything. He could count and understand time. And, despite the fact that the right side of his body was paralyzed, neither his tongue nor his facial muscles had been affected by the paralysis. In other words, Tan-Tan did not lack the cognitive or motor skills necessary for talking. Nevertheless, the only utterance that came out his mouth was just "tan tan." His problem must have been, therefore, a language-specific impairment. Also, the anamnesis for the first few years of Tan-Tan's disease showed, crucially, that lack of language was his *only* evident deficit at that time; the paralysis of his limbs occurred later.

Tan-Tan died on April 17, 1861, and Broca did an autopsy of his brain. After careful examination, he reached the conclusion that a lesion in Tan-Tan's left frontal lobe must have been the cause of his loss of language. Broca had discovered the first anatomic evidence for localization of a specific brain function. Shortly thereafter, Broca gave a talk at the Anthropological Society that would change our conception of how the brain works: "Loss of Speech, Chronic Softening, Partial Destruction of the Left Frontal Lobe of the Brain" (Broca 1861). Not all of Broca's colleagues accepted his conclusions, and some, such as the famous neurologist Pierre Flourens, continued to argue that the brain's high functions could not be localized in any specific area of the brain. But by that time the road to studying the biological basis of language had already been taken.

A little less than a century later, in 1957, at the Massachusetts Institute of Technology in Cambridge, Massachusetts, a young professor by the name of Noam Chomsky had just published a short monograph based on his ponderous doctoral thesis (Chomsky 1957), which as yet had no publisher; in the end it took nearly twenty years for his entire thesis to be published (see Chomsky 1975b). Chomsky found himself in a particularly favorable cultural environment: his father, a famous Hebrew scholar, had introduced him to linguistics before turning him over to another famous linguist, a Russian, Dr. Zelig Harris, at the University of Pennsylvania, which Chomsky entered at the age of sixteen. In those years (partly spent at Harvard as a Harvard junior fellow), in addition to pursuing his linguistics studies, Chomsky was exposed to logic, mathematics, and theories of computation, which brought him in touch with the thinking of Alan Turing, a British mathematician who, among other things, was responsible for a rigorous definition of what an algorithm is.

Furthermore, in the fifties interesting communication theories were being developed at nearby MIT, where Chomsky started teaching in 1955. In particular, Claude E. Shannon's information theory was circulating, according to which the grammars of human languages could be interpreted using statistics. At that time Chomsky was also working at MIT in the Research Laboratory of Electronics (RLE), where an effort was under way to try to build machines for automated translation. The results of the research at RLE were making people hope for a quick method for automated translation (especially from Russian—it was the Cold War), automated archiving of printed material on the basis of content, and automated writing of abstracts for archived papers. These ambitious goals, which at that time were assumed to be reachable, also triggered crucial research projects focused on understanding how human thinking works: "There was an ubiquitous and overwhelming feeling around the Laboratory that with the new insights of cybernetics and the newly developed techniques of information theory the final breakthrough towards a full understanding of the complexities of communication 'in the animal and the machine' had been achieved" (Bar Hillel 1970, 294).

Chomsky immediately realized that this conception of language was not acceptable and that human minds could not be assessed as machines. Language cannot be represented as just a sequence of symbols regulated by statistical rules. Linguistics is a science like all other empirical sciences: experiments are necessary to obtain results. Theory cannot be derived from data alone. Like a falling rock, which does not have the law of gravity written on it, a speaking person, when he utters a sentence, does not also utter the rule of grammar that governs it. With a series of indestructible arguments—drawing on the rigorous methods he learned from his studies of abstract formal systems (involving, for example, recursive nested dependencies)—Chomsky showed not only that the structure of grammar is more complex than the statistically based model, but that complexity itself immediately raised a fundamental problem concerning language acquisition in children: "The fact that all normal children acquire essentially comparable grammar of great complexity with remarkable rapidity suggests that human beings are somehow specially designed to do this, with data-handling or 'hypothesis-formulating' ability of unknown character and complexity" (Chomsky 1959, 57).

The jump had occurred: linguistics could no longer ignore the problem of language acquisition; linguistics in fact had to consider the specific biologically determined "design" that allows human beings to develop this capacity. The path had opened for the study of the formal properties that characterize all and only human grammars, the so-called "Universal Grammar."

In January of 1962, a child was born in England and his parents named him Christopher. Six weeks after his birth, he was diagnosed with brain damage that would have major consequences for the rest of his life. He learned to speak and walk late, but from the age of three on he had a burning passion for books. Unlike most children, he did not favor illustrated books or fairy tales. He liked reading dictionaries, phone books, and books illustrating the flags and currencies of the world. His parents were left astonished when they realized, that, around the age of three, Christopher was already able to read the advertisements printed in local newspapers. Even more strange, he could read them, irrespective of their position: upside down, right side up, or sideways. A next remarkable step in Christopher's development occurred when he first encountered technical papers written in foreign languages. From that moment on, learning foreign languages became his absolute passion. Any occasion was good for learning a new language, which among other things, made him locally famous. Christopher's talent was exceptional—for instance, it was enough for him to hear his brother-in-law speak Polish for him to learn it. What had happened to Christopher that could explain his behavior? Clinically, his pathology was never diagnosed with certainty, although a careful analysis of the patient showed some typical characteristics of autism, such as insensitivity to irony. The results of the most common intelligence tests showed that Christopher was well below average.

Neil Smith and Ianthi-Maria Tsimpli teach linguistics at University College, London, and the University of Newcastle-on-Tyne, respectively. The meeting between the two linguists and Christopher would produce unique results with implications for the study of the relationship between mind and language. First of all, Christopher's mental development was unique. It was already known, of course, that there are some people who develop exceptional mnemonic skills—such as memorizing entire calendars or big phone books, and others who are able to perfectly perform virtuoso pieces on a musical instrument or render extremely complex drawings. People with these exceptional skills are often unable to live a normal life or simply take care of themselves. They are often autistic, and even more often, demonstrate impaired linguistic skills. This is why

Christopher's "talent" was already exceptional by itself: it was exactly his linguistic capacity that had developed in an extraordinary way. Other cases of people with great linguistic skills accompanied by cognitive deficits were known by that time, such as the cases described by Giuseppe Cossu and John Marshall (1986); these people, however, spoke only one language. Christopher was the first recorded case in which a dissociation between language and other cognitive skills coincided with the knowledge of many languages and the ability to learn new ones.

The meeting between two linguists and Christopher yielded a crucial experiment. By that time, in addition to his first language, English, Christopher knew, with varying degrees of competence, Danish, Dutch, Finnish, French, German, Modern Greek, Hindi, Italian, Norwegian, Polish, Portuguese, Russian, Spanish, Swedish, Turkish, and Welsh. It would not have added much to what we already knew about Christopher to try to test him while he was learning another new language. For this reason, Smith and Tsimpli came up with a radically different experiment. The idea that inspired them is found in Chomsky (1991, 40), quoted in Smith-Tsimpli (1995, 137): "Knowing something about [universal grammar], we can easily design 'languages' that will be unattainable by the language faculty," where Universal Grammar means, as already mentioned, the set of properties that characterize all and only human languages. With this, Smith and Tsimpli knew how to proceed: they invented a vocabulary and a grammar, called this language Epun, and included "impossible" rules—that is, rules that violate the properties of Universal Grammar. The result left no doubt: no matter how hard he tried, Christopher could not learn the impossible rules—unlike control subjects who were able to use their general intelligence to learn them. The mechanisms of general intelligence that the control subjects used, in fact, had nothing to do with spontaneous language learning. On the other hand, Christopher, who could only count on his language faculty, was not able to correct himself, thus providing unequivocal psychological evidence for the distinction between possible rules and impossible rules based on mere linguistic generalizations.

In the century between Tan-Tan and Christopher, our knowledge of the structures of language advanced enormously, perhaps more than ever before in the history of linguistics. Moreover, thanks to techniques that combine radiology and computer science, we are now able to explore the functional architecture of the brains of healthy human subjects in vivo.

We no longer need to wait for disease or damage to provide us with research material, nor do we need to limit ourselves to doing autopsies to explore how the brain works.

The journey that this book will take you on starts here. We will return to Broca, once we are armed with contemporary knowledge of linguistics. Then we will see how *the distinction between possible and impossible rules has not only psychological relevance, but also specific neuropsychological relevance.* We will do so by looking at the fundamental aspects of two neuroimaging experiments that I designed with the essential collaboration of different teams of researchers. The first experiment provided evidence of the autonomous nature of syntax with respect to the brain's activities by isolating a dedicated neuronal net. The second experiment showed us how such a net is able to recognize only the possible rules while it ignores the impossible rules. Both experiments made use of invented languages: in these two experiments, the discovery of what is real was guided by the unreal.

The winner of the Nobel Prize for Medicine or Physiology in 1969, Salvador E. Luria, used to say that in the end, every discipline is characterized by the ten or so fundamental experiments that are at its foundation. I cannot say for sure that the experiments described here will be counted among the top ten experiments in the discipline that has been called biolinguistics, but I hope that they will at least contribute to a better understanding of what such an experiment will need to be.

1 Hidden Texture

There are few people who haven't stopped at least once to think about language for a moment. Every day, our brains hear and produce sentences continuously. Even now, your eyes are following a string of black signs that conveys ideas and images that were produced by a different brain in a different place at a different time. If I wanted, I could, simply by writing, activate images in your mind that may not have been there before: *A long line of lizards crossed the desert without even stopping to dream*. It is quite unlikely that you have already encountered this sentence. Nevertheless, the image was created in your mind with no effort, just by your scanning that string of black symbols.

How does this information-transmitting "code" work? Terms and concepts from your school days may come to mind, perhaps only vaguely: "grammar," "verb tense," "helping verbs," "subjects," "predicates" and so on. For many, studying language in this way was nothing but a boring task of labeling—about as exciting as eating stale bread. For some, however, language is a never-ending source of amazement: What do we know about such a complex system today? How did we acquire our language ability as kids? Why is it so difficult to learn a foreign language as an adult? What makes human language different from the languages of the other animals? What are the structures in our brains that are responsible for language? In this book we will consider some aspects of these questions. We will see how the analysis of language is not just a dry description of entangled regularities but a major path to the understanding of crucial properties of human nature. In particular, we will focus on a fascinating enigma that is now just beginning to be unraveled: *Why aren't all the grammars that we can think of realized across the languages of the world?* We will see how modern brain-imaging techniques can help us answer this question. We will have to proceed step by step, and make an effort to "forget" some of the notions we learned in school by giving them new definitions and new substance. We will need to "disassemble" what we already know in order to reassemble it according to a new schema. This schema is more abstract and better suited to capturing aspects of language that have, in the last fifty years, revolutionized our way of thinking about language. Let's start with the notion of grammar.

For many of us, grammar is an analysis of words that makes reference to common linguistic categories such as nouns, verbs and articles—called "parts of speech." These categories are in contrast to the categories used in the practice of diagramming sentences, a method of analysis that includes categories such as subject, predicate, modifiers, objects, and so forth.[1] If we return to the sentence about the lizard, at the start of the chapter, we could say that, according to an analysis based on parts of speech, *a* is an indefinite article, *lizards* is a common plural noun describing an animal, and so forth. If, on the other hand, we wanted to diagram the sentence, one way to do it would be to say that *a long line of lizards* is the subject, *of lizards* is a modifier within the subject, and so forth. These ideas constitute a conventional notion of what grammar is.

In this book, however, we will consider a more abstract notion of "grammar." We will not limit ourselves to parts-of-speech analysis or sentence diagrams. Instead, we will try to understand how grammar works, as if we were looking at a clock whose inner workings we want to

1. These terms have a long tradition that goes back to classical Greek thought. "Subject," for example, is the translation made by the fifth-century A.D. Christian philosopher Boethius, who wrote the *Consolatio Philosophiae* while waiting to be executed in Pavia. Literally, the term means "that was thrown under," hence "that stays under" (analogous to the word *understand* in English). It is the direct translation into Latin of the ancient Greek *hypokeimenon*. Why should something that "stays under" be a "subject"? Recall that the notion of substance itself has the same etymology. Roughly, this is because, in the ontological model that was largely elaborated in Aristotelian thought, the idea was that under all properties there existed just pure things in the world, entities; thus, a subject is just what is "under" a property (or a set of properties). The reason I am bringing up this etymology here—and I could have done the same thing for almost every linguistic term, *case, copula, predicate, aspect,* and so on—is to remind readers that there is no theory-neutral term. All terms are in fact the result of a philosophical presupposition, and failing to grasp it could undermine the research itself. Moreover, although terms like *subject* appear to have been used without interruption since ancient times, we all know that this continuity is only superficial. In the course of time, terms of this type inevitably change their original meaning. The result: Even though in most cases we are prepared to acknowledge that some basic core intuition that the term in question had when it was originally coined has indeed survived, we should nevertheless treat such terms with great care.

understand in detail without being allowed to look inside. What are the parts that it is made of? How are they connected? When do they activate? In sum, we want to know everything that makes a clock a clock. But since we cannot open it, we can only describe it observing how it behaves, by observing what it cannot do, by manipulating its hands, and if malfunctions occur, by learning from them.

It is common within this area of scientific investigation to define a grammar as a filter of combinations of primitive elements. I will explain. Let's imagine that the primitive elements are simply the words of a language. A grammar can then be seen, in a rough way, as divided into at least two fundamental components: an inventory of all the primitive elements (the lexicon) and the rules that determine how these primitive elements can be combined (filters). These filters eliminate some of the combinations that would otherwise be available. For instance, if we consider a fragment of the English lexicon that contains the words *Dante, one, Beatrice, day, saw, astonished, was*, we can have sentences like: *One day Dante saw Beatrice and was astonished*; or *One day Beatrice saw Dante and was astonished;* or *Dante saw Beatrice*; or *Beatrice was astonished*; or even *Dante saw Beatrice one day and was astonished*. However, there are many examples of impossible combinations, including: *Dante and astonished was one day Beatrice*; and *Astonished Beatrice one*.

This way of looking at grammar is very different from the grammar from our school memories. We will distinguish between *selective* grammars and *constructive* grammars following the spirit of the classical terminological distinction that Massimo Piattelli-Palmarini (1989) originally introduced into linguistics from biology.[2] In textbooks, you will never find the rules that eliminate the impossible combinations of words (selective grammars). You only find the rules that describe the possible combinations (constructive grammars), and among these rules, you are likely to find just those that lead to "proper" language rather than actual spoken language. Let's spend some time on these two opposite conceptions of grammar—constructive and selective. A comparison may help us.

Imagine that we buy a cookbook instead of a grammar text. What do we expect to find in the book? We find how the ingredients are combined, how much of each ingredient to use, and cooking times. In short, we expect to find the directions to build all the dishes of a certain cuisine, step

2. Piattelli-Palmarini (1980) uses *instructive* versus *selective*. I prefer to use *constructive* versus *selective* because selection, too, can be seen as a form of instruction.

by step, *even if it is repetitive*. I will explain using Italian cuisine: if I am cooking spaghetti with pesto, at a certain point I will find the direction "Put salt in the boiling water." If I am cooking penne with tomato sauce and basil, I will find this very same direction—"Put salt in the boiling water." This redundancy does not bother us, and it can even be practically very useful. In some sense, traditional grammars have the very same style: we find directions that tell us how words can be combined, step by step, until they form a complete sentence. In doing so, we never build pieces of sentences that must be thrown away because they are impossible, though the directions that we are following may be repetitive. If, however, we conceived the cookbook as a filter of combinations, the cookbook would be very different. It would just give a list of ingredients and a list of negative instructions: which ingredients *cannot* be combined together, which cook times are *not* possible, and so on. You can also think of it as a filter that selects the good dishes from the bad ones after you tried all possible combinations. In the first book you would never find recipes that combine coffee with anchovies, tuna with whipped cream, or recipes that require pasta to cook for eight days; rather, you would find positive instructions on how to combine the ingredients in the proper way. The second cookbook instead would list negative instructions only. This cookbook would be, for sure, less user-friendly, but it would express, without repetition, what the various dishes of a given cuisine have in common. You would no longer need to repeat a given direction in each recipe that it applies to. It would only be necessary to give it once. Actually, the notion of "recipe" itself would disappear. The price that we have to pay for this brevity is that if you go in a kitchen with only this book, you might get disgusting food. Randomly mixing all possible ingredients, in all possible amounts, according to all possible ways of cooking, eventually selecting only those which were compatible, will also produce inedible things. But it certainly has a number of advantages: it may lead to the discovery of new dishes, potentially all possible dishes, and, most important, it would make explicit the principles that regulate cuisine in general, rather than the principles that regulate a single dish. If we look at grammar as a filter of combinations, then we have to expect to "cook" unacceptable sentences. But, by finding the impossible combinations, we will discover those general principles that help us understand how grammar works.

Of course, in the same way that "recipes" disappear from our culinary example, in a selective grammar the rules (in the way we generally understand them) will disappear in the sense that they will be derived from the interaction of more general (and in a certain sense, more abstract) principles. "In early work in generative grammar it was assumed, as in traditional grammar, that there are rules such as 'passive,' 'relativization,' 'question-formation,' etc.... These 'rules' are decomposed into the more fundamental elements of the subsystems of rules and principles.... This development, largely in the work of the past ten years, represents a substantial break from earlier generative grammar, or from the traditional grammar on which it was in part modelled.... The notions of 'passive,' 'relativization,' etc., can be reconstructed as processes of a more general nature, with a functional role in grammar, but they are not 'rules of grammar'" (Chomsky 1981, 7).[3]

The simplest grammar you can imagine, which is in fact trivial, is one that allows for all the possible combinations of all the primitive elements: that is to say, a grammar with no filters. But there is not a known natural language with a grammar that allows for all the combinations of words.[4] To study the grammar of a language, therefore, is to recognize, first of all, all the primitive elements of the language and then all the rules for combining those elements. Before reflecting on this "selective" aspect of grammar, let's look at some simple examples in which the primitive elements are not words.

First, consider the "material base" of a grammar of a natural language: the sounds that form a language—the "phones" and the "phonemes," as we will see shortly. When we talk, we produce sounds. Often we let ourselves be conditioned by writing, and we imagine that sounds correspond, more or less, to letters. We imagine that the alphabetic symbols are unique instructions on how to position the mouth, tongue, and other

3. It is interesting to notice that in the full quote, which for brevity I have not included in the text, Chomsky makes an explicit comparison between this new approach to syntax and the great structuralist tradition of the Prague school of phonology. Chomsky adds that this change "is reminiscent of the move from phonemes to features in the phonology of the Prague school, though in the present case the 'features' (e.g., the principles of case, government, and binding theory) are considerably more abstract and their properties and interaction much more intricate" (Chomsky 1981, 7). "Generative grammar" here just means "explicit grammar" as Chomsky (2005) noted. We will return to this notion shortly.

4. For the sake of brevity we will use *grammars* to refer to the "grammars of natural languages," which differ from "artificial grammars" such as FORTRAN, Prolog, Basic, or "made-up" languages such as Esperanto or Giuseppe Peano's "latino sine flexione" (see, for example, Peano 1930). A simple example of a fragment of an invented grammar, inconsistent with Universal Grammar, can be found in Moro (1997c).

parts of the articulatory apparatus in order to produce a sound. But it's not like this. Although there is a certain correspondence between sounds and letters, there are many aspects of our production of sounds that we do not perceive unless we are guided. We all know, for example, that in English the string of letters *th* in the word *mother* is pronounced as just one sound and not as the combination of the sounds that correspond to the letters *t* and *h,* as in the word *penthouse*. Or, consider the words *sacks* and *sax*. We all know they are pronounced in exactly the same way, so they must have the same number of sounds. Nevertheless, *sacks* has five letters and *sax* has only three. These are well-known examples in which sounds and letters do not correspond. There are also hidden cases you would not think of. Consider the letter *n*. It's natural to think that this letter always corresponds to the same movement of the articulatory apparatus in the mouth. But this is not the case. Let's look at the words *incapable, intolerable,* and *inferior.* How is the *n* pronounced in these three words? It is easy to see that these *n*-sounds are produced in different ways. In the case of *incapable*, the *n*-sound is produced by blocking the air flow out of the mouth—by placing the base of the tongue against the velum (or soft palate)—and redirecting the air through the nostrils. In the case of *intolerable*, *n* is still pronounced by blocking the airflow out of the mouth with the tongue, but now the tongue is placed right above the upper teeth (the so-called alveolar region). Finally, the *n* in *inferior* results from blocking the airflow by bringing the upper teeth against the lower lips. The reason for these differences is pretty clear. The *n* is followed by different consonants, and the way in which the airflow out of the mouth is blocked depends on the following consonant. Nevertheless, we transcribe these three different sounds with the same letter, *n,* because naturally you are not going to use one *n*-sound when you should use the other, and if you try, it's not going to be easy and the result will sound funny.

Something similar happens with *s*: this letter is also pronounced differently according to the sound following it. Consider two words like *seen* and *soon*. When you say *seen*, you utter the *s* with your lips straight, whereas when you say *soon* you round your lips. The reason of this difference is pretty clear in this case as well: it is vowels that matter now. The vowel that follows *s* in *seen* is pronounced with the lips straight, while the vowel that follows the *s* in *soon* is pronounced with the lips rounded. The conclusion we draw from these examples is that we unconsciously make our pronunciation of a string of sounds easier by anticipating the articulation of one sound when we utter the preceding one.

But the world of sound is not mysterious just because of the "physical" facts, such as the example we just saw. There are also distinctions that show that the physical, articulatory factor is surely important but by no means the only relevant one. The very same sound can, in fact, have a completely different *psychological impact* from one language to another. In order to appreciate this psychological dimension, linguists make a distinction between *phones* (or sounds) and *phonemes* according to the following definition.

Whenever two words with different meanings are exactly the same except for one sound in the same position, we call the two sounds that make the difference in meaning *phonemes* and the pair of words *a minimal pair*.[5] For instance, the fact that *top* and *pop* are two different words in English shows that /t/ and /p/ are two phonemes in English (notice that the pair *top/pot* would not qualify as a minimal pair). The fact that the notion of phoneme is not crucially based on physical reality but rather is a psychological entity is also clarified by the fact that two distinct sounds may not correspond to two phonemes. The words *lap* and *pal* have two different *l*-sounds, but these two *l*-sounds are not separate phonemes in English because one can never find a minimal pair distinguished by these two sounds. The situation here is confused by the fact that the same letter *l* is used but it is useful to think about it. First, let's see why they are two different sounds. Pay attention to the position of your tongue when you produce the *l*-sound in *lap*—usually called *light l:* the tip of your tongue touches the alveolar region right behind your upper teeth and the remaining part of the tongue stays flat more or less at the same height as the tip. In the *l*-sound in *pal*—usually called *dark l*—the tip of the tongue still touches the alveolar region, but the body of the tongue, especially the back part, now rises toward the soft palate. Although these two sounds are different, they are not two different phonemes in English because there is no pair of words in English that differ only by these two sounds in the same position. Their distribution is predictable from the context where they occur. Roughly, dark l is found only at the end of a syllable (as in *pal,* where it occurs at the end of the last syllable and therefore at

5. For a detailed introduction to phonetics and phonology, see Kenstovicz (1993); Clark, Fletcher, and Yallop (2006); Ladefoged (2006); for an influential (and quite controversial) approach to phonology and articulatory theory see Liberman (1996). Phonemes are conventionally written between slashes (e.g. /a/) and phones are written between brackets (e.g., [a]). The phonetic symbols used here are from the International Phonetic Association (IPA).

the end of the whole word) or before a consonant such as in *plot or palter*. Light *l* is never found in these contexts or positions but is found in all others (see Kenstovicz (1993) and Hayes 2000 for a much more detailed description of the distribution of the two *l*-sounds). Technically, when this happens—that is, when the occurrence of two or more phones is completely predictable from the context, we say that those phones are *allophones* of the same phoneme. Therefore, the phoneme /l/ is realized by two allophones [l] and [ɫ]. (Slashes and brackets are part of the transcription system for phonemes and allophones of the International Phonetic Alphabet, or IPA. So, for example, if you transcribe *pal* and *lap* by indicating phonemes you get /pal/ and /lap/; if you want to be more specific and highlight the allophones instead you write [paɫ] and [lap].) All in all, although a phoneme is realized by just one phone in many cases, you should not think of a phoneme as something physical (acoustic or articulatory). The phoneme represents a class of sounds and is essentially a psychological phenomenon.[6]

6. Phonemes are also defined as "the smallest linguistic unit into which the flow of speech can be segmented." We use "the flow of speech" because phonemes are not the smallest linguistic unit in absolute terms. In fact, each phoneme can be seen as the sum of a series of primitive characteristics called *distinctive features*. To give an idea as to what a distinctive feature is, consider the following example: The two consonant phonemes /t/ and /d/ are identical except for voicing: *t* is voiceless, pronounced without vibration of the vocal cords, whereas *d* is voiced—the vocal cords vibrate when you pronounce it. The same contrast distinguishes /p/ from /b/ and /f/ from /v/. Voicing, therefore, becomes a characteristic that is common to groups of sounds—and this makes it a "distinctive feature." Voicing cannot be expressed in isolation but always occurs with other features. With this technique of systematic comparison among sounds, the Prague school of phonology—founded by Prince Nikolai Trubetzkoy—revolutionized phonology in the 1930s. Every phoneme was characterized as the sum of all the distinctive features that distinguish it from the other phonemes (Trubetzkoy 1939/1969).

This was the realization of the structuralist dream, according to which every linguistic element has value in a system—or more radically it can be identified—only insofar as it is distinguishable from another element, as on a chessboard where the value of a piece is established only in relation to the other pieces. Later, Roman Jakobson—with Trubetzkoy, a cofounder of the Prague Linguistic Circle—pushed this approach even further by suggesting that *all* the features were defined for *all* the phonemes according to a binary schema of presence/absence of a certain characteristic. For example, +voiced versus −voiced. This provided the base for generative phonology (Jakobson and Halle 1956), and, as Chomsky (1981) says, the model for generative syntax as well. For a matrix of the binary features of English, see Chomsky and Halle (1968) and Kenstovicz (1993).

Moreover, not all languages have the same phonemes, a fact that can be understood only if we assume that the distinctive nature of a sound is not physical but psychological. For instance, consider the words *ice* and *eyes*. Let's ignore their spellings, which for historical reasons look very different, and focus on the way we pronounce them. They sound identical except for the last sound. The IPA transcription may help us here: *ice* is transcribed as [ais] and *eyes* is transcribed as [aiz]. *Ice* and *eyes* differ only in their last sound [s] and [z], which are, therefore, phonemes in English and should be written as /s/ and /z/. But this is not true for Italian, for example, which has both sounds—*sballo* (''buzz'') has a *z*-sound, and *stallo* (''stall'') has an *s*-sound—but has no pair of words with different meanings that only differ by these two sounds in the same position: in other words, in Italian the pair [s] and [z] does not qualify as a pair of phonemes. This is similar to what we saw earlier for [l] and [ɫ] in English. Using the terminology that we introduced then, we say that [s] and [z] are allophones of the same phoneme in Italian, and by convention we call this phoneme /s/. Therefore, not all languages have the same number of phonemes, just as not all languages have the same number of say prepositions. For instance, Standard American English has about 38 phonemes, whereas Standard Italian has 30. There are languages like Pirahã, spoken in Brazil, that have only 10, and languages like !xû, spoken in Africa, that have 141.

How many words can be built from a set of phonemes? A simple combinatory calculation would indicate that Standard American English could have up to 238 millions of billions of different words, each made of eleven phonemes. Some of the combinations, such as *linguistics,* exist, and others, such as *gislisctuin,* do not.[7] Here, we are dealing with a filtering mechanism. What is the filter that reduces the huge number of words built out of a certain phoneme inventory? Certainly there is a ''historic'' filter, since a word like *typewriter* was not available in the sixteenth century for obvious reasons. But this historic filter is not the only one. There are other reasons why certain combinations of phonemes are not found. One of the most powerful natural filters that reduce the potential inventory of phoneme combinations is syllable structure. In English, a syllable always has a core, or ''nucleus,'' containing a vowel (see also section 1.2.3. Recursion). The nucleus may stand alone, or it may be preceded by up to three consonants and followed by up to four consonants (such as *a, do,*

7. On the important notion of ''redundancy'' in linguistics, see Gillette and Wit (1999).

up, tap, trap, stamp, sprint, sixths). These and other constraints on syllable structures filter out strings such as *strx*, *strxthsa* and *astrxths*, which are not words since do not have a nucleus or they have too many consonants before or after the nucleus.

This is not, of course, the only filter that grammar imposes. Moving to a level of representation that is different than the one of sounds, the level of *morphemes,* the smallest meaningful unit in a language, the same argument holds. Morphemes cannot be combined freely.[8] A standard way of categorizing morphemes is to distinguish between "free morphemes" and "bound morphemes." A free morpheme can be pronounced in isolation, which makes it a word. Examples include the conjunction *and* or the noun *table*. Bound morphemes are found in combination with other bound morphemes or with free morphemes. For instance, the Italian word *tavolo,* table, contains two bound morphemes: *tavol-,* which means "table," and the morpheme -*o*, which expresses the meaning of singular. In English, the bound morpheme -*s* that conveys the meaning of plural in is bound to free morphemes such as the noun *table* to form the bimorphemic noun *tables.* The morpheme -*s* can be combined with morphemes other than *table*: *chair*-*s*, *car*-*s*, etc. Also, more than two morphemes can be combined: the word *interestingly* has three: *interest*-, -*ing*-, -*ly*.

As with phones and phonemes, not all morphemes can be freely combined: there are rules that filter out logically possible combinations. For instance, the morpheme *dis*- more or less conveys the meaning of "not" and can be combined with the monomorphimic word *honest* to form *dishonest* (that is, not honest). Similarly, the morpheme *in*- can also mean "not" and can be combined with a monomorphic word such as *coherent* to form *incoherent* (not coherent). Nevertheless, the morphemes *dis*- and *in*- do not freely attach to just any word: *dis*- does not combine with the word *coherent* to form something like *discoherent*, nor can the morpheme *in*- be combined with *honest* to form *inhonest*, though there is nothing phonologically wrong with this words and it is clear what these nonwords would mean. Simply, they are words that are not attested now but that could perhaps be in use in some future variety of English. Similarly, the morpheme -*er* attaches to the end of verbs such as *play, examine,* and *read* to form the nouns *player, examiner,* and *reader*, respectively,

8. The property of a linguistic code such as human language to form a very high number of morphemes (and therefore words and sentences) out of elements lacking meaning such phonemes is called *double articulation* (see Akmajian et al. 1995 and Hockett 1960).

and others like them. Still, *-er* cannot attach to the end of any verb: *arrive* plus *-er* cannot be combined to form *arriver—arriver,* one who arrives, is not a word in English. Once again, the violation is not at the phonetic nor phonological level. Aside from simply accidental historical facts, there are other deeper reasons for the filtering out of some combinations: semantic reasons and reasons that depend on the different syntactic properties of *arrive* as opposed to *play*, *examine*, and so forth. Whatever the reason behind the filtering out of some combinations of morphemes,[9] what is relevant for us is the realization that there is a filter in this domain of grammar as well and that speakers of English, or Italian, or German, unconsciously *knows* all these filters for their own language.

So far we have dealt with aspects of sound combination (phonology, in the broad sense) and word structure (morphology). These are not the only components of a natural language. The situation seems to get more complicated when we move to another level, the level of word combination, or "syntax."[10] We will spend more time on syntax than phonology or morphology because it constitutes the heart of the neuroimaging experiments I will present in the second half of the book. An important note: we will not define the notion of "word." Although the meaning is intuitively clear, the task of giving a formal definition of "word" is far from trivial. We will behave like the mathematicians who work with numbers or sets without defining "number" or "set," and, borrowing the term from mathematics, we will call this approach "naïve": our syntax therefore will be a "naïve syntax," at least in the sense that we will not give a definition of "word" (on the expression "naïve syntax," see Graffi 1991).

Let's start with a look at the behavior of two Italian verbs (the reason will soon be clear). *Arrivare* (to arrive) and *dormire* (to sleep) are two examples of intransitive verbs: like their English equivalents, they do not take a direct object. **Maria arriva i fiori* in Italian and its equivalent in English, **Mary arrives the flowers,* are both ungrammatical. (It is common practice in linguistics to place an asterisk [*] before a linguistic

9. See Spencer and Zwicky (2001) for a complete overview of morphological issues; see Burzio (1986) for the impact of syntax on these morphological aspects.

10. *Syntax* is an ancient word, unlike *semantics, morphology,* and *phonology,* which were created during the nineteenth and twentieth centuries (*semantics*, for example was first introduced by Michel Bréal, a French philologist, who became professor of comparative grammar at the Collège de France in 1864). A treatise on syntax by Apollonius Dyscolus, written in the second century A.D., is usually considered the first such work in Western culture.

form that is unacceptable or ungrammatical.) Similarly, *Gianni dorme il cane* and its English equivalent *John sleeps the dog* are ungrammatical. On the other hand, a transitive verb, such as *gradire* (to enjoy) needs a direct object: *Maria gradisce i fiori* and *Mary enjoys flowers,* versus **Maria gradisce* and **Mary enjoys.* The property expressed by an intransitive verb such as *arrivare* (to arrive) or *dormire* (to sleep) is usually said to stay on the subject, rather than "transit" to another element.[11] These two verbs are therefore expected to behave in the same way syntactically. But we will soon see how different these two Italian verbs are.

Consider two simple sentences: *Molti gnomi arrivano di sera* ("Many dwarves arrive at night") and *Molti gnomi dormono di sera* ("Many dwarves sleep at night"). If we change the tense from simple present to a complex form with a helping verb (a tense called in Italian *passato prossimo,* morphologically similar to the English present perfect but with a slightly different meaning that is sometimes closer to the English simple past), we obtain: *Molti gnomi sono arrivati di sera* (literally, "Many dwarfs are arrived at night," but translated into English as "Many dwarfs arrived at night") and *Molti gnomi hanno dormito di sera* (literally, "Many dwarfs have slept at night," translated as "Many dwarfs slept at night"). These two new sentences show two important differences (note underlined words) with respect to the sentences we started with. First, the helping verbs are different. *Arrivare* takes *essere* (to be) as its helping verb (*sono* [are] is the third-person plural present tense of *essere*) similar to the English progressive construction *is running,* where the helping verb is to be, as in *John is running*. In this respect, *arrivare* is different from *to*

11. The subject of a verb and its direct or indirect objects are called *arguments* or *valences* of the verb (the term *arguments* is borrowed from mathematics, where an argument refers to the values that can be assigned to the variables of a function: f(x, y . . .); the term *valences*, instead, is borrowed from chemistry and indicates the number of bonds that an atom can establish with other atoms). It is common to use the term *bi-argumental verbs* to refer to transitive verbs such as *to enjoy* (*somebody enjoys something*); *mono-argumental* for intransitive verbs such as *to arrive* (*somebody arrived*); and *tri-argumental* for verbs such as *give* (somebody gives something to somebody) or *to put* (somebody puts something somewhere). There are also *zero-argument verbs* (or constructions) such as verbs referring to weather occurrences: *to rain, to snow, to sleet,* and so forth. Note that circumstantial elements such as *on the balcony* in *It rained on the balcony* do not count as arguments, but they are called *adjuncts* in that they *add* information to the whole sentence. See Carnie (2006) for a standard illustration of the notion of argument and Hale and Keyser (2002) for an advanced presentation of these issues.

arrive in English, since the latter takes *to have* as its helping verb in the past tense (*have arrived* versus **are arrived*). On the other hand, *dormire* takes *avere* (to have) (*hanno* [have] is the third-person plural present tense), similar to *to sleep* in English (*have slept*). If we switch the helping verbs in the Italian sentences and combine them with the other main verb, the results are completely ungrammatical: **Molti gnomi hanno arrivati di sera* and **Molti gnomi sono dormito di sera*.

The second important difference between the two sentences is the main verb endings. The past participle *arrivati* ends with -*i,* which is the marker for masculine plural. This marker occurs redundantly in these sentences, being found at the end of the two words that form the subject, *molti* and *gnomi*. In other words, the past participle "agrees" with the subject in gender and number (we will talk more about agreement later). This is not the case with the past participle *dormito,* slept, though the subject is the same. The final vowel -*o* in this case is the default nonagreeing form. Once again, if we switch the pattern and replace the agreeing form *arrivati* with the nonagreeing *arrivato*, and the nonagreeing *dormito* with the agreeing *dormiti*, the results are totally ungrammatical: **Molti gnomi sono arrivato di sera* and **Molti gnomi hanno dormiti di sera*.

These and other differences among intransitive verbs depend on various factors that are neither cultural nor historical. Their discovery is one of the most relevant findings of the last century regarding syntax, and has motivated the division of intransitive verbs into two distinct subcategories: those that allow for purely intransitive constructions, such as *dormire,* and those that allow for the so-called "unaccusative" constructions, such as *arrivare*. One of the empirical justifications for this split comes from the differences in helping verbs in Italian (about this, see Perlmutter's 1978 and Burzio's 1986 pioneering works). Also in this case, a crucial point that all these differences share is that they are not immediately accessible to introspection. In order to find the "filters" that block certain combinations, you need to think and try to build examples, similar to the research process that is followed to investigate other areas of the natural world.

We looked at Italian because the contrast between verbs such as *dormire* and *arrivare* is clear and because Italian was the first language, historically, in which this distinction was discovered and discussed in detail. But we can find systematic differences between the same verbs in English as well. Consider, for example, the two sentences *There have arrived three kids at the party* and **There have slept three kids at the party.* You may not quite like the first sentence, but there is little doubt that it sounds more grammatical than the second sentence. Why is it that the *there*-construction—in which *there* is in the usual subject position and the subject comes after the verb—sounds much better with the verb *arrive* than with *sleep,* although they are both intransitive verbs? Whatever the answer is, the central point here is that although it's likely that you cannot answer this question immediately,[12] your intuitions about which sentence sounds better are clear. In this case, as in the case above from Italian, grammar functions as a syntactic filter in the sense that it limits the number of possible combinations of words even if, at first glance, we do not see any rational or nonrational reason that accounts for this constraint.

Let's go back to Italian and look at a new pair of examples and a new rule. Imagine a painter who is painting a fresco in a cathedral with a vaulted ceiling. An Italian says the following sentence: *Il pittore dipinse due volte della cattedrale di Pavia* ("The painter painted two vaults of the cathedral in Pavia") and *Il pittore dipinse due volte lo stesso affresco* ("The painter painted the same fresco twice," literally, "The painter painted two times the same fresco"). Notice that in Italian, *due volte* is ambiguous: it can mean "two vaults" and behave as a direct object, as it did in the first sentence, or it can mean "two times" and behave as a temporal modifier, as it did in the second sentence (unlike in English, in Italian, temporal modifiers can occur between the verb and the direct object). In Italian, the pronoun *ne* (roughly, "of them") can replace a noun. If we use *ne* in the two sentences above to replace the noun *volte,* we get the following results: *Il pittore ne dipinse due della cattedrale di Pavia* ("The painter of them painted two of the cathedral in Pavia") and **Il pittore ne dipinse due lo stesso affresco*. No Italian speaker would accept the second sentence. Why? *Ne* seems to be able to replace a noun when it serves as part of a direct object, whereas *ne* cannot replace nouns when they are used as a temporal modifier. Even if it looks more like a description of the facts than an explanation, let's stick with this. What is really surprising is that we now have two rules concerning Italian: the rule concerning the choice of helping verbs with intransitive verbs such as *dormire* and *arrivere* that we looked at earlier, and the rule about *ne.* The surprising fact is that these two rules do not behave independently but interact with each other. Let's look at the following two sentences: *Sono arrivati molti gnomi di sera* ("Many dwarfs arrived in the evening," literally, "Are

12. For unaccusatives, see Carnie (2006) for a standard illustration based on Burzio (1986) and Moro (1997a) and Hale and Keyser (2002) for some alternative proposals.

arrived many dwarfs in evening") and *Hanno dormito molti gnomi di sera* ("Many dwarfs slept in the evening," literally, "Have slept many dwarfs in evening"). Let's apply the rule for *ne* to these two sentences, as we did earlier with the sentences with the verb *dipingere.* In particular, let's replace the noun *gnomi* with *ne*: *Ne sono arrivati molti di sera* ("Many arrived in the evening," literally, "Of-them are arrived many in evening") and **Ne hanno dormito molti di sera* (literally, "of them have slept many in evening"). The first sentence is grammatical but the second one is not. The explanation for the behavior of *ne* that we suggested earlier certainly does not account for this contrast here: it makes reference to the direct object—but neither *arrivere* nor *dormire* has a direct object.[13] In this case, the grammar works as a syntactic filter in that it restricts the number of possible combinations of words, although at first glance, we do not see any reason for this restriction.

The interaction of these two rules in Italian—the choice of helping verb and the use of the pronoun *ne*—is surprising and raises the general question we are approaching here once more: How do speakers know what they know if they are unable to explain it? The hypothesis that we have been given specific instructions is very remote: it is improbable that the syntactic distinction that we just discussed—which linguists did not notice for such a long time—is explicitly given to children while they acquire their native language. Children are certainly not exposed to this kind of instruction—and neither are adults. Where does this knowledge of grammar come from? In order to answer this question, which is now compelling, we need to ask another question that, in a certain sense, comes first. A question we have not yet formulated in an explicit way. *What is it that we know when we know a language?*

We will discuss this question, and many others, in detail. From what we have seen so far, at least two important facts are clear. First, *we do not always have an explicit understanding as to why we apply certain grammatical rules.* Second, *rules interact in an intricate way* and this seems to show that the system is much more complex than just a list of filters. The system is an interactive net in which touching one point moves others.[14] Grammar, therefore, looks like a Rubik's Cube (a hand-held puzzle that was popular years ago). Nine squares of identical size and color are found

13. We are ignoring the fact that *dormire* allows for idiomatic expressions with an "internal object," for example, *dormire sonni tranquilli* (sleep a good sleep").

14. Although we will focus only on syntax, all areas of grammar exhibit similar interactive netlike behavior.

on each of the Rubik's cube's six uniquely colored faces. Squares can be rotated along the three axes, leaving the central square of each face fixed and generating combinations that elude our immediate intuition—at least that of most people. A person studying grammar has to deal with a Rubik's Cube of words. If one hopes to find the way to reach the desired combination, one must take into account the entire system and not just an isolated part. Also, the effects of each move are so intricate that they cannot be immediately grasped by intuition. In order to have a good model of the system, we must, therefore, reflect, try, experiment, and sometimes start all over again.

How do people who study a language manage to keep their orientation within such a complex system? The only possibility—excluding mystical solutions—is to follow a method. Since the scientific method (which Husserl defined as "Galilean research style") has given good results in other scientific fields, such as physics—one option is to adopt the fundamentals of this method, with some adjustments. In the following section, we will address ourselves to methodological issues as applied to linguistics.

Three basic questions demand answers: Why does syntax have limits? Why does it have precisely these limits and not others? How does a child acquire the knowledge of these limits? We will discuss these issues only after presenting at least some methodological basics and a simple collection, a sample, of syntactic rules. In particular, in the final chapter—which is frankly speculative—we will try to probe these questions in a deeper way. Let's now turn the discussion to three methodological issues that I consider pivotal: simplicity, error, and formalism.

1.1 Methodological Issues

Human language is a universe. I cannot say whether it is more complicated than the physical universe, and I do not believe that this question makes much sense—even if it is true that human language is used to describe the physical universe, and therefore the problem of relative complexity can bring us to nontrivial considerations.

But this is not the point. Human language is a universe in the sense that by means of it, different and complex events can take place. With language it is possible to describe, to emote, to convince, to think, to condemn, to order, to analyze, to lie ... If the task of a linguist were to explain *everything* concerning language, it is likely that knowledge would advance little. The same would be true if a physicist wanted to describe everything in the universe—the enterprise would not even be conceivable.

The only hope is to circumscribe the domain of investigation by choosing a method. Let's start with the first crucial methodological notion right away: simplicity.

1.1.1 The Three Degrees of Simplicity

The notion of simplicity is not simple at all. Certainly, it is simple in trivial cases: it is simpler to multiply a number by 2 than to take the square root of 2. This kind of simplicity is understood intuitively but can also be formalized by calculating both the steps that are needed to obtain the result and the "magnitude" of the structured set of numbers (that is, integers, rational numbers, real numbers, complex numbers—each of which "contains" the other in a progressive way) by means of which the calculus is computed. But when we are dealing with facts other than numbers—that is, the majority of the world's phenomena—the situation is not so trivial. What is simpler, an elephant or a mouse? A rose or an earthworm? There are no immediate answers to these questions.

In physics it often assumed, sometimes implicitly, that a theory is simpler than another if fewer principles are needed to account for the same data. Is this true? Look at the development of modern heliocentric theory. Kepler corrected the erroneous idea that celestial bodies follow circular orbits in favor of the idea of elliptical orbits. Before Kepler, the geocentric theory was, in fact, the simplest, since it required fewer corrections then the heliocentric one proposed by Copernicus—but it was false (Verdet 1990). Nevertheless, the scientists, with a kind of perseverance that often appears to go beyond reason, chose the less simple theory and demonstrated the value of intuition as a guide. In the end, but only in the end, people realized that Kepler's proposed theory was not only more empirically adequate, but also simpler and significantly more extendable than the other theory. Moreover, it is not uncommon to say that simplicity coincides with elegance—a vague notion of symmetry, lack of ambiguity and coherence as applied to scientific theories. As a matter of fact, you often hear that the more elegant a theory, the simpler it is. But in this way, you jump out of the frying pan and into the fire—the notion of "elegance" is certainly not simpler to define and does not help us get closer to the truth. Interestingly, although in a slightly different context, Albert Einstein wrote "I meticulously follow the rule of the great theoretician L. Boltzman, that is, the question of elegance should be left to tailors and shoemakers" (quoted in Infeld 1950). In other words, sometimes elegance must be put aside in pursuit of clarity and truth, whatever that term means.

If then we turn to linguistics, the problems are certainly no less intricate. We need to distinguish at least three different kinds of simplicity: the simplicity of the data to be observed, the simplicity of the questions to be asked, and the simplicity of the theories that account for the data. If we do not keep these three levels apart, it will be difficult to understand the limits and the methods that I intend to illustrate from the experimental area between linguistics and neuroscience. Let's start with the simplicity of the data.

1.1.1.1 The Simplicity of the Data It is better not to describe everything. A paradox that Jorge Luis Borges made famous in one of his short stories illustrates the point. If a map had to describe all the features of a place—down to the smallest detail, it would so closely mimic the place that it would become completely useless. In the same way, when you decide to describe a phenomenon, you need to assume a partial point of view—that is, you have to give up on the ambition of providing an immediate explanation of everything. Of course, there is no recipe for choosing a correct point of view: on the contrary, every point of view is correct in the appropriate context. What matters is that the point of view that we choose be appropriate for the goal at hand. If we go back to the map example, you would choose to include relief details, or what trees have been planted, or property lines, according to what you need the map for. The data have to be simplified or, as they sometimes say, "idealized."

Language presents a similar case. Language is like the world of natural phenomena, in that when we decide to describe it, we need to choose a point of view. Of course, every choice in this area as well constitutes both an advantage and a limit. Nevertheless, we cannot avoid this: once again, we need to directly verify if the advantage is bigger than the accompanying limit and whether it is suited to the goal. Furthermore, it is possible that what is reduced to an irrelevant fact for one person becomes the center of attention for someone else.

This happens often in physics. Consider the birth of dynamics, with Galileo Galilei. One of his most famous propositions, stated informally, is "An object in motion will continue moving infinitely, following a uniform rectilinear motion [in a straight line], unless an external force acts on it." In a sense, Galileo couldn't experience all the factors involved in this principle experimentally: the infinite—at least the spatial, temporal one—is not accessibly to humans, who live a finite life. Therefore, how could Galileo end up proposing a hypothesis that is not verifiable? The fundamental idea was to "idealize" the data. For instance, when we apply a force to a sphere on a smooth surface, the sphere will move, and at a certain point will stop. Nevertheless, if we progressively reduce friction and observe a corresponding increase on motion, we can logically conclude that the sphere would move infinitely with uniform rectilinear motion if friction were reduced to zero. This is what is sometimes synthetically referred to by just saying "if we ignore friction."

The "idealization"—ignoring some factors by reducing them to the minimum—therefore, the foundation of the experiment. To be sure, when physicists centuries later started to study thermodynamics, that phenomenon in dynamics that is reduced to the minimum and as a matter of fact disregarded—namely, friction—became relevant. In conclusion, there are no phenomena that should *always* be disregarded: the scientist will choose, each time, which aspects should be disregarded and which should not. In doing so, the scientist is not guided by infallible algorithms but by intuition. Only in the end will the scientist know if his choice was good—only when the scientist sees whether the new theory allowed him or her to account for apparently different things in a uniform way, to discover new things, or to simplify the vision of the world.

Linguists behave in the same way. *Language, in all its manifestations, is like the universe of natural phenomena. What can and must be disregarded by some scholars becomes the center of attention for others.* This is not surprising. Language can be studied from so many different perspectives that there is, certainly, no privileged view. It is studied by the philologist, who wants to travel backward to the source of the original texts; by the historical linguist, who wants to reconstruct dead languages or the family tree of languages; by the jurist, who has to understand how to formulate a law in the best way; by the literary critic, who aims to discover poetic techniques; by the psychoanalyst, who investigates the unconscious thoughts that drive the emotions of people; by the clinician, who has to understand what damage occurred after a brain lesion caused aphasia; by the advertising executive, who wants to convince the public to purchase; and of course, among others, by the general linguist, who is trying to understand what the *structures* of this code are, independent of other aspects of language and of specific languages.

As linguists, then, we are compelled to make a methodological choice—actually, more than one. In our case, we will decide to study some structural aspects of this code, those that are usually called syntax. To put it simply, though we will return to this theme many times, syntax is the study of word combinations. This is our limited domain of interest. We will try, therefore, to minimize the phenomena that do not appear relevant for syntax. That is, to *simplify* the domain. In the end, each of us will decide whether this move is fruitful or not. Nevertheless, we need to ask ourselves a preliminary question. Is it legitimate to study this aspect autonomously? Could it be the case, instead, that what we call syntax is the result of the interaction of some other component? In other words, can syntax be studied in isolation? Is it autonomous? There is no logical, intuitive, algorithmic answer here. Once again, we need to reason about the data in a scientific way, that is, experimentally. Let's see how.

Syntax, as we said, is the study of word combinations. Certainly, this is not the only component of grammar. For instance, there is semantics—the study of possible meanings based on syntax and the lexicon. As we saw, there is phonology, which is the study of the possible sounds and their combinations in the languages of the world, and morphology, which studies the structure of words. There is also pragmatics, which studies the role that context and the speaker's intention plays in determining the content of an act of communication in concrete situations.[15] All these disciplines are ways to read what we see as a unitary phenomenon: language.[16] We are in a situation that is similar to the world of natural phenomena. There is only one world of natural phenomena, and the fact that we have physics, chemistry, and the subdivisions of kinematics, dynamics, thermodynamics, and so forth is just a way to build partitions useful to research. If a lit torch falls from a tower, how do we decide if we are dealing with a physical, chemical, or thermodynamic phenomenon? It will be all these together. It is the researcher who decides whether it is appropriate to divide the phenomenon into different aspects and study them autonomously, according to what he or she is interested in. Once again, idealization makes this procedure possible. But the guarantee that separating, say, thermodynamics from chemistry is correct depends on the possibility of identifying a class of phenomena that is coherent with respect to the two domains as we define them. In other words, if in one domain you ignore the facts that are relevant in the other domain, this will not hinder the investigation.

Therefore, to decide whether we can build a theory of syntax, we need to see whether it is autonomous with respect to other domains of grammar. In particular, it will be crucial for syntax to be autonomous with respect to semantics and phonology. What would convince us that this is

15. For an overview of pragmatics, see Levinson (1983).

16. As for the idea that language is *not* a unitary phenomenon, see the section on Mendelian linguistics in chapter 2.

really the case? Let's break the problem down. First, what would convince us that syntax is autonomous from semantics?

Semantics, by definition, studies possible meanings. As with the notion of "word," we won't define "meaning." Instead, we will admit to having just an intuitive idea of this term—for which, by the way, there is a copious bibliography.[17] One of the fundamental properties of semantics is the so-called "principle of compositionality," according to which the meaning of a sentence results from combining the meanings of its parts.

Let's take, for instance, the case of sentential negation.[18] When we interpret the meaning of the sentence *John will not say that Peter will leave*, we start combining *will* and *leave*. Next, we progressively combine what we got with *Peter, that,* and *say*. It is at this point that we add the negation *not* in order to conjoin the subject *John* and the auxiliary *will* with the complex predicate *say that Peter will leave*. Roughly speaking, we can say that the meaning of the entire sentence coincides with a characterization of the conditions under which the sentence is true or false. Therefore, syntax and semantics are not totally independent. For instance, if the negation *not* occurred right before *leave* instead of *say*, the meaning would be different, since the syntax would be different though the words would be the same: *John will say that Peter will not leave*. This time, negation would be interpreted by applying it to the conjunction between the subject *Peter* and the auxiliary *will* and the predicate *leave*.

In fact, we do not want to say that semantics and syntax are independent. How could they be, since they are two possible ways to look at a unitary phenomenon such as human language? What we want to show is something different: that syntax is not *completely* reducible to semantics. This seems to be the case, as we shall now see.

Let's take the words *a, is, circle, red,* and *square* and try to build strings by selecting some of these words at random: *A is red circle a; Red red a square; Red is a red; A circle is red; Circle is a a; A circle is square.* Among all these strings, only some are syntactically possible, according to our intuition: *a circle is red* and *a circle is square*. Immediately we realized that the latter sentence is a contradiction. If you try to

17. To orient yourself in this sea of publications, you can start by looking at Chierchia and McConnell-Ginet (2000) and references cited there.

18. The negation literature is vast. See Horn (1987) for an exhaustive treatment and Zanuttini (1997) for a critical analysis that is especially focused on Romance languages. For a biolinguistics approach to negation see Tettamanti et al. (2006).

compute the sentence *a circle is square,* you reach the paradox of combining the meanings of *square* and *circle* and produce an unimaginable object, a square circle. Of course, most of the time when we pronounce sentences we do not utter contradictions; otherwise, communication would be useless on the semantic level. Why, then, is this case interesting? Because it shows in a straightforward way that a contradictory sentence (*A circle is square*)—a kind of semantically useless sentence—can, nevertheless, have the same structure as a fully acceptable sentence (*A circle is red*). In other words, a sentence that is well formed from the syntactic point of view may not be well formed from the semantic point of view, at least to the extent that a contradiction is not well formed semantically. This simple example allows us to conclude that semantics and syntax are not completely reducible one to the other.

Let's move now to phonology. What would convince us that syntax is not completely reducible to phonology or, more precisely, to the phonological structure of a sentence? A first piece of evidence comes from reading this very text. Your eyes are detecting the variation of light on the white page in front of you as carried from the black letters of the alphabet. If you were listening to someone pronouncing these words, rather than you reading them, the syntactic structure would be conveyed by mechanical actions on the set of molecules of the air (compression and rarefaction), the sound, rather than an electromagnetic modulation, the light. But both the spoken and the read sentence would have the same syntax at a more abstract level. This first piece of evidence may not be completely convincing, since the objection could be raised that the graphic symbols carried by light are associated with sounds. There is, however, also a way to investigate the relationship between syntax and phonology within one single medium.

How can we convince ourselves that syntax is not completely reducible to the medium? We should try to build an experiment in which the same string of written or spoken words allows for two different syntactic structures. If we succeed, we can no longer think that syntax can be completely reduced to its medium—sound or writing.

To address this issue, linguists often cite sentences such as *I saw the king with the telescope,* where the same string of sounds (and graphic symbols) can convey two very different syntactic structures. This sentence can have two interpretations: according to one, I had a telescope and used it to see the king; according to the other, it's the king who had the telescope and I saw the king with it. Since the two meanings are different, though the sounds and the writing are the same, it is not unreasonable to say that there are two different syntactic structures in order to justify these two different interpretations. Often, it is our world knowledge that helps us make clear which structure is relevant. If I say *I saw a stork flying home*, I can mean that I was flying home and saw a stork on my way. I can also mean that I was on the ground and saw a stork that was flying home. But if I say: *I saw Ireland flying home,* I do not have any doubt, unless I think islands fly. In some sense, the technique is the same when we want to show that the meaning of a word is independent of its sound. In this case, homonymy (one spelling or pronunciation of a word with more than one meaning) is the central piece of evidence. For instance, the word *bank* can refer to either a financial institution or the land right next to a river. Similarly, we could say that there are "homonym" sentences, like the one cited, that, without varying the physical medium, have two meanings, showing that syntax is irreducible to phonology and, more generally, to the physical medium that carries the linguistic code.

To summarize: With the understanding that the partition in domains of study of a phenomenon is not unique nor necessary, and that this partition can be evaluated only on the base of its effectiveness in discovering new facts and better understanding facts we already know, we have good reason to consider syntax as an independent domain of investigation, at least with respect to two other important domains of grammar: semantics and phonology. By idealizing the field of investigation, we have *simplified* it, as in physics, when we say friction doesn't matter. This kind of simplification will be quite useful in describing the experiments that will be presented in chapter 2.

1.1.1.2 The Simplicity of the Questions The notion of simplicity that coincides with idealization—the simplicity of the data—does not exhaust the notion of simplicity. A second way to understand simplicity is *the simplicity of the questions.* Confronting the complexity of the universe—either the physical or linguistic one—we feel lost and paralyzed if we try to understand everything. This has occurred often in the history of science. In biology, for instance, people arrived at an understanding of how more complex organisms worked only when they realized that in order to investigate the genetic structure of organisms, it was simpler to focus on the structure of an apparently insignificant organism such as the fruit fly (*Drosophila melanogaster*) by modifying its genes—rather than asking overly general questions about the totality of living species.

Similarly, in the second half of the nineteenth century the abbot Gregor Mendel discovered the laws of genetics by inquiring why his pea plants showed characteristics or traits, such as flower color and the external seed texture, that could vary by "discontinuous steps" in a systematic way (Mendel 1866/1901).

Once the domain of investigation is restricted, it is always necessary to try to formulate simple questions and hope that they will help us grasp some substantial aspect of the phenomenon without ambiguity, and in turn shed light on all the complexity and make it possible to think up an experiment to verify if the theory is correct.[19] Let's consider a concrete example. Imagine being in the countryside, and making a fire near a waterfall. In this scenario, we will observe, among other things, two different objects that are making different movements: the smoke that goes up and the water that goes down. Why?

This reality has always been under everyone's eyes. But the interpretations have not been identical throughout the history of scientific thought. In Aristotelian physics, the description of these phenomena brought people to think that there were at least two different kinds of natural movement due to two different forces: one force that pulls down (the force that acts on the water) and one force that pushes up (the force that acts on the smoke). More generally, Aristotelian physics assumed that there were different forces that made objects move toward their natural seats, according to the elements they were made of. So physics initially described these two different movements as resulting from two different principles—thus "surrendering," so to speak, to the initial sensory data without thinking beyond them.

The modern scientific mentality finds this answer to the simple question we raised unsatisfactory: the explanation for one or more phenomena together cannot be more complex than the descriptions of the phenomena themselves. In our example, saying that there are *two* principles that account for *two* different movements does not gain us anything. Aristotelian physics stated exactly this, even if upward and downward movement did not concern just smoke and water but all the objects on earth, which were

19. Mathematics differs from experimental sciences in a significant way. Sometimes, in mathematics, it is the simple question that cannot be addressed first. Consider the famous "Goldbach's conjecture," according to which every even number bigger than 2 is the sum of two prime numbers. The question associated with this conjecture is simple, but the answer is so difficult that it does not yet exist.

grouped into two different classes. It is interesting to note that by the time of Lucretius (the first century B.C.) people had already realized the redundancy of this way of thinking: it was not necessary to postulate two distinct forces. It was already understood that it is sufficient to assume that there is just one force—the force that pulls all objects down. But if the object's density is smaller than the density of the body in which it is immersed, the body with the small density rises (Lucretius 1975, 184–205).[20]

A simple experiment can convince us that an object can move either up or down, according to the kind of environment in which it is found. A balloon inflated with cold air will fall if left free in a room, but it will rise if left free at the bottom of a swimming pool full of water. No surprise: The difference depends on the different densities of the environments in which the balloon is. In fact, if the air in the balloon is warmer, and therefore less dense, than the air in the room, the balloon will rise like a tiny hot-air balloon. Thus, the smoke goes up because it is less dense than air and the water goes down because it is more dense than air.

This simple example should help us understand that there is substantial difference between explanation and description, at least in terms of simplicity: in order for a theory to be the explanation of a phenomenon, it needs to be simpler than the description of the phenomenon. It is not easy to measure simplicity, but in some cases, like the one just mentioned, intuition helps us: a theory is simpler if the number of phenomena that it describes is larger than the number of principles that it assumes. If I need *two* forces to describe *two* different instances of movement, I don't gain much. This example requires some careful consideration.

The advancement of the theory often occurs with phenomena that seem trivial, so trivial that we do not even wonder why they are the way they are. The movement of the smoke and the movement of a body that falls to the ground have been under everyone's eyes forever—like the cycle of day and night, light and dark, hot and cold, and the colors of the rainbow. Nevertheless, only some people manage to suggest new ideas, because only some people see the contrast among facts that look simple. When faced with common phenomena that most people take for granted—such as rising smoke and falling water—some will instead feel surprise and a need to explain, and these people advance our knowledge

20. Galileo Galilei arrived at a similar conclusion, as Geymonat (1957/1965) shows, likely through the influence of Archimedean mechanics.

of the world.[21] In order to accomplish this one must edit reality by means of abstraction. Or, to use the words of Jean-Baptiste Perrin, the winner of the Nobel Prize for Physics in 1926, one needs to ‘‘explain what is visible and complicated by means of what is simple and invisible’’ (quoted in Jacob 1970/1974). But between what is visible and what is simple, the scientist will always choose what is simple.

Linguistics borrows the same research logic based on simple questions. Actually, it offers a case in which the contrast between two facts that are intuitively evident and apparently irreconcilable is even stronger, since the phenomena are commonly observed by everyone. Let’s try to summarize the two extremes of a ‘‘classical’’ linguistic question with the following two sentences. First, as we saw, languages vary in complex ways and their structure is not always accessible to the direct introspection of speakers. Second, any language is acquired by children in an essentially spontaneous way, in a short period of time, and only at an age when their general intellectual skills look *less* powerful than those of adults (usually within the first four years of life). Why? This is a very informal way to formulate the problem, maybe a bit simplistic, but the intuition at the base of the contrast should be evident. Similarly, the theoretical problem that this contrast raises should be evident. How can we reconcile these two phenomena? In other words, how can we arrive at a theory that derives the two phenomena from the same principles? I will anticipate the answer that I will try to illustrate in the second part of the book.

Modern linguistics aims to explain language acquisition in children by hypothesizing a specific biological guide, some sort of ‘‘grid’’ or ‘‘framework’’ that only gives the degrees of freedom within which the individual (and therefore collective) experience can move. One of the fundamental goals of modern linguists is, then, exactly to discover this grid, this hidden structure: in short, *we can even think of modern linguistics as a theory of the limits of experience in language acquisition.* In this book, I will try, through the lens of syntax, to illustrate this way of looking at modern linguistics, and show how linguistics and the neurosciences converge toward the deciphering of the mystery of human language.

21. A famous case is the question of why light goes through glass but not, say, iron. In one of his books, Richard Feynman, the winner of the Nobel Prize for Physics in 1965, starts from this point to show one of the central aspects of quantum electrodynamics (Feynman 1985). From being amazed by a simple fact, one can arrive at totally unexpected and complex results.

1.1.1.3 The Simplicity of the Theories The third way of understanding the notion of simplicity is the *simplicity of the theories*. In the cases we saw in the previous section, a simple question about the movement of smoke and water took us to a simpler theory. Therefore, in a certain sense, we have already dealt with the theme of the simplicity of the theories. In many areas, however—for example, psychology—the notion of simplicity plays a much more crucial role in building the model than just at the methodological level, that is, requiring that a theory has as few algorithms or calculations as possible. It may be useful to start with a comparison with arithmetic.

Let's pretend we are observing human beings while they are counting and that we have to describe their knowledge of arithmetic. In principle, there would be two theories: one is that people archive in their mind all the sums, subtractions, multiplications, and divisions that are possible among a huge, but finite, collection of numbers, for example, the numbers that one can pronounce, write, or think of in one's life. Every time one encounters an operation, one's brain recovers the corresponding structure from the archives and interprets it.[22] Counting, then, would be an activity that is very close to looking up words in a dictionary. If the archive were sufficiently large, this could perhaps be a good way to characterize the knowledge of arithmetic. In the end, every person lives a life limited by time. Therefore, in practice nobody needs to archive an infinite number of sums, subtractions, or numbers that require an infinite life to think of, write, or pronounce. This model could effectively *simulate* a human's knowledge of arithmetic but, of course, nobody would accept this model as real, that is, as psychologically plausible; it is too complex if compared to the obvious alternative: every person archives only the notion of number and successor—or more specifically Peano's axioms—the four basic operations, and the rules to combine numbers and operations. Knowing arithmetic, then, is likely to mean knowing a few rules for combining a few symbols, rather than an endless menu of prepackaged operations (for a discussion on the knowledge of mathematics see Dehaene 1999).

22. This would also imply the unwanted odd consequence that if we had a number that would take, say, more than half of the average life span of human beings to think, write, or pronounce and multiply it by 2, the result would not be defined, for it would require more than an entire lifetime to think, write, or pronounce it.

Borrowing a term from the branch of mathematics known as recursion theory, we can state that a grammatical model that builds fresh structures from a finite set of elements on the base of a finite set of principles whenever called upon to do so—rather than accessing an archive of prepackaged sentences—is a *generative* model.[23] This model is also completely explicit, in the sense that it makes no reference to any kind of undeclared or ambiguous knowledge of the speaker. For example, no speaker of English who buys a French grammar book expects to find a rule saying that an article precedes a noun. The reason is obvious: the rule is common to both languages. On the other hand, if the same speaker studied Basque, the rule would certainly be stated, because in that language articles follow nouns.

A generative grammar of a certain language, then, is just a completely explicit grammar (Chomsky 1995), that is, a combinatorial mechanism that does not take anything for granted and describes all and only possible structures of a language. Sticking to our example, it would be a grammar that always indicates where an article must be put with respect to a noun it refers to even if that language is familiar or is the native language of the reader. By definition, in fact, a generative grammar for a certain language would give *all* and *only* the rules of that language, including of course the syntactic rules.

As you can see, strictly speaking there is no *logical* need to prefer the generative model, rather there is a *psychological* one. Other descriptive models can be more suitable to different goals, such as, for example, teaching a foreign language (no native English speaker needs to be instructed about the position of the article when learning Italian) or build machines which simulate human knowledge of language for computerized procedures (where the archive model might work well). The simplicity of the generative-grammar model as compared to a gigantic archive of sentences makes it the only acceptable model from the psychological point of view, which is, after all, the point of view of humans as biological organisms. The analogy with the knowledge of arithmetic becomes evident here: the only plausible psychological model for the knowledge of arithmetic is the knowledge of the notion of number (and successor), the four

23. It is irrelevant here whether you think of a constructive or a selective grammar. The idea of grammar as a combinatorial procedure rather than as an archive would fit with both perspectives, that is, whether you combine good structures step by step or you filter out all possible combinations at the end. For the sake of simplicity, I do not discuss this issue in the text.

basic operations, and the rules to combine numbers and operations rather than the knowledge of a gigantic archive of specific sums, subtractions, multiplications, and divisions.[24]

But notice that when it comes to arithmetic a further factor is involved. We need to identify it to avoid confusion. The rules of syntactic combination in the grammar are not invented, that is, they do not have to be taught explicitly in school; the rules of syntactic notation in arithmetic, instead, must be taught explicitly because they were invented, although their format might certainly be influenced by the language spoken in the community where they were first proposed (incidentally, mathematical notation is itself a most interesting cognitive capacity of humans; see Dehaene 2005). In fact, there are many different mathematical notations. In the so-called "Polish notation," for example, $16 + 1$ would be written as $+(16, 1)$, similar to when we write $f(x, y)$ in mathematics to indicate a function with two variables, rather than writing, say, xfy. The situation is different for syntax: we use the rules of syntactic combination spontaneously, that is, without consciously knowing them. We don't even know how many syntactic rules a language has, although we can infer that their number must be finite—otherwise no one would ever finish learning them and be able to speak coherently. Syntactic rules have to be discovered in the same way that a law of physics has to be discovered. We will come back to this issue in the next section.

Finally, notice that the choice in favor of a combinatorial mechanism for grammar or arithmetic as opposed to a huge archive would also make language acquisition amenable to a psychologically plausible explanation. The very idea that the knowledge of language or arithmetic comes from memorizing and mastering a huge archive containing lists of full expressions rather than from a combinatorial mechanism of finite elements governed by filters makes the process of acquisition by children a virtually impossible phenomenon, unless of course, one admits that children's memory is something totally different from what we know about adults' memory.

To conclude, we summarize the discussion of the notion of simplicity in the following way. The search for simplicity is an important engine for scientific research. Often, this search starts from the astonishment caused

24. In some sense it is as if we were applying a naturalized version of the so-called Ockham's Razor to the mental models, according to which entities cannot be multiplied unless necessary, taking into account psychology and not just a general principle of economy.

in the observation of apparently irreconcilable facts; the search is almost always discretional in the sense that it usually issues from an individual's intuition. The simplicity of a theory is measured according to its capacity to discover new data with the same set of principles or operations or derive known data from a smaller set of principles or operations. In investigating of the facts of the mind, more specifically, the search for simplicity stands out as more than a mere methodological or logical requirement: it becomes psychological. The psychological plausibility of a simple generative—that is, combinatorial (and explicit)—model versus the archive model is intuitively evident. We will see this criterion of simplicity in action when we talk about pronouns in the second section of this chapter.

1.1.2 Errors

There are arguably few other areas in which the definition of errors is as natural as it is in grammar. This should not surprise us for at least two reasons. On the one hand, grammar, even at an intuitive level, is one of the most complex systems that we deal with every day (this is obvious to people who begin to study a foreign language). It is, therefore, also obvious that the number of errors produced is high or is at least higher than in a simple system. On the other hand, although it is true that we deal with other complex systems every day (consider the complexity of one of our internal organs, or a cell phone or a railway system), it is also true that grammar can be observed without sophisticated equipment and that it is constantly under our conscious examination, both when we produce and when we listen to or read sentences. It is not by chance, therefore, that the notion of "error" is directly connected to grammar. Actually, the concomitance of these two facts—complexity of the generative grammar and easy detectability of errors—makes the production and identification of grammatical errors (in the sense of generative grammar, not "conventional" grammar) so easy that they are generally seen, perhaps along with mathematical mistakes, as the quintessential type of error.

In this section, I would like to show that the notion of error is connected to grammar in a more complex way than we usually think. By starting from the more traditional meaning, we will explore three types of errors: error as *deviation* in relation to literary production, error as *omission* with respect to the recorded data, and error as a *tool* of scientific investigation.

1.1.2.1 Error as Deviation The term *grammar* was first used in classical Greece. Until the time of Plato and Aristotle, this term, or more correctly, *téchne grammatiké* (literally, art of grammar), was used just to refer to the capacity to correctly use letters (*grámmata* is from the verb *grâpho,* "I engrave," which refers to the most ancient writing technique, engraving on stone). Later, grammar stabilized into a series of complex paradigms that codified the correct use of language.[25] It is not by chance that one of the most important moments of the stabilization of the standard grammatical model occurred during the Hellenistic period, especially the second century B.C., just when it was decided to collect all the known literary production in the Greek language in large cities like, for example, Alexandria in Egypt. In order to collect, you need to organize and exclude. In order to exclude, you need a standard for comparison.

A harsh and extremely interesting debate between the two most important philological schools of the day, that of the Analogists of Alexandria and that of the Anomalists of Pergamus, gave rise to a grammatical model that has survived until today and serves as the primary basis for modern grammar books.[26] Consequently it is not surprising that the person who is commonly acknowledged to have written the first Greek grammar in Alexandria, Dionysus Thrax (about 170–90 B.C.), stated that "grammar is the practical knowledge of the general usages of poets and

25. Reflection on the nature of language, which we now call "philosophy of language," was not at all absent in Greek thought (just think of Plato's dialogue *Cratylus*), but it was not part of a grammarian's competence.

26. According to the Analogists, based in Alexandria (especially Aristophanes of Byzantium and Aristarchus of Samothrace, who lived during the second century B.C., but also Julius Caesar himself), grammars are conventionally built out of the analogical process, the symmetry that is found among similar forms, as in verbal inflection and nominal declension. But according to the Anomalists, based in Pergamon (especially Krates of Mallo, a contemporary and rival of Aristarchus), it is exactly the natural, unconventional deviation from the standard that generates meaningful structure. The more an element differs from the others with respect to monotone schemes, the more significant it is. Thinking of the Anomalist viewpoint, it is impossible not to recall one of the fundamental dogmas of Saussurian structuralism: the value of an element is defined only in opposition to the others, rather than in an absolute way. De Saussure loved to make the analogy between grammar and chess: it is useless to know merely the position of a rook on a chess board; what matters is knowing where the others pieces are with respect to the rook. In a system, what is important is the difference. Anomaly and analogy are two archetypes that have never been abandoned in linguistics and more generally constitute two paradigmatic distinctions in the way to look at the biological world.

prose writers" (Robins 1997, 38).[27] Clearly, the literary model is considered the absolute standard for comparison; spoken language must follow it to be considered correct. The priority given to literary language over spoken language has persisted over the centuries. For instance, Italian was created from the language spoken in Florence. This choice was due to the prestige of Tuscan writers such as Dante Alighieri, Francesco Petrarch, and Giovanni Boccaccio and the influence of their works on the language.

Given this cultural background, it is not surprising that the grammatical error is, for the most part, what deviates from the literary standard. It does not matter if a certain expression is "productive," that is, extremely frequent in the spoken language of large social, regional, and cultural areas. The fundamental duty of a teacher is to filter the linguistic production of students by excluding dialectal expressions, family lexicon, and slang expressions in favor of the literary model. This occurs at any level of grammar—phonological (pronunciation), morphological (paradigms), lexical-semantic (words), and syntactic (rules to combine words). Let's look at an example from morphology, the choice of the form *I* versus *me* for the first-person singular masculine pronoun.[28] Your instinct may be to say in casual conversation *Me and my best friend went to Los Angeles last weekend*. But your teachers are like to have told you that it is wrong and that you should instead say *My best friend and I went to Los Angeles for the weekend*. Similarly, you may want to choose to say both *I wish I was more focused on my goals* and *I wish I were more focused on my goals*; teachers would point out that only the latter sentence is correct after the pronoun *I* and the verb *wish*.

We are not here to discuss whether these corrections are appropriate or not. It is definitely important—indispensable, actually—that young people be trained to use language in the best possible way and recognize different styles and rules. What we are interested in here, is to observe that this notion of "error" is inherently bound to the deviation from a stylistic and thus a social and cultural norm. There is nothing *intrinsically*

27. This grammar did not include syntax, however. Apollonius Dyscolus, a grammarian who lived in the second century A.D. is often credited with that accomplishment.

28. *Masculine* and *feminine*, from the Latin words expressing male versus female, only express a binary distinction. There is nothing inherently masculine in a noun like *cappello* (hat) or an adjective like *nero* (black). These two terms are used in linguistics only because they are the prototypes of all the binary distinctions, similarly to the pair *weak* and *strong*.

"wrong" in this kind of error. Actually, what is considered an error at a certain time could very well become the norm in the future; and what is an error in one language can be correct in a different language. For instance, the double negation in *I don't see nothing* is considered an "error" in Standard American English, but it is perfectly fine in other varieties of (American) English. In Italian and Spanish it is the only option. In general, all attempts to crystallize a language into a grammar that would remain stable over time have failed, except perhaps for some cases of spelling. Language, the philosopher of language Ludwig Wittgenstein said, "takes care of itself."

1.1.2.2 Error as Omission So error as deviation from the stylistic norm has been the most natural interpretation of "error" for at least the last two thousand years. During the twentieth century, however, a new conception of error developed alongside the old, normative one. This new notion reflected a fundamental revolution in linguistic thinking. Linguists no longer pay attention to just the evaluation of attested forms and deviations from the norm. *Modern linguists are also interested in the possible forms that are not attested, forms that are not used and in fact do not exist.* How is it possible to hold such a paradoxical position? Why study something that doesn't exist? To understand this revolutionary theoretical and methodological leap, it is important to have an idea of the historical moment when it developed.

Until the first half of the twentieth century, linguists almost exclusively analyzed texts.[29] They were also interested in texts collected from everyday spoken language, and this was a departure from tradition, stemming mainly from the fact that in addition to prestigious literary texts, language scholars were also interested in less elevated styles, jargons, and non-written languages like those spoken by Native American tribes. The difference in approach was not, however, substantial: in the end, linguists were still dealing with the classification of bodies of linguistic data (corpora) by means of various tools, from statistics to more typical linguistic tools. This phase of linguistics coincided with and interacted with behaviorism in psychology. According to behaviorists, the object of study should be only the reaction to a stimulus, not what's behind it, that is the (human)

29. This is a rough simplification. See Robins (1997) for a brief overview of linguistics in the twentieth century and Graffi (2001) for a critical in-depth discussion of syntax that in many respects is paradigmatic of linguistics in the twentieth century.

mind. In the case of language, then, linguists should have been confined to the sentences that were *actually* produced in a given community. The capacity to produce and understand an infinite number of sentences—and therefore the structure of mind as well—remained by definition inaccessible to scientific investigation. Linguistics changed its object of study around the mid-1950s, thanks to the work of an American linguist, Noam Chomsky, who invented and developed the notion "generative grammar." Chomsky's first book, *Syntactic Structures* (1957), includes most of the intuitions presented in this methodological introduction.

Many modern linguists no longer study a collection of sentences actually produced by a certain linguistic community; instead they study the "implicit" knowledge that each speaker of that community has of his/her grammar to produce *any* sentence in that language—his/her so-called linguistic competence.[30] Here, "any sentence" practically means "an infinite number." Although it is true that no human can produce an infinite number of sentences, or sentences of infinite length, it is also true that the number of possible combinations of words in a language is so huge and goes so far beyond the number of sentences that a person will actually hear or produce over a lifetime that we can say that to all intents and purposes the number of combinations is virtually infinite. For modern linguists, the importance of attested forms fades: *the attention moves from the product to the mental capacity* that has allowed this product to be generated.

This change of perspective reflects a fundamental fact about human language that we are all at least implicitly aware of: every person can understand and produce a practically infinite number of sentences that have never been heard or produced before. (Descartes, in the seventeenth century, recognized this capacity as a distinctive feature of our species, and his insight was reinforced by von Humboldt in the nineteenth century.) The rules that produce this potentially infinite repertoire are not immediately accessible to introspection. To decipher human capacity to produce an infinite number of sentences means, therefore, discovering the rules that govern production, rather than just describing the product.

30. "Competence" is in opposition to "performance," which refers to the concrete application of competence in order to produce a sentence. Competence is like the knowledge of an entire map; performance is the movement along a specific path between two specific points on the map. In the best of all possible worlds, the representation of performance would be just competence plus time. In reality, experimental research has shown that many other factors play a role, such as *parsing,* the sequential analysis of a sentence.

Every speaker explicitly knows *some* of the rules of his own language, but the number and the complexity of all the rules are clearly not accessible to anyone. For example, how many speakers of English can say what the rules are that determine the ordering of the subject and the helping verb in a sentence. For instance, you say *he will come later but he won't help us with the party,* with the underlined subject, *he,* of the first clause preceding the underlined helping verb *will* (or its contracted form, *'ll,* if it sounds better to you). But you also say sentences like the following in which the helping verb precedes the subject: *Not only will he come later, but he won't help us with the party.* How many native speakers of English know what the rule that accounts for this word order difference is? Not very many, most likely. Nevertheless, they know how to apply the rule without any doubt. In fact, they know that the following sentence, in which they did not apply the rule, is not good sentences in English: **Not only he will come later, but he won't help us with the party*. This is just a simple case that shows that the knowledge of a rule that the native speaker of a certain language uses is not necessarily explicit. Actually, most rules are not accessible to speakers' direct introspection, as noticed earlier. One of the clearest methodological assumptions of modern linguistics is that speakers' minds have to be investigated by means of experiments, conjectures, confutations, and, finally, theories.

How do linguists use this assumption as a basis for their actual linguistic research? A syntactician formulates a hypothesis about a rule or a group of rules in a language, builds a sentence, and then asks a speaker of that language if the sentence "sounds good" or not. Depending to the answer, the syntactician corrects the hypothesis and asks new questions. For instance, by looking at a contrast like *Paul says he is coming* versus *He says Paul is coming*, the syntactician could start hypothesizing a rule according to which a pronoun, *he*, cannot refer to a noun, *Paul*, if the pronoun precedes the noun. Then the syntactician builds more complex sentences in order to test this hypothesis: *When the teacher says he is lazy, Paul complains to the Queen*. In this way, the syntactician finds out that the hypothesized rule is wrong and tries new strategies (we will see what the rules that account for these sentences in the next sections).

Modern linguistics, then, is an empirical science like all the others: what supports a linguistic theory is experiments, the possibility to repeat the relevant facts in the same conditions (the acceptability of a linguistic structure here), the acceptability of a linguistic structure. It is interesting to note that modern linguistics has borrowed a typical behaviorist technique: the attempt to get an answer or response to a stimulus. The crucial difference from behaviorist models is in the way modern linguistics makes use of these data: to decipher the structure of the human mind (in this case, speakers' capacity to produce an infinite number of sentences), not just to catalogue sentences that have been actually uttered.

What then is the meaning of error in this theoretical and cultural model? Definitely not deviation from a stylist norm. Let us go back to our example about the word order of subjects and helping verbs. There is no doubt that **Not only he will come later, but he won't help us with the party* is ungrammatical in English. Also, it is unlikely that such a sentence is ever produced, except in case of a slip of the tongue (slips of the tongue are themselves of interests to linguists). On the other hand, the frequencies of expressions such as *Me and my best friend* and *I wish I was there* is much higher. Therefore, the error we are talking about now is not an attested form, one that deviates from the stylistic norm and that is socially marked, and that may repulse "well-educated" people. It is instead the "absent" form, the form that is rejected all the time, the omitted form that no speaker ever produces, like the sequence noun plus article in English (**house the*). Linguists try to account exactly for these systematic omissions, which cannot be just tracked back to casual or conventional historical facts. If syntax were a heap of random rules that had piled up by accident in the course of history, we would expect that *any* combination of rules would be turn up sooner or later in a certain language; in fact, the data we have do not support such infinite variability among the syntaxes of different languages.

One of the great achievements of the linguistics of the twentieth century was the hypothesis that not all possible combinations of syntactic rules are actually attested, that is, used in actual language. Another American scholar, Joseph Greenberg, made a significant contribution to this hypothesis. Greenberg (1963) studied a large sample of languages from different language families, then formulated fundamental generalizations about possible combinations, which he called "implicational universals," that are at the basis of "linguistic typology." This subfield of linguistics aims at classifying languages independently of their historical ancestors and finding hidden generalizations among them, or "natural classes."[31]

31. "Natural class" is a term borrowed from phonology. It is a set of phones or phonemes that share certain common phonetic features. A natural class can be identified by using fewer features than would be necessary to describe each phone contained in it individually. It is not only a convenient descriptive device, though: a natural class plays a significant role in capturing the regularities showing up in the phonological systems of human languages.

For example, in a typological framework languages are grouped together by comparing structural properties such as the position of the Subject (S), the (inflected) Verb (V) and the Direct Object (O) in declarative affirmative main sentences. Assuming this grid, it turns out that the majority of human languages are included in the natural class where the Subject precedes the Direct Object (SVO, VSO, SOV) whereas the rarest—and for many linguists nonexistent type—would be the mirror image of SVO languages: namely, OVS.

As a simple example of Greenberg's implicational universal number 4 we can have the following paraphrase: "With overwhelmingly greater than chance frequency, in a language of the type SOV prepositions follows the nouns they are associated with." Thus, for example, in Japanese, where one says *Nori wa ocha o nomimashita* (literally, Nori-subj. tea-obj. drank; "Nori drank tea")—where the object precedes the verb—one also says *Tokyo ni* (literally, Tokyo to, "to Tokyo")—putting the preposition after the noun it refers to.

The central point of Greenberg's discoveries is the predictive power of one phenomenon with respect to another: if, in a language, some syntactic properties hold, then some others can be expected to hold as well. It directly follows from this logic that not all syntactic properties are compatible with each other; in other words, *languages cannot vary indefinitely, since the syntax of natural languages tends to exhibit clusters of homogeneous properties.*

With his fundamental research, Greenberg's goal was to build a typological description of data; he made no attempt to infer anything about human mental structure or any potential innate or biological bases for these regularities he discovered.[32] Despite his lack of interest in making such inferences, his results are not at all incompatible with the investigation of language knowledge in biological terms that characterizes the generative school (see Graffi 1980, among others). Actually, Greenberg's results offer an extremely rich empirical basis that converges with many generativist theoretical assumptions. Stated briefly, *generative grammar turned out to be the theory that identifies the class of possible human languages as they are constrained by biological restrictions.*[33] This does not

32. I use the term *innate* to refer to what comes before experience, what is determined just by biological limits and possible structural, physical limits in a very general sense.

33. In the next chapters we shall see that "possible language" in practice coincides with "learnable language." The tension between the two notions, however, is not completely resolved, in light of the neuroimaging experiments in chapter 2.

contradict the initial definition of generative grammar as an "explicit theory of all and only the sentences in a language." Indeed, the discoveries made regarding grammar as a formal, explicit system are the necessary logical presuppositions upon which the theory of language as a biologically determined system is grounded. The knowledge of the clusters of homogeneous properties in the syntax of various languages in fact constitutes fundamental, indispensable support for research projects probing for biological constraints to grammars.

In conclusion, we are definitely far from the initial meaning of error as deviation from a literary norm, although we have not abandoned this first meaning. What matters is that a different meaning has made its way next to the traditional one: error as omission.

1.1.2.3 Error as Tool We have seen how the notion of error has changed from the traditional meaning of deviation from a norm to the modern meaning of absent or unusable forms. There is a third meaning of error, which differs from both the traditional and the error-as-omission meanings: error as an element that has a heuristic value—as an element that is useful for the discovery of new facts, as a tool. Consider the following sentences we introduced earlier: *The circle is red, The circle is square,* and *Circle the is red*. It is obvious that the first sentence is the only acceptable one. The other two contain some anomalies: the second one contains a contradiction, and the third is a word mix, not a "well-formed" structure. It is equally obvious that the error in the second sentence (*The circle is square*) is not of the same kind as the one in the third sentence (*Circle the is red*). The two sentences are "erroneous" in two different ways: the second sentence is erroneous because it is impossible to imagine a circle that is square, and the third one is erroneous since its word order is never attested, or used, in English. An endless number of examples of either kind of error can be produced: *Red is black*, *Round fat this is big belly*, *Happiness is cooked*, *Happiness emotion is an*, and so forth. On the basis of simple experiments of comparison of different errors, linguists have hypothesized that there are two different levels of representation of the structures in speakers' minds: a level where it is computed whether the meaning of the different parts that form a structure are compatible with each other, and a level where it is computed whether the word order (more generally, syntax) is correct.

The fact that we can produce different kinds of errors leads us to make hypotheses on the structure of language that otherwise would be empirically unjustified. This is why I said earlier that error acquires a "heuristic value," since linguists turn it into a tool to distinguish different components of grammar, as we just saw, and, more generally, different components of our minds. These different kinds of errors are not limited to the case we just discussed; they can be reproduced in all domains of grammar. The more kinds of errors we are able to distinguish, the better we can justify different levels of representations in the grammar, for instance, the morphological level with respect to the phonological and syntactic level.

On the other hand, the meaning of error as a tool for scientific investigation becomes apparent also in a completely different context—in studies of the brain. The argument gets more complicated in this case and it is better to deal with it separately. The heuristic aspect of error will become a central pillar of the experimental paradigm used to investigate the brain in the discussion in chapter 2.

Let's conclude this short survey of the meanings of errors in linguistics. Our primary aim is to highlight the fact that the notion of error has acquired a meaning and a role that are very different from those we are traditionally used to. Error has become a founding central factor in the theory and practice of modern research. Error no longer means just a deviation from a stylistic norm; it has become a significant object of study and also a research tool. We have moved from a simple definition of error in grammar to the characterization of a more complex typology. A typology that in some sense overturns things and brings us to identify what we could call, using an oxymoron, "a grammar of errors."

1.1.3 Formalism

What do we mean by *formalism*? There are many definitions extant, but for our purposes a workable definition is to think of formalism as a way to translate concepts and relations among concepts into symbols and formulas that connect them in a generative way. Formalism has a huge impact on scientific research. Consider the big leap forward that occurred in physics in the seventeenth century after the invention of the calculus by Leibniz and Newton, or the swift development of the study of chemistry when a symbolic system was invented that was based of experimental models that could be developed theoretically without direct access to chemical components. Formalism in science did not develop all at once. The formulas that we use nowadays evolved from techniques that have developed step by step over millennia, starting at least as far back as the Arabic algebraic calculus, proceeding through the big innovations in calculus in the seventeenth century and the explosion of analytical calculus and abstract algebra in the nineteenth century and arriving at the innovations that were introduced in the last century to handle the concepts of quantum mechanics, such as the "Feynman's diagrams" of Richard Feynman, who won the Nobel Prize for Physics in 1965.

In linguistics, too, the use of formalism has triggered previously unimaginable developments. In the study of syntax, the experience that was accumulated in manipulating symbolic elements from formal logics from the end of the nineteenth century to the beginning of the twentieth century has brought huge advances. The use of formalism in science brings several other advantages that cannot be so easily detected right away. We will soon see that formalism has three properties: it is concise; it is deductive; and, most important, it is heuristic. These three characteristics cannot be clearly seen right away, and I think it is useful to try to understand them separately.

1.1.3.1 Formalism Is Concise Let's start with a simple example. If we go back to the physics that we learned in school, many will know Newton's Law of Universal Gravitation, according to which *the force that attracts one mass to another is identical to the product of the two masses divided by the distance between the two masses multiplied by itself, the whole thing multiplied by a certain number that is called the "gravitational constant."* How many characters have we used to express this central law of classical physics? Two hundred six, not counting the spaces. We can do much better by using the formalism of physics. If we call the force that attracts one mass to another F, if m_1 is one mass and m_2 is the other, if r is the distance between the masses, and G is the number that expresses the gravitational constant, the long sentence describing Newton's Law of Universal Gravitation can be expressed as $F = G(m_1 m_2/r^2)$ (See Feynman 1965/1989 for critical remarks on the "language" of the laws of physics). What have we gained? We have certainly saved ink. More important, we managed to express the very same concept in a much more compact way: we used 12 characters instead of 206. The advantage is evident. Formalism is *concise*.

But this is not, of course, the most important advantage of the use of formalism in science. Formalism allows us to manipulate symbols in such a way as to advance our knowledge of nature, those mathematical symbols—in a broad sense—in which, as Galileo said, the book of nature is written.

1.1.3.2 Formalism Is Deductive A second and less trivial aspect of formalism than merely saving space on the page is that it allows us to determine an element of the relation based on the other elements. For instance, if we don't know the value of the constant G in Newton's Law of Universal Gravitation but we have enough other data, we can determine the value of G by manipulating the formula according to the rules of calculus and get $G = F/(m_1 m_2/r^2)$. This result is far from insignificant. If we had used a vague descriptive sentence, we could have had to work much harder to get the value of the constant, although in principle this can be done. Unlike the prose version of the law, the formula is based on an apparatus of formal relations that allow us to manipulate symbols without ambiguity and in this way get the definition of the element in terms of the others. This fact, together with the fact that these symbols are numbers, has enormous advantages and is the reason why we say that formalism is *deductive*.

If this were the only advantage, besides the speed of calculation, it still wouldn't be dramatically remarkable. It is a fundamental fact that concise expressions allow us to see analogies among different phenomena in a very convenient way. For example, physics equations, the result of an extremely complex formalism, are able to make the similarities between classes of apparently unrelated phenomena "visible," which allows for significant scientific advancement. A clear example of these analogies is Coulomb's Law, which describes the forces between electrically charged bodies. The force F with which the two distinct charges q_1 and q_2 interact is inversely proportional to the square of the distance r that separates the two charges. Formally, the representation of this law is $F = k(q_1 q_2/r^2)$, where k is constant. The analogy with the Law of Universal Gravitation—and the suggestive theoretical consequences—cannot be missed: formalism just makes it evident.

1.1.3.3 Formalism Is Heuristic The third aspect of formalism, the heuristic one, the one that is probably most difficult to understand, is of the greatest importance to us in this journey through the foundations of syntax. Perhaps the most useful comparison for understanding the value of the heuristic aspects of formalism comes from chemistry. Many of us know that lines, letters, and numbers are now used in chemical formulas to represent relationships among atoms and other chemical entities. Take $C_{10}H_{12}N_2O$, the serotonin molecule (figure 1.1). Many will remember that the famous "benzene ring" (represented by the hexagonal form in the formula) discovered by Friedrich August Kekulé (1865)—while he was dozing in front of a fireplace, it is said—was the result of the combinatorial manipulation of those symbols according to the laws

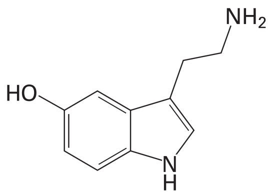


Figure 1.1
Lines, letters, and numbers used in the chemical formula for serotonin to represent relationships among atoms.

of chemistry. At that time, however, there was no direct experimental evidence clearly indicating that the physical organization of that molecule coincided with that "ring" form: formalism was suggesting it.

In this case, formalism dramatically preceded experimental confirmation. Another interesting example of the use of formalism as an aid in discovery is described in James Watson's book *The Double Helix* (1968). Watson reminds us that when he already had in his hands all of the "pieces" of the structure of the DNA molecule, he had someone build the "mechanic" models of the atoms and the bonds between the atoms in order to help him divine the correct form, the double helix, which was in competition with other possibilities, including a triple helix. Actually, at a certain point he was so impatient, he says, that he himself cut the various elements on pieces of cardboard, trying to combine them in such a way that the whole thing would hold together. At that point, says Watson, the structure "emerged by itself." Somehow, the definition of the primitive elements and the rules of combination had been so well thought out that the real structure turned out to be the only possibility.

Let's even assume that there is a bit of creative and heroic narration in these two stories. It still seems to be true that if we start from good primitive elements and good rules of combination, formalism certainly helps in building new hypotheses and producing discoveries. Let's stop here. This digression on formalism has brought us to understand some fundamental, interconnected properties: concise, deductive, and heuristic. How does all this relate to linguistics?

Formalism plays a decisive role in linguistics as well. One example of formalism's concise nature in linguistics is that we can talk about V, N, A, or P, instead of directly mentioning verbs, nouns, adjectives, or prepositions. Sentences such as *Dante loves Beatrix* or *Newton knew Leibniz* can be represented, at a certain level of abstraction, as noun-verb-noun constructions (N V N); longer sentences can be represented by our increasing the number of symbols as needed, according to the parts of speech that we are interested in analyzing. However, formalism's conciseness per se is not very useful in linguistics. The deductive property has applications in linguistic formalism as well, although at the present moment the symbolic system of "calculus" is not nearly as developed as the mathematical one. That said, it is not unusual to define categories on the basis on formulas. It is possible to determine the definition of some linguistic categories (such as subject, predicate, direct object, and so on) on the basis of the combination of primitive categories and thus capture many analogies, as was the case with "unaccusative constructions" studied by Burzio (1986). Actually, it is exactly the heuristic aspect that resonates most in linguistics, especially in the field of generative syntax. In order to better understand this aspect, though, we first need to learn some fundamental aspects of syntax: we will be able to construe structural representations that in a sense resemble the chemical representations illustrated here, where words (or morphemes) will be the atoms and sentences the molecules built upon them. We do this in the next section, and then go back to the heuristic property of grammar in the section 1.3, "The Ark of Babel," and in chapter 2, "The Form of Grammar."

These methodological considerations now enable us to think of a possible list of questions concerning the universe of language, with the awareness that we are going to take a nonobjective position, one that may not necessarily be correct. We will be able to determine how effective these questions are only if they bring us to an effective extension of our knowledge—even if this means that we risk opening the field to new and more complex questions.

Let us consider a list of classical questions of modern linguistics—not exclusively of generative linguistics. What do we know when we know a language? How do children get to know it? How is this knowledge biologically implemented? How much can languages vary one from another? How has this capacity evolved in our species, if it ever evolved? Why do languages change? How can we know more than one language? Following the methodological premises we have chosen, we will not investigate all the properties of languages and all these questions. Rather, we too will choose a "simpler" idealized prospective. Not only will we restrict our attention to the domain of syntax, but we will also only focus on two simple questions: First, *does syntactic processing selectively activate a specific neuronal net?* Second, *have the existing boundaries of variation in the syntax of human languages any neuropsychological correlates?* We will see that this simplification will have to be pursued further if we want to build a feasible experimental paradigm. But before we explore the data on the functional architecture of the brain, the next preliminary step will be to highlight the fundamental aspects of syntax that will be at play in the neuroimaging experiments.

1.2 A Sample of Syntax

Among all the other properties of human languages, it is the richness of the syntactic structure of the code that seemed to be the crucial difference between the human and the other systems of animal communication. Certainly, animal languages can be extremely complex, with surprising regularities.[34] But given what we know, there is no other species that can manipulate words or their equivalents, if there are any, with a level of complexity that is even remotely comparable with the syntax of human languages. In this sample of syntax, we will see what properties characterize the syntax of human languages, but before examining them, we will briefly deal with the question of primate language (primates are a superorder of mammals that includes apes and monkeys). Observing the limits of the language of apes is useful because it provides a different point of view for appreciating the unique characteristics of human languages.

You often hear the claim that primates such as chimpanzees or gorillas are able to communicate in a sophisticated way. This is certainly true, but it is also true that virtually none of the typical features of the syntax of human languages are found in primate language. This has been known at least since the end of the 1970s but is often forgotten. In this regard, it is interesting to examine the classic work by Terrace and colleagues (1979). Focusing on chimpanzees (*Pan troglodytes*), the species that is

34. See Hockett (1960), on the identification of the features of animal languages. Interestingly, the question "can animals talk?" reminds me of another question addressed in the fundamental work by Turing (1950, 433ff.): "I propose to consider the question, "Can machines think?" This should begin with definitions of the meaning of the terms 'machine' and 'think.' The definitions might be framed so as to reflect so far as possible the normal use of the words, but this attitude is dangerous. If the meaning of the words 'machine' and 'think' are to be found by examining how they are commonly used it is difficult to escape the conclusion that the meaning and the answer to the question, 'Can machines think?' is to be sought in a statistical survey such as a Gallup poll. But this is absurd. [. . .] The original question, 'Can machines think?' I believe to be too meaningless to deserve discussion. Nevertheless I believe that at the end of the century the use of words and general educated opinion will have altered so much that one will be able to speak of machines thinking without expecting to be contradicted."

genetically closest to *Homo sapiens*, they studied the potential that chimpanzees have to learn the syntax of a human language. The case is extremely interesting, not only because it is known that the genetic difference between chimpanzees and *Homo sapiens* is a little less than 2 percent of their respective genomes. Terrace and colleagues showed beyond doubt that the syntax of chimpanzees does not reach the complexity of the syntax of a child that hasn't yet attained linguistic maturity, not to mention the complexity of the syntax of human adults.

The background to their study was that in the 1970s, especially in the United States, people began experimenting with the learning of human language in primates. In some research groups, including those led by the Gardners, Premack, and Rumbaugh (described in Terrace et al. 1979), chimpanzees were taught forms of artificial languages based on small colorful objects or on American Sign Language (ASL; see Neidle et al. 1999). If you have read articles on these experiments, you will remember the chimpanzees Washoe, Sarah, and Lana as minor stars of science, who were able to astonish through their learning of human language, according to the researchers. The most famous case, perhaps, is Koko, a female gorilla who allegedly learned four hundred different "words" of ASL.

The research implied that testing the chimps for linguistic ability using not sound production, for which primates are at an anatomical disadvantage, but different symbolic systems—in particular ASL—gave encouraging results in support of the hypotheses that primates can manipulate symbols at the same level of complexity as human beings. The work by Terrace and colleagues, however, that will be described briefly here demolished the idea that if the chimps were not obliged to use sounds to communicate, they would be able to use the same language as humans. It should come as no surprise if I state that the linguistic component in which primates fail dramatically is syntax. The second author of the Terrace et al. paper was Laura Petitto, a Canadian researcher who has also recently made important contributions to the study of ASL (see, for example, Pettitto et al. 2003). Her research group raised a baby chimpanzee, Neam Chimpsky, from the age of two weeks to four years (Later on, she was nicknamed just Nim in an attempt to obscure the obvious Noam Chomsky take-off). For four years, Nim lived in a house with a "family" of humans who communicated with each other in ASL. For four years, the researchers meticulously recorded each bit of progress Nim made. In a short time, Nim managed to learn to sign 125 different ASL "words." The researchers were extremely scrupulous in their list of learned words. A word was considered learned only if two conditions were satisfied: First, at least three observers (members of Nim's "family") had to independently record Nim's use of that word. Second, Nim spontaneously had to use this word again within five days. With this criteria in place, we can be confident that Nim's list of words was not overestimated. Nim was truly able to acquire a small ASL vocabulary.

The unwelcome surprise with respect to the previous researchers' overly optimistic conclusions came from the examination of Nim's "sentences"—her strings of ASL words. The researchers transcribed and filmed almost 25,000 sentences and the data underwent a detailed and complex statistical and linguistic examination. First of all, more than half of these "sentences" were made of only two ASL signs, and their order was unpredictable (Nim signed *Eat banana* or *Banana eat* to indicate the same meaning). In the cases with more than two signs, the "sentence" often contained the simple repetition of an element that had just been produced (*Banana eat banana*) or even the repetition of the entire sequence (*Eat banana eat banana*). Furthermore, there was no "sentence" with more than two signs in which the extra sign had a structural function. Let me explain. Laura Petitto uses a very clear example. Almost every time a child says *Sit daddy chair,* what the child means with *daddy* right before *chair* is that the chair belongs to daddy. This is not just a random juxtaposition of words but a meaningful juxtaposition. The addition of the word *daddy* between *sit* and *chair* has a precise structural function: it specifies a person's ownership of an object. Similarly, adults will say *Sit on daddy's chair* after full acquisition of English morphology. If the child had uttered the three words in a different order, for instance *daddy sit chair*, we can imagine that he or she may have meant something completely different—that daddy sits on the chair. Word order—syntax—matters in a crucial way. In sum, not only did Nim's "sentences" with two signs have no structure, but Nim did not show by increasing the number of signs that she wanted to enrich the simpler structures: she was just able to produce unconnected strings of signs. Put another way, if we allow ourselves to imagine than Nim could have used three ASL signs to produce the two sentences *Daddy sit chair* and *Sit daddy chair*, these two sentences would have meant exactly the same thing to her.

The differences between Nim and a small child do not end here. The average length of sentences produced by some of the children who were involved in the experiment when they were about twenty-six months of age was about the same as Nim's (for the sake of brevity, I won't go into the methods used to calculate the average length of sentences). When Nim was fifty-two months, her sentence lengths remained about the same while the children of that age were producing sentences eight times longer. Another substantial difference was observed between Nim and the children: In 71 percent of the cases, Nim signed at the same time as the people she was talking to, whereas the children learned very early that speaking is a "game" in which you take turns: one speaks and the other listens (which says a lot about the behavior of certain adults, but this is another story). Also, in many cases, Nim simply imitated the interlocutor without producing signs autonomously.

In summary, Laura Petitto and her colleagues clearly showed with their elegant experiment that although the use of ASL can facilitate the learning of single words by primates, like the baby chimpanzee Nim, *the syntax of a chimpanzee cannot be even remotely compared to the syntax of children who are acquiring their native language and even less to the syntax of adults*. Chimpanzees never reach the early level that children reach and definitely never reach the final level that humans reach. The richness of the syntax of the linguistic code remains, therefore, a trait of our species. *Homo grammaticalis,* if we can use the name, is in the end a proof that human language stands out as an isolated point in the evolutionary tree, in Gould's (1977a) sense. More specifically, there is no "less-evolved" equivalent to the syntax of human languages in the languages of other species, unless we radically dilute the notion of syntax as applied to human languages in order to cover far less complex communication systems. Such a dilution would be similar to stretching the notion of bird flight so that it covers human jumping. In the next section, we will see precisely what we mean when we speak of the "richness" of the syntax of human languages. But it is worthwhile to anticipate here, so as to bear it in mind, the fundamental fact that *the syntax of human languages, in spite of the linearity of the acoustic or written signal, cannot be modeled as a flat concatenation of words but hides a much more complex structure.* We can now abandon the term "sentences" when we talk about primates. Primate communications are not sentences at all. They are just simple sequences of few signs with nothing in common with the language that you and I are using at this moment.

Let's go back to humans. Chomsky has used a very suggestive formulation to describe the capacity of a child to acquire his or her native language: acquiring a language is something that *happens* to a child, not something that a child *does* (see Chomsky 1988). This is identical to the development of a child's liver or lungs. This is why a chimpanzee cannot acquire a human language—not even with great teachers. To repeat one of Chomsky's witty sayings (quoted in Pinker 1994): a spider does not learn to build a web because a very skillful spider taught her how, but because she has a spider brain. The ability to build a web is therefore an instinct in the same way that Darwin called a human's capacity to learn to walk upright an instinct (Darwin 1862). Similarly, within some limits that we will see later, children do not learn their native language because someone taught them, but because they have human brains.[35]

As usual, we will limit ourselves to the examination of syntax. When children acquire linguistic signs—the vocabulary of their own language—they proceed by imitating the association between the sound (the signifier) and meaning (the signified). The association between the signified (the concept of apple as a fruit) and signifier (the corresponding sound) is totally arbitrary. A child cannot know in advance that the concept of "apple" is associated with the sound corresponding to the pronunciation of *apple*, *mela, pomme,* or *Apfel, besides obvious expectations of the type of sounds that occur in a given language (for example, no Italian child would expect a cluster of consonants [pf] as you find in the German Apfel).* Linguists speak about "Saussurian arbitrariness" in homage to Ferdinand de Saussure, the scholar who first investigated the linguistic sign as composed of a signified (the concept) and a signifier (the sound) and clearly stated the arbitrary nature of their relation.[36] Once this point is clear, we are left with just the sample of syntax. We will not see everything, but we will have theoretical and terminological tools sufficient to understand the neuroimaging experiments that are described in the second part of the book.[37]

35. It has often been stated that it would be more appropriate to speak of *acquiring* a language than of *learning* it. Personally, I don't find this distinction particularly enlightening. Therefore, I will use both terms according to what sounds better in each context. For instance, we don't say *learn the knowledge of English* but rather *acquire the knowledge of English.* Regardless of which term is used, the process of mastering a native language is not just one of acquiring an outside structure but is largely dependent on a biologically predetermined project. I hope that by the end of this chapter this issue will be clear.

36. A sophisticated theory of linguistic signs that in some aspects reminds us of the modern one was elaborated in ancient Greece by the Stoics, in particular, Chrysippus in the third century B.C., and had the concept of *lektón,* "what can be said," as its main tenet.

37. It makes sense to ask why humans have associated signifieds to auditory signifiers, since it is certainly possible for us to express ourselves in a rich way by means of sign language, that is, with visual signifiers. The fact that an auditory sign can be perceived from afar may be crucial. Nevertheless, it doesn't look totally implausible to consider the role that laughing and crying may have played. They are auditory facts and imply a semantic interpretation, if we exclude tickling and physical pain and triggers. But this says nothing about the other distinctive aspects of human language.

We start our examination of syntax by adopting the Galilean style of research that is based on idealization, as described in the methodological section when we discussed simplicity. From this moment on, in fact, we will deal only with the syntax of human languages, ignoring other components of grammar such as phonology and the differences among individuals speaking the same language. Is it correct to deal just with syntax and ignore semantics, morphology, phonology, and pragmatics? This question is nonsensical, as we already said. We cannot ask whether it is correct or not: we can only ask whether it is useful. This is a methodological choice that is based on data that show how syntax can be investigated autonomously with respect to the other components of language, though, as a matter of fact, it never appears in isolation. In other words, we are idealizing the area of investigation by reducing the other factors as much as possible.

A brief reminder: We know that syntax is autonomous with respect to the medium that conveys it (writing, gesture, or sound); syntax is also autonomous within each medium (remember the sentence *I saw the king with the telescope*). We also know that the laws that govern syntax do not seem to be totally reducible to other components of grammar. In sum, we start from the hypothesis that syntax is autonomous. There are at least two aspects of syntax that are important to keep in mind because of their methodological consequences. We saw that the system of rules that governs the syntax of human languages is, for the most part, inaccessible to the direct introspection of native speakers; in other words, it is largely unconscious. We also saw that syntax constitutes a very complex net. Investigating this net requires linguists to make the same efforts as scientists studying any other aspect of the natural world: theory cannot be expected to spontaneously emerge from the data. *It is not enough to hear speaking to develop a theory of language, just as it is not enough to look at the sun to develop a heliocentric theory.* In order to have a good linguistic theory—a theory that helps us understand language structures and how they are acquired—we need to proceed with trials, confutations, hypotheses, models, and experiments. In brief, we need to do what is usually done in all the empirical sciences.

We will talk about linearity discreteness, recursion (and hierarchy), dependence, and locality.[38] These terms—only partially understood at this point—will substantially help us to understand how we have managed to investigate the brain in order to increase our knowledge of how it works

38. This partition does not correspond to any official canon. It is just an approach I took for illustrating the architecture of syntax.

with respect to language and especially to syntax. As the section title indicates, we will limit ourselves to a sample of data, but it is a sample designed to provide a general overview of the foundations of syntax.[39] For the rest of our discussion, the word "syntax" will encompass everything related to the ways words are combined, including linearity, discreteness, recursion, hierarchy, dependence, and locality.

1.2.1 Linearity

Let's start from a clear and unequivocal fact. *The very first piece of data that the syntax of natural languages offers us as empirical fact is before your eyes at this moment as you read: the linguistic code, the "signal," is linear, or monodimensional*: the signal consists of words, strung one after the other. It is of little importance whether your eyes are scanning a white sheet where black signs absorb light and the white that surrounds them is reflected from the page and perceived by the retina (visual signals that the brain reinterprets "as sounds"), or whether someone is reading this to you. In any case, words follow one after the other in a linear sequence. Called linearization by Saussure, this structural aspect "is evident, but looks as if we have always forgotten to state it, no doubt because it was considered too simple. Nevertheless, it is fundamental and its consequences are incalculable" (Saussure 1922/1974). Under the disarming simplicity of a row of words, we will see how a bidimensional (and possibly multidimensional) system is hidden that actually controls all the rules of the syntax of human languages.

In some sense it is like looking at a tapestry: seen from the front, it looks like many little points of different colors, one next to the other, forming a vivid image. But if we look at the back, we discover a complex structure of woven threads that makes almost no visual sense. Individual threads emerge on the surface and then dive, disappearing on this side while creating the image of the tapestry on the other. *In a way, syntax resembles a tapestry: if we look at it superficially, it appears as a simple row of words, arranged next to each other in a sound and coherent way. But if we manage to look at it "from behind," we discover the hidden and intricate structure that connects them at a distance.*

This aspect of syntax will become clear with the next examples. We will also return to it in chapter 3, which is the most speculative, because I intend to mention the frontiers of modern research in which the issue of the linearization of the syntactic symbol has taken on a central role. For now,

39. Those who are interested in going deeper into these issues can refer to the texts mentioned in the forward to this book and referenced throughout.

we will not ask why the signal is linear, nor what the consequences of this structural property are. We will just observe that the phenomenon is strictly related to our biological structure and to the natural world which we are part of. Let's just register this fundamental property and move on.

1.2.2 Discreteness

The second fundamental property of the syntax of human languages is discreteness. What is hidden behind this term, whose conventional meaning seems to be unrelated to syntax? "Discrete," as a scientific term, is the opposite of "continuous." Sometimes the term "digital" is used instead of "discrete."[40] The infinite set of "natural" numbers (natural numbers being the set of positive integers, that is, 1, 2, 3, etc. or non-negative integers, that is, the set of 0 plus positive integers) is discrete. For instance, between a natural number n and its successor, $n + 1$, there is no other natural number. On the other hand, the set of "real" numbers is a continuum, because, roughly speaking, there are no "gaps" in their distribution: between any two real numbers (e.g., 1.4141356 ... and $\sqrt{2}$) you can always find an infinity of real numbers. More technically, any bounded infinite sequence of real numbers always has a limit value (for example, the sequence 1.4, 1.41, 1.414, 1.4142, ... has limit value $\sqrt{2}$). A piano keyboard, on the other hand, is discrete, because between two neighboring keys there is no "key and a half," whereas the sounds a violin string makes when it is lightly touched by a bow are continuous because an infinite number of sounds can be produced between two possible positions of the finger on the string by simply moving the finger that pushed the string. So what does it mean to say that syntax is discrete? In practice, it means that *between a sentence with n words and a sentence with* $n + 1$ *words there are no "intermediate" sentences. The phenomenon is very simple, but its effect is not simple at all. It is as if linguistic information travels in separate packages, from the syntactic point of view.*[41]

40. It is interesting to notice that the etymology of the word sheds light on the issue. *Discrete* derives from the Latin word *discernere,* to separate. *Digital* derives from the Latin *digitum,* finger, for prototpically one can use fingers for discrete counting.

41. Discreteness is not an exclusive property of syntax. Compare the vowel sounds in *bay* and *bee*. Start pronouncing the vowel in *bay* and gradually move to the vowel sound in *bee*. There are many vowel sounds in between, in fact infinite, since there is a continuous movement of the vocal apparatus to shift from one sound to the other. Nevertheless, in English, there are just no words with vowels that are between the vowels in *bay* and *bee*. This indicates that we split the vowel space into discrete packages although the sound is continuous.

This makes syntax profoundly different from the physical system that implements it and clarifies what it means to say that syntax is discrete. If you look at the acoustic waveform of a sentence you immediately realize that sound information is continuous. When you pronounce a sentence, you do not pause between each word. The idea that you pause between words—or more generally that there is a physical clue signaling the words boundaries—is just an illusion stemming from two distinct factors: our habit of dealing with written language—where, in languages using an alphabet, blank spaces are inserted between words—and our brain's capacity to identify separate words within a continuous string of sound based on a dictionary. It is not easy to grasp this without seeing an acoustic waveform, but you can get the idea by thinking of a sentence without spaces between words in a language that is not your own.

If you read *nomathematicianunderstandstheproof*, you can recognize each word. You know that *derstand* is not a word, even though it could be. On the other hand, if you read the translation of this very same string in a language you do not know, let's say Italian, *nessunmatematicocapisceladimostrazione,* the difficulties would be immediately clear. In fact, every time we speak, we deal with a continuous flow of signals, but we effortlessly fragment the continuous and reduce it to a combination of discrete units.

There are well-established techniques to help you see how difficult it is to determine the boundary between one word and another or, more basically, just the effort to learn a foreign language, unless that language is of course very similar to your mother tongue. Constructing graphs from sound recordings is one example. In the picture below, we see the graph that corresponds to the sentence *No mathematician understands the proof* as usually uttered by a native speaker of English. The variation of air pressure is on the vertical axis and pronunciation time is on the horizontal axis: the higher the pressure the taller the peak.

It is clear from figure 1.2 that identifying the boundaries between the five words in the sentence is basically impossible. There are moments when the sound decreases, but these drops in pressure cannot be immediately associated with word boundaries. There are no images of four silences, physical signals that allow us to deduce that we pronounced a five-word sentence.

If on the other hand the very same sentence is pronounced with a short pause between each word (*no*—pause—*mathematician*—pause—*understands*—pause—*the*—pause—*proof*), the resulting graph is very

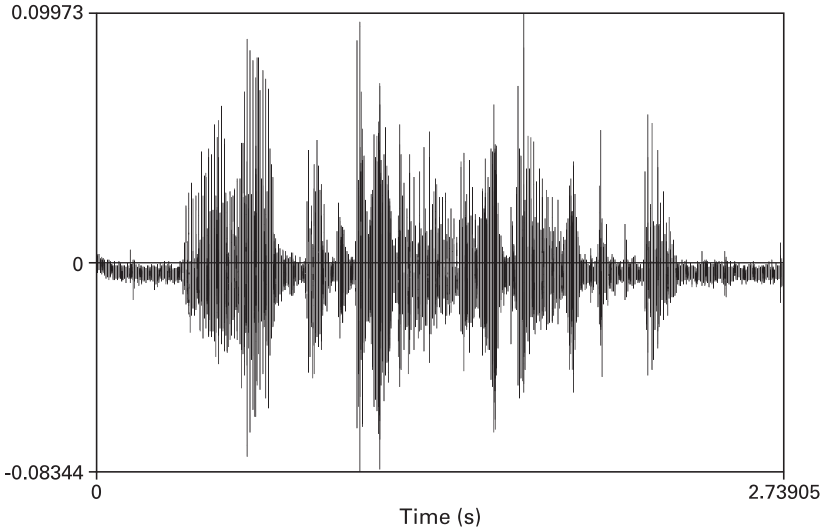


Figure 1.2
Acoustic waveform of the sentence *No mathematician understands the proof* as usually uttered by a native speaker.

different: the clear flat areas in the graph now correspond to pauses and to the spaces between the words in the written sentence (see figure 1.3).

As you can see, five chunks of air pressure variation, separated by pauses (silences), are recognizable and can be linked to the five words of the sentence. But this only confirms that the sound flow in normal speech is continuous, because in normal situations people do not pronounce sentences word by word, with pauses between words. Also, there are pressure drops within chunks, that is, within each word. It is somewhat like when we look at a rainbow. Although we know that what we see is the result of progressive imperceptible changes in the length of the electromagnetic waves, still we perceive it as a finite number of emerging blocks of uniform colors, pitches of light with shades at their borders, to which we can assign names. In summary, it is not at all easy to directly translate the physical message as illustrated by such a waveform image into the message perceived by our brains. This fact raises a fundamental problem, which is still partially unresolved: how can children acquiring their native language, who certainly don't know the vocabulary yet, split a continuous stream of sound into words? Significant progress has been made in this research area, largely based on the analysis of sound frequencies,

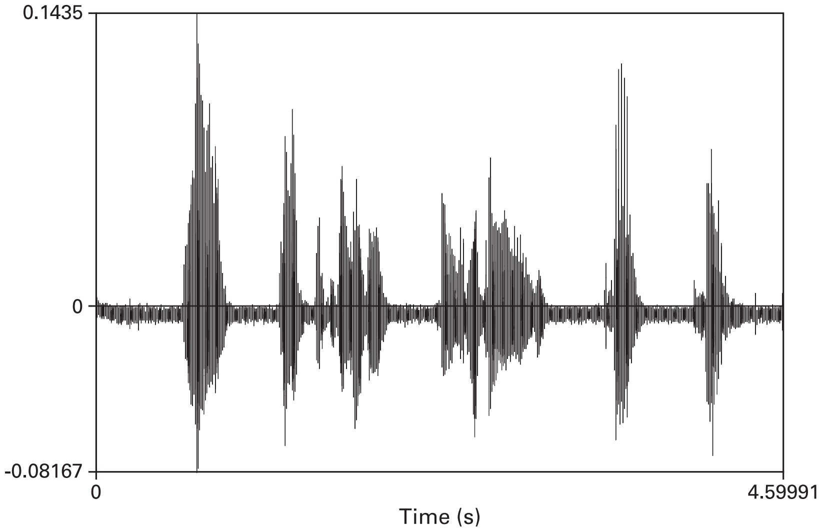


Figure 1.3
Acoustic waveform of the sentence *No mathematician understands the proof* when uttered with a short pause between each word.

of syllable structure, and of "prosodic" phenomena—phenomena that involve phenomena such as stress, tone, and intonation in a linguistic expression (see the pioneering work of Jacques Mehler as described in Peña et al. 2002, Dupoux and Mehler 1990, Nespor, Guasti, and Christophe 1996; for a different view, see Saffran, Aslin, and Newport 1996, among others; for an interesting correlation of word recognition with punctuation, see Steinhauber 2003).

In the history of linguistics, there have been many attempts to define the notion *word* formally, and even independently from acoustic cues. It is interesting that the effort to define the concept of *word* contrasts sharply with the fact that we all have a clear pretheoretical intuition of this notion. Even children can use the word "word." If I say "Don't say that word" to a child, I don't need to give him or her a syntax lesson in order to convey what I mean. In fact, although its definition is complex, the notion of *word* is undoubtedly part of everyday language. One of the most influential approaches to defining *word* has been undertaken by Jespersen (1922). According to him, two tests can suitably be exploited to identify a word, from a syntactic point of view. Simply put, a certain sequence of sounds is a word if it cannot be interrupted and if its subparts cannot be rearranged. For example, in the sequence in French *il aime*, "he loves," pronounced as a phonological unit, /ilɛm/, we can recognize two words, *il* and *aime*, because they can be found in different positions—such as in *Aime-t-il Catherine?* ("Does he love Catherine?"—literally, Loves-he Catherine?)—and they can be interrupted, as in *Il ne aime pas les frites* ("He doesn't like French fries," literally, He not loves not the French fries). These two tests, often referred to as "positional mobility" and "uninterruptability" (see Lyons 1968), may be sufficient to solve the problem of identification of words, but they certainly cannot be considered as a suitable model for language acquisition: children do not try to move or interrupt sequences of sounds in order to get words; rather, they rely on different inputs, as suggested in the cited works, such as, for example, prosodical and phonological regularities.

All in all, discreteness in syntax and the contrast with the acoustic medium constitute a very clear example of the tension between the physical world and a cognitive system. If we were to represent it synthetically, we could just observe that *the ear hears sounds; the brain, sentences.*[42]

1.2.3 Recursion

Another fundamental property of syntax is recursion. The term is borrowed from a branch of mathematics and is used in linguistics with a slightly simplified meaning. In linguistics, recursion means, roughly, the repetition of a process on the same structure endlessly. It is such a central element that it is often considered to be the fundamental characteristic of the syntax of human languages (see Hauser, Chomsky, and Fitch 1992). But what does it actually mean to say that a process can be repeated on the same structure endlessly? Let's start with a simple example. Most readers will as children have played with recursion in their schoolyard chants: *This is the song that doesn't end. Yes, it goes on and on, my friend. Some people started it not knowing what it was, and they'll continue singing it forever just because this is the song that doesn't end* . . . The fun of this little game is that you can make a never-ending song by continually attaching the beginning of the song to its end without stopping.

On a "simple" level, recursion refers to the fact that a sentence can always be made longer. If I say *Andy says that Mary knows that Peter*

42. This is also true at the simpler levels of phonemes, morphemes, and words. There is no physical evidence for these units, as we saw. They are the result from our brains' processing of perceptual inputs.

believes that Mark is intelligent, I immediately realize that the procedure can be extended endlessly by adding other clauses such as *Andy says that Mary knows that Peter believes that Mark is intelligent and thinks that Rose and Angel claim that life is beautiful.* You can add new words to the beginning as well as the end of a sentence. If I say *Peter knows that Paul will defend him*, I can also easily imagine saying *Mary is sure that Peter knows that Paul will defend him.*

At first glance, recursion seems to coincide solely with the ability to produce endless sentences by adding a string of words to the beginning (the right) or to the end (the left)—but it is not that simple. Recursion is similar to the fact that we cannot imagine the largest possible number: we can always add another number, or digit—even if just one—and have a larger number. With this perspective, recursion brings together two human capacities par excellence: language and mathematics. Much has been said about this likeness. In both cases, a human being has the notion of *successor* and has the potential to produce an infinite number of structures of infinite length (if we ignore limits on human memory and standard conversation lengths).

In linguistics, recursion also means something more complex than just increasing the length of a string of words by adding more words to its right or left side. Delving deeper into this aspect of recursion, we encounter another characteristic property of syntax, one that is often overlooked, perhaps because it comes naturally. Before considering it directly, let's look at an example. Imagine saying these two sentences: *A man is eating the pizza* and *A man is reading a book*. They are two different sentences, each with a subject, verb, and complement (the direct object), as we learned in school. It is not hard to imagine combining the two sentences: *A man who is eating the pizza is reading a book.* I built this new longer sentence, which is more complex and contains a sentence within it, namely the substring *who is eating the pizza.* This combinatory process is called *recursion: a structure of a certain kind (a sentence, for example) that contains structures of the same kind (another sentence).*

Are there general rules for combining structures so that a structure of type X contains a structure of the same type X? The answer is yes, and the discovery of these rules has in fact been one of the central themes in the study of syntax, since at least the 1950s. We will look at the fundamental aspects of these recursive rules, though in a simplified way. In brief, recursion in linguistics identifies the human capacity to produce potentially infinite structures: in more technical but equivalent terms, there is no upper limit to the quantity of information a person can convey by combining words. We are now ready to address the study of the specific implementation of recursion in linguistics, which we call *syntactic hierarchy.*

Not all hierarchical structures are recursive, whereas all recursive structures are hierarchical. Syllable structures, for example, provide a good example to show that recursion and hierarchy are different notions. A syllable—from the ancient Greek "syn" (with) and "lambano" (take)—is a unit of organization of a sequence of speech sounds and may vary across languages. The most typical syllable you can find among the languages of the world is a sequence of one consonant (C) followed by one vowel (V) optionally followed by another consonant: CVC, as in the monosyllabic word *cat* or as in the bisyllabic *napkin*. These three units are grouped into hierarchical subunits for different empirical reasons: the first consonant constitutes the onset; the subunit made by the vowel and the other consonant, on the other hand, constitutes the rhyme; the rhyme is further subdivided into the nucleus (which is the only obligatory element of a syllable) represented here by the vowel and the coda, the last consonant. Clearly, it would make no sense to assume that this structure is also recursive, because there cannot be another syllable inside a syllable. For the sake of brevity we will use *hierarchy* to mean "syntactic hierarchical structure with recursive properties."

Let's introduce a linguistic object that does not have the same tradition as the other syntactic notions that we have discussed so far. We have already noticed that the notion of *word* is part of our general knowledge, before any attempt to give a formal definition. This is also true for our notion of *sentence*: we "know" very well what we mean when we say "Don't say that sentence," "this sentence is false," but if we try to formally define "sentence," we run up against an overwhelming number of problems.

What is a sentence, then? Is it enough to say that it is a string of words with meaning? Of course not. *The beautiful house* and *The house is beautiful* are both strings of words with meaning, but we know that only the latter is a sentence. Also, we have seen that meaning is not a good way to identify sentences. If I say, *A circle is square* and *Circle is square a,* I understand that the former looks like a sentence but the latter does not, even though the former is nonsensical—it is a contradiction. Also in this case we don't need to deal with the problem of defining *sentence* explicitly. Instead, we will adopt the "naïve" view, as we did with the notion of *word*: We will take for granted that we have a notion of *sentence*—the intuitive notion we have—without defining it rigorously. Graffi (2001) shows that about three hundred definitions of *sentence* have been used since the

1930s—evidence[43] that access to linguistic structures by introspection cannot be taken for granted and is less than natural. In linguistics, as in any experimental science, definitions of concepts do not spontaneously arise from the facts in a direct and unequivocal way. Among the different options, you need to think and often you have to choose the one that gives the best results according to the goal you want to achieve.

We have established that *word* and *sentence* are syntactic notions,[44] and that sentences are made of words. And now, a natural question arises: When words are put together, are words organized in units that are smaller than a sentence but bigger than just one word? If they exist, do these combinations have interesting structures? The answer is yes, and these combinations will let us more precisely understand what we mean by recursion in linguistics. The next step is to recognize those structures, which we call phrases. We will see that words combine according to a particular geometry that gives rise to organization based on *hierarchy* rather than on simple linear combinations. What does *hierarchy* mean in the context of syntax? The notion of phrase will answer this question.

Let's look at a simple example: *A mathematician saw pictures*. This is a sentence, and it is made of words. The sentence can be lengthened. Let's focus on the word *pictures*. You can say *A mathematician saw pictures of Mary*, or *A mathematician saw many pictures* or you can combine these two extensions of *pictures* by saying *A mathematician saw many pictures of Mary*. You get the impression that you have "expanded" the word *pictures* by having additional elements like *many* and *of Mary* "gravitate" around it. You intuitively know that *many* and *of Mary* refer more directly to *pictures* than to *mathematician*. The string *many pictures of Mary* is called a phrase.

43. One of the most ancient definitions of sentence—one that has survived until modern times—is given by Aristotle in *De interpretatione* (1938). It aims at capturing declarative sentences, "only a subset of what we would instinctively consider a sentence. What is a declarative sentence?... Not every sentence is a statement-making sentence, but only those in which there is truth or falsity. There is not truth or falsity in all sentences: a prayer is a sentence but is neither true nor false" (chapter 4, 17a, 3–6). So *the house is beautiful* is a sentence but *the beautiful house* is not. When we refer to a sentence as a syntactic structure, we often use the term *clause;* in this work both sentence and *clauses* will be used, depending on the context.

44. The notion of *word* is also the privileged object of study of morphology and phonology. Here, we are just considering *word* to be the primitive element of syntax.

Is there a nonintuitive way to identify phrases? If you look at just a known language, maybe your own native language, this question may look irrelevant: the intuition that *many* and *of Mary* do not combine directly with *mathematician* but with *pictures* would be sufficient. Nevertheless, for historical reasons, when linguists started dealing with syntax as an experimental science, they had to come up with techniques for identifying phrases in languages whose structures are different from the structures of known languages. An essential boost to this field of research came at the beginning of the 1900s from American linguists who began to investigate languages other than Indo-European languages (examples of Indo-European languages include Greek, Latin, Sanskrit, and Romance and Germanic languages; see Comrie 1981). In particular, I am referring to the period when American linguists started studying the native languages of North America, whose structures are quite different from the Indo-European languages linguists were used to. The familiar grammatical notions that worked well for classical and Indo-European languages did not work nearly as well for native North American languages.[45] Researchers invented what they called "constituency tests" to identify phrases in these differently structured languages. We will look at two constituency tests and use English in order to keep things simple. Let's go back to our sentence *A mathematician saw many pictures of Mary.*

One constituency tests is the so-called "cleft formation test," the point of which is to figure out whether a particular string of words is a phrase. The procedure is simple and it will be clear why this is called "cleft." Pick a sentence, choose a string of words X within the sentence, then build a new sentence with the following structure: *It is X that* followed by the original sentence but without the string X. Suppose we want to determine whether, in our original sentence, *many pictures of Mary* is a phrase. *Many pictures of Mary* is our X. Using the cleft formation test, we build the following sentence: *It is many pictures of Mary that a mathematician saw.* This is a possible English sentence, and therefore, we say that the string of words *many pictures of Mary* is a phrase (in fact, it is a noun phrase). What happens if we consider the string *pictures of* as X in the same sentence? The cleft formation test yields **It is pictures of that a*

45. The famous case of using these languages for military purposes is often mentioned. During the Second World War, the Allied Forces sometimes used languages of the Navajo (in the Na-Dené language group) to communicate with each other in code; these languages could not easily be deciphered by the enemy.

mathematician saw many Mary. This is not a possible—certainly not an acceptable—sentence. The string of words *pictures of* behaves in a different way than *many pictures of Mary* and therefore is not a phrase. This is a way to show that words are grouped as smaller units within sentences in a way that is *not* based on intuition. This test is extremely useful when we are dealing with unknown languages.

In theory, if we had a native speaker and lots of time, we could perform a systematic cut-and-paste operation as in the cleft formation test on a sufficiently large sample of a language to get a good list of its phrases and then ask the native speaker whether the sentences we have built using the cleft formation protocol are acceptable sentences. Furthermore, we may also learn that some phrases can be replaced with others (we say that they "commute") and build equivalence classes. For instance, if we look at the sentences *A mathematician saw* [*many pictures of Mary*] and *Peter bought* [*three portraits of Paul*] we can switch [*many pictures of Mary*] and [*three portraits of Paul*] and produce the following two sentences: *A mathematician saw* [*three portraits of Paul*] and *Peter bought* [*many pictures of Mary*]. If two phrases can replace each other, then we conclude that they are of the same kind.[46] Moreover, if we apply this "cut, paste, and switch" procedure to English, we would be able to build phrases other than noun phrases. In English, we also have adjective phrases, preposition phrases, and verb phrases, such as those in brackets in the following sentences: *Peter is* [*very proud of Mary*]; *The butcher lives* [*right around the corner*]; *A guy wants to* [*fully understand the theorem*]. None of these phrases in brackets can be switched with a noun phrase like [*many pictures of Mary*], which therefore is not part of any of these equivalence classes.

Sometimes constituency tests can also be useful for revealing interesting facts about your own language. Let's look at the following two sentences:

46. The linguist has to pay particular attention to decide whether two phrases can or cannot be switched in light of agreement restrictions. For instance, in the sentences *[A friend of Mary's] is running* and *[The friends of Mary's] are running,* we could mistakenly conclude that [*a friend of Mary's*] and [*the friends of Mary's*] are not part of the same equivalence class because they cannot be switched (**A friend of Mary's are running* and **The friends of Mary's is running* are unacceptable). This is why the sample must be large. In fact, there is virtually an infinite number of contexts in which this switching is possible, for example, *I know [a friend of Mary's]* and *I met [the friends of Mary's],* which would lead us to conclude that the two phrases are part of the same class.

John put on the sweater and *John spit on the sweater*. Since the two sentences only differ in the verb, you may think that their syntactic structures are identical. But if you apply the cleft formation test, you see that this is not the case. **It is on the sweater that John put* is unacceptable, whereas *It is on the sweater that John spit* is fine. This reveals a structural difference that was unexpected, since the two sentences only differ by a verb. Thanks to the cleft construction test, we realize that the relation between the verb *put* and *on* is different than that between the verb *spit* and *on*. It looks as if *on* forms a unit with the verb *put*, whereas there is more freedom between *on* and *spit*. The cleft formation test, therefore, yields clear and unexpected results when applied systematically.

Another test for identifying phrases is called the substitution test. In English, a group of words can be replaced with pronouns. How can we use this property to find phrases? Let's use the same example one more time. *A mathematician saw many pictures of Mary* can be turned into *A mathematician saw them* which shows that the string *many pictures of Mary* behaves as a unit, a phrase. If we replace the string *saw many* with *them* similar to what we did earlier, the result **A mathematician them pictures of Mary* is unacceptable. Just as we found using the cleft test, the substitution test shows that *saw many* is not a phrase. The two tests converge in showing that the words in the string *many pictures of Mary* are bound in a "preferential" way that creates a "phrase": they are not sentences and they are not simple strings of words. If they were just strings of words, we would not be able to account for why the cleft formation and substitution tests do not always work in the same way with any string of words.

We have reached an interesting point: we now know that the string *many pictures of Mary* is a phrase. Does this phrase have its own internal structure? In other words, is the string of words *many pictures of Mary* a flat string or does it have a hierarchical internal organization? There are many ways to answer yes to this question and to the question of the internal structure of phrases in general and across languages. The one we will explore here relies on the substitution test and on Italian, which has a pronominal unstressed, or clitic, form *ne* that English lacks. *Ne*—whose meaning can be roughly translated as "of-them" or "of-it," depending on the particular case—can replace parts of a phrase according to precise structural rules (see Belletti and Rizzi 1981). The study of the syntax of this element—whose syntax had been virtually neglected before the birth of generative grammar—provided crucial data for the development of the theory of grammar in the 1970s. Let's see what happens if we apply the substitution test with *ne* to the Italian sentence *Un matematico vide [molte foto di Maria]* ("A mathematician saw [many pictures of Mary]"). Focusing on the noun phrase [*molte foto di Maria*] ("many pictures of Mary"), a first result is that *ne* can replace the sequence *foto di Maria* ("pictures of Mary") contained in the full phrase yielding *Un matematico ne vide* [*molte*] ("A mathematician saw many of them"). *Ne* cannot, however, replace the substring *molte foto* because it leads to the impossible string **Un matematico ne vide di Maria* ("A mathematician saw of them of Mary"). What does this test teach us? That Italian has a pronominal form *ne* that can replace the substring *foto di Maria* leaving *molte,* but cannot replace the substring *molte foto* leaving *di Maria* behind. We conclude that the noun phrase *molte foto di Maria* has an asymmetric internal structure in which the substring *foto di Maria* behaves as a unit (as the substitution test shows), while the substring *molte foto* does not, paralleling the conclusion we draw when identifying full phrases with the substitution tests.[47] Similar tests have been replicated in other languages, including English, showing that this asymmetrical structure of phrases applies generally.

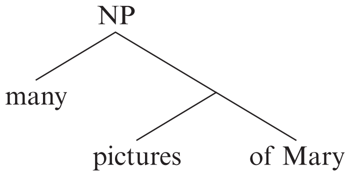
Although *pictures* may or may not come with other elements that complete the phrase, like *many* or *of Mary,* it is awkward to omit *pictures* out of the blue and get a sentence that is fully acceptable. If there is no shared background information, and someone suddenly asks you a question like *Why are you laughing?,* you cannot answer **Well, the mathematician saw* [*many of Mary*], though you could answer *Well, the mathematician saw [many pictures]*, or *Well, the mathematician saw [pictures of Mary],* or even just *Well, the mathematician saw [pictures]*. We technically call the element around which the phrase is built—*pictures,* in our example—the *head* of the phrase

To summarize this section: First, *words are grouped together to form units that we call "phrases," which in turn are grouped to form sentences.* Second, *phrases have an internal asymmetric structure.* Asymmetry results from the fact that the head and what follows can be replaced with a pronominal element, whereas the head and what comes before cannot. Looking at the order of the linear sequence, we call what follows the head the

47. The most perceptive reader may have wondered why I have not applied the cleft formation test to prove the asymmetry. This test cannot be used for independent reasons. In general, a constituency test is only a *sufficient* but not *necessary* condition for the identification of a phrase.

complement and what precedes the head the *specifier*. The term *specifier* has been chosen for noun phrases because it aims to capture the intuition that an element such as *many,* which precedes the head in *many pictures of Mary*, specifies the quantity that is expressed by the phrasal subunit it is applied to, that is, *pictures of Mary*. These elements—the specifier and the complement—are generally optional. More precisely, whether their presence is obligatory depends on the head of the phrase. In the case of the phrase we are considering, a noun phrase, or NP (called this because the indispensable element at the head of this phrase is a noun), it is possible to give a ‘‘geometric'' representation of the asymmetry by means of a so-called *tree diagram*. The tree diagram is a *bidimensional representation* of the connections that are found in the continuous line of a string of words, the hierarchical representation of the elements as presented in the two-dimensional space of a sheet of paper or a blackboard.[48]

In the case of *many pictures of Mary*, we can build a bidimensional representation, as in the graph below. The hierarchical relationship between the specifier, the head, and the complement are made graphically explicit, in the sense that the head, *pictures,* and the complement *of Mary*—which in the substitution form constitute a subunit—are represented at the same level. They are said to be *adjacent*. The head and the complement are directly connected by two branches of the syntactic tree and the specifier *many* is adjacent to the ‘‘node,'' the two branches grouping *pictures* with *of Mary* meet:



The specifier (*many*), then, is at a higher hierarchical level than the subunits that consist of the phrase's head and complement. We say that the specifier is *prominent* to the head and the complement. We will see that

48. It is interesting to ask whether it is possible or at least empirically useful to move from representations in a bidimensional space to tri- or multidimensional representation in the syntactic space. To this day there is still no cogent need for a multidimensional model, nor is there evidence that a multidimensional representation would be untenable. Simply, it seems that we capture all the useful generalization by means of a two-dimensional space. Using a play on words, it could be interesting to find ‘‘non-Euclidian'' grammars, that is grammars whose representations require a metrics different from the standard one.

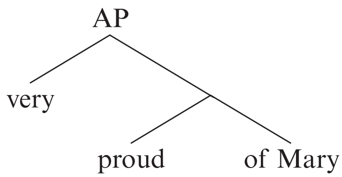
the notion of *prominence* is central to many areas of syntax—we will not provide a formally rigorous definition here. We can, however, adopt the following simplified technical definition: an element X is *prominent* with respect to element Y if the element Y is contained within a node Z that is adjacent to X.[49] To repeat: In this tree, the head is *pictures,* the complement is *of Mary,* and the specifier is *many,* which is prominent to the head and the complement. The head determines the label of the entire phrase, so in this case we have a noun phrase, or NP. The head is the only element in the phrase that is always obligatory.

A fundamental discovery stemming from the structuralist theories and successively refined in generative grammar is that there are not only noun phrases, like the one we just discussed, but also other kinds of phrases and, crucially, that *all* these phrases have the very same asymmetric structure that we observed in the noun phrase *many pictures of Mary*. The first kinds of phrases to be identified—those within square brackets—were adjective phrases (*Sebastian is [very proud of the lullaby]*), noun phrases (*Isaac knows [many stories about apples]*), prepositional phrases (*The butcher lives [just in this house]*), and verb phrases (*A student wants to [completely understand the theorem]*). In all these cases, we find the same asymmetric structure that we found in [*Many pictures of Mary*] and represent it in the same way, by means of a two-dimensional tree diagram. The terms *specifier, head,* and *complement* apply to these phrases as well. The lexical characteristics of the head of a phrase determine the type of phrase, and the needed specifiers and complements, vary according to the lexical characteristics of the head, just as in noun phrases, where a certain noun may or may not require or allow a complement (*his hat *of John* versus *his picture (of John)*). Thus, the adjective phrase (AP) has an adjective as its head; the noun phrase (NP), a noun; the prepositional phrase (PP), a preposition; and the verb phrase (VP); a verb. In the four branch representations shown below, all heads are accompanied by a specifier and a complement:

49. The notion of prominence is one of the notions of "command"—which also includes c-command and m-command—which play a crucial role in syntax. Note that if one lets *Z* be identical to *Y*, prominence becomes a reflexive relation. When it is not reflexive it is sometimes called asymmetric prominence (hence asymmetric c-command; see Kayne 1994 for a detailed discussion).

The first formal definition of this notion goes back at least to the seminal work by Reinhart (1976); Frank and Vijay-Shankar (2001) have developed a grammar where the notion of command is a primitive in the system (see also Frank 2002).

(1)



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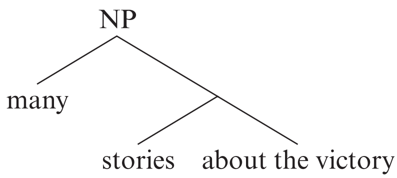

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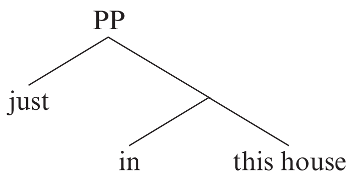

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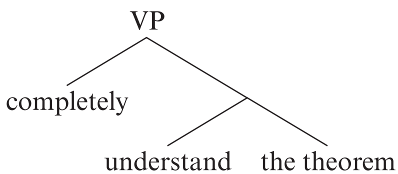
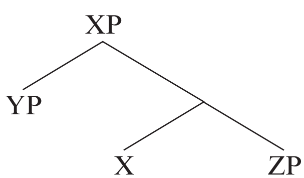


If we use X to refer to any phrase head (N, V, P, or A), we can generalize and say that for all phrases the asymmetric structure that follows holds:



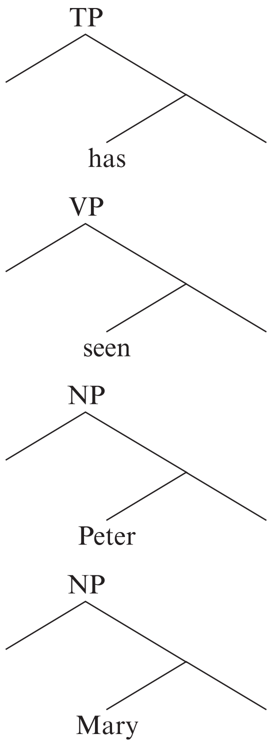
In the structure above, the specifier YP and the complement ZP are phrases and can have the same asymmetric structure as the phrase that contains them.[50] For instance, in the verb phrase *completely understand*

50. This representation by means of syntactic trees is often called X-bar theory, because an increasingly larger numbers of bars was used to refer to the different levels within the same phrase. This is also a way to underline the structural homogeneity of phrases using the variable X. For the sake of simplicity, we will not use the bar system and will just refer to generic heads X and phrases XP. For the original description of X-bar theory see Chomsky (1970), Jackendoff (1977), Stowell (1981); see Graffi (2001) for a critical overview.

the theorem, the complement of the head V, *understand,* is the noun phrase *the theorem* and the adverb *completely* is the specifier. The specifier can also be a full phrase. In our example, *completely understand the theorem,* it is not easy to see why this is so. It will be much clearer when we see the structure of full sentences. Nevertheless, one can grasp the idea of having full-phrase specifiers independent of sentence structure by considering the following example. The two strings *many pictures of John* and *two girls*, are both noun phrases, like those in the sentences *I found [two girls]* or *I found [many pictures of John]*. The specifiers of these noun phrases are *two* and *many*, respectively. Combining these pieces yields the sentence *I found two girls' pictures of John*, where *two girls' pictures of John* is a noun phrase, as shown by the grammaticality of a cleft sentence such as *It's two girls' pictures of John that I found* (as opposed to **It's two girls' that I found pictures of John*). The string *two girls* and the specifier *many* cannot occur together in a noun phrase; the result would be **two girls' many pictures of John*. In other words, we say that they are in complementary distribution. Therefore, putting aside the role of the morphological marker -*s*, it is reasonable to conclude that *two girls* is now the specifier of *pictures of John.*

On the basis of similar tests it has been concluded that *phrases follow a homogeneous, recursive and asymmetric structure where the entire phrase shares the structure of its specifier and complement.* The picture of phrase theory that I'm giving here is very simplified, but it is all we need for understanding how the neuroimaging experiments have been built. Notice that each node has a maximum of two branches departing from it. This principle, called the *binary branching principle*, was proposed by Richard Kayne (1994) and it is fundamentally important for understanding syntax, as are all principles that try to make a theory more restrictive by reducing the possibility of variability to the minimum. We will see in chapter 3 how this principle can be deduced from an axiom that is actually able to derive many properties of phrase structure.

So far we have observed the structure of the bricks from which the sentence is constructed. A very important discovery in syntactic theory that goes back at least to Chomsky (1986b) is that *the whole sentence has a phrase structure of the same kind as the phrases it is made of.* As usual, let's start with a simple example: four independent phrases with just their heads—that is, phrases whose specifier and complement positions are not filled with lexical material. Let's imagine a fragment of English that contains the following four heads and their associated phrase structures: *seen, Peter, Mary,* and *has.* Each head is assigned the following syntactic tree, indicating the hierarchical structure of each phrase.

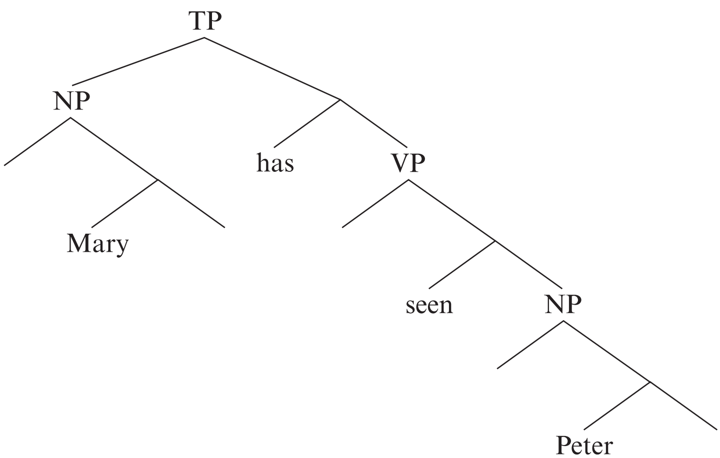


Of the four phrases above, the only new addition to the set of phrases we have considered so far is the phrase with *has* as the head. This phrase is called a *tense phrase* (TP) because the helping verb here, *has,* is specifying the tense of the main verb; so, for example, if we change *has* into *had* we just change the tense, leaving all other factors equal.[51]

51. *Has* also specifies the mood, the aspect, and subject agreement, but we will disregard these other features expressed by the helping verbs and more generally by verbal inflection—the ending of a verbal form such as *walk-s, walk-ed* and *walk-ing*).

Note also that if there is no helping verb, the structure of the sentence is more complex. This is irrelevant for our purposes, so I will describe it in brief. The idea is that verbal inflection (*-ed* in *walked*) is represented as an independent head exactly in the same way as helping verbs. Since this head takes the VP as its complement, the head of the VP is in the opposite order with respect to how it is pronounced (*-ed* … *walk-*). The hypothesis is that the head of the VP moves to the left of the head of the TP with an operation that is called syntactic movement (or in this case, more precisely, affix hopping). This operation is one of the kinds of dependence that we will discuss in the next sections and has played a crucial role in the development of generative grammar since it was first studied in

How can we put these pieces together to get a connected structure? If these were wooden construction toys, there would be many possible structures. I will let the curious readers go through the several correct and incorrect combinations on their own, and I will present just one correct combination below, corresponding to the sentence *Mary has seen Peter* (the only other well-formed combination would be *Peter has seen Mary*):[52]



Chomsky 1957. There are other independent reasons to hypothesize that a lower head can move to the left of the head that selects the phrase that contains it. This phenomenon is found in sentences such as *Gianni ne racconta molte su Roma,* "Gianni tells many of them about Rome" (literally, Gianni of-them tells many about Rome), where the pronoun *ne,* of-them, is the head of the NP complement of *racconta,* tells. The corresponding sentence without movement would be *Gianni racconta molte storie su Roma*, "Gianni tells many stories about Rome." Syntactically, the head V is to the head T as the head N is to the head V; V is the head of the complement of T and is to the right of T, just as N is the head of the complement of V and is to the right of V. After the "head-to-head" movement transformation, V immediately precedes T, just as N immediately precedes V. See the textbooks cited in the Preface to this book for a detailed analysis. See Carnie 2006. See Moro (1988), Pollock (1989), and Belletti (1990) for an analysis of the structure of tense as an independent head (and for other features related to sentence structure, the so-called Split-Infl hypothesis); see Chomsky (1995, chapter 1), for an updated summary of the empirical conditions leading to affix hopping and Graffi (2001) for a detailed critical account.

52. All the other combinations would produce an incorrect word order. The only exception would be the combination in which the head *Maria* would take TP as its complement. In this case, the structure would be assigned the wrong label, NP, since the string *Mary has seen Peter* cannot replace other NPs (for example, *I have met Mary* and **I have met Mary has seen Peter*).

The syntactic tree above gives stronger, more evident support to what we said about recursion: the schema XP (where X is any head) repeats itself within the very same XP schema. Things could be made even more complex if we iterated the same schema many times.

This structure also allows us to go back to the familiar notion of diagramming sentences, showing us the subject, *Mary,* the predicate, *has seen Peter,* and the direct object, *Peter*. These notions can now be defined in so-called *configurational* terms: the subject can be defined as the most prominent noun phrase in the sentence structure and the direct object as the noun phrase that is adjacent to the verb. This is one of the most important innovations that generative grammar introduced to sentence analysis (for a detailed discussion, see Graffi 2001).[53]

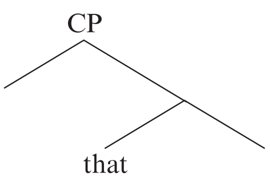
In order to fully understand the structure of sentences, there is still one more head to be uncovered. It will be useful for understanding the content of the next section which is about the differences among languages. Consider the sentence *Mary has said that John has seen those pictures.* Which phrase does the word *that* belong to? The answer is not straightforward. Let's reason by analogy with noun phrases and exploit the techniques that we have introduced for investigating phrase structure (this procedure cannot be generalized to all cases, but it works with the structure of the sentence and the structure of the noun phrase).

If the sentence had been *The weaver has found a body* and we had wondered what the word *a* belonged to, what could we have done? It we applied the clef test and the substitution test and we would get the following results: *It is a body that the weaver has found* and *The weaver has found it,* but not **It is body that the weaver has found a* nor **The weaver has found a it*. We would have easily concluded that the word *a* belonged to the noun phrase because it cannot be left behind in either the cleft or the substitution test.

Now, let's go back to our initial sentence *Mary has said that John has seen those pictures*, and apply the same technique to the word *that*. We would get the following results: *It is that John has seen those pictures that Mary has said* and *Mary has said it* but not **It is John has seen those pictures that Mary has said that* nor **Mary has said that it.* What can we

53. Readers who are interested in a nonstandard view of sentence structure based on an analysis of sentences with the verb *to be* can look at Moro (1997a). These sentences are peculiar because the most prominent noun phrase in the sentence structure may be a predicate, not a subject, and the subject may be found embedded in the verb phrase. We will return to these sentences in the last chapter.

conclude from these tests? That the word *that* somehow belongs to the structure of the embedded clause, because it cannot be left behind by the cleft or substitution tests.[54] Since the tense phrase (TP) already has a specifier (the NP subject *Peter*), we can hypothesize that *that* is the head of an independent phrase; this type of phrase is called a "complementizer" because it turns a clause into the complement of a verb, as in *Mary has said that John has seen those pictures,* or into the complement of a noun, as in *The fact that Mary has said that John has seen those pictures.* This complementizer phrase (CP) has a head and a specifier and will combine with the other phrases according to the usual rules.[55] In conclusion, the last phrase that we need in order to represent a sentence like *Mary has said that John has seen those pictures* is the following:

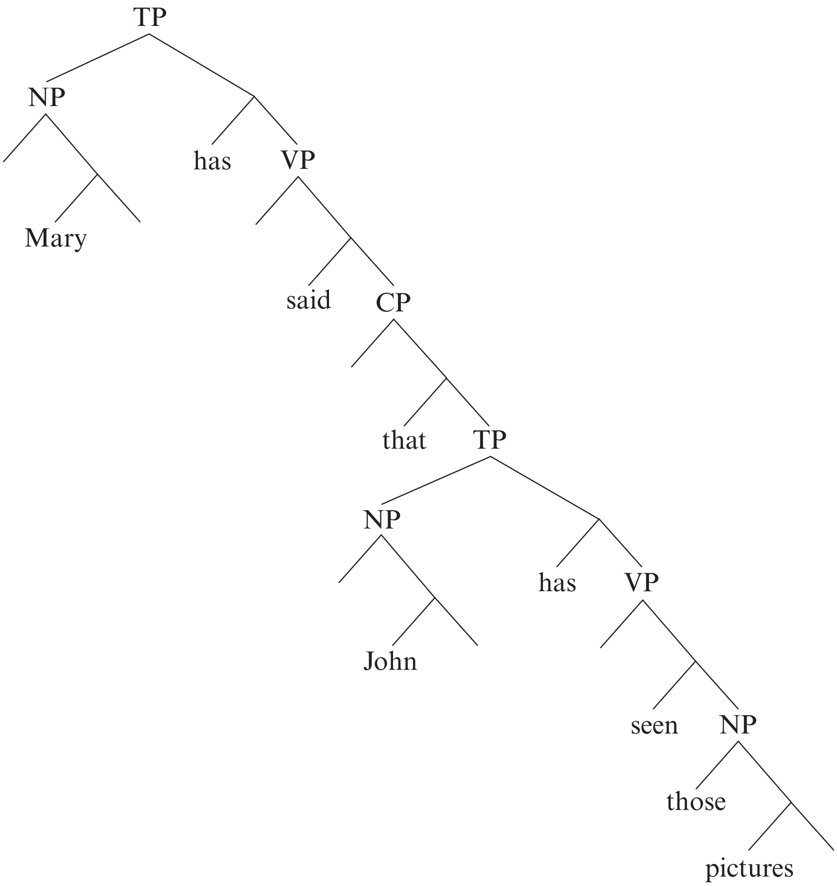


The complement of the head *that* is the TP that contains the elements of the embedded clause (the NP specifier *John* playing the role of the subject, the head *has*, and the VP complement *seen those pictures* playing the role of the predicate), while the CP itself is the complement of the head of the verb phrase *said* in the TP of the main clause (*Mary has said that John has seen those pictures,* where the NP *Mary* is the specifier playing the role of the subject, *has* is the head, and *said that John has seen those pictures* is the VP complement playing the role of the predicate).

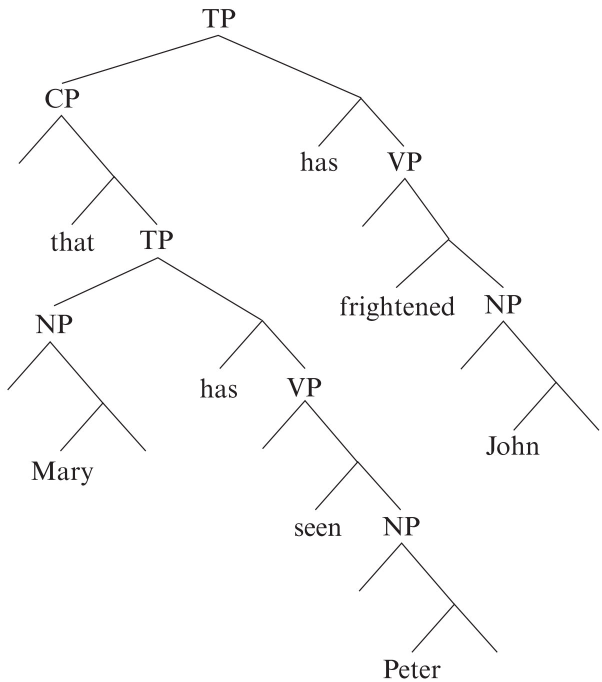
54. We may wonder whether an indefinite article like *a* is the head of an independent phrase as well. For simplicity, I did not address this issue, since it would not change the argument that I am developing. Nevertheless, the answer is affirmative. Starting with the work of Abney (1987), it is usually assumed that elements such as definite and indefinite articles are heads of independent phrases as well. See Abney's work for evidence in support of this hypothesis.

55. Elements that may occur in the position of the specifier of CP—which is automatically made available by phrase structure once an extra head such as C is assumed—will be discussed in section 3.2. I can anticipate that this position can host interrogative elements such as *when*. A minimal pair is the following: *John knows Mary left* and *John knows when Mary left*. In the second case the temporal adverbial *when* is inserted in the specifier position of the dependent clause.

The sentence *Mary has said that John has seen those pictures* is assigned the following syntactic tree, where a TP structure contains another TP structure and they both contain phrases of the same kind: NPs, APs, and VPs. This exemplifies the principle of recursion within the hierarchical structure that we have been discussing.



Structures can be even more complex. The subject of a clause can be another clause, a case in which recursion as we have defined it for syntax is even more evident. Take the sentence *That Mary has seen Peter has frightened John*. What is its syntactic tree? We need to take into account the fact that the subject—*That Mary has seen Peter*—is itself a clause. Therefore, the tree will have the following complex structure:



In this case, a CP is the specifier of a TP and contains the same elements as the main clause: TP, VP, and MP. This structure provides further evidence of recursion (and hierarchy) in syntax, in that it is a sentence that contains a sentence as its subpart.

To summarize, when we utter and interpret syntactic structures, we automatically assign each of them a syntactic tree that is always based on the same asymmetric hierarchical model of the kind "specifier–head–complement." I have discussed simple sentences and intentionally avoided the many issues that every linguist has to deal with in assigning the right syntactic structure to more complex sentences. Nevertheless, what we have seen should give an idea of the syntactician's research domain, which is similar to that of the chemist who reconstructs the structural schemes of molecules by knowing the atoms' valences and their rules of composition. The linguist reconstructs the structures of sentences starting from the words and the rules of phrasal composition as if they were molecules.

We have reached the end of our dealings with the issue of hierarchy and recursion in syntax. The phrasal structures we gave to sentences like *Mary has said that John has seen those pictures* or *That Mary has seen*

Peter has frightened John give us a good idea of what is meant by recursive hierarchical structure in syntax, where a structure of the kind X can be found within another structure of the same kind X, thus establishing different levels of prominence. In particular, we have observed the following: First, a sentence is made of hierarchically organized subunits that are called phrases and is not just a simple string of words. Second, the structure of these phrases is the same, except for the nature of the head; it is always asymmetric in the same way: it doesn't matter if we are dealing with NPs, VPs, APs, PPs, TPs, or CPs. Third, the structure of the sentence itself follows the same asymmetric skeleton ''specifier-head-complement'' via the TP and the CP split skeleton, which can be repeated recursively.

This is all we can say from our simple overview, or sample, of the syntax of natural languages, as far as the issue of recursion and hierarchy is concerned; but it is sufficient for formulating the hypotheses that will allow us to understand the neuroimaging experiments. It is worth noting, however, that this geometric property is not ''necessary'' at all: we can imagine a possible language that does *not* have this asymmetric recursive geometry, but it would be very different from the syntax of any natural language and such a human language does not exist. Human languages and only human languages are endowed with this property; it doesn't seem that any other species uses a similar architecture, not even primates, as we saw earlier.[56]

The existence of this architecture raises interesting questions. Why does it exist? It is not required by logical necessity. It would be very easy to create an artificial language that does *not* have a recursive structure with this geometry. We won't try to answer this question now. Nevertheless, it is worth observing that if a child is born with this architecture, then the possible number of word combinations for forming phrases is drastically reduced. Let's try to understand the situation, just in combinatory terms, with a couple of examples. Let's use the following convention to keep things compact: I will group the words that are under the same node in the syntactic tree in parenthesis. If we have a string of three words, a, b, and c, we have three possible combinations: abc, a(bc), and (ab)c. If we

56. Interestingly, this asymmetric partition of sentence structure in subject, predicate, and tense was recognized at least as early as Aristotle's work. See Moro (1997a, appendix) for an illustration of this theory, which relies on the possibility of representing tense as separated by the verb, as in *John caused the riot* vs. *John was the cause of the riot*.

add even just one word, d, giving us four words, the number of combinations becomes eleven: abcd, (ab)cd, a(bc)d, ab(cd), (ab)(cd), (abc)d, a(bcd), ((ab)c)d, (a(bc))d, a((bc)d), and a(b(cd)). If we now move to five words, the number of combinations jumps to forty-five. But if we apply the asymmetric geometric filter to the syntactic structure that we just saw, the number of combinations is drastically reduced. Because of the binary branching principle, in the case of four words, only five combinations are left since the combinations abcd, (ab)cd, a(dc)d, ab(cd), (abc)d, and a(bcd) contain nodes from which three branches depart.[57] The consequences of this for language acquisition are significant and will be discussed in chapter 3.

1.2.4 Dependence

Another central theme in syntax is *dependence, the capacity that words have to enter preferential relationships with other words (at a distance).* In addition to being combined to form a phrase, there are many ways in which words can do this. Let me explain. A typical case of dependence is number agreement—the fact that a verb or a noun is singular or plural, given certain syntactic circumstances.[58] We say *this dog* and not **these dog, those houses* and not **that houses, John runs* and not **John run, the children run* and not **the children runs*. How can we interpret these phenomena? A plausible hypothesis is that the head of the phrases *those dogs* and *that dog* is the same, *dog*, with an extra morpheme at the end, *-s,* to form the plural. What happens when *dog* or *dogs* enters into agreement with the specifier of its own noun phrase? From the repertoire of the possible demonstratives—*that, those, this, these*—we choose the one that shares the same morphological properties with the noun, in this case, number. Agreement, therefore, would be just selection of compatible morphemes. You may wonder why a natural language repeats the same

57. Mathematicians call the number of combinations of parentheses, given a fixed sequence of elements with binary combinations, "Catalano's numbers," from the name of the mathematician who gave the combinatory formula in 1838. If we include nonbinary combinations, the numbers are called "Catalano's super numbers" or "Schröder's numbers," from the name of another nineteenth-century mathematician who was studying combinatory problems. I owe these tidbits on the history of mathematics to Piergiorgio Odifreddi.

58. The distinction between singular and plural is present in English and in many other languages, but is not the only way to express number. Ancient Greek, for example, also had "dual" number, when there were exactly two elements were the subjects of predication.

piece of information on both the noun and the demonstrative, thus creating a certain amount of redundancy. This phenomenon is even more striking in languages such as Italian that have a richer agreement system. In the noun phrase *quello strano suo nuovo cappello nero* (literally, that-masc.sing strange-masc.sing his-masc.sing new-masc.sing hat-masc.sing black-masc.sing; "that strange new black hat of his") the same morpheme, *-o,* (masculine,[59] singular) is repeated at the end of each word six times. Whatever the explanation, language exhibits redundancy, and agreement is just one example of this.

Agreement is not the only case of syntactic dependence. I will mention two more types of dependence that will be useful for understanding the last theme of this sample of syntax: locality. It is important to notice the following crucial methodological issue: when I talk about dependence, I am just referring to a heterogeneous list of phenomena. At the moment, there is no unified theory of syntactic dependences, though it would be quite valuable to find a unitary primitive notion behind this term. But our list is still a good start toward describing how the syntax of human languages works. Let's proceed then by highlighting cases of dependence other than the case of agreement.

A second interesting case of a preferential dependence among words in a sentence is called *pronominal coreference.* This brings us to a brief discussion of pronouns. Let's take a step back. It is commonly assumed that we can use language to directly or indirectly refer to things or concepts in the world. If I say *the tables* or *happiness,* I am referring to specific (set of) objects or concepts. If I say, *to love* or *to hug,* I am referring to a relation between individuals. If I say *her,* I am referring to a female being. But how do words get us in touch with reality?

There is a long tradition of studies on the connection between words and the world. Linguistic thought has dealt with this issue at various times since its origins and this issue is still at the center of philosophical and linguistic debate. We should at least mention the Stoics, the Modists, the school of Port-Royal, and the analytical philosophy from the twentieth century. The Modists are a good example of the level of complexity of this issue (see Robins 1997 and Lepschy 1994). In the thirteenth and

59. Masculine and feminine in Italian only express a binary distinction. There is nothing typically masculine in a masculine noun like *naso,* nose, or feminine in a feminine noun like *testa*, head. The terms *masculine* and *feminine* are used as lables in linguistics only because they are just the prototypes of all the binary distinctions, similarly to the pair *weak* and *strong*.

fourteenth centuries, the Modists, also known as speculative grammarians, tried to found grammar on logic and ontology. Their name comes from their central idea: that the parts of speech (*modi significandi,* in Latin) should be made to correlate with the categories of thought (*modi intelligendi*) and with reality (*modi essendi*). Their thinking went: the noun and the pronoun are the parts of speech that refer to what is stable in the world, whereas the verb and the participle refer to what undergoes change. The goal of this great speculative enterprise was to discover a universal grammar that was common to all languages. Roger Bacon, a British philosopher and theologian called Doctor Mirabilis who taught at the Universities of Paris and Oxford in the thirteenth century, famously said of this theory of language: "Grammatica una et eadem est secundum substantiam in omnibus linguis, licet accidentaliter varietur" ("Grammar, in its substance, is one and the same in all languages, although there can be accidental differences"; quoted in Wallerand 1913). The development of this notion continues; in fact, even recently, the debate on the nature of the capacity that words have to refer to reality has been at the center of the philosophical and linguistic debate and has continued to trigger a diversity of opinions. Why are we talking about this unsolved issue to illustrate a case of syntactic dependence? Because once we make it clear that we do *not* want to address the problem of how words refer to things or concepts in the world, we are still left with the legitimate question of whether there are words that refer to what other words within the same sentence refer to (coreference). The answer is yes, and pronouns are a striking case. By looking at simple examples of pronominal coreference, we will see an interesting case of syntactic dependence that is very useful for understanding the foundations of the architecture of syntax as a whole. We may not improve our understanding of how we refer to reality, but we can certainly improve our understanding of the inner texture of language.

Let's consider the sentence *John says that he is tired*. There can be coreference between *John* and *he*, meaning that it is possible that *he* is interpreted as referring to the same entity as *John;* of course *he* could also refer to someone else, such as one of John's friends. It is as if the sentence *John says that he is tired* could be paraphrased as *John says that John is tired.* But a model of interpretation of pronouns based on the simple-minded idea that pronouns can be interpreted by just substituting the noun they refer to—as if grammar were guided by a principle of parsimony to reduce the effort in uttering a sentence—can be shown not to be correct. There are various ways to show this. Let's consider an example such as [*The man who wants to marry her*] *thinks that* [*the woman who loves him*] *is very beautiful*. We have two complex noun phrases in brackets and two pronouns, *her* and *him*. If pronouns just replaced nouns (or noun phrases, as in this case) that corefer, it should be possible to replace one with the other exactly as we did for the case *John says that he is tired,* yielding *John says that John is tired*. Surprisingly enough, this substitution that may look intuitively straightforward cannot be applied in our second case. Imagine replacing the pronoun *him* with the noun phrase [*the man who wants to marry her*]. You get *[The man who wants to marry her] thinks that [the woman that loves [the man who wants to marry her]] is very beautiful.* We could keep going *ad infinitum* by replacing a pronoun with a noun phrase with a pronoun that could then be replaced. The conclusion is that this is not a possible interpretive rule, since it would be repeated infinite times without ever reaching an end: the interpretation of pronoun must be governed by a more complex mechanism.[60] What matters here is understanding that pronominal coreference is one of the threads in the tapestry of the linguistic code that connect words with each other. We will see that pronouns also play a role in another fundamental property of syntax, locality. It is important to realize that these threads are not infinite: grammar has a complex but not unlimited texture. Agreement and coreference are only two of the tapestry's threads that relate words to each other within the hierarchical architecture that we have just started to explore.

Before concluding this section on dependence, let's look at some examples of an additional, rather surprising, type of dependence that makes human syntax unique among communication codes. Consider the sentence *Simon tells [many stories].* The noun phrase *many stories* is the direct object of *to tell,* or if we use the notion that we are discussing, we would say that [*many stories*] thematically depends on the verb *to tell*.[61]

60. This is often called "Bach's paradox" (see Bach and Harms 1968).

61. The interpretative role of a phrase is often called the thematic role, which is also evident from the expression "thematically depends." There is a limited number of thematic roles (agent, patient, experiencer, beneficiary). For instance, *Andy* has the thematic role of patient in the sentence *Larry hit Andy,* in which *Andy* occurs as the complement of the verb *hit,* and also in the sentence *Andy was hit by Larry*, in which Andy occurs as the subject of the verb *hit*. The theory of thematic roles is an important area of research in syntax. Among the many central questions it touches on, it looks for an explanation of the relative cross-linguistic invariance of the mapping between syntactic positions and thematic role assignments (also known as the acronym UTAH: uniformity of thematic assignment

The same phrase can be the direct object of other verbs, as in *Andy knows [many stories],* where *many stories* depends on the verb *to know*. Let's make the situation more complicated by considering the sentence *Simon knows that Andy tells [many stories]*. The noun phrase *many stories* can, in principle, be the direct object of a verb like *to tell* or *to know*. In this specific case, *many stories* is the direct object of *tells* and not *knows.* Why? This doesn't sound like a very sharp question. A straightforward answer could be that *many stories* is closer to *tells* than to *knows* and what matters for dependence is the closer verb. Is this intuition correct? No. It is enough to look at the following example: *[Which stories] does Simon know that Andy tells?* Now, the closest verb to the noun phrase *which stories* is *know*, but *which stories* still depends on *tells*. In fact, if in this sentence we try to insert a possible direct object for the verb *to tell* such as [*a fairy tale*] the result would be sharply ungrammatical: **[Which stories] does Simon know that Andy tells [a fairy tale]?* Why, is the sentence ungrammatical? Notice that there is nothing wrong in the string of words *Simon knows that Andy tells a fairy tale*: all the elements are interpreted correctly here. Nor has any principle of phrasal composition been violated: the architecture of phrases has been respected. The ungrammaticality is rather due to the fact that when you add [*which stories*] at the beginning of the sentence, this element would be left uninterrupted because *to tell* has already exhausted all its possible dependences: it has a subject (*Andy*) and a direct object (*a fairy tale*). In theoretical linguistics, the impossibility of adding elements that do not receive an interpretation, even though they fit into the phrasal structure, is usually called the "principle of full interpretation." The sentence **[Which stories] does Simon know that Andy tells a fairy tale?* violates this principle in the same way as the sentence **Who do you think to go?*

This evidence reveals an important fact that makes the structure of syntax much more complex than ever thought. The dependence among words can be "distorted" by a change in word order; that is, dependence can resist at a distance when the sentence is transformed in its sequence. This kind of distortion is technically called "syntactic movement," as if the "true" position of the phrase *which stories* in the sentence *Which stories does Simon know that Andy tells?* was the same as the position of *many stories* in the

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hypothesis). An important line of research is exactly the one that tried to deduce these invariants and the relatively small numbers of thematic roles in languages from the geometrical structure of syntactic trees. In this regard, the study by Hale and Keyser (2002) is fundamental.

sentence *Simon knows that Andy tells many stories*—in other words, as if the phrase [*which stories*] had moved from the position of direct object of *to tell*. Historically, Chomsky used the term *transformation,* which he borrowed from the work of his mentor, Zelig Harris, but used with a different meaning, to capture the idea that active, affirmative declarative sentences, the so-called "kernel" of grammar, constitute the starting point for the interpretation of grammatical functions (see Chomsky 1956/1965) and that from them all the other types of sentences—interrogative, negative, and so forth—are derived.[62] The term was so successful that for decades people talked about "generative-transformational grammar," nowadays often shortened to the more compact phrase "generative grammar," though this is not as accurate.

In the first models, a transformation was expressed just by numbering the phrases (or the words) in the initial structure and by giving an instruction on how to reorganize them and introduce the appropriate morphemes. But starting at least from Chomsky (1975a), a different way, call trace theory, has been used to express transformations. One way to describe these facts is to assume that a sentence like *Which stories does Simon know that Andy tells?* includes a syntactic element in its structure that lacks phonological content (technically called *empty category*). This element occupies the position of direct object of *to tell* and is directly connected to the moved element. It is, so to speak, "memory" in the phrase structure—a "trace" indicated with the symbol t placed in the position where the syntactic element has moved from: *Which stories does Simon know that Andy tells t?* These days we prefer to say that the moved element leaves an "unpronounced copy" of the moved phrase (copy theory).

62. For Harris, transformations were just equivalence classes among sentence types (See Graffi 2001); in other words, there was no structural type of sentence that would be the model to "start" from. Notice that, in principle, even in transformational grammar one could assume, for example, that the affirmative sentence is derived from the interrogative one, that is, that the object of a transitive verb is generated in the specifier position of the complementizer and then moved down to the object position. However, this has never been proposed for it is totally psychologically counterintuitive. This is an interesting case where our intuitions guide the analysis: assuming that affirmative declarative active nonsubordinate sentences are the basic sentences and all the other types (negative, interrogative, imperative, passives, etc.) are derived from them rather than vice versa was in fact one of the leading hypothesis at the very origin of transformational generative grammar (and virtually implicit in any grammar, as far as I know). However, the effectiveness of this hypothesis can only to be evaluated in terms of its heuristic force and simplicity on global grounds.

This aspect is irrelevant at this point but will become relevant in the last chapter. (The reader can find a detailed discussion of these themes in Chomsky 1995; for an alternative view see Moro 2004; see Graffi 2001 for a critical survey.) In the last chapter I shall return to this topic by presenting the standard theory and discussing some important implications that syntactic movement has on the relation between biology and linguistics.

Movement theory is, without a doubt, one of the central themes in modern syntax. To quote Chomsky, the fact that some lexical elements appear displaced with respect to the structural position in which they are interpreted is "an irreducible fact ... expressed somehow in every contemporary theory of language" (Chomsky 1995, 222). The fundamental idea is that movement is a pervasive operation of syntactic structures that can unify many facts that look unrelated on the surface. To give at least one example of the kind of linguistic aspects in which movement theory is implicated, independent of the principle of full interpretation, let's look at the phenomenon of case assignment.

"Case" in linguistic terms is a particular morpheme in some languages that nouns, pronouns, articles, and adjectives have to mark their structural dependence with respect to the various grammatical functions that they perform. Latin, ancient Greek, Russian, and German are languages with very rich case systems. In Latin, if I say *Paul-us Petr-um vidit,* the morphemes at the end of the nouns allow us to interpret the sentence as "Paul saw Peter." But if I say *Paul-um Petr-us vidit,* I am saying exactly the opposite, *Peter saw Paul.* In English, the case system shows up only in the pronominal system. The sentence *John saw her,* with the third-person female singular pronoun in the accusative case in the direct object position, is grammatical, whereas the sentence **John saw she,* with the third-person female singular pronoun in the nominative case in the direct object position, and the sentence **Her saw John,* with the third-person female singular pronoun in the accusative case in the subject position, are not. This example clearly shows that in this simple sentence accusative case can be assigned only to the direct object of the verb.[63]

63. I say "in this simple sentence" because there are structures in which accusative case can appear on the subject, as in the embedded infinitival clause in the following sentence: *John believed her to have seen Peter;* compare it with the corresponding clause where the nominative case appears on the subject in the embedded finite clause *John believed that she had seen Peter.*

The connection between case assignment and movement becomes clear as soon as the behavior of interrogative pronouns is observed. Interrogative pronouns can bear case: *who* is nominative whereas *whom* is accusative (though *who* is often used by English speakers as both nominative and accusative). So I can only say *Who left?,* not **Whom left?* or **Her left*. What happens if the interrogative pronoun refers to the direct object? An interesting example for us is the sentence *Whom has John seen?,* where it is clear that the interrogative pronoun can exhibit accusative case. It follows that an accusative element can be in a preverbal position, though not in **Her left*. The movement hypothesis can account for these facts in a natural way: the interrogative pronoun *whom* exhibits accusative case because it has moved from the position of direct object, where pronouns can receive this morphological feature (as in *John saw her*). In conclusion, we can find data supporting the movement hypothesis, which pervades syntax, based on the morphological shape of words, which is independent of the interpretative facts that we saw earlier (like the dependence of *which stories* from *to tell*).

Finally, notice that syntactic movement is not found in any natural or artificial code other than human language. In other words, syntactic movement is a property that exclusively characterizes the syntax of all and only natural languages, just like the recursive structure that generates asymmetric phrases. As far as recursion and dependence are concerned, then, this is all that we need for approaching the neuroimaging experiments that will come later in the book. We will return to the issue of syntactic movement and its justifications in the last chapter. *To summarize, dependence is a heterogeneous series of syntactic properties—agreement, coreference, case, and thematic dependence—that relate features of the words combined in a sentence. These properties are not infinite and constitute one of the salient characteristics of the hidden texture of human language.*

1.2.5 Locality

Dependence leads us to the last concept we want to touch on in our sample of syntax: locality. So far we have seen that syntax has its own recursive and asymmetric geometry, that words can have dependences, and that dependences can be maintained despite distances created when a structure is "distorted" by movement. Locality is a sort of "antidependence," a filter that blocks some potentially available dependences. Crucially, this filter is sensitive only to the geometry of syntax resulting from recursive phrase structure—one of our central themes. Our "Rubik's cube" of words is taking shape. So, what is locality?

If recursion in a language guarantees that there is no upper limit to the amount of computable structure (except for the biological limits of memory and length of life), then *locality is a limit on dependences: it is a filter that eliminates some potentially available dependences and thereby reduces the quantity of computable information in a syntactic structure.* In the last chapter, we will reflect on the reason why this filter exists and on its impact on the theory of language acquisition in children. For now, we will limit ourselves to observing three cases of locality related to the three cases of dependence that we have just discussed: agreement, pronominal coreference, and syntactic movement.

The first case of locality that we will examine is the locality of the agreement of the verb with the noun-phrase subject. We said that agreement is a type of dependence that is easy to observe. Let's consider the sentence *A mathematician tells many stories*. Here, the agreements are satisfied and the sentence is grammatical. Let's now consider the string **A mathematician tell many stories*. This is an ungrammatical string in Standard American English if it is construed as a sentence—but is it ever possible to find this string in this language? The answer is yes. It is enough to add a few words to the left of the string to show this: *The boys who know a mathematician tell many stories.* This sentence is undoubtedly grammatical, although it contains the string we just discounted: **a mathematician tell many stories*. Understanding why is not difficult to grasp: the new sentence includes a plural noun phrase, [*the boys*], that behaves as the subject of *tell,* which is why the sentence works. How can we capture this intuitive fact in a formal way?

This is a typical, simple example of locality: the verb agrees with the closest subject in terms of prominence. The noun phrase [*a mathematician*] is not the subject but is contained in a larger phrase that is itself the subject of the sentence: *[the boys who know [a mathematician]].* How is it that we can say that this string, *the boys who know a mathematician,* is a phrase? It is enough to apply the cleft formation test to the sentence in which the string is found, giving us: *It is the boys who know a mathematician that tell many stories.* If we leave behind *who know a mathematician* we get **It is the boys that who know a mathematician tell many stories*, a sentence that is clearly ungrammatical. Therefore, verbal agreement provides our first simple example of locality: the verb cannot agree with a noun phrase [*a mathematician*] that is embedded in another noun phrase [*the boys who know* [*a mathematician*]], although it is closer in the string of words. In order to realize its agreement, the verb is somehow "forced" to select the most prominent noun phrase within the hierarchical structure that contains it—that is, the least embedded one—and *ignore the linear sequence*, as if part of the string were invisible to the computation of agreement relations.

Pronominal coreference is a second interesting case of locality. Consider the sentence *Peter says that he is tired*—we look at this sentence earlier when we discussed dependence. *He* can corefer with *Peter*—or not. Now consider the sentence: *He says that Peter is tired*. There is no doubt: in this case, *he* can absolutely not corefer with *Peter*. Coreference is blocked. Why? The most natural and immediate answer that comes to mind is that since a pronoun replaces a noun, the noun must come first in order for the pronoun to be able to corefer to it. This explanation, however, is completely wrong. Consider the following sentence: *When the teacher says that he didn't do the assignment, Peter always comes up with lame excuses*. In this case, *he* can easily corefer with *Peter*. Clearly, the rule that came to our minds right away is wrong: as it often happens in scientific observation, the first superficial piece of data can be tricky.

It is also important to notice that the effect of locality that we just discussed does not rely on the linear order between the pronoun and the noun (and how could it, since the examples show that it doesn't matter?). This is the same conclusion that we reached earlier when we looked at agreement between verbs and subjects. Like agreement, locality relies on the "hierarchical structure" of the sentence in which the pronoun is found—that is, on prominence. In order to describe the locality constraints of this simple case of pronominal coreference, we will need the notion of the *domain* of a pronoun—a notion that is based on the hierarchy of phrase structure. We define the domain of a pronoun as the smallest clause—the smallest TP—that contains the pronoun (see Lasnik 1976 for the original proposal, and Haegeman 1997 and Carnie 2006 for an updated illustration of the principles governing coreference). Given this, the principle that regulates our two sentences is easy to formulate: "A pronoun cannot corefer with a noun that is within its domain."

Let's go back to our examples to see how this principle works in concrete terms, indicating by square brackets the domain of the pronoun. Why is coreference possible between *he* and the proper noun in *John says that [he is tired]* and *When the teacher says that [he didn't do the assignment], Peter always comes up with lame excuses* while it is not in *[He says that John is tired]*? This is because, in the third sentence, the domain of the pronoun is the entire sentence *He says that John is tired*. Therefore, *John* is within the domain of the pronoun. On the other hand, in the first two sentences, the domain of the pronoun is just the clause *he is tired* or *he didn't do the assignment*, respectively. Therefore, *John* is not in the domain of the pronoun. It follows that coreference is possible only in the first two sentences. This, of course, is just a fragment of the theory of pronouns, but it should be enough to show that—in the same way as agreement dependence—what matters for pronominal coreference dependence is not the linear order of the words in the string, but the geometry of the sentence structure.[64]

Another interesting example of locality is related to the syntactic movement that we illustrated earlier with examples of interrogative sentences and case assignment. I will be extremely brief because my only intent is to show some basic properties of filters in action that limit syntactic distortion due to movement. Not all phrases can move anywhere within the syntactic string. What can move where is one of the most important research topics in contemporary linguistics.[65]

A first simple example of locality constraint on movement comes from the comparison of sentences such as *I think that Mom wants to talk* [*with this nurse*] *before meeting* [*with the chief doctor*]. We will try to apply the syntactic movement of interrogative clauses to the two prepositional phrases in the square brackets. The resulting contrast is striking: although it is possible to say *[With which nurse] do you think that Mom wants to talk before meeting [with the chief doctor]?* it is completely ungrammatical to say: **[With which chief doctor] do you think that Mom wants to talk [with this nurse] before meeting?* The movement is blocked out of the embedded adverbial clause *before meeting with the chief doctor.* Nothing in principle tells us why one movement is allowed while the other is not. The explanation of these facts is not immediately obvious. Nevertheless, we can get an idea of how things work by means of an analogy. Let's

64. The notion of domain can be expressed in terms of prominence, but I will not illustrate this point due to reasons of space and because it is not strictly relevant (see, for example, Chomsky 1995, chapter 1).

65. This is not the only interesting question that the theory of syntactic movement raises. In the last chapter, we will address another important question: *Why* do phrases move? How far phrases can move and why they move are two different questions. so they can be examined separately. The idea that movement is restricted in a significant way in human language was first systematically explored within generative grammar in seminal work by Joseph Emonds (1976), who proposed the *structure-preserving principle* to constrain movement on a principled base (see also Graffi 2001 for historical notes).

reason as follows. Take the sentence *Frank took a picture of the lights during a trip to the Alps*. The following corresponding interrogative sentence is ungrammatical: **Which mountains did Frank take a picture of the lights during a trip to?* The phrase *which mountains* cannot be moved from within the phrase *during a trip to*. Notice that nothing prevents movement from the same phrase per se. In a different syntactic context, in fact, movement would be possible. Consider the sentence *Frank took a trip to the Alps*, and the corresponding interrogative sentence: *Which mountains did Frank take a trip to?* This sentence is fully grammatical. Why is there a difference? Descriptively, the role of *a trip to the Alps* is different in the two sentences. In the sentence *Frank took a picture of the lights during a trip to the Alps*, it is part of an adverbial element, whereas in *Frank took a trip to the Alps* it is the complement of the verb *to take*. Complements add information to the clause in a different way with respect to adjuncts. Adjuncts are phrases that just add unnecessary information, at least from a syntactic point of view, whereas complements are required by the verb and are therefore necessary. The sentence *Frank took a picture of the lights* would be grammatical, although less informative than *Frank took a picture of the lights during a trip to the Alps.* But **Frank took* instead of *Frank took a trip to the Alps* would be not only incomplete but also ungrammatical because the verb requires a complement. Usually adjuncts are formally expressed by adding an extra branch to the usual XP phrase skeleton as opposed to complements, although to preserve the binary branching principle, discussed earlier, some significant adjustment is required.[66]

Whatever the formal implementation is, the question arises: Could it be the case that it is not possible to move an element out of a phrase unless it is a complement of that phrase? In other words, could it be that the geometrical structure of the phrase matters for locality? In fact, if the string of words *meeting with the chief doctor* were not adverbial but instead were the complement of a verb as in *You think that Mom likes meeting [with the chief doctor]*, the movement of *with the chief doctor* would be possible: *[With which chief doctor] do you think that Mom likes meeting?*

The phrases out of which syntactic movement cannot occur are usually called *islands,* following John R. Ross's (1967) seminal suggestion. This

66. Technically, one possible way to preserve the binary principle and include adjuncts is to assume that the label of the node the adjunct attaches to is duplicated, creating an extra layer. See Haegeman (1997) and Carnie (2006) for a general illustration of the notion of *adjunct*.

kind of constraint crucially depends on the geometric structure of a tree and on the grammatical functions that the structure conveys, such as complement or adjunct.[67] Two apparently unrelated facts can now be explained in a unified way under the same locality restriction based on purely geometrical factors, according to which extraction is not possible from phrases that are not complements of a certain head. From a methodological point of view, this appears to be a genuine case of explanation in terms of simplicity: two facts captured by one single principle. But this is not the last kind of constraint.

Here is an example of a different kind of locality violation concerning syntactic movement. Consider the sentence *Peter knows that Americans elected Carter in 1976*. We can build an embedded interrogative clause by transforming the sentence into *Peter knows [which president] Americans elected in 1976* or we can move the phrase [*which president*] farther away and obtain the sentence: *[Which president] does Peter know that Americans elected in 1976?* We can also have the phrase [*in which year*] make a short move and produce *Peter knows [in which year] Americans elected Carter,* or the same phrase can make a longer move: *[In which year] does Peter know that Americans elected Carter?* But not all the sentences in which both phrases move are possible. The sentence *[In which year] does Peter know [which president] Americans elected?* cannot be interpreted as a question about the year in which a certain president was elected, as if it were moving from the embedded sentence. Rather, the only interpretation that comes to mind is the awkward question about the year in which Peter (presently) has that knowledge, as if it were moved from the matrix sentence. On the other hand, the following sentence is possible with only the relevant interpretation (given the right scenario): *[Which president] does Peter know [in which year] Americans elected?* This shows that [*which president*] can move from the embedded clause. The violation of the locality principle related to the interpretation

67. This geometrical approach has been pursued most notably by Pesetsky (1982)—in a framework called "path theory," Huang (1982)—proposing the condition-on-extraction domain (CED)—and Kayne (1984)—within the theory of connectedness. If this approach to locality turns out to be correct, another central theme of the syntax of natural languages could be entirely derived from the geometry of phrase structure, besides movement (according to the dynamic antisymmetry that we will look at in the last chapter) and thematic role assignment (as proposed by Hale and Keyser 2002) leading to the attractive hypothesis where all central processes of grammar can be reduced to a computation based solely on the geometry of phrase structure.

of sentences like *[In which year] does Peter know [which president] Americans elected?*—where [*in which year*] incorrectly moves from the embedded clause—is usually called *intervention effect*: a certain syntactic item (in this case, an interrogative element such as *which president*) blocks the movement of another item (in this case, the interrogative element *in which year*). Violations of this kind are often referred to as *relativized minimality violations*. The notion of (rigid) minimality, introduced by Chomsky (1986b) for a different domain, is one of the central notions of syntax. Relativized minimality, instead, was proposed by Luigi Rizzi's (1990) seminal work, to emphasize the fact that the intervening element blocks only elements of *a similar kind,* according to a fine-grained partition among phrases. So for example, only *wh*- phrases can intervene to block movement of *wh*- phrases. In our case, however, the situation is even more complicated. A noun phrase (*which president*) blocks the movement of a prepositional phrase (*in which year*)—but not the other way around, even though both are interrogative phrases. For this reasons, the theory of relativized minimality must include other factors to predict locality effects, such as, for example, the notion of referentiality to distinguish between noun phrases and prepositional phrases. Prototypically, according to Rizzi, elements like noun phrases that play the role of arguments of predicates are assigned an "index" expressing their capacity to refer to things in the world, whereas prepositional phrases that play the role of adjunct are not. Relying on this kind of asymmetries (and the mechanism of index assignment) the different capacities among elements to cross over elements of the same type is captured (see Manzini 1992 for a critical survey of locality theories, including relativized minimality).

Finally, there are other locality principles concerning syntactic movement that do not make reference to either the notion of complement or of intervention, but instead try to characterize the specific properties of the syntactic context that is right next to the point from which a certain element is moved. A very simple case is the agreement requirement. Agreement is triggered between a moved element and an element that is right next to it, as if agreement were signaling the starting point of the moved element. For instance, in Italian, when you use certain types of clitic pronouns as the complement of a complex verb, the pronoun moves to the left of the verb and it triggers agreement on the past participle of the verb. In the sentence *Umberto ha visto tre montagne in Alto Adige* ("Umberto has seen three mountains in Alto Adige"), the ending *-o* of the verb *visto* ("seen") signals that it is the default form of the verb, which happens to be masculine singular. *Tre montagne* is plural feminine in

Italian, as shown by the final -*e* in the noun *montagne*. If we replace it with the corresponding clitic pronoun *le* (feminine plural, as shown by the final -*e*), we get: *Umberto le ha viste in Alto Adige* ("Umberto saw them in Alto Adige"), where the past participle *viste* with the ending -*e* agrees with the clitic pronoun *le* in gender and number. **Umberto le ha visto in Alto Adige*, where the past participle *visto* no longer agrees with the clitic *le,* is completely ungrammatical. Thus, agreement is relevant to locality. In fact, agreement on the past participle can be intuitively regarded as a way to characterize the specific properties of the point in the structure from which the movement originated. In other words, it could be that an element can move up to a certain point in the structure only if—in order for it to arrive at that point—its trajectory can be marked by elements that agree with it. As Chomsky puts it: "It is not unreasonable that Universal Grammar requires the presence of an empty category to be signaled somehow by phonetically realized elements" (Chomsky 1981: 251). The requirement of agreement is only one of the possible strategies by which universal grammar "signals" the presence of an empty category, in this case, a trace. In other structures or in other languages, this principle could be satisfied in other ways, for instance, by making use of resumptive pronouns or specific morphological or lexical characteristics of the words that are close to the empty category (see Rizzi 1990 for a cross-linguistic discussion of different strategies to satisfy this requirement). This further locality principle is often called *empty-category principle* (ECP), an allusion to the hole the moved element leaves behind in the string of words, and it interacts in a complex way with other locality conditions, such as those based on intervenors.

These discoveries regarding movement constraints have given a critical push to the development of the study of human language. Historically, they may have been the first concrete and striking example of the general goals of generative grammar. With the discoveries of the structural conditions that allow or prevent movement, the strategies that were available for discovering the class of "all and only" possible grammars started to become clear. The historical development of this evolution is extremely interesting if you want to understand the distinctive features of generative grammar. John R. Ross's Ph.D. dissertation was one of the turning points of this development (published without substantial changes as *Infinite Syntax;* see Ross 1986). In his work, Ross went from the search for constraints on movement in different linguistic structures to a reflection on the general principles that underlie such constraints, which opened the door to a new research program. Ross—the originator of the term *is-*

land for the structures from which movement is blocked—explicitly states (1967), "The constraints ... which I will propose are often of such a complex nature that to state them as constraints on rules in particular languages would greatly increase the power of transformational rules.... So I will tentatively assume that many of the constraints I have arrived at in my investigation of the few languages I am familiar with are universal" (8). From this work on, it has been quite clear that *if linguistic theory wants to keep the characteristics of simplicity that are proper to a scientific theory, it can only be a universal theory on the constraints on possible syntactic combinations.*

For the twenty years that followed Ross's dissertation in 1967, this attempt was one of the most impassioned and fruitful research avenues in syntax. Locality conditions such as those we mentioned earlier are the fruit of the research from these years. As new specific constraints were discovered in different languages, linguists tried to reduce them to a finite number of general classes, until they arrived at the locality principles—such as intervention effects—and the empty-category principle, which we just discussed. From the mid-1980s on, most of the research moved beyond attempts to unify the locality criteria, but without reaching any generally accepted results (see Chomsky 1986b for one example). (For more on the historical evolution of locality, see Manzini 1992, Roberts 1988, and Moro 1993. An alternative view that runs contrary to attempts to unify locality conditions on empirical grounds can be found in the appendix in Moro 2000.)

Nevertheless, there is no doubt that the success in finding similarities between constraints that looked independent, and the possibility of reducing them to progressively more abstract schemas has been successful. This very fact has constituted one of the first valid pieces of empirical evidence in favor of the hypothesis that there is a crucial component of language that is innate, meaning that it precedes experience. Without generalizations and abstractions of this sort, this hypothesis would have remained a dream, a "philosophical" metaphor. By showing in a concrete way how these constraints are common to all languages and too complex and intricate to be learned from explicit stimuli, the hypothesis that universal grammar is biologically determined has become scientifically plausible, on the basis of purely linguistic data and not on neurobiological data.

Finally, it is interesting to notice that *it was the partially failed attempt to give a universal account for locality phenomena that triggered a radical change of approach toward the problem of language variation*, since it allowed for the identification of simple, clear, and systematic examples of cross-linguistic variation in universal grammar (see Graffi 2001 for a critical discussion; on the original empirical base on which the model of universal grammar has been built, see Kayne 1980, Taraldsen 1978, and Rizzi 1980). For instance, Rizzi observed that the movement of a relative pronoun across an embedded interrogative clause in Italian produces a different result than in other languages. There is a sharp contrast between Italian and English in this regard. In Italian it is basically fine to say: *Ecco l'incarico che non so proprio a chi potremmo affidare,* the English equivalent, **Here is the job that I really don't know who we could assign to,* is ungrammatical, showing that the embedded interrogative clause is an island in English. The fundamental fact is that since it was not possible to reduce this and other variations to the complex net of principles of universal grammars that were identical across languages, people agreed that this complex net of principles should just been seen to allow points of variation that could not be further broken down, which were later called *parameters*. In Italian, embedded interrogative clauses do not constitute islands, which means they do not count as barriers for movement, whereas they do in languages English and German. In the twenty years that followed this realization in the late 1970s, linguists tried to find other minimal and systematic differences among languages, other parameters that would allow for the maintenance of the hypothesis of a universal net of principles along with the recognition of differences across languages. The cross-linguistic differences in locality constraints were the first primary impetus for the "principles and parameters" model, which nowadays constitutes the standard model for modern syntax (ever since its first formulation in Chomsky 1981). We will often return to it.

To conclude our camera's rapid pan of the landscape of locality issues, I want to emphasize once again the characteristic shared by all the examples I have mentioned: the fact that *the various locality principles are independent of the linear order of words but are instead based on the hierarchical structure of phrases.* For the sake of simplicity, I will focus on a particularly clear example. *I say that a man who eats [this pizza] reads [this book].* If the two bracketed phrases were equally free to move, the following two sentences would both be grammatical: *[Which book] do I say that a man who eats this pizza reads?* And **[Which pizza] do I say that a man who eats reads this book?* But only one is grammatical. The movement in the second sentence is not allowed, just like in the case of movement out of adverbial phrases we saw while discussing cases like **[With which chief doctor] do you think that Mom wants to talk [with this nurse] before meeting?* Why? Those who know a bit of grammar might find the *description* of this contrast trivial: if the phrase is contained in a relative clause, the movement is blocked, whereas if the phrase is in the main clause, the movement is possible. But the *explanation* of this contrast is much more abstract and relies on conditions that are similar to those that we discussed while accounting for the impossibility of movement out of adverbial clauses. In fact, a relative clause is in a certain sense like an adverbial clause: it expresses an "additional" property much as *before meeting the chief doctor* expresses an additional property in the sentence we discussed before. Also, a relative clause is part of the phrase whose head it modifies. For instance, if you say *Francis loves a girl who writes poems*, a constituency test based on cleft would yield the following contrast: *It's a girl who writes poems that Francis loves* versus *It's a girl that Francis loves who writes poems*. This is sufficient to show that *a girl who writes* poems is a phrase, more specifically a noun phrase. Therefore, the clause *who writes poems* is part of the noun phrase—is embedded in it. If we now go back to the issue of movement, the relevant thing to notice is that that linear order does not matter at all. If you thought that the reason for the contrast we showed was tied to the fact that you can only move the last phrase as opposed to those that precede it, the following example should change your mind. *I brought [a pizza] to a man who was reading [a book]* is fine, but moving the last phrase to the beginning yields the following ungrammatical sentence **[Which book] did I bring [a pizza] to a man who was reading.* In this case, it is clear that the hierarchical structure rather than linear order matters. As we have seen in all the examples in our overview of syntax so far, it is not possible to extract a complement out of a relative clause, and the fact that the moved phrase is at the end of a string of words is irrelevant. Hierarchy is the real hidden structure of syntax and this fact is strongly supported by both the study of adults' knowledge of syntax and the data coming from children's spontaneous acquisition of their native language (on this specific issue, see Guasti 2002, Yang 2003).

We end our sampling of syntax here. In chapter 2 we will see how indispensable the knowledge of the fundamental elements of syntax is for understanding how the neuroimaging experiments have been designed. For the sake of convenience, we will refer to the hierarchy and the locality that characterize the syntax of human languages as *structure dependence,* a commonly used term. Structure dependence, being crucially based on hierarchy, is in sharp contrast with the most evident physical aspect of the linguistic code, its linearity. Two words can have a dependence-relation-like agreement at a distance, and that distance can be increased as much as you like, as in *John runs; John who knows many girls runs; John who says that his brother knows many girls runs*; and so on. If dependence relations were sensitive just to linearity, this could not be explained. Linearity alone is not enough to account for the facts; additional dimensions are needed to represent dependence relations which are in fact actions at a distance. In conclusion, structure dependence, based on hierarchy and locality, constitutes the hidden texture of the "tapestry" that is the syntax of natural languages. Structure dependence is exactly what will guide us toward the pivotally important brain experiment described in the second part of the book.

But before concluding, we need to explicitly mention a very strong idealization that we have implicitly assumed thus far. We have primarily limited our observation to a single language—English. But it is obvious that this level of simplification is not fully satisfactory, especially if the problem is to understand how the human brain works, which is potentially not limited to the knowledge of just one language. In other words, *we need to explore whether the syntactic theory presented by focusing on one single language is compatible with all languages in the world.* The question cannot be avoided. Therefore, in the next section, we will deal look at the relationship between the syntactic theory that we have illustrated with data from English and some Italian and the diversity among the many languages in the world. I will show the fundamental elements that have contributed to making the theory of universal grammar scientifically plausible by making reference to purely linguistic data. In chapter 2, I will verify if this theory is convergent with what we know about how the brain functions.

1.3 The Ark of Babel

What is Noah's ark if not the dream of gathering a representative sample of life forms? In the Bible (Genesis 7:1) it is recounted that Noah built an enormous vessel that could contain male-female pairs of every living kind in order to preserve all life forms from imminent death by flood. What would have happened if such a universal flood had occurred after 1953.[68] A little lifeboat would have been enough for Noah to carry a sin-

68. In 1953 the famous article by James Watson and Francis Crick was published on the double helix structure of DNA. I take this date as a reference point, but not everything regarding the importance of DNA for classifying living species was understood right away (on the history of molecular biology see Judson 1979).

gle test tube with the DNA of any living being. After all, we have known since the last century that between an elephant and a butterfly there is no qualitative difference—only a difference in order and number within the four-letter alphabet that makes the sequence of the genetic code: G, A, T, and C, which stand for the four nitrogenous bases that make up the long chains of DNA in all living beings. In theory, a modern Noah could take the DNA in that test tube, rearrange the letters, and come up with a recipe for any possible animal, not just for those that already exist.

1.3.1 Limits of Variation

Imagine an ark of speakers instead of animals—an ark of Babel where the dream of a collection of all possible languages could be realized. Most catalogues listing the world's spoken languages (see, for example, Comrie 1981) claim that there are six to seven thousand in the world today—and that's not counting the dialects within individual languages.[69] Noah the linguist would need an entire fleet to carry (a pair of) people for each language or dialect.

In contemporary linguistics, a reduction process that started in the last century and that still continues today is analogous to the work in genetics that would have allowed a modern Noah to have all the material to create all possible living forms in one test tube. *Nowadays, linguists are convinced that as far as syntax is concerned, all the grammars in the world are just variations on a universal mold.* The mold contains a few points of variation, but since the mold is very complex, these points of variation produce differences on the surface that are so dramatic that the hypothesis of a unique model appears totally counterintuitive and beyond the reach of sensory experience. Something similar to the sensations one feels when listening to Bach's *Goldberg Variations* (BWV 988): a set of thirty variations of the same bass line of an aria for harpsichord published by Johann Sebastian Bach in 1741. The variations—often considered an encyclopedic sum of Baroque music—are so complex that even if they are based on a single ground bass theme it is very hard for a nonexpert musician to recognize the underlying common pattern. The same thing happens when we are exposed to human languages. Normally, no one ever imagines or feels

69. Max Weinreich's saying is now a classic: "A language is a dialect with an army and a navy."

that the syntax of English and that of Japanese can be reduced to the same universal mold with minimal variation.[70]

Let's go back to Noah the linguist. If the theory of Universal Grammar adopted here is true and if we ignore the lexical differences among languages, then just one person would be sufficient in our ark of Babel to create all the possible grammar. Once the combinatory principles are found, it would be possible to recover the syntax of all languages from just one, even if they turned out to look very different in the same way that an elephant looks very different from a butterfly. With just one language and an adequate combinatory theory, we could recover the syntaxes of all existing languages as well as the lost ones. In fact, from just one language we could recover the entire class of possible syntaxes, including those that have never been spoken and those that may never be spoken. However, this is still a dream—and perhaps finding the class of possible life forms is a dream for biologists as well. Today, nobody is able to recover the class of all possible syntaxes from just one language, in the same way that nobody can recover all the animals starting from just one animal. Neither can we give the recipe for universal grammar, but it is no longer just a mirage or a metaphor: with a healthy portion of optimism, we can start to see a glimpse of the possible architecture.

Linguistics and biology have much in common: instead of reconstructing all the possible forms from just one sample, both the biologist and the linguist have chosen the comparative path. Biologists, both naturalists and geneticists, compare significant samples of different forms rather than analyzing just one single form in depth. In linguistics, the beginnings of systematic comparison in Europe go back to the start of the fourteenth century and Dante's *De vulgari eloquentia*, but the pace picked up dramatically about two hundred years ago, at least for European linguistics. In fact, modern linguistics, as a scientific domain, coincides with the birth of the process of comparing similar languages in order to build the genealogical tree of the different linguistic families.

In the 1970s, Richard Kayne was the first to perform in-depth studies of comparative generative syntax. Very recently, the notion of comparison in linguistics has been refined even more, still thanks to Kayne's

70. In this case, too, we have a symbolic date, 1981, the year Noam Chomsky's famous Pisa lectures were published (Chomsky 1981). Chomsky gave these lectures at the Scuola Normale Superiore in Pisa in 1979, before an audience of scholars who would make a significant contribution to the development of the model we are talking about (See Graffi 2001, 449–62).

work, by the introduction of the distinction between *microcomparison*, the comparison of very similar languages, such as the paradigmatic study of Italian dialects, and *macrocomparison,* when languages from different families and types are compared, such as the comparison of Romance and Germanic languages or Indo-European and Sino-Tibetan languages.[71] The comparative work is sharpening more and more. (For a critical examination of the state of the art in parameter theory and in a concrete proposal on the extension of the comparative method to historical reconstruction, see Lightfoot 1991, Longobardi 2003, Roberts 2004, and Gianollo et al. forthcoming.)

How is it possible that the syntax of languages as different as English and Japanese are variations of the same template—come from the same architecture? This fact is definitely surprising. But imagine being a biologist in the nineteenth century: Wouldn't you have been surprised to learn that the difference between, say, an elephant and a butterfly is quantitative—resting on a variation in order and number of four elements within a complex molecule? Yet modern biology has managed to produce such a reduction—even if biologists themselves often say that the idea of reconstructing a living being from DNA is still a pipedream, and our knowledge falls far short for actually replicating life.

71. See Comrie (1981) for a catalogue of the languages of the world. If you are interested in the comparison between the genealogical tree of the languages of the world and the genetic study of populations, see Cavalli Sforza (1996/2000). Cavalli Sforza's fundamental studies, a benchmark of modern population genetics, show that the genealogical trees of languages can be largely superimposed on the genealogical trees of populations, which in turn are based on genetic distances from the earliest common ancestors, who probably lived in Africa about 100,000 years ago). The common-ancestor hypothesis and thus the hypothesis that all human languages come from the same language is fully compatible with the hypothesis of the existence of a universal grammar, but the common-ancestor hypothesis does not allow us to decide whether similarities among languages are due to an ontogenetic biological nature or, more simply, to a progressive differentiation from the accidentally formed original model that is due to error accumulation. Our study of the brain's reaction to impossible grammars is therefore empirically different from the hypothesis that all languages are historically derived from a common language, and it has totally different consequences both empirical and theoretical. Also, the proposed genealogical trees of languages are almost exclusively based on phonological, lexical, and morphological comparison. Syntactic taxonomies, in particular those based on Joseph Greenberg's fundamental work, are intrinsically ahistoric (see Graffi 1980). Given the importance of syntax for characterizing human languages, the lack of syntactic contribution to the building of genealogical trees of language is no small thing.

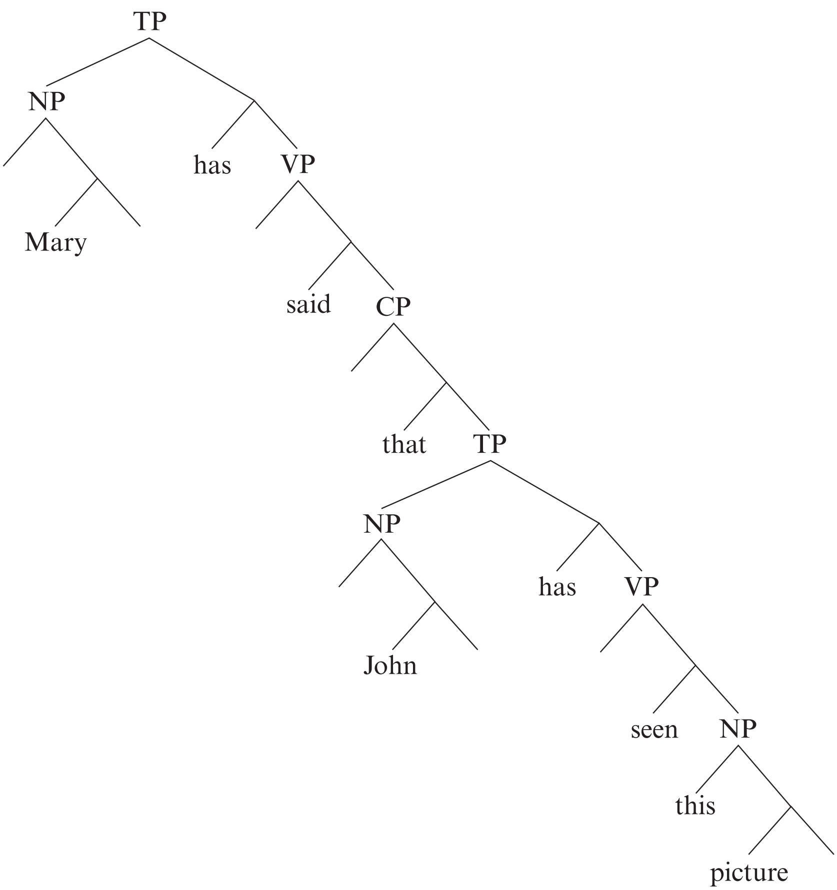
Let's stick with this dream, and strengthen it with a comparison. Think of the elements the world is made of: iron, hydrogen, chromium, uranium, and so forth. At first glance we seem to be dealing with very different elements: What does hydrogen have in common with iron? This looks similar to the elephant and butterfly in biology. Once the structure of an element was discovered and the general combinatory laws were found, the *qualitative* differences among elements was reduced to the *quantitative* differences among subcomponents of those elements. Mendeleev's "Periodic Table of Elements" showed how one could systematically go from one element to another by increasing the numbers of protons, neutrons, electrons and so on, according to a rational schema (Mendeleev 1869). Wouldn't a chemist in the eighteenth century have been amazed to learn that the differences between elements were quantitative and not qualitative?

Modern linguists dream of arriving at a "periodic table" of human languages. The metaphor has been circulating among linguists for a while and was fleshed out in Mark Baker's book *Atoms of Language* (Baker 2001). I believe that we should still consider it a metaphor. *Unfortunately, we do not yet have a "table of languages" that is comparable to the table of elements, and I don't believe that it is just a matter of time until we get it.* The problem is that we have not yet fully understood what the aspects are that make one language vary from another in *all* its components, even if we just look at syntax. Nevertheless, I agree that a certain optimism is not completely out of place. In some cases we have been able to find tiny differences between different languages that produce spectacular global effects in a system as complex as the syntax of a language—just as minimal differences make us think of the elephant and the butterfly, or iron and hydrogen as variations on the same theme. What is the empirical basis for such optimism? Let's look at a simple example. Consider the following four strings of words:

(Group 1)
a. Mary has said that John has seen this picture
b. Mary John this picture seen has that said has
c. Mary this John picture seen that has said has
d. John Mary has that said has seen picture this

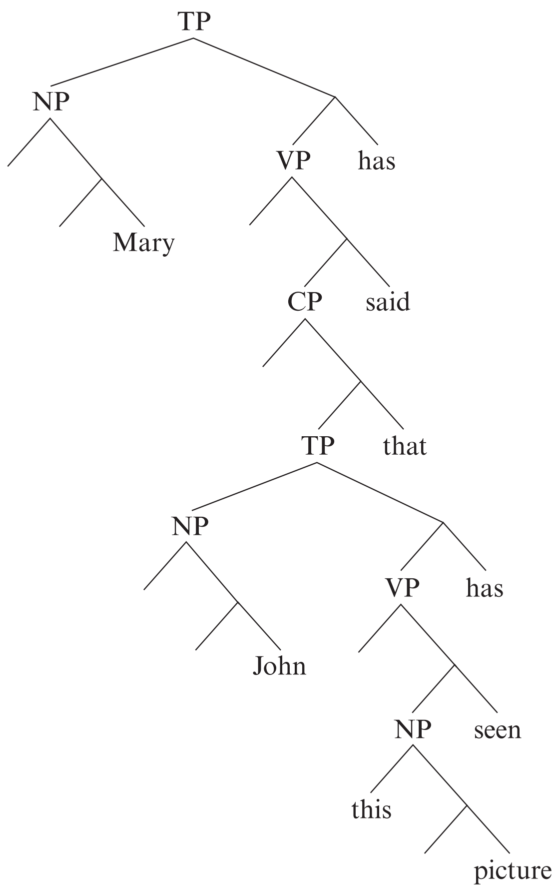
What is the difference among these sentences? Only the first one is grammatical and it is the only one that can be correctly considered a sentence. Each of the others is a hodgepodge of words. Nevertheless, despite appearances there is an order in all this disorder. Let's go back to the syntactic tree that we saw in the previous section—the bidimensional representation of the hierarchical and asymmetric structure that we assigned to various English phrases. There you will find the solution to this puzzle. In fact, the two of the apparently messy strings in group 1 are, in fact, strictly related and are not at all random strings of words: the first and second can be obtained from the same phrasal structure, whereas the others are truly chaotic. The only difference between the first two strings is in the linear order of the head and the complement in each phrase: the head precedes the complement in the first string, whereas if follows the complement in the second; the specifier always precedes the head-complement complex. *The apparent disorder is traced back to a unified order by assuming a minimal change in the underlying system.*

Let's go step by step. First, let's show the structure of the first string of words, *Mary has said that John has seen this picture*:



In the sentence above, the order of the specifier, the head and the complement is the same as the one we saw in previous trees, according to the general XP skeleton: the head precedes the complement and follows the specifier in each phrase (TP, VP, NP or CP) in a specific asymmetric pattern.

Let's now try flipping the order of the head and the complement in each phrase—in other words, have the head follow the complement rather than precede it:



The result produces the word order in the second sentence. It is not surprising that a small change can cause such a spectacular result. On the basis of this simple order variation within phrasal structure, we can observe the effect of recombining the linear order of the words in the sentence by building the corresponding syntactic tree in which the complement precedes the head in each phrase (discussed in more detail in the final chapter). The linear order that corresponds to the new syntactic tree, in which the complement precedes the head in each phrase, should be read in the same way as the syntactic tree for the actual English sentence: the words must be read, starting from the top-left-most word and following the tree, moving from left to right. In English, the head of the noun phrase *Mary* is followed by the head of the tense phrase *has,* which is followed by the head of the verb phrase *said,* and so on. The same thing can be done with the syntactic tree of the second sentence, which initially looked so chaotic. The head of the noun phrase *Mary* precedes the head of the noun phrase *John,* which precedes the specifier *this* of the noun phrase *this picture,* and so on until we reach the complete sentence in italics *Mary John this picture seen has that said has*. In this way, we get exactly the word order in the second sentence with just one structural change: the switching of the order of the head and the complement in each phrase. This minimal change has, nevertheless, had an extremely visible macroscopic effect that makes the syntactic tree and its corresponding linear order of words unrecognizable with respect to the previous one. The third and fourth sentences are truly chaotic: there is no hope of deriving them from the architecture shared by the previous two with minimal and systematic changes.

For what concerns us here, the interesting thing is that the second sentence has the word order of the Japanese equivalent of the first sentence. *Thus, Japanese is "the opposite" of English in terms of the linear order of head and complement, which is a central aspect of the syntactic structure.* In our example, we have built a new language. It is made of Japanese syntax and English words—call it "Japlish." No child has ever acquired such a chimera of a language, but it does not go against the fundamental nature of language. We will go back to the plausibility of this theory in the last chapter.

In my opinion, the heuristic value of formalism that we discussed in the section on method seems especially clear in this case. Like the double helix, the representation of syntactic structure has, *mutatis mutandis,* suggested the discovery of a new structure and opened the field to a new research area.[72] Even though this is just a simple example with a slightly artificial flavor, the main point should be clear: *even minimal variations in a complex system trigger dramatic maximal differences in syntax, as in*

72. We will come back to this aspect in the final chapter, which is devoted to speculation on future research.

biology. Most of the research in syntax in the last twenty years has focused precisely on finding these minimal and systematic differences. The syntactic elements that vary among languages are called *parameters,* and the elements in the system that do not vary are called *principles.* This is why the model that currently dominates in linguistics is called the "principles and parameters" model. The parameter that we just manipulated concerning the internal structure of phrases is called the "head-complement parameter," but there are others, and the debate on the nature and format of parameters remains open. What is universal within this component of syntax is that—independent of linear order variations—every phrase is made of a specifier, a head, and a complement and that the specifier is prominent on the head-complement sequence. What is language-specific is the linear order of the specifier, the head, and the complement within each and every phrase.

It goes without saying that the hypothesis of the existence of parameters to account for language variation raises important questions that are intricate and fascinating. How many parameters are there? Are there universal principles that determine the format of possible parameters? Does every parameter have only binary choices?[73] At what age does a child know which value he or she has to assign to each parameter? Do parameters have a default value or does a child have to face a completely arbitrary choice? Are parameter values chosen simultaneously or is there an order? Can the value of a parameter influence another? And finally, possibly the most radical question: Why do parameters exist?[74]

As usual, new ideas trigger new questions that cannot always be answered right away. But there is at least one thing we know for sure. In order to see it, we will assume the simplest model in which there are only binary parameters that are all independent of each other. If there were only one parameter, only two syntaxes would be possible. If there

73. The idea that parameters only have binary values is extremely widespread in contemporary linguistics. This hypothesis is so strong that, as originally suggested by James Higginbotham, parameters are often simply referred to as "switches."

74. For those interested in the critical development of this issue, see Longobardi (2003) and Kayne (2003). For the empirical base of the notion of parameter, Chomsky, in his "Pisa lectures," relied substantially on Kayne (1980), Taraldsen (1978), Rizzi (1980), and Hyams (1986). See Graffi (2001) for a critical history of the development of this research program. See also Manzini and Wexler (1987) and Wexler (1993) for the illustration of the *subset principle,* one of the first and most powerful explanatory principles concerning parameter setting and language acquisition.

were two, four syntaxes would be possible. If there were four, sixteen syntaxes would be possible, and so on. In general, n parameters would generate 2^n syntaxes. This is rather surprising. If parameter theory turns out to be correct, thirteen parameters would generate 8,192 different syntaxes, which would be more than enough to account for the six to seven thousand languages that are usually assumed to be currently spoken (even if we assume that they all have a different syntax). With sixteen parameters we could generate 65,536 different syntaxes, about ten times the estimated number of currently spoken languages. Though this is obviously an oversimplified model and an extremely large number of questions are raised, it is easy to see how important parameter theory is for defining the class of human languages. Its consequences for the theory of language acquisition are also clear. A child choosing from among thirty parameters wuld have more than 1 billion different syntaxes—1,073,741,824, to be exact—available. This number goes well beyond the number of human languages that we can reasonably assume have been spoken since humans first appeared. With three more parameters, we would even go beyond the number of living human beings—which is now around 6,400,000,000—and reach 8,589,934,592 different syntaxes.

All the questions we raised earlier and many others remain open, especially the important question of whether there are general principles limiting the number and the formats of parameters and the even more important question of why parameters exist at all. So far there is no answer to the latter question. One possibility, the less interesting one, is that they are accidental: they are just there for no reason and simply they are compatible with the system of principles. A more interesting possibility would be rather that *parameters are just the degrees of freedom that must be tolerated to preserve the system of principles.*[75] In other words, they are not separate instructions, but they are generated by the system of principles itself. As for the other question on the format and number of parameters, from a purely methodological point of view, if there were no restrictive generalization on the format of parameters, the theory would be too weak. If each cross-linguistic difference corresponded to a different parameter, principles and parameters theory would end up being equivalent to a description of the difference, rather than an explanation of it. This possibility cannot be excluded in principle, but if it is true, then the

75. It may be useful to compare parameters with ambiguities in language. *Mutatis mutandis*, one can say that ambiguity in language is the price that must be paid for having a system that is not too complex.

simplicity requirement we discussed earlier will no longer hold. Among other consequences, the theory of language acquisition that would result from such a theory of language would look much less credible. Children's fast language acquisition and the absence of "mixed" structures in learning and attested language varieties would have to be accounted for differently. In fact, all conceivable combinations of syntactic rules should be available, rendering the very notion of parameter meaningless. In other words, if we assume as many parameters as the differences among languages, it would be like assuming that there are two different forces that apply to smoke and water when it comes to their natural movements. The theory would be as complicated as reality.

1.3.2 Grammar as a Limit for Experience

Before concluding this section about the dream of a collection of all existing or possible languages—what I have called the Ark of Babel—I would like briefly to touch on the issue of how the concept of parameters affects how we answer the question: How do we acquire our native language? The answer to this may be the most revolutionary aspect of modern linguistics. There are at least two models that have always represented the two archetypes of language acquisition and more generally of any process of learning. One is the "tabula rasa" model, according to which every language is gradually built in the speaker's mind on a foundation of attempts regulated by experience. The other model we can call "tabula inscripta," for symmetry, and according to it, language develops in a child as a biologically determined project. The theory of universal grammar, which is based on the principles and parameters model, is an innovation that in some ways reconciles these two extremes.

The "tabula inscripta" model should be rejected right away. It is well known that the development of a specific language does not result from a project that is completely determined by biology. If two adults who speak the same language give birth to a child who ends up being raised in an environment where a different language is spoken, the child will acquire the second language as easily as a child whose biological parents speak that language. The language of the biological parents will not emerge, and it will not interfere with the language the child is exposed to nor with the language of the child's descendents.

The "tabula rasa" model also does not describe the existing data. If the child really were a linguistic tabula rasa, there would be at least three important and major consequences. First, the number of possible syntaxes should be infinite, since there would not be any predetermined limit to the syntactic structure of a language. Second, children lack the guidance coming from the parameters and thus would have to deal with a level of complexity that is practically inaccessible in such a short time and without explicit instruction. Third, we would also expect children to make mistakes that they never make in real life. This experimental side of the research—the evaluation of possible mistakes—now constitutes an autonomous scientific domain that is giving great results to the research and corroborates theoretical proposals in a substantial way.[76] Yang (2003) suggests a fascinating hypothesis that brings the impact of parameter theory to its maximal limit with respect to error evaluation. According to Yang *the mistakes that a child can make during language acquisition are all forms that are possible in other languages.* If this were to be confirmed by data, the notion of mistakes in grammar—except for lapses or slips of the tongue—would disappear or would have to be completely rethought. In fact, it is very likely that the study of language pathologies and cognitive neurosciences in general will provide data that support the principles and parameters model. We will return to this in the next chapter.

Thus, neither the tabula rasa nor the tabula inscripta model can describe the class of possible human languages and language acquisition. Which model, then, should we adopt for language acquisition? The most likely hypothesis of human language acquisition, one that conforms to the principles and parameters theory, is that it is part of the human biological endowment (thus the new hypothesis shares some features of the tabula inscripta hypothesis) but that it also has degrees of freedom that must be fixed on the basis of experience (thus the new hypothesis shares some features of the tabula rasa hypothesis). *Learning means using experience to fix the structural aspects that are left free by the biologically determined schema.*

Though it may sound paradoxical, the principles and parameters model can correctly be seen as a model of *the limits of experience* and therefore as a model of the possible influence of imitation on language acquisition (even though the hypothesis contains elements that rest on the idea of innate traits). This model tells us how much experience can influence structure, which is not news to anyone who knows a bit about biology. The genome fixes the degrees of freedom of the organisms within which

76. See Dupoux and Mehler (1990), Crain and Thornton (1998), Crain and Lillo-Martin (1999), and Yang (2003). For a general overview of language acquisition work, see Guasti (2002).

experience and environment can play a role. For instance, both a sunflower and a baobab seedling need watering, but they become two different kinds of plant. The difference does not depend on the kind or quantity of water: the sunflower seed and baobab seed contain the limits of the degrees of freedom that will constrain the plants that will develop from them. Even though environmental conditions such as light and wind can and do influence the final shapes of plants, nobody would expect that anything but a baobab would develop from a baobab seed. In the same way, a child needs a certain nourishment—the experience of listening to people speaking a language—in order for the language to develop. This experience-based association is responsible for dramatic differences in the signs of a language (in the Saussurean sense), especially with respect to the signifiers (assuming that the repertoire of signified is not unbounded). But syntax can only vary within certain limits, and these are established by our species' biologically determined schema. *If you talk to a dog a lot, the dog will not speak English, in the same way that watering a sunflower seed a lot will not get you a baobab.*

Jacques Mehler (1974) suggested an even more radical view of language acquisition on the basis of a preformed schema: language acquisition should be conceived as *forgetting* what is not needed in terms of our experiences. From this point of view, syntax is learned via a selective process, not by accretion, that is, by building, step by step, the complex net that in the end produces the Rubik's Cube of words. What does the child have to forget? In the principles and parameters model, acquiring the syntax of a language means that a child has to eliminate—"forget"—the parameter values that are not compatible with the surrounding linguistic environment.

To understand how this model works, we can go back to the comparison of syntactic structures in English and Japanese. Regarding the head-complement parameter, for a child acquiring Japanese it would be enough to eliminate the option in which the head precedes the complement and extend that kind of linear order to all phrases. In principle this could occur after just one exposure to the relevant option. A child acquiring English will eliminate the opposite option. A child who hears the prepositional phrase *after the dinner* will know that in the phrases of this language, the head precedes the complement and will therefore eliminate the other option. But a child who hears the prepositional phrase *yuusyoku go* ("dinner after") will know that the head follows the complement in all the phrases of that language. According to this theory, just a few stimuli are enough for a child to automatically infer the parameter setting of the language he or she has been exposed to and at the same time to forget the other available parameter values. This is radically different from the hypothesis that grammar is built little by little by increasing the level of complexity. It is as if the brain did not build anything but just eliminates useless structures. Metaphorically, *according to the acquisition by selection model, the brain is a trashcan, though perhaps the best of all possible trashcans.*

Learning by forgetting applies not only to syntax only but to all the other domains of language—which corroborates this theory. Take phonology. An adult native speaker of Chinese does not recognize the difference between the phones [l] and [r]. It is not that the Chinese adult literally does not hear the difference. He or she hears it for sure, since the two physical stimuli are different. But because the two sounds are not phonemes in Chinese, the adult speaker can no longer give them the psychological weight that they have in English. That is why adult Chinese have a hard time distinguishing between *load* and *road*. Similarly, an adult native speaker of Italian will have problems with English vowels. To the Italian speaker, *ship* and *sheep* sound the same, since Italian has the vowel [i] as in *sheep* but not the "short" vowel sound [ɪ] as in *ship*. The adult native speaker of English has trouble distinguishing between single and double consonants (*geminate* consonants), which are not found in English, but are found in Italian and some other languages. The words *papa* (pope) and *pappa* (food) are two different and easily distinguishable words in Italian but sound the same to an English speaker.

Let's summarize this model of language acquisition. Acquiring the various components of a language means forgetting the values that are not compatible with experience, within the limits of variation that are imposed by a biologically determined guide. This is a modified tabula inscripta model, in which the system allows for degrees of freedom. *This model differs from both a tabula rasa model and a pure tabula inscripta model, which has no degrees of freedom. We can dub this model "tabula praeparata"* (from the Latin *prae-parare*, to set in advance).

1.3.3 Simple Languages and Complex Languages: The Genesis of Creoles

One of the implications of the hypothesis that there is no biologically determined guidance in language acquisition is that different languages could vary in complexity. In a strictly biological analogy, if someone proposed that the liver or the pancreas are cultural products, we could expect to have populations with organs of different complexity. This hypothesis is of course ridiculous, because we take for granted that although organs may vary slightly according to the environment where the people live, they cannot vary so dramatically that there are substantial differences in complexity. *Similarly, if we assume biologically determined guidance, we need to assume that languages do not vary in complexity.* We need to be careful, however, or we could be misled. There are two kinds of considerations that corroborate the hypothesis that languages do not vary in complexity: one is comparative and the other is based on the acquisition of artificial languages. Let's take a look at each of them.

It is commonly thought that some languages are "difficult" and others are "easy." Indeed, Spanish may look easy for an Italian, but what would a native Japanese speaker think? Moreover, certain aspects of the grammar can be easier in some languages than others. This type of "easiness" does not say anything about the "global complexity" of a grammar, and the hypothesis we present here is that languages do not differ in *global* complexity, despite the complexity in the details of how the language is expressed. In fact, there are at least two empirically and conceptually distinct empirical lines: on the one hand, one can compare different existing languages and try to see if there is any significative difference in complexity among them; on the other, one can try to see if the evolution of a single language (or a set of languages) produces any sensible difference in complexity.

Actually, nobody has yet found a scientifically acceptable measure of natural language complexity (both in the sense of human language as opposed to other languages and one specific language as opposed to another). Languages that seem easy in some aspects are more difficult in others. For example, verbal morphology (the forms that verbs can take) is certainly easier in English than it is in Italian. Aside from a few irregular verbs, English verbal morphology is minimal, making a change in the spelling of a verb in the third-person singular present, the past tense, in passive and the progressive forms. The infinitive *to paint* turns into *paint-s*, *paint-ed* and *paint-ing*. Italian, by contrast, has an extremely rich verbal morphology that distinguishes person, number, tense, mood, and so on. The verb *cantare,* to sing, turns into *cant-o* for first-person singular present indicative, "I sing," *cant-i* for second-person singular present indicative, "you sing," *cant-ai* for first-person singular past indicative, "I sang," *cant-erei* for first person singular present conditional, "I would sing," and so on. In some respects, however, the syntax of English is more complex than Italian. For instance, the subject *John* and the verb *arrives* can be combined only into the string *John arrives* and not **Arrives John*. In Italian, both *Gianni arriva* and *arriva Gianni* are correct. If we assume that syntax is a set of filters that eliminate impossible combinations, we have to conclude that English has one more filter in its syntax according to which the subject cannot follow the verb in declarative sentences, thus the syntax is more complex in this respect. It is important to repeat the idea that until we have an explicit matrix to measure the complexity of different languages, we will have to settle for limited evaluations such as the one above. The most plausible conclusion, however, is still that languages do not differ in *global* complexity.

Although we lack both a matrix and direct experimental tests for language complexity, Derek Bickerton's work (1983, 1984) on so-called "artificial" languages has opened an interesting new perspective for dealing with this problem. As a first approximation, by "artificial language" I mean those human languages that are invented under particular historical conditions, such as those regions where the contact communities speaking different languages forced the invention of new words and rules to allow communication in an easy way.

Bickerton's work may provide the only current evidence supporting the idea that languages naturally tend toward the same amount of complexity. The central and radically new aspect of Bickerton's research was that he studied language acquisition in children who were exposed to "pidgin"; a pidgin is a simplified language that developed for reasons of commerce. In fact, rather than a single stable language, a pidgin is a special environment where many languages are confused and reassembled in a simplified form which may differ from speaker to speaker in a significant way. In this sense, it would perhaps be better to speak of "pidgin environments" rather than "pidgin languages." The alleged etymology of *pidgin* from the distortion in English of the word *Beijing* indicates that the first language so labeled developed from the contact between English merchants moving to China and local Chinese populations. Later, *pidgin* came to refer to all simplified languages, and languages that develop in this manner, for a restricted purpose, are, in fact, simplified. Tense is formed by combining an adverb with an uninflected verb, so that the equivalent of *went* would be something like *to go yesterday*; helping verbs are omitted or simplified; and so on. In addition, Bickerton (1983) states, "One of the main characteristics of pidgin ... is its variability from speaker to speaker. Each immigrant seems to have gone about the task of inventing a makeshift language in some individual way" (117). Babel to the max: as many languages as there are speakers!

Bickerton had the original idea of observing what happens if children are exposed to a pidgin environment at the time when they would normally be exposed to a complete native language. Situations like this are extremely rare; they occur only when children are raised by someone who speaks only pidgin to them.

In Hawaii, Bickerton found one such rare and scientifically favorable situation. He examined what had happened to the simplified grammar in adults who had learned such a grammar as their first language. Despite the limit of a strong idealization of the data, the results were surprising. Bickerton found that the first generation of children of this group of people who spoke pidgin "recomplicated" the language by introducing all those components that the simplified language did not have and in some cases by introducing new ones. This was the birth of a Creole language. The adjective *Creole*, which historically was assigned to those who were born in Latin America from French, Spanish, or Portuguese parents (*Creole* comes from the Spanish *criollo,* which means racially mixed, locally born), refers nowadays to languages that developed from commercial contacts. Their lexicons draw on more than one language, but the languages themselves do *not* exhibit the simplified characteristics of the pidgins. Instead they embody the same richness and stability of any other natural language with respect to all components of grammar, in particular, the morphological and syntactic structure. Let's look at a simple example of the structure of Creoles by focusing on some verbal properties (see Bickerton 1984).

Let's first consider this issue from an abstract point of view. There are many ways that verbs can contribute to the meaning of a sentence, aside from the proper meaning expressed by the root of the verb. Let's examine four properties of differing complexities by looking at action verbs. First, the verb can refer to an action in the past (anteriority), as in *paint-ed.* Second, the verb can refer to actions that have not been realized (unreality), as in the sentence *If I had time, I would paint,* in which the verb *paint* with the helping verb *would* refers to a painting action that has not occurred. Third, the verb can refer to an action that is completed or that is still going on (continuity). In English, the *-ing* form at the end of a verb and the helping verb *to be* express an ongoing action, *I was painting,* whereas the simple past usually refers to a completed action, *I painted.* Finally, verbs that refer to events that have a natural culmination can convey whether the culmination point has been reached (telicity, from the Greek *télos,* meaning "final purpose or conclusion").[77] In English there is no

77. I am using this term to refer to the property of an event that can be expressed with *any* verb. This deviates from the traditional grammatical use whereby about certain verbs (or constructs) are telic.

way for the verb to convey this, not even by means of a helping verb. It must be said explicitly. *Paolo went to Rome to marry Francesca* does not say whether the wedding occurred or not. In order to know that it did, something else must be added: *Paolo went to Rome to marry Francesca and he made it (*or *he didn't make it*). In the simplified grammars of pidgins, genereally none of the properties of anteriority, unreality, continuity, and telicity are codified. Creoles are surprisingly different from pidgins in this respect.

When Bickerton looked at these properties in Hawaiian Creole, he found that children who were speaking it had introduced all these properties into the grammar, going even beyond the languages from which the pidgin developed. For instance, to say that a person went to see another person, the children were using two different forms: *go see* if the meeting actually took place, and *for see* if the meeting had not happened or if the speaker didn't know. Don't be misled by the fact that the words in these strings look like English—the children used *for* and *go* just to introduce the verb. In sum, a property that could not be directly expressed on the verb in one of the languages from which the Hawaiian pidgin had developed—English—had been grammaticalized in the Creole spontaneously and without instruction. But the surprises don't stop here. Each of the first three properties—anteriority, unreality and continuity—has been grammaticalized in Hawaiian Creole. In order to express these properties, children put a different word before the verb: *bin* for anteriority, *go* for unreality, and *stay* for continuity. Let's look at some examples. The equivalent of the sentence *he walks* in Hawaiian Creole is *he walk*, which is very similar to English except for the lack of the third-person singular morpheme -*s*. The equivalent of he walked is *he bin walk*; *he would walk* is *he go walk*; and *he is walking* is *he stay walk*. These three properties can also combine with each other. For example, *he been go walk* expresses both anteriority and unreality, as *he would have walked* does in English. *He been stay walking* expresses both anteriority and continuity as *he was walking* does in English. Finally, *he go stay walk* expresses both unreality and continuity as *he would be walking* does in English. These three properties of anteriority, unreality, and continuity can also all be combined in English, as in *He would have been walking*. What would the order of the three helping verbs be in Hawaiian Creole? The examples show that *bin* precedes both *go* and *stay* and *go* precedes *stay*, therefore, the only coherent order is *he bin go stay walk.*

This result alone is interesting, but Bickerton's research generated another crucial and related result. He later studied Creoles in other parts of the world that had originated from pidgins that developed from different languages than the languages Hawaiian pidgin developed from: for example, Haitian Creole and the Creole spoken in Surinam (formerly Dutch Guyana).[78] He found out, not only, that these other Creoles had words that were equivalent to *bin, go,* and *stay,* but also that the relative order of the three words that expressed anteriority, unreality, and continuity was exactly the same. This revealed that they had a common structure. There was nothing "logical" or necessary in that specific linear order of the words that are associated with these features: anteriority, unreality, and continuity could very well be combined in a different linear order. If the fact that the order does not vary within the Creoles cannot be reduced to some property of reality or logic, then the most plausible hypothesis is that it is in some way the result of predetermined structural properties and not experience.

The results speak for themselves. *It would be very difficult to explain the process of "recomplication" from a pidgin to a Creole and the structural homogeneity of the grammars of Creoles that developed from different pidgins if there were not a biologically determined predisposition that shapes the structure of a language, and of syntax in particular.* This is why Bickerton talks about the "language bioprogram hypothesis," which substantially corroborates the hypothesis that Chomsky formulated on a comparative basis, according to which the structure of languages does not vary randomly but follows a unitary biological mechanism.

It would be incorrect, however, to think that the results of Bickerton's research only give further support to this hypothesis. As a matter of fact, Bickerton (1983, 191) proposes a vision that is compatible with the principles and parameters model—that is the model we referred to in the previous sections of this chapter which goes back to Chomsky 1981 first formulation—but does not overlap completely:

> The universal grammar conjectured by Chomsky is a computing device, somehow realized neurologically, that makes a wide range of grammatical models available to the child. According to Chomsky, the child must then 'select' which of the available grammatical models matches the grammar of the language into which the child is born. The evidence from [C]reole languages suggests that first-language acquisition is mediated by an innate device of a rather different kind. Instead of making a range of grammatical models available, the device provides the child with a single and fairly specific grammatical model. It was only in pidgin-

78. Creolization of a pidgin language may also affect sign language—not surprisingly, since sign language is just one of the possible human languages: interesting and original data are illustrated by Ann Senghas (see, for example, Senghas and Coppola 2001).

speaking communities, where there was no grammatical model that could compete with the child's innate grammar, that the innate grammatical model was not eventually suppressed. The innate grammar was then clothed in whatever vocabulary was locally available and gave rise to the [C]reole language heard today.

The hypothesis of the genesis of Creoles is not, however, *in opposition* to Chomsky's hypothesis. If anything, it makes it more precise. It supports the validity of the hypothesis that among the attested languages, there are no differences in complexity, even if there is a specific syntax that, in specific and rare conditions, seems to be the most immediate expression of the biologically determined project of human language. On the one hand, if language were a convention one would expect people over the centuries to design languages that got simpler and simpler, to facilitate learning and communication. But if we assume that language springs from a predetermined biological matrix, asking whether there are languages that are simpler than others is like asking whether there are populations whose members have a simpler liver or pancreas—an obviously nonsensical question: a "simpler" liver either is not a liver or does not exist.

Let's conclude this chapter with a naturalistic example that linguists mention sometimes to show how biologically determined language acquisition models similar to the one that we have described are present in nature in species other than humans. Although the degree of complexity is incomparably smaller, the similarity is so surprising that it is worth mentioning. It is the famous case of birdsong acquisition in some varieties of birds such as American cowbirds (*Molothrus ater*; see Goldstein, King, and West 2003). In that species, there are different "dialects" (different melodies) according to the regions where the bird populations are found. Researchers studied the learning mechanism of birdsong in a specific group, and the results are amazing.

The first piece of data: only male birds sing. Where do they learn their "dialect"? From the females of the same species—more precisely, from their mothers, but indirectly. The little bird starts by producing a range of apparently unstructured sounds, analogous to the repetitive babbling of human babies. It is only when the little bird "tunes" itself to the singing of the males of the group does the mother—who of course recognizes the songs of the males—give "positive" feedback to the young bird. In this way, the little bird "fixes" the variety of his group among all the possible varieties and "forgets" the others.

The second piece of data: if the little bird is moved to a different group, it can learn another dialect in the same "spontaneous" way, but this has to happen within a certain window of time (about seven months). Once the window closes, the brain of the little bird loses plasticity and is no longer able to retrieve any language or dialect. This is a strike against supporters of the idea that language in nature is learned only by teaching or by imitation. But in any case the comparison of humans with birds is just suggestive. It would be as if we would expect children to be able to acquire a language from mute mothers. But this is just an analogy, and human language is much more complex. Nevertheless, animal behavior may still give us data that corroborate theories about human beings.[79]

We are finally at the end of the first leg of our journey.[80] We have learned enough to negotiate the second leg of our journey—the story of the encounter between two different intellectual cultures: the neurosciences and theoretical linguistics. How will this encounter go? Will it produce results? If this were a mystery novel, the author would have to

79. Nature seems to rely on the selective model for more than just language. Niel Jerne, the winner in 1984 of the Nobel Prize for Medicine or Physiology, titled his *lectio magistralis* before the king of Sweden "The Generative Grammar of the Immune System" (Jerne 1985). The central idea—which was later substantially modified—was that the immune system does not produce antibodies for each antigen at the moment they are needed, but it is endowed with an abundant number of antibodies, only some of which will be used, as necessary over the course of life. As in Massimo Piattelli-Palmarini's (1989) effective imagery, it is a somewhat like the difference between having a tailor make you a customized suit or buying a suit at a department store. Nature seems to work in the latter way: it saves by offering a large selection of general merchandise. Similarly, it is plausible to think that in a child's mind, there is space for billions of possible syntaxes and that only those that fit with external inputs will be selected.

80. The model that I have presented here is part of the contemporary generative grammar standard model. This model is not static or monolithic, nor does it lack critiques or suggestions for change. Actually, as shown by Chomsky's recent work and some other work in generative grammar, the model is being discussed all the time. If this were not the case, the model might desiccate and be abandoned. What happened to the so-called "neogrammarians" who supported a view of linguistics that dominated the nineteenth Century should not happen here—but optimism is not always justified. See Anna Morpurgo Davies's (1996) enlightening words: "Neo-grammarians' success partially depends on the fact that they could easily teach their students how to apply the model. . . . The dislike towards them depended in part on the fact that a good number of their followers applied the model mechanically without too much effort dedicated to question its foundations" (346).

face a classical strategic choice: whether to reveal the culprit from the very beginning and then show how the investigator solves the mystery by means of a series of clues or to follow the opposite strategy and leave the reader in the dark regarding the identity of the culprit and just allow the reader to follow the investigation step by step. I chose the first option, not without risk: I will reveal the two key questions the two experiments that I will present raise and the answer that we came to right away. Curiosity regarding the method, if not the motive, should provide the suspense.

The first question to be answered: Does syntax, which linguists assume to be autonomous with respect to other components of grammar, correlate with autonomous dedicated neuronal activity? Or is this hypothesis of the autonomy of syntax as elaborated by theoretical linguistics not at all compatible with what we know about brain structure?

The second question to be answered: Are the limits that characterize the class of possible human languages—for instance, the fact that all rules are based on hierarchy rather linear order—only epiphenomena in some sense, or, even worse, just historical accidents or social conventions? Or does this difference correspond to a precise neuronal reaction?

Of course, there are no a priori answers to these questions. *There is no logical reason why theoretical linguistics should have developed a model that is isomorphic to the actual functioning of the brain.* After all, the empirical data of the two fields are completely different: one is based on the comparison of linguistic regularities, the other on biological data. But we will see, surprisingly enough, that there is a significant convergence: not only does syntax activate specific neuronal nets that are different from those activated by other components of grammar, but conversely the limit of variation among possible grammars is conditioned by the neurofunctional architecture of the human brain.[81] This not only increases our knowledge of the empirical data on the nature of language, but also has a non-negligible epistemological impact because it shows a substantial

81. The terms *neuronal nets* and *neural nets* are both found in scientific texts. I chose to refer to *neuronal nets* in this book to stress that I am not talking about *simulation* but *description* of the function of biological structures. *Neural net* is mainly associated with the theory of mathematical models (related to the so-called theory of "connectionism," a pillar of artificial intelligence) that try to simulate the human central nervous system by building artificial nets, which are often labeled ANN, for "artificial neural networks" (see the criticism of this program in the classic work by Minsky and Papert 1969, and the more recent Chester 1993).

convergence between two theories that are based on totally different empirical data. And, as we said earlier, this convergence cannot be derived a priori unless we make an ad hoc assumption.

In the next chapter we will focus on these questions by presenting two relevant neuroimaging experiments. Once again, before being able to make a direct comparison, we need to acquire some basic knowledge that we need to understand how the two cultures talk to each other.

2 Language in the Brain

Who designed the boundaries of Babel, that is, the limits that are imposed on the format of human languages? In the previous chapter, we reached the conclusion that the syntax of human languages is based on the hierarchical organization of words (structure dependence), despite the fact that the physical form of the linguistic signal is linear—words are arranged in a sequential order. Why are there no languages that have rules based on linear order? Is this a cultural, conventional, or historical fact? Has someone eliminated these impossible languages or did they never exist? Is this an accidental fact that could very well change in the next centuries or millennia? Or is it rather a structural effect that depends on the functional architecture of the brain? We now have the opportunity to approach this enigma from a new perspective.

Only fifty years ago, in a book that has become a classic in the neurosciences, Eric Lenneberg (1967, 2) felt the need to write (not without a polemic slant),

> A biological investigation into language must seem paradoxical as it is so widely assumed that languages consist of arbitrary, cultural conventions. Wittgenstein and his followers speak of the *word game*, thus likening languages to the arbitrary set of rules encountered in parlor games and sports. It is acceptable usage to speak of the psychology of bridge or poker, but a treatise on the biological foundation of contract bridge would not seem to be an interesting topic. The rules of natural languages do bear some superficial resemblance to the rules of a game, but I hope to make it obvious in the following chapters that there are major and fundamental differences between rules of languages and rules of games. The former are biologically determined; the latter are arbitrary."

In the fifty years since 1957, research into human language has developed beyond all expectations, up to the point that Lenneberg's passage now sounds anachronistic to most people.

Two different research areas have turned the situation upside down since Lenneberg's book was written: theoretical linguistics, especially generative grammar, and the neurosciences. In this chapter, I will show—based on two neuroimaging experiments that I took part in—how these two areas not only *can* talk to each other but *must*, in order for the research to proceed.

Fifty years of work in generative linguistics have shown the implausibility of the hypothesis that language structure is a cultural fact, totally modeled on the basis of conventional decisions, and thus have made Lenneberg's defense look anachronistic. The most likely current hypothesis regarding the relationship of human language and brain structure is that the limits of the syntaxes of human languages are due to a biological matrix. As we saw, linguists based this conclusion on two different types of data. One type is largely comparative data; these studies highlight structural identities in languages, including Creoles, that are very far from each other geographically or diachronically. The other source of relevant data is the observation of children acquiring their native language and was present as early as the first writings by Noam Chomsky (cf. Chomsky 1959). Although they are exposed to fragmented data, within roughly the first four years of life children converge toward the syntax of their native language without going through a path of random trial and error, as would be likely if there was no biologically determined guidance. No child, for instance, randomly switches the order of the specifier and the complement in phrases. Even if we rely only on our personal experiences, not scientific observations, we will not find among the many mistakes that children make utterances such as *house the* instead of *the house*, or *table on* instead of *on the table*.[1] If anything, children tend to make morphological mistakes, saying, for example *foot* and *foots* on the basis of the analogy between *hand* and *hands*.

How can we answer this fundamental question on the boundaries that are imposed on the linguistic Babel? If you look for evidence that indicates determined biological guidance, in the best of possible worlds this hypothesis will be corroborated by biological data.[2] Therefore, I will try

1. Aphasic patients don't make these kinds of mistakes either (Stefano Cappa, personal communication). This suggests that at least this syntactic component is much more resistant than others, for example, the morphological component, which is damaged in "agrammatic" patients (but see Cotelli et al. 2007 and, for a critical comment on language acquisition and aphasiology, Caramazza 1994).

2. In principle, however, biological data are not needed in order to prove that language has a biological matrix. Linguistic data could very well be enough: evidence from errors, from language acquisition in children and adults, from pathologies, or simply from a nonrandom absence of structures. On the other hand, of course, direct data are straightforward evidence.

to show how we can find interesting biological data by means of neuroimaging techniques. Of course, we will not be able to find *all* the biological constraints on grammars, nor will we be able to conclude that we have discovered the details of the biological matrix that guides children's acquisition of their native language. However, *we will be able to find direct clues that show that the boundaries of the linguistic Babel are directly related to the functional architecture of the (adult) brain.* At this point, the hypothesis that language is just a cultural and historical product, or a tacit social "contract," will be much more difficult to accept.

Is there direct proof for the existence of biologically determined guidance? What we are really asking is: What other kinds of data, aside from what linguists have uncovered, could shed light on the biologically determined limits of the syntaxes of human languages? I will certainly not give a categorical answer. The term *biological* constitutes both the core and the limit of this question. This term signals that we are moving from one level of analysis to a very different one, at least from an empirical point of view: from reflections on syntax to reflections on the brain. How legitimate or even possible is this move? The question is certainly not new. Just think about the previously mentioned classic work by Eric Lenneberg (1967), where a large volume of clinical neuroanatomic and anthropological data was presented in support of this hypothesis (for a more recent survey, see Caplan 1992). In this chapter I will focus on just one aspect of this research area: how the new neuroimaging techniques corroborate the hypothesis of a biologically determined guide of syntax in a new and unexpected way. More specifically, I will try to show how *the neurofunctional structure of the adult human brain is sensitive to the differences between the rules that follow the universal principles of syntax—in particular, hierarchical structure—and rules that violate them.* This will provide data in favor of the hypothesis that human language is constrained by the functional architecture of the brain.

Let's not get optimistic too early. We certainly know a lot about the brain's neural anatomy and cytoarchitecture—the kinds of cells that form it—but we know much less about its physiology and, more generally, about the brain's functioning and even its anatomy, compared to what we know about other organs.[3] For obvious reasons of both complexity (a brain is much more complicated than a pancreas or an ear)

3. To learn more about hodology, the science of connectional anatomy, recently revitalized by developments based on diffusion-based imaging and computational analysis of effective connectivity, see Catani (2007).

and ethics, we know even less about cognitive faculties, especially language. Experimentation on the human brain has not profited particularly from experimentation on similar organs of animals of other species. Ignoring all the legitimate ethical questions regarding experimentation on animals, it is nevertheless a fact that we know much more about the physiology of the eye, just to mention one example, than we do about the physiology of the human brain, because experimentation has been done on animals whose eyes are very similar to human eyes (see, for example, Hubel and Wiesel 1962 and Blakemore and Melvill Jones 1977 about the cat's visual system and Marr 1982 for a seminal treatise on vision). We have seen that no animal linguistic code can be compared to that of humans. All animals communicate, of course, but none use syntax as humans do, and none seem to have the same syntactic richness that we were introduced to in the chapter 1: the properties of linearity, discreteness, recursion, dependence and locality—in short, structure dependence. When it comes to studying brain structures as related to syntax, brain experimentation cannot go through the short cut of the comparison between species.

Throughout the nineteenth Century, knowledge of how the brain functions developed from clinical evidence. It was virtually always necessary to wait for a "breakdown"—a trauma, an ischemia, a tumor, a birth defect or any symptomatic damage—before one could do any investigation with a view toward deducing anything about the human brain function. Usually a relation would be established between a symptom and the location of a brain lesion that was observed during macroscopic and microscopic investigation during the autopsy. In healthy brains, the only way to gain access to brain function in vivo was by measuring the reaction times to stimuli and, starting in the 1930s, to the rudimentary measures of the electric brain activity of the encephalon by means of the electroencephalogram.[4]

The situation was similar, in a way, to deciphering the internal structure of a star: How can we know what is inside the sun without being able to manipulate or directly test its mechanisms and consistency? The only way is to interpret the signs that arrive from its interior. At the present time we can't establish a lab or send a probe *into* the sun. For a long time, we were in the same situation with the brain: our human "star" did not allow for direct investigations in vivo.

4. For a history of the origins of the neurosciences, see the classic Finger (1994) and the introductory chapter in Rapp (2001).

Things changed radically in the twentieth century. New biochemical, genetic, and radiological investigation techniques have led us to new methodological strategies that are still developing. Actually, it is not unreasonable to say that we are just at the beginning of this search.[5] Researchers in the field have started talking about neurotransmitters such as dopamine and serotonin (endogenous molecules that mediate neuronal activity), neuronal nets, genetic studies, brain embryogenesis, genes that regulate the cortex, such as gene Emx2, discovered by Edoardo Boncinelli (1999b), the modular structure of superior functions, and, more recently, thanks to an influential discovery by Giacomo Rizzolatti and his group, the existence of "mirror neurons" (see Gallese et al. 1996, and for an extensive critical review, Rizzolatti, Fogassi, and Gallese 2002).[6] New and important discoveries may come from the use of electrophysiological techniques such as evoked potentials, which give a detailed map of the electric activities of the cortex, or transcranial magnetic stimulation (TMS), in which the cortical activity is altered by the modulation of a strong local magnetic field through the bones of the skull, or functional surgery—which because of its invasive nature is reserved for cases in which the subject is in such a pathological condition that surgery is the only option—where the same alteration effect is produced by placing the electrode directly on the portion of the cerebral cortex of an awake patient whose skull has been partially removed. (For a simplified description of these techniques, see Calvin and Ojemann 1994.) Finally, in vivo brain investigation makes use of a combination of neuroradiology and computer science in order to produce investigative techniques such as functional magnetic resonance imaging (fMRI) or positron emission

5. This section is based on Cappa (2004), Cappa (in press), Friston (1997), and Kaan and Swaab (2002); see also Opitz and Friederici (2004). For a general overview, see Denes and Pizzamiglio (1999) and Kandel, Schwartz, and Jessel (2000).

6. Mirror neurons have been isolated in monkeys, and it has been suggested that the same system exists in humans (see Tettamanti et al. 2005b): to simplify, when individual X sees individual Y perform an action, in some cases, X's brain activates the same net of neurons that Y's brain activates in performing that action. This is why they are called "mirror neurons." In principle, it is not unreasonable to assume that their role in animal communication and in learning is fundamental. As far as human language is concerned, however, it seems to me that as far as syntax is concerned, it is quite dubious that mirror neurons may play a role in language acquisition, given the fact that the hierarchical structure that is at the base of all grammatical relations is flattened into a linear code at the physical level (we will come back to this in the last chapter).

tomography (PET). How and when all these pieces of information will be integrated into an exhaustive theory of human brain has yet to come.

Let's stop here. The reader should not be frustrated or confused by this roundup. As I said at the very beginning, I am not even dreaming about writing a textbook of general linguistics or a textbook on neurosciences. The point is the following: we have reached the real raison d'etre of this book, we have arrived at the meeting point of the two cultures—the cognitive neurosciences, in particular neuroimaging techniques, and theoretical linguistics—and we have reached the fundamental question at the center of our journey: What do these two disciplines have to say to each other about language? The obvious limit of this book is the author, who is telling you only one side of the story of this meeting: all that I will tell you is what a linguist has learned by interacting with neuroscientists. A partial view is not negative, per se, as long as it is explicit and does not profess to tell the whole story. After all, curiosity and amazement are the real engines in any scientific experience, and what you will find in this story is a linguist's amazement and curiosity. The reader will be left with just the task of learning the neuroscientist's point of view. But it could end up being that the jobs of neuroscientists and linguists will become outdated and a brand-new job will emerge.

Studies on language pathology and the function of the brain for linguistic production are certainly numerous and have a very long tradition. If you are looking for a date when study of the study began, many would point to 1861, when the French neurologist and anthropologist Pierre-Paul Broca—whom we met in the prologue—described a patient with severe linguistic deficits, which he named aphemia. For most of his life the patient could only answer questions with the syllables *tan-tan*. Broca realized that this patient did not present deficits of muscular control of the tongue and face. Also, based on the anamnesis, the patient had kept a normal level of intelligence and had been able to perform normal cognitive tasks, except for the last few years of his life. So, his only symptom was linguistic in nature and was unrelated to other cognitive functions. This patient, who became known to history as "Tan-Tan" (or "Tan"), was examined by Broca after his death (Broca 1861). The autopsy revealed damage to the third circumvolution of the cerebral cortex of the inferior frontal gyrus of the left hemisphere of the brain—called since then Broca's area (figure 2.1).

Broca did more than discover the functional and morphological asymmetry of the two hemispheres. He was also the first to verify the hypothe-

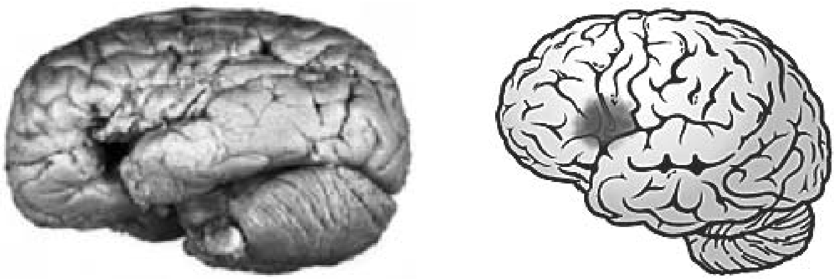


Figure 2.1
(a) Tan-Tan's brain. (b) In this drawing of the human brain, Broca's area—a portion of the cerebral cortex corresponding to the left inferior frontal gyrus—is highlighted.

sis that a specific portion of the cerebral cortex, the inferior frontal gyrus of the left hemisphere, was the physical location of a specific cognitive function, language.

Later, in 1909, the German Korbinian Brodmann published a map of the brain that is often referred to as "Brodmann's numeration." It catalogued about fifty different areas according to anatomical observations concerning histological tissues (the cytoarchitecture) of the cortex (Brodmann 1909). Brodmann, in this map, called Broca's area 44–45; area 44 is the so-called "opercular section" and area 45, the "triangular section" of Broca's area (see figure 2.2; the part corresponding to Broca's area is circled).

Other maps of the brain were proposed, mainly from the German school, in which Karl Kleist was an important figure, and researchers tried to connect the various areas to the functions that were related on them (for a critical view of the histological individuation of the brain's areas, see Lennenberg 1967).

Nowadays we do not think that things are that simple. There is no single "language area," and it is likely that there is no single area specifically dedicated to any one function. The brain activates complex nets, and the "areas" are merely zones of preferential activation, but they are not exclusively responsible for a certain function. Brodmann's numeration, with its "neolocationist" character, is certainly a useful and practical reference, but in certain aspects it looks like the projection of our way of thinking on that mass of neurons that constitute the brain: "The amount of new data ... is growing dramatically, but it could easily reach the saturation level if we do not manage to find the key to 'reason' in the same

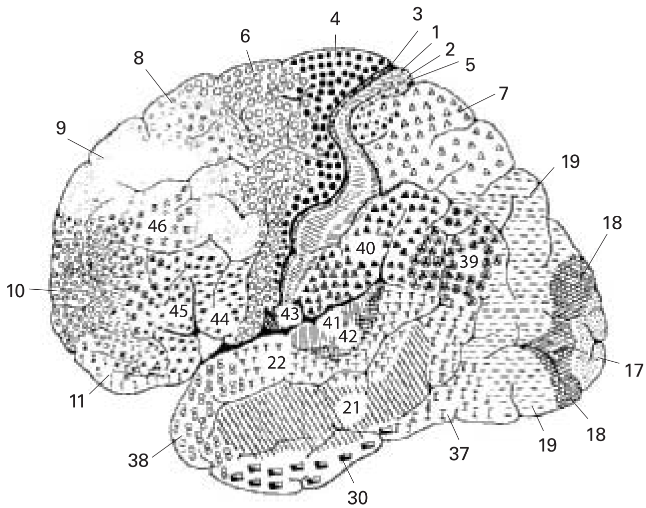


Figure 2.2
A representation of the cerebral area and the so-called Brodmann's numeration for the left hemisphere; Broca's area largely coincides with areas 44 and 45.

way as our brain actually reasons in planning and performing its tasks" (Boncinelli 1999a, 167). Work by the Prussian neurologist Carl Wernicke (1874) that was done about twenty years earlier than Broca's work showed that other cortical areas, including the posterior half of the superior temporal gyrus of the left hemisphere, were involved in language pathologies, so it was obvious that the situation was much more complex.

In the most advanced neuroscience centers of the early twenty-first century, researchers know that such a rigid locationist theory is implausible. It is known that there are various areas that, if damaged correlate with language deficits and that symptoms vary as well. Even the term *Broca's aphasia* is quite problematic. First, the pathology Tan-Tan suffered from nowadays would be classified as "anarthria" (see Lebrun 1982) and the damage associated with it also includes larger areas, including the anterior temporal cortex (Déjerine 1914). Even if the expression "Broca's aphasia" continues to be used, this label now indicates the loss of the capacity to produce fluent speech and the concurrent omission of bound or free grammatical morphemes such as prepositions, the copula, helping verbs, or verbal suffixes. Today, many neurologists consider the label

Broca's aphasia obsolete because it cannot convey the various shades that characterize the complex phenomenon of aphasia in general.[7,8]

But the central question we are asking is another: Does all we know as linguists about the way the syntaxes of natural languages work based on the comparative methodology (autonomy and structure dependence, essentially) constitute a completely separate model from the one that the neurosciences provide us with, or are there nontrivial contact points? In other words: *Are the empirical foundations of linguistics and language neurobiology "incommensurable" or can they be compared?*

Let's immediately get rid of a mirage: nobody yet knows how you go from the principles that allow us to form sentences to the neurons or the cortical areas that underlie this task. If we knew, then the two fields would already have been unified. This unification is not on the horizon and it is not even clear what it will mean and that it will ever occur.[9] Wondering whether the regularities discovered by linguists can be connected to cortical brain activity in a nontrivial way is, therefore, an obligatory preliminary step, but certainly not the final goal. This specific question does not resolve another question, which may be even more radical: Why are there *these* linguistic regularities and not others? We will be able to offer only some bold speculation in the last chapter.

Let's now start our brief journey through neuroimaging techniques and two experiments that make use of them.

7. See critical overviews from different perspectives in Caramazza (1994), Gainotti (1999), Basso and Cubelli (1999), and Kandel, Schwartz, and Jessel (2000); see also Caplan (1992), Grodzinsky (2000), Rapp (2001), Fava (2002) and Basso (2003).

8. Wernicke (1874) also has a pathology named after him, "Wernicke's aphasia." Patients who suffer from Wernicke's aphasia have comprehension difficulties and speak fluently but with frequent paraphasias (swapping words with similar meanings or other correlations), repetitions, and so on. Unlike the case with Broca's aphasia, there are no grammatical morpheme omissions. Jakobson (1956) proposed an interpretation for these two aphasias within de Saussure's conceptual frame. There is also the so-called "conduction aphasia," which occurs when the arcuate fasciculus—an anatomical structure that connects Broca's area with Wernicke's area—is damaged or cut. Unlike Broca's aphasia, patients with conduction aphasia can't repeat the words that are told to them. Keep in mind that the caveats we mentioned in the main test apply to all these classifications.

9. For a critical discussion of the questions of the unification between linguistics and other disciplines, see Chomsky (1988) and, for neurobiology in particular, see Poeppel (1996) and Dehaene (1997).

2.1 Seeing Thought

Since in vivo brain exploration started, around the beginning of the 1980s, we have no longer needed to wait for mental breakdowns or death from brain diseases to get data about brain function or to make use of autopsies or the neuroradiological localization of lesions. Thanks to the combination of neuroimaging techniques and computer science, we have come up with ways to observe some aspects of brain function on healthy live subjects. I will give a brief description of the two techniques that were used in the experiments that I will discuss: functional magnetic resonance imaging (fMRI) and positron emission tomography (PET).[10]

What exactly do these two techniques measure? They are very different methodologies, although computational analysis of the data plays an important role in both. Without going too deeply into the technical details, we will get an idea of the limits and advantages of these methodologies in order to understand how language experiments are conceived. As with any instrument, the structure of these diagnostic machines radically conditions the entire experimental procedure. We would not comprehend anything without understanding how these machines work.

To understand what PET and fMRI measure, we need to have some idea of some aspects of brain architecture. Gerald Edelman (2001), the winner in 1972 of the Nobel Prize for Medicine or Physiology, has said, "Perhaps the most important general observation that can be made about the brain is that its anatomy is the most important thing about it" (38). This claim may sound provocative or reductive; the consequences of the brain's anatomic architecture on its function are not yet completely clear and indeed continually surprise us. In fact, even the anatomical description of the brain is incomplete: for example, the detailed organization, the hodology, of white matter is virtually unknown, as mentioned earlier (Catani 2007). Nevertheless, let's move ahead using a schematic illustration of some major characteristics of the brain.

The brain is a very complex organ that is made of various anatomical parts that are interlaced in an entangled system. It is roughly divided into two hemispheres that are largely symmetrical on the macroanatomical scale and are connected with a horizontal lamina, or layer, of white matter called the corpus callosum. The surface of the brain is covered with

10. Murray E. Phelps provided the foundations for the PET methodology in 1975. Paul C. Lauterbur's studies led to fMRI in 1973, and for this he won the Nobel Prize for Medicine or Physiology in 2003.

gray matter, the cerebral cortex, which is made of variable layers of neurons with an average thickness of 4 millimeters. It has many folds. The deepest folds are called fissures and divide each of the hemispheres into lobes: frontal, parietal, temporal, occipital, limbic, insula. Folds that are not as deep divide the lobes into gyri. This folding system greatly increases the brain's cortical surface, by more than twice the surface area that it would have if it were not folded. Other mammals make use of this system as well, but in humans it reaches its peak in relation to the volume of the skull. As a proportion of body weight, the amount of gray matter in the human brain is larger than that of any other mammal. The white matter, in contrast, is made of bundles of nerve fibers that, starting from the neurons on the cortex, connect other areas of the cortex with each other, both within the same hemisphere and between hemispheres, via the corpus callosum. White matter also receives nerve impulses from and transmits nerve impulses to the spinal cord through a complex net of bundles of nerves that irradiate out to the whole organism.

The cerebral cortex is an intricate net of neurons that transmit electric impulses to each other through the white matter, thanks to a complex metabolic activity that involves glucose consumption. The cerebral cortex plays a fundamental role in cognitive processes, for ultimately all such processes depend on the electric activity of neurons. Nowadays, the idea that the brain's gray matter is an intricate net of neurons is taken for granted. But Boncinelli reminds us (Boncinelli 1999a; see also Levi-Montalcini 1987 for a comment on this issue) that

> . . . this claim—which is so obvious nowadays—was accepted by the scientific community only in the first half of [the twentieth] century. Even when it was completely accepted that the remaining part of the body was made of cells, the brain was assigned a special place and considered a continuous net of organic material that was not fragmented into simpler independent units. This theory was called the "Reticular Theory" of the nervous system and had famous supporters. . . . It was only around 1950, with the advent of the electronic microscope, that we obtained the definitive proof of the cellular nature of the nervous system and of the brain.

This was indeed a major over the understandings of ancient and medieval medicine. Although Hippocrates of Kos had already discounted the idea fifty years earlier, Aristotle, in the fourth century B.C., thought that the brain's function was to cool down the blood—an organic radiator—and that "psychological" functions depended on other organs. The term *melancholy* was once used to refer to depression and means "black bile," and memory was thought to reside in the heart, the expression *by heart* being linguistic fossil referring to this. This theory lasted a long time, which may have been due to reticence toward dissecting human corpses, despite the widespread use of this practice with animals.

Throughout the Middle Ages it was thought that cognitive faculties were regulated by the flowing of the "liquor" that runs in the brain's meninges and cerebral ventricles—a mixture of cephalorachidian liquid, a water solution containing glucose, oxygen and proteins. We now know that the ventricles are four cavities in the encephalic mass whose partial function is to protect the brain from the effects of rough movement such as shaking. In the medieval theory, however, ventricles were assumed to be able to regulate many cognitive, sensory, and motor functions. The first description of the ventricular system goes back to two doctors from Alexandria in the third century B.C., Herophilos and Erasistratus. There is an explicit trace of this theory in Augustine's authoritative writings (1982, book 7, chapter 18):

> The medical writers point out that there are three ventricles in the brain. One of these, which is in the front near the face, is the one from which all sensations come; the second, which is in the back of the brain near the neck, is the one from which all motion comes; the third, which is between the first two, is where the medical writers place the seat of memory.... The medical writers say that the existence of these ventricles has been proved by clear indications in cases in which these parts of the brain have been affected by some disease or pathological condition.

Interestingly, the assertion that there were just three ventricles was seen in the Middle Ages as functional and related to a tripartite distinction of the cognitive activities, which was also Aristotelian in nature: *phantasia*, that is the capacity to generate images by means of sensations (in ancient Greek *phainomai* means "I appear" and the same root can be seen in *phantom*); *ratio,* that is, the capacity to produce logical thought and judgments; and *memoria,* the memory. The theory of ventricles did not survive the rationalist and anthropocentric momentum that developed from the Renaissance. The idea that the cephalorachidian liquid functioned as a mediator between cognitive activities (*phantasia, ratio,* and *memoria*) could not be experimentally proved of course, and this forced Augustine (1982, book 7, chapter 18) to introduce another entity, the soul: "The soul, however, acts on these parts of the brain as on its own organs. It is not the same thing as they are; on the contrary, it vivifies and rules all parts, and through them it provides for the body and for this life in virtue of which man was made a living being."

The transition from the "theory of ventricles" to a "theory of the cortex" for psychological activities of the brain started with Andrea Vesalio in Padua in the sixteenth century. Vesalio explained the implausibility of the theory of ventricles by comparison, pointing out that since animals also have a sophisticated ventricular system, it could not be responsible for the intellectual superiority of humans. Vesalio stopped halfway, though, in that he only offered negative evidence toward the theory of ventricles. As for the nature of intellectual faculties in humans, he attributed them to the spirit rather than to the activity of the cortex. It was only with Thomas Willis's anatomical work, *Cerebri Anatome*, in the seventeenth century that the central role of the cortex in intellectual activities was first recognized. Willis also used the comparative method, but focused on the richness of gyri—the convolutions of the surface of the brain—which is an exclusive characteristic of the human brain. The psychological theory that Willis promulgated was not much different from Aristotelian dogma. Not until the nineteenth century, with Broca and the controversial experience of Gall's phrenology did science develop an experimental theory of intellectual activities that was based on the cortex.

Thanks to the techniques of microscopic investigation, modern biology has discovered even more surprising facts about brain structure than the simple observation of the cellular nature of the cortex. The human brain, which is on average just 2 percent by body weight, contains about 100 billion neurons. The number of connections that neurons establish with each other by means of synapses (Greek for "contacts" from *syn* "with" and *hapto* "touch") reaches around 1 million billions, an astonishing number that, together with the complexity of these nets, help us to have an idea why we still know so little about the cortex. Edelman and Tononi (2000) suggest that the number of possible neuronal circuits is on the order of 10 followed by at least 1 million zeros, an astonishing number, since the number of particles in the known universe is on the order of 10 followed by "only" 72 zeros. Also, the electrochemical communication between neurons, unlike most artificial electric circuits, is not the "all or none" kind that is either on or off, but instead ranges over all the values between the two extremes. This fact greatly increases the information encoding possibilities: neurons do not work like electric switches, but can assume potentially infinite number of intermediate values, the only restriction being the molecular limits of cell structure at the level of quantum theory interactions (see Penrose 1989 for a critical discussion of this issue). The entirety of our individual histories—memories, perceptions, tastes, voluntary movements, emotions, judgments, decisions, and, our concern here, our grammars—run through this net. We know very little about how these synaptic contacts develop (see Changeux 1983/1985 for a suggestive proposal that converges with the learning-by-forgetting hypothesis that we briefly mentioned in the first chapter). But we do know that at least at a macroscopic level synaptic contacts are guided by growth factors, proteinaceaous substances such as nerve growth factor, or NGF, discovered in the early 1950s by Rita Levi-Montalcini (1987/1988), winner of the 1986 Nobel Prize for Medicine or Physiology, and that individual experience is capable of significantly affecting the plasticity of this net—a fact that makes every human's brain a unique and unreplicable object.

Given this level of complexity, understanding how we get from the structure of an interrogative sentence to that of a neuronal net or, even more simply, how we encode the notion of *word* is still an object of speculation or fantasy. Something can, nevertheless, be done: we can measure the energy that portions of the cerebral cortex use while tasks are performed and try to infer some significant data. Let's now look at how neuroimaging techniques help us to do this.

When an electric impulse is transmitted between two neurons, energy produced by the cellular metabolism is used. The metabolism strictly depends on the oxygen coming from the blood. PET and fMRI are techniques that can measure blood flow (hemodynamics) in the various regions of the cerebral cortex, though the way the two technologies do the measuring is completely different.[11]

In PET, the subject is usually injected with a radioactive variant of a biologically compatible atom such as oxygen-15 by means of water molecules ($H_2{}^{15}O$) or more rarely nonbiological markers bound to biologically compatible atoms such as carbon-11 (for instance, the tracer [^{11}C] Raclopride). The radioactive substance circulates in the organism and reaches the brain, which is 70 to 80 percent water (only 3 to 4 percent of this water is part of the hematic component, or, in less technical terms, the blood). The radioactive decay of the oxygen isotope molecule is quite rapid, its half life varying from two minutes to two hours, and produces positively charged particles called positrons (from which the name positron emission tomography is derived). When positrons collide with

11. This is not the only use of these two techniques. In general, PET and fMRI can be used to observe the various densities of the human body's anatomic tissues in both the encephalon and other parts of the body for research and diagnosis.

nearby electrons (called annihilation), two gamma photons moving in opposite directions at 180 degrees are produced. A "ring" of sensors around the subject's head detects the photons and reconstructs their origin by means of a sophisticated mathematical calculation. In other words, water molecules containing the oxygen isotope function as tracers of blood flow or other possible metabolic variables that are at play. Different numbers of particles correspond to different amounts of water, indicating different amounts of blood reaching certain areas of the cortex. In conclusion, blood flow variation is proportional to neuronal metabolic activity. These measurements (in spatiotemporal terms it can reach a resolution of 6 cubic millimeters per 30 seconds with PET, typically) are usually projected onto grayscale anatomical brain maps created by averaging the maps of a significantly large sample. The projections indicate the level of activation of the different areas of the brain by means of colored areas. These maps can be "slices" of the encephalon in axial, coronal, and sagittal projections or can be three-dimension perspective images of portions of the encephalon.

Functional magnetic resonance imaging (fMRI) is a different type of procedure, though for our purposes the results of the measurements are similar. It is based on the phenomenon that masses of atoms modify their normal orientation when they are put in a strong magnetic field, as if they were "combed" and ordered by the magnetic field, like needles on a piece of paper under the effect of a magnet. The nuclei of fundamental elements such as hydrogen rotate around their own axes. The artificial conditions produced by the strong magnetic field make it possible to measure the average rotational movement of atoms of the same kind. If energy is administered to these atoms as radio waves of a certain frequency, the rotational movement of the atoms gains energy and varies in direction, entering into *resonance* with the source of the energy—something like when you strum a guitar string and other strings start vibrating spontaneously, or when you are swinging on a swing set and you increase the oscillation of the swing itself by pumping with your legs, following the same rhythm of the oscillation. When the administration of energy ends, the atoms—still immersed in the magnetic field—return a part of the acquired energy as radio signals and go back to their equilibrium state. At this point a system of antennae captures the energy that the atoms return as radio waves, and a computer with a specific statistical and mathematical program, by repeating this process many times, reconstructs the image on the basis of the quantity and origin of the energy that is released by the atoms.

How does this give us information about brain function? The measurement of the resonance reaction of the hydrogen atoms is usually used to test biological tissues, since these atoms are present in many biologically compatible elements, such as water molecules, and therefore blood. The hydrogen nuclei are normally randomly oriented in biological tissues. During the fMRI acquisition, the subject is put into a strong magnetic field commonly between from 0.5 to 4 tesla (the unit of measurement of the magnetic field in these kinds of studies) and is subjected to radio wave bundles with short-duration pulses. These bundles give indications about the various parameters of the physiology and anatomy of the tissues under examination. The reaction times of hydrogen atoms differ according to their molecular environment. From these differences, it is possible to acquire information about the tissue under examination. For example, if the hydrogen is in a water-rich environment, like blood, the returned energy will be different than if the hydrogen is in adipose tissue.

Like studies done with PET, these anatomical-functional studies focus on the blood flow to the brain. Depending on the task, the metabolic activities of neurons require more or less blood, and therefore more or less water, and hydrogen. During these tasks, fMRI measures the different concentrations of oxygenated and deoxygenated hemoglobin, which are two molecules found in the blood that have different oxygen levels. In particular, their hydrogen atoms react differently to the waves that are emitted in the magnetic field they are within. When a cortical area is activated, the amount of blood flow that reaches it becomes larger than the amount of oxygen the area uses. Consequently, the amount of oxygenated hemoglobin in the deoxygenated blood coming from the activated area is larger than the amount of deoxygenated hemoglobin.

The measure of the oxygenation level of the blood is an endogenous tracer. In other words, by measuring the difference between the oxygenated and deoxygenated hemoglobin in a certain area, fMRI provides a measure of the hemodynamics of that portion of the cortex. This is why this method is referred to as BOLD (blood-oxygen-level dependent). Local measures of blood flow are often referred to as rCBF (regional cerebral blood flow).

As with PET, a relation is established between regional hemodynamic differences and neuronal activity. Statistical results of fMRI can also be projected on anatomic maps of the brain, producing characteristic images and maps. Unlike PET, fMRI is considered a noninvasive technique, since it does not expose subjects to ionizing radiation and thus can be also applied to women and children for research purposes, unlike PET, which is reserved for males because of the potential damage to female egg cells and to the developing organism of children in general. As for spatio-temporal resolution, the limits of fMRI are shorter than those of PET for reasons tied to the physiology of the hemodynamic reaction: the temporal limits are about two to ten seconds and the spatial resolution goes down to 1 to 3 cubic millimeters.

When I first started thinking about an experiment to visualize the functioning of the brain in vivo with respect to linguistic tasks, my illusion was that if we put subjects inside one of these machines and have them perform a linguistic task, we would have interesting data right away. It is not like that at all.

Let's imagine putting a subject inside a PET or fMRI machine. The machines look similar: they are both toroidal structures, that is, big fat rings that encircle the part of the body under examination. Suppose you want to locate the brain activity involved in the task of reading a written text in the subject's native language. Usually the subject, who is lying down on the test bed, reads text rolling across a monitor in silence. What would you find? You would see that basically the *whole* cortex and, more generally, the *whole* brain is activated while cognitive and non-cognitive functions are being performed. The subject would move the eyes to read the words that are rolling across the monitor, process the symbols in order to translate them into linguistic structures, sense hot or cold, pay attention to irrelevant details, feel nervous or hungry, hear unusual noises, or get distracted and think of other things. In short, subjects can feel all sensations and thoughts that can busy your mind while you are reading. Therefore, you would not be able to recognize the brain activity that results exclusively from reading. Even if you measure the hemodynamics of a subject at rest, you will find that the cortex is still pretty active. Except for cases of pathology, no areas or portions of the net in the brain ever shuts down.

Your initial enthusiasm for these machines may dissipate in the face of the realization that the objective data that you dreamed of obtaining was just an illusion and that reality does not reveal itself on its own. But then, what are PET and fMRI useful for? We need to devise a method to highlight the activity we want to study with respect to all the other concomitant activities. With a simple example, I will touch on an investigation technique that is widely used in neuroimaging, called the subtractive method (see Friston 1997 for a detailed critical discussion on the various methods that are used to interpret the data from different neuroimaging techniques).

The subtractive method is one of the most commonly used methods and one of the first to be developed. What is it? Why "subtraction"? Consider a simple example concerning a motor task: A subject taps the four fingers of his right hand, or opens and closes his right hand, while observing a black screen, and the hemodynamic cortex activity is measured (either with PET or fMRI) as he does this. This task will certainly activate the portion of the cortex that is dedicated to motor control. In particular, a significant activation of the hemisphere that is contra lateral with respect to the hand that is used will be observed (the right hemisphere controls the movements of the left part of the body and vice versa). You will also see cognitive activities and other independent activities such as eye movement that will activate additional areas of the cortex. This result will not allow you to isolate the cortical activities that are dedicated exclusively to motor control. You will need to take a second measurement and for this you ask the subject to go back into the machine and just observe the black screen, as he did in the first part of the experiment, and do nothing else. You now have two sets of measurements of cortical hemodynamics: one while task X, finger tapping, is performed at the same time as task Y, observing the black screen, and another one while only task, Y, is performed. In order to collect the most homogeneous results possible from all the subjects, they are asked to perform a simple cognitive task to prevent them from getting distracted and letting their minds wander. For example, subjects may be asked to count backward from one hundred to zero by odd numbers; this should require a minimal effort from the subjects without interfering with the task that is being measured.

The subtractive method is based on the hypothesis that if you "subtract," point by point, the hemodynamics values of the cortex that you got from the two phases of the experiment, the points of the cortex in which identical activation values were recorded in both phases of the experiment would cancel each other out, leaving just the points where the values were different. In the best of possible worlds, the residual differences between the two measures would precisely correspond to the tapping of the fingers of the right hand on the thumb, that is, the area that is associated with task X in the corresponding anatomical map would be localized (see figure 2.3).

In figure 2.3 the areas within the light lines correspond to the regions of the cerebral cortex that are selectively involved with the motor task. The idea of the subtractive method is, therefore, to "mask" the irrelevant activations with respect to what is under investigation by subtracting the

Figure 2.3
This image, obtained with the subtractive method, indicates the regions of the cerebral cortex that are selectively involved with the motor task of tapping the fingers of the right hand.

activities of two different tasks. (In figure 2.3 the shape of the brain looks abnormal. This shape is derived from a mathematical procedure that projects the portions of the cortex that are in the folds onto the external surface. It is as if the brain were inflated like a balloon in order to highlight the cortical activations that would otherwise be hidden in the usual anatomical structure.)

The subtractive method works on the basis of the fact that *the localization of the cortical activation that corresponds to a certain task results from the comparison of two different tasks.* More generally, a comparison is always needed, either between two different tasks (the straightforward subtractive method) or between two different moments of the same task (the parametric method). This experimental point needs to be understood very well because it will be crucial when we deal with the problem of localizing the neuronal nets of the cortex that are selectively associated with syntax. We can even anticipate the central question that will be raised in the first experiment, which by now the reader might have already thought of: *Can we design two tasks such that their difference gives relevant information regarding cortical activity that selectively corresponds to syntax processes?* We will return to this point in the next section. For now, let's just keep this goal in mind and finish these methodological preliminaries.

In order to conclude this brief tour of neuroimaging methods, we need to illustrate, in a simplified manner, how we can actually get information by using the subtractive method in a more articulate way. Imagine having data from a certain task and thus having localized an associated cortical net such as that involved in the finger tapping study. Now imagine asking a subject to tap not all the fingers but every other finger and measuring this. A first behavioral measurement, as it is technically called, concerns the level at which the subject masters the task. In this simple case, it is likely that, after some training, the subject improves, measurement after measurement, until mistakes are no longer made. In the meantime, we have also measured the activation of the cortical net that is associated with this task and possibly we can notice an increased in blood flow in that area—another thing we know thanks to the subtraction method—along with increased movement mastery with a decrease in errors. We now have two pieces of information with respect to a certain task: the mastery level and the regional cortical blood flow level. By appropriately combining the measures relative to this piece of information, we can draw a cartesian graph where a line that expresses an average of values indicates how the two kinds of information correlate, with each reported on one axis. In general, graphs of this kind can be very useful for comparing two different tasks. For instance, they can show how flow increases in a net correspond to increases in accuracy of a certain movement while the very same net is deactivated if a different movement is performed. This is not a real example, but it will be useful when we talk about language acquisition in the third section of this chapter, "Possible Grammars versus Impossible Grammars."

Investigating the structure of a net of millions of billions of neurons in the cortex by examining the regional hematic flow of portions of this unconceivable tapestry is like observing Earth from Mars and trying to reconstruct the maps of the cities of the world by having airport passenger flow as the only type of data. The enterprise is hopeless. Some interesting pieces of data can be found, at least regarding the relative sizes of the cities and their connections, and possibly dramatic mistakes. This is just about the level that we are at these days: we do not have a way to "see thought," but we can still obtain some nontrivial data, as if we could detect its "movements."

Before we move to language, however, it is important that a fundamental point be completely clear. We referred in a very simple way to a motor task: the tapping of the fingers of a hand on the thumb of the same hand. The subtractive method does not seem particularly problematic, at least in this case. Resting and viewing a black screen are subtracted from the movement, and the difference is associated with the cortical net that is responsible for controlling that movement. But what happens when a cognitive task is observed? The situation gets much more complicated. Imagine that you want to use the subtraction method to locate the cortical net that is responsible for object recognition by seeing. You could of course measure the cortical blood flow while objects are recognized and subtract from it a state of rest in which the subject observes a black screen. However, the situation in this case is more complicated: we are not dealing with just the presence or absence of a movement—something that can be simply suspended. By using the subtractive method, you are implicitly assuming that object recognition simply "adds" to the observation of a black screen. But is this correct? It could be the case that observing a black screen implies a cognitive operation that is at least partially similar to object recognition. Therefore, we cannot be sure that the subtractive method does not influence the data, if used incorrectly. The results can only be evaluated on global grounds, in terms of the predictive force of the paradigm.

An even more complicated case could be the localization of the cortical nets that distinguish the recognition of an object from the capacity to assign a name to it. In this case, the entangling of cognitive activities looks even more difficult to disentangle. The researcher can only try to see whether the data collected in this way are compatible with the expectations of the model that has been built by studying a certain cognitive activity. For instance, the researcher could assume that recognition of an object always has to go through the search for its name and therefore could try to repeat the experiment by using abstract geometrical forms and check to see whether the results are different. Let's stop here. It is clear, even at an intuitive level, that the analyses of a motor task and of a cognitive task imply very different empirical problems. Subtracting or comparing the absence or presence of a movement, although complex, is much simpler than investigating a cognitive activity. Also, no one has a global theory of how the mind works. As I have remarked many times—and as is clear to those who have thought, even a little, about this issue—no technique, even if very sophisticated, and even less a machine, can by itself provide a theory of how the human mind works.

The illusion of "seeing thought" is still an illusion, although we are starting to accumulate data that we did not have before. It is unclear whether this illusion will ever dissolve, due to an explicit theory. Our species may never reach a complete unification of the material and psychological functioning of cognitive processes, a theory in which "mind" and "brain" are one thing. But, as always happens in science (physics is a particularly good example), you can proceed with analysis even without the certainty of reaching "a theory of everything." What is important is formulating simple questions that take a feasible experimental path and that advance at least one step toward achieving the goal of a better and simpler understanding of the observed phenomena. This is what I will do in the next two sections of the chapter.

We will see how what we know about syntax and what we know about the brain can be compared by means of the neuroimaging techniques. We have no a priori expectations. Linguistics and neurobiology were born and have developed independently. It is therefore an empirical problem, one whose solution can come only from an experimental examination of reality and not from a logical analysis. However, we should be aware that reality does not reveal itself on its own: no machine makes nature talk spontaneously. The process of comprehension always occurs from our adopting the Galilean research style that we discussed in depth in chapter 1.

2.2 The Autonomy of Syntax: How to Fool the Brain with Errors

We now have all the elements needed to describe the experiments. We have a simplified linguistic theory, a model of brain functioning, albeit one that is rudimentary with respect to the complexity of what we can see, and machines that allow us to verify whether the expectations of the model produce measurable effects. When we built our first experiment, there had already been investigations on the relation between syntax and cortical activities, but all the works aimed to measure the increase of syntactic complexity, not syntax per se as opposed to other components of grammar (see the review in Kaan and Swaab 2002). Embick et al. 2000, on the other hand—which we will briefly refer to—is an exception; still, our experiment differs substantially from it, as we will soon see.

The increase of syntactic complexity is certainly an extremely interesting index, but it triggers an increase in memory load, and therefore, the specific contributions of the syntactic component and memory cannot be easily distinguished from memory activity.[12] What we were looking for in this first experiment was a different thing: we were looking at whether syntax, independent from other components of grammar, included memory load, or complexity—correlates with a dedicated neuronal activity.

It is now time to take the notion of simplicity seriously: we need to idealize the data up to the point where we can formulate a question that is simple enough so that we can design an experiment. In this journey be-

12. It has been shown that the activation of Broca's area clearly correlates with the modulation of verbal working memory (see Paulesu, Frith, and Frackowiack 1993).

tween grammar and brain, we have already done a substantial amount of idealization: when we discussed the properties of human language, we focused not only on grammar, rather than on communication or other aspects, but, more narrowly, on syntax, which is the real distinction between human and animal languages. Of course, the new neuroimaging techniques give rise to many questions about syntax, but we need to carefully think about what we want to investigate on the basis of the limits imposed by the machines, and keep in mind that machines never "speak on their own": the choice of the theoretical framework is always decisive.

Among all the possible questions—and they are really virtually infinite, as numerous as the syntactic properties of human languages—the first experiment will show how we managed to find a way to verify at least *one* fundamental property of syntax: it is autonomous with respect to the other components of grammar. For the sake of simplicity and convenience, let me briefly remind you of what we mean by "autonomy of syntax." We certainly do not mean that syntax is independent from the rest of grammar. Actually, it is quite the opposite. Language is a seamless universe. It is the linguist who imposes the division into modules such as phonology, morphology, semantics, and syntax, similar to the naturalist who imposes the distinctions of chemistry, physics, and biology over all existence. Why, then, do we speak of the "autonomy of syntax"? To highlight the fact that the rules for combining words are not completely reducible to other components of grammar: syntactic principles have their own laws (linearity, discreteness, recursion, dependence, and locality) that do not apply to other aspects of language, although syntax (and its rules) and the other aspects of language are at play simultaneously. This leads us to the real problem and the question that our experiment aimed to answer: *How can we verify that syntax is autonomously represented in the brain if syntactic processes take place simultaneously with other linguistic processes?* Now that we have an idea about the limits of the measurement of cortical activity—in particular, the limits of the subtractive method—we can fully understand what the problem is: to come up with tasks that allow syntax to emerge as autonomous even though grammar is completely at play in any linguistic task. In other words, unlike with motor tasks such as finger tapping, syntax cannot be simply suspended while the subject processes linguistic structures; there *is* no human language without syntax.

This problem is purely empirical: we need to find a way to compare two tasks so that the syntactic component is isolated from other linguistic aspects. But this is not the only problem—in fact, it may even be secondary. The real problem is that this experiment requires a nontrivial epistemological reflection. Let's consider our initial question carefully. We are admitting that theoretical linguistics has, by means of language regularities, established a certain partition of the linguistic universe according to which a number of facts, called syntax, can be distinguished from others. We also know that the brain can be investigated by means of the analysis of blood flow and that we can, using these data, recover information about its biological organization. What ensures that the two models are compatible? What ensures that syntax can be isolated in hemodynamic terms? Nobody can exclude a priori that the two fields have arrived at models that are coherent if considered separately but incompatible if considered together. This must be borne in mind as we move through the first experiment. It would not be good to start by assuming that the linguistic model and neurobiological functioning coincide.

Let's go back to the central question: How can we isolate syntax in the brain if it is at play simultaneously with other components of grammar? We cannot subtract a syntactic task from a general linguistic task: virtually *every* linguistic utterance involves syntax. Note, however, that it is certainly possible to perform certain linguistic tasks that do *not* involve syntax. For instance, it is possible to analyze the cortical response to the reading of a word list—say, a list of verbs or a list of nouns—and obtain interesting results (see, for example, Perani et al. 1999). A random list of words or a list of words of the same type (nouns, verbs, adjectives) surely pertains to language, but it is something quite different from the string of words that forms a sentence. But the opposite is not possible: if a task involves syntax—if a subject reads or listens to a sentence—this will automatically trigger other linguistic components, such as semantic, phonological, and so forth. Although in both cases we are dealing with sets of words, with sentences there is also the complex syntactic structure connecting them, that is, structure dependence. Therefore, direct investigation of lists of words versus sentences that employs the subtraction method would not make sense: lists of words with no syntactic structure and lists of words connected by syntax are just not homogeneous entities. But there is a way to get around this. We can make use of the notion of error (discussed in depth in the chapter 1, section 1.1.2.3, "Error as Tool") —in a heuristic way.

If we cannot isolate syntax because it is always activated simultaneously with other components of grammar, we can try to turn the situation upside down: *since syntax cannot be suspended, in order to verify the hypothesis that it is autonomously represented in the brain, we can produce selective errors in all the components of grammar and verify whether the cortical reaction to syntactic error recognition differs from the reaction to other kinds of errors.* If it is different, this would suggest that syntax is autonomously represented in the brain. In fact, even though we still may not be able to say that we have produced a *direct* proof of the autonomy of syntax, the indirect proof would be self-evident: otherwise, why should syntactic error recognition activate neuronal nets that are different from those that are activated with the recognition of other kinds of errors?

Relying on this intuition—the recognition of selective errors—let's describe the experimental paradigm. We want to build errors of different kinds that allow us to show how syntactic competence activates cortical nets that are different from other kinds of linguistic competence. Let's start with a preliminary linguistic problem: What happens when you produce a syntactic error? Consider the simple transitive sentence *The lion devoured the chicken.* Let's now produce a syntactic error by mixing the words: *Chicken lion the the devoured.* This sentence does not follow the syntax of English: its syntax is corrupted. Nevertheless—and this is important—you can still recover the meaning of the sentence. Given your knowledge of the lexicon and of how the world works, you can reconstruct the meaning of the sentence and understand that a lion devoured a chicken. Equivalently, you can reconstruct the structure of the sentence by putting subject and object in the right positions, still based on your knowledge of the world. In summary, the semantics of this sentence does not seem to be irreparably damaged by the syntactic errors in this sample. On the other hand, if you consider another transitive sentence *The lion devoured the crocodile* and apply the word mixing operation and get *Crocodile lion the the devoured*, how can you recover the meaning? Your lexical and world knowledge brings you to an ambiguous solution: either the lion devoured the crocodile or the crocodile devoured the lion. Both options are available in our world. In this case, the semantics has been irreparably damaged by the syntactic error. How can we avoid this effect of affecting an area other than syntax when we construe syntactic errors? If we do not solve this preliminary issue, the entire strategy of construing selective errors to see if syntax is autonomously represented in the brain would be hopelessly undermined.

There is an exit strategy. We needed to find a way to "fool" the brain so that semantics would not be involved. Semantics is a complex notion, but one way, though a simplistic way, to understand it is as the specific language competence that the brain makes use of to build the meaning of a sentence and assign the sentence its "truth conditions," as determined by the meanings of its constituent expressions and the rules used to combine them (principle of compositionality). By truth conditions we mean the capacity to say whether the sentence is true or false under some given circumstances. In the simple case of an affirmative sentence like *The lion devoured the crocodile,* we know that it is true if and only if it is true that the lion devoured the crocodile.[13] The exit strategy to avoid syntactic errors with semantic consequences we chose was not to use words that have meaning, but rather to come up with lexical roots that do not refer to any concept, action, or object.

This is quite a delicate point and deserves some considerations which I will try to put in simple terms. In first approximation, when we think of the meaning of a word in fact we think of at least two components: the capacity of certain words to refer to objects, properties, or concepts of the world (like when we say *dog*, or *run*, or *happiness*) or the capacity to build relations among them. In the latter sense, for example, it is assumed that the meaning of an indefinite article like *a* in *a dog runs* is to say that the intersection between the set of dogs and the set of runners is not empty, since it contains (at least) one element; the meaning of *every*, instead, in a sentence like *every dog runs,* is that the set of dogs is a proper subset of the set of runners (see the seminal work by Barwise et al. [1981] on this issue).[14] Both components contribute to the meaning of a sen-

13. This association is often called Tarski's principle, from the name of the scholar who first presented it formally, triggering a number of complex and very articulated studies that are still continuing (cf. Chierchia and McConnell Ginet [2000]).

14. This is often called the theory of generalized quantifiers after the seminal paper by Barwise and Cooper (1981); see Chierchia and McConnell Ginet (2000) for a detailed illustration of this theory. One of the major contributions of this theory is to have discovered a surprising universal of language that pertains to semantics. Synthetically, it can be illustrated as follows. Assuming that all determiners—like *a*, *every*, *some*, etc.—are relations between sets and defining a relation R between two sets A and B as *conservative* if and only if it also holds between A and the intersection between A and B, it has been discovered that every determiner in every language is conservative. This is surprising because in principle non-conservative determiners are logically conceivable (think, for example, of a complex determiner meaning "all non"): consequently, the absence of non-conservative determiners can only be plausibly explained as a consequence of a universal boundary imposed on semantics by the biological guide governing languages.

tence. For the sake of simplicity, let us call the former type of semantics "lexical semantics," following a quite common practice of calling "lexicon" the set of words that can refer to objects, properties, or concepts only.

Synthetically, our idea was to overcome the obstacle of semantic triggering by skipping completely over lexical semantics while still maintaining the other aspects of semantics, such as those conveyed by articles (see again Chierchia and McConnell Ginet 2000 for a detailed and critical illustration of these two distinct aspects of semantics). The hypothesis was that by skipping one component of semantics—that is, lexical semantics—semantics was nullified in toto, since there was no possible way to assign a truth value to the corresponding sentence. This will be made much clearer by considering the actual stimuli we used, but I can anticipate the idea by giving a sample sentence like *The gulk ganfed the brals*: while only lexical semantics is unavailable (there is no meaning for *gulk*, *ganf* or *bral*), whereas the article *the*, the plural morpheme *-s* and the past suffix on the verb *-ed* are available, clearly there is no way whatever to assign a truth value to the sentence. Synthetically, there is no meaning for this sentence.

Despite the many possible objections to this strategy of semantic neutralization, in the end we were making a bet based on a model and known facts. Only the final results would tell us whether we were right or not. Also, an imaginary language had never been used before in neuroimaging experiments about syntax—not even by Embick and his colleagues (2000), though they did use errors in various components in order to measure cortical activities.

The experiment was conducted in Italy on native speakers of Italian.[15] We used sentences that were made primarily of invented words but looked and sounded like Italian sentences, which we called "pseudo-words" and "pseudo-sentences," respectively. All pseudo-words had the regular Italian suffixes expressing number, gender, tense, and so on. In addition to the invented words there were still actual articles, demonstrative pronouns, indefinite pronouns, helping verbs, negations, impersonal pronouns, copulas (linking verbs such as, prototypically, *to be*), quantifiers such as *ogni* (every) and *tutti* (all) and prepositions. Four of the fifty-two pseudo-sentences that we actually used are listed. The second

15. This experiment was the result of collaboration between the Istituto di Neuroscienze e Bioimmagini-CNR in Milan, the Università "Vita-Salute" San Raffaele, and the Istituto Scientifico San Raffaele IRCCS, also in Milan.

line in each example is a word-by-word translation of the non-pseudo-words with glosses for the suffixes; the third line is an approximate rendering in English.

(Group 1)
Il gulco gianigevale brale.
The.MASC.SG gulc-MASC.SG gianig-3SG.PAST the.FEM.PL bral-FEM.PL.
"The gulk ganfed the brals."

Molte grapotte amionarono.
Many.FEM.PL grappott-FEM.PL amion-3PL.PAST.
"Many grapots atted."

Ogni ditra ha milenato il fiommo.
Every ditr-FEM.SG has milen-PASTPART.NEUT the.MASC.SG fiomm.MASC.SG.
"Every blick has milened the flust."

Le corle furono featide.
The.FEM.PL corl-FEM.PL were featid-PASTPART.FEM.PL.
"The coives were featided."

In our experiment, following what we just observed concerning lexical semantics, we used pseudo-words to replace lexical roots of "open-class" words but not "closed-class" words. This is another useful terminological device to distinguish words that is different from the distinction based on their capacity to refer to objects, properties, or concepts or relations between sets we illustrated before while talking about lexical semantics. Open classes are sets of words to which new members can always be added, such as verbs, adjectives, nouns, and some adverbs. Closed classes are those that do not allow for new members, including articles, demonstrative pronouns, indefinite pronouns, helping verbs, negations, impersonal pronouns, copulas, quantifiers such as *every* and *all*, and prepositions. Although it is always possible for the vocabulary of a language to acquire a new verb—in fact, this happens all the time, as even individuals coin new verbs and nouns—the number of articles or prepositions cannot be voluntarily increased by an individual speaker. It does not seem likely that sometime in the future, a new preposition, *wiby,* will be added to the set of preposition used in English. This is because the meanings of closed-class words have a different nature with respect to the meanings of open-class words. Open-class words refer to sets of objects, properties, or concepts in the world; closed-class words do not do this but instead connect open class words with each other. It is virtually impossible to control closed-class words. Moreover, none of our pseudo-sentences can be said to be true of false, because none of the open-class words refer to concepts or sets of objects in the world.[16]

Finally, punctuation was completely absent in our pseudo-sentences. This was done in order to avoid giving any clue about the sentence intonation that could make the subjects think of specific distortions of the sentences.[17]

We had two expectations. First, that our syntactic errors would not have any semantic effects, because we completely avoided lexical semantics. Second, since it is well known that the processing of linguistic structures that contain meaningful words activates cortical areas in the temporal regions which are specifically responsible for lexical retrieval—as when one consults a dictionary, we were expecting not to have activations in those regions, thus making it easier to isolate syntax (see Dhond et al. 2001; for the neuronal correlates of the notions of nouns and verbs, see Perani et al. 1999). This was the starting point, but getting rid of lexical semantics, of course, did not suffice for us to reach our goal of isolating syntax.

There were at least two more components that we had to try to separate out from the grammar complex in order to isolate the cortical activities tied to syntax: the phonological and the morpho-syntactic components. Morpho-syntax is the set of morphological effects that depends on syntactic structure, prototypically subject verb agreement. For instance, **The kids runs* is ungrammatical because of a morpho-syntactic error, because the suffix morpheme on the verb, *-s,* is the wrong one to

16. Incidentally, building sentences made of pseudo-words was hard work. We had to create different kinds of syntactic structure that would not trigger ambiguous readings and avoid pseudo-words with strong evocative power. It is interesting that given a series of pseudo-words, most subjects spontaneously assigned the meaning of "animal name" to noun phrases. Therefore, a *gulco* immediately reminds of an animal, and not, say, of a vegetable, a color, a flavor or a feeling. This cannot be accidental given the potential vastness of other kinds of repertoires (the names of flowers, foods, body parts, and so forth). It could have an adaptive origin; the need for humans to identify other living beings before anything else.

17. This is a little bit like when people say, in Italian, *Paola, bacia volentieri* "Paola, (he/she) kisses willingly": the direct object (*Paola*) is found in preverbal position, is separated from the verb by a comma, and is pronounced with a marked intonation; the "understood" subject—remember that Italian, unlike English, allows for subject omission in cases like this—could be any unspecified individual. On the other hand, the sentence *Paola bacia volentieri,* "Paola kisses willingly," refers to the fact that it is Paola who likes kissing. In our stimuli, we wanted to prevent our subjects from being misled by these kinds of facts.

use with that subject. In chapter 1 we spoke of morphology as the study of morpheme structure or composition, but morphology per se was not affected by this experiment, in which we were interested in whether syntax and morpho-syntax were distinct in the functional architecture of the brain. As for phonology, of course, we simply aimed at producing illegal sequences of phonemes like impossible clusters of consonants, something like *panzstrge* in English, where *nzstrg* is surely not a possible string of consonants in this language; thus *panzstrge* is not a possible word. Incidentally, building sentences made of pseudo words was hard work, from many points of view. Among other things, we had to create different kinds of syntactic structure that would not trigger ambiguous readings and avoid pseudo words with strong evocative power.

Let's go back to our experimental paradigm and follow its construction step by step. After eliminating lexical semantics, we now needed to build selective errors at the phonological, morpho-syntactic, and syntactic levels. For all three levels, we used the same pseudo-sentences and inserted the different kinds of errors. The four sentences below give an idea of what we did at the phonological level. They are based on the sentences in group 1, but have been changed so that each includes one word with a phonological error (underlined).

(Group 2)

Il gulco gianigzleva le brale.
The.masc.sg gulc-masc.sg gianigzl-3sg.past the.fem.pl bral-fem.pl.

Molte grapotrte amionarono.
Many.FEM.PL grapotrt-FEM.PL amion-3PL.PAST.

Ogni ditra ha milenaclto il fiommo.
Every ditr-FEM.SG has milenaclt-PASTPART the.MASC.SG fiomm-MASC.SG.

Le cofrsle furono featide.
The.FEM.PL cofrsl-FEM.PL were featid-PASTPART.FEM.PL.

All the pseudo-words in the sentences in group 2 can be pronounced by Italian speakers, but they would not consider these words "natural," that is, their phonological competence tells them that these words are not part of the Italian vocabulary. For instance, the word *grapotrte* is not a possible word in Italian since Italian phonological rules never allow for the string *trt*. Similarly, the word *milenaclto* is not a possible Italian word because it contains the string of letters and sounds *clt,* which is not allowed in Italian. On the other hand, words such as *amionarono* or *fiommo* in the sentences in group 2 could very well be taken as words in the Italian vocabulary, although they are not. Subjects were given a list of pseudo-sentences and had to identify those with phonological errors.

As for the morpho-syntactic level, we built pseudo-sentences without phonological errors but with incorrect agreement, which is the gender or number dependence between nouns, verbs, adjectives, and articles in Italian and many other languages. In English, agreement is limited to just the plural *-s* suffix on most nouns and third person singular in the present tense of the indicative in practically all verbs but the copula. In Italian and other languages, the morphological structure of nouns, verbs, adjectives, and articles varies in a richer way according to number and gender. For instance, the Italian noun *ragazzo* means "boy." It is a bimorphemic word containing the lexical morpheme *ragazz-* and the grammatical morpheme *-o*, espressing masculine singular. If the last vowel is *-i* instead of *-o,* the noun becomes *ragazzi* and means "boys." If it is *-a,* the resulting noun, *ragazza,* means "girl." If it is *-e,* the resulting noun, *ragazze,* means "girls." Articles' and adjectives' endings also change in Italian according to the number and the gender of the noun (the agreement morphemes are underlined in the following examples): *questo strano ragazzo,* this weird boy, quest*i strani ragazzi,* these weird boys, *questa strana ragazza,* this weird girl, and *queste strane ragazze,* these weird girls. Some verbal forms, too, show number and gender agreement, such as the past participle in the following sentences: *questo strano ragazzo è arrivato,* "this weird boy has arrived" (literally, is arrived), or *questi strani ragazzi sono arrivati,* "these weird boys have arrived" (literally, are arrived). The examples in group 3 show how we changed the sentences in group 1 to have one word for each sentence with an agreement error (the wrong pairings between noun and verb gender and number suffix morphemes are indicated by underlining; those of articles and adjectives are not highlighted here).

(Group 3)

Il gulco ha gianigiata le brale.

The.MASC.SG gulc-MASC.SG has gianig-3FEM.SG.PASTPAST the.FEM.PL. bral-FEM.PL.

Molti grapotti sono stata amionati.

Many.MASC.PL grappott-MASC.PL have been.FEM.SG amion-PASTPART.MASC.PL.

Ogni ditra hanno milenato il fiommo.

Every ditr.FEM.SG have.3PL milen.PASTPART the.MASC.SG fiomm.MASC.SG.

Le corl<u>e</u> <u>fu</u> featide.
The.fem.pl corl-fem.pl was featid-pastPart.fem.pl.

Each word in the sentences in group 3 sounds like a possible Italian word, but each sentence contains a wrong agreement morpheme pairing (underlined). The "morpho-syntactic" tie—the choice of the correct morphology given a specific syntactic context—is broken. An Italian speaker who hears the top sentence in group 3, *Molti grapotti sono stata amionati,* immediately knows that *stata,* the helping verb used to form a compound past tense, does not agree with anything, since it is feminine singular (*-a*) and there are no feminine singular noun phrases in the sentence. It should be *stati*, which is masculine plural. Therefore, the sentence contains a morpho-syntactic error: morpho-syntax requires the right word to be in the right place, and this is not the case. The actual syntax—the structured sequence of words—is still intact, however.

Finally, we dealt with syntactic errors: the essential target of our experiment. We came up with a list of pseudo-sentences where all the words were possible Italian words, all the suffixes (and therefore, agreement forms) were compatible with each other, and the only problem was the syntactic order of the elements. Four examples are given in group 4. Once again, they are based on the examples in (1), but in each example the word order has been changed. They look like sentences with distorted word order: a quantifier following its noun (*grappotte molte*), an article that follows its noun (*fiommo il*), a noun between a helping verb and the main verb (*furono corle featide*). On the other hand, they did not contain any phonological or morphosyntatic errors.

(Group 4)
Gulco il gianigeva le brale.
gulc-masc.sg the.masc.sg gianig-3sg.past the.fem.pl bral-fem.pl.

Grapotte molte amionarono.
Grappott-fem.pl many.fem.pl amion.3sg.pl.past.

Ogni ditra ha milenato fiommo il.
Every ditr-fem.sg has milen-pastPart fiomm-masc.sg the.masc.sg.

Delle furono corle featide.
Some.fem.pl were corl-fem.pl featid-pastPart.fem.pl.

We now have all the ingredients for our experimental paradigm. Let me once again briefly summarize the main idea. Syntax, by definition, cannot be suspended when a sentence is formed. Therefore, to isolate the syntactic component in the brain via subtraction it is necessary to use error-recognition tasks that concern different levels of grammar. If different cortical activations correspond to the recognition of different kinds of errors, we would have strong evidence in favor of the hypothesis that syntax activates autonomous neuronal circuits, a finding that would coincide with the modular structure of linguistic theory. This strategy, including the use of pseudo-words to nullify the semantic component, has given some interesting results. Let's take a quick look at a few other technical details of the experiment.

Once the stimuli were prepared, the PET experiment was technically built in the following way. We had eleven subjects who all were male native speakers of Italian, right-handed, with no known neurological problems, twenty-six years old on average, and with a homogenous education level.[18] They entered the machine and received an injection of water molecules with radioactive oxygen isotopes as tracers. Their first task was to read pseudo-sentences without errors such as those in group 1 on a screen while they were lying on the PET table. The date thus collected would serve as a baseline to subtract visual processes from the recognition of various kinds of error. The hope was to see some differences in the regional cerebral blood flow and therefore see differences in the activation of different neuronal nets. The subject would push a button after reading each sentence correctly. Later, for each of the three domains of grammar—phonology, morphology (by the way of morpho-syntax), and syntax proper—the subject was presented with nine pseudo-sentences containing errors, randomly mixed up with four "correct" pseudo-sentences, for a total of twelve trials for each subject. Each time, the subject had to push a button after recognizing the sentence as correct or after finding an error.

Our hope was to see the activation of different areas according to the kind of error, and ultimately to see whether syntax activates a different area than other components of grammar.[19] To keep things simple, I will present here just the stylized images of the experiment's results. (For precise technical data on the experiment, see Moro et al. 2001.)

18. We needed all right-handed subjects because a significant percentage of left-handed subjects develop the functions of the language in the analogous portion in the right hemisphere. This would have "dirtied" our data, making them impossible to evaluate.

19. I will not give the detailed results with the "stereotaxic coordinates"—triplets of numbers corresponding to the axes of the Cartesian space that precisely identify the specific point of the encephalon where a certain neuronal activity occurred.

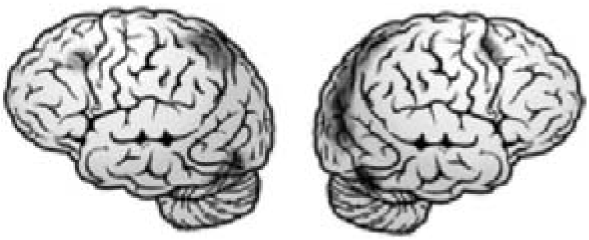


Figure 2.4
The darkened areas in the left and right hemispheres correspond to the common areas that were activated for the recognition of any of the three kinds of error.

The first result is a stylized three-dimensional image (see figure 2.4) of the brain showing the areas corresponding to the subtraction of the reading of correct pseudo-sentences from the reading of all sentences containing errors without separating out the three different kinds of errors. This result is already welcome: it tells us that error recognition in general is "visible" as an isolated fact at the cortical level. The notion of grammatical error coincides with the precise neurobiological activity, as witnessed by different levels of blood flow in certain cortical areas of the brain.

The activation of large portions of the parietal areas together with the activation of the opercular part of Broca's area (area 44) is likely to be due to the subjects' attempt to fix the sentence when they recognize an error instead of simply reading it, as with "correct" pseudo-sentences (Wojciulik and Kanwisher 1999). The lack of activation of the temporal areas shows that eliminating lexical semantics by using meaningless invented words was effective: these are the areas in the brain that are assumed to be activated when one is searching for word meaning (see, for instance, Dhond et al. 2001).

Let's now move to a more detailed, though still simplified, analysis of the kinds of errors. In the images that we will see, I will just show the most important contrast: the one between morpho-syntactic error recognition and syntactic error recognition.

Let's start with morpho-syntactic errors—a combination of phonologically possible words that are in correct order but have the wrong agreement, like the examples in group 3.

Less of the cortex is activated than in the previous case, which included all types of error. Therefore, simplifying a bit, the image resulting from subtraction techniques between the various tasks highlights the portions of the cortex that are activated specifically in the case of morpho-

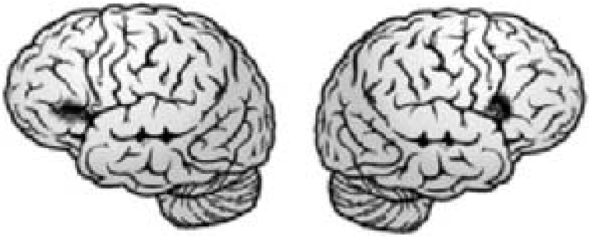


Figure 2.5
The darkened areas correspond to the areas that are activated by morpho-syntactic error recognition.

syntactic error recognition. In particular, a deep component of the triangular part of Broca's area (area 45) and a corresponding area in the right hemisphere are activated (see figure 2.5). As for this unexpected collateral result, we suggested that the right hemisphere homologue of Broca's area might be involved in abstract thinking that arises in meta-linguistic judgments—judgments about language such as grammaticality judgments. Our tentative conclusion is based on studying subjects whose corpus callosum, the tissue that connects the two hemispheres, was cut. Although these subjects showed a severe reduction in their capacity to use syntactic information, they were still able to judge whether a given sentence was grammatical or not (Gazzaniga 1980; Gazzaniga et al. 1984; Baynes and Gazziniga 1988).[20]

The crucial part of our experiment was designed to establish whether syntactic error recognition correlates with the activation of a dedicated neuronal net. If so, it would give us a way to isolate syntactic competence from other kinds of linguistic competence. The sentences were built from phonologically possible words with the correct agreement but in the wrong order, like the examples in group 4. I will report the results of this last case of subtraction in greater detail by including the activation map of some subcortical portions of the brain which are crucial for our specific goal. Nobody can conclude a priori that there are any actual differences in brain activation between morpho-syntactic and syntactic errors: the subtractive method can be decisive for generating data in support of this hypothesis.

20. The activation of the right homologue of Broca's area could also be simply the result of the wiring inside the brain that tends to produce symmetrical connections "by default," a much less interesting hypothesis but one that could be true.

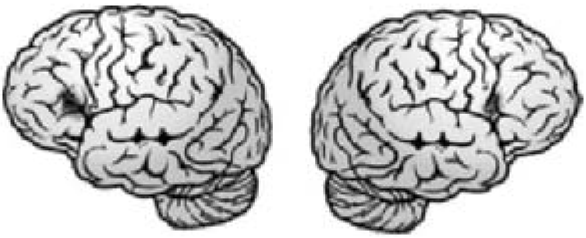


Figure 2.6
The darkened areas correspond to the cortical areas that are activated by syntactical error recognition.

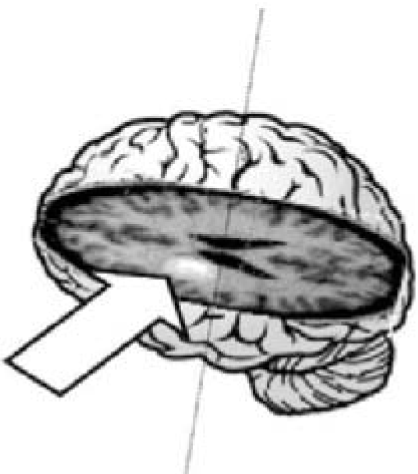


Figure 2.7
This axial projection shows the activation of the subencephalic component of the net that is dedicated to syntax, the left caudate nucleus.

In the first image we see cortical activation; in the image in figure 2.6 it is due specifically to syntactic error recognition.

The highlighted area is very small. Moreover, this image and the image showing morpho-syntactic errors (figure 2.5) superficially exhibit similar cortical activations: a deep component of the triangular part or Broca's area (area 45) and its homologue in the right hemisphere. Should we conclude that there is no difference between morpho-syntactic and syntactic error recognition? The next image (figure 2.7), an axial projection, shows other portions of the brain besides the cerebral cortex; some subcortical portions of the brain and the deep component of Broca's area are activated in the case of syntactic errors, unlike what happens with morpho-syntactic errors. The most important structure here is the left caudate nucleus, which is a component of a group of subcortical structures that have important cognitive functions, are made of gray matter, and are called basal ganglia (they also include the putamen and the globus pallidus). Another area that is activated is the cortex of the insula, which is a deep portion of the cerebral cortex, located under the frontal lobes.

Finally, we have reached the central point: *syntactic error recognition involves a complex net that is not seen when other types of errors are perceived; and this net is not represented in just one cortical area but in an integrated group of different portions of the brain* that includes the deep component of the triangular part of Broca's area (area 45), its homologue in the right hemisphere, the left caudate nucleus, and the insula. (Figure 2.7 shows only the activation of the left caudate nucleus.[21] This image is to be associated with the preceding one. The activation of the insula is not shown, for the sake of simplicity.)

Figure 2.7 clearly shows the activated subcortical brain component, the left caudate nucleus, which, crucially, is *not* activated in morpho-syntactic error recognition. Thus we can infer that not only is there no single area for language in general (which was already clear twenty years after Broca's discovery, thanks to Wernicke's studies [1874]), but there is no single area for syntax. (Subcortical areas such as the caudate nucleus are involved in neurological pathologies such as Parkinson's disease, which suggests interesting research possibilities that are still preliminary; see Moro et al. 2001.)[22]

These results have two major consequences. We have given explicit, empirical data with respect to the functional architecture of the brain. Not surprisingly, we saw that Broca's area is selectively involved for syntax within a complex net that also involves the activation of subcortical portions of the brain. We have also reached a significant epistemological result. If—and I would like to emphasize *if*—these experiments are confirmed, we could conclude that theoretical linguists' distinctions, in particular the hypothesis of the "autonomy of syntax," are not incompatible

21. For the activation of the homologue of Broca's area in the right hemisphere, see Moro et al. (2001).

22. Involvement of basal ganglia is confirmed in a recent study (Tettamanti et al. 2005a), where the modulation of the neurotransmitter dopamine is studied in relation to the linguistic task of error recognition, according to the test suggested in Moro et al. (2001).

with what we know about the neurobiological architecture of the brain. In fact, syntax can be isolated not only by observing comparative linguistic regularities but also by measuring blood flow in the cerebral cortex and subcortical components. Most likely the reader now has new questions that I likely would not be able to answer. If so, it would be evidence that the contact of two disciplines that usually move independently can generate new areas of investigation.[23]

Let's conclude the description of our first experiment on the neuronal localization of syntax with a general remark. The hypothesis that one area corresponds to one function and vice versa, pure locationism, is certainly too rigid, for two reasons: One function involves more than one cortical area and more than one portion of the brain. Conversely the cortical areas and the portions of the brain that are involved can play a role in other functions as well. In fact, Broca's area is also activated for nonlinguistic activities, including music (see, for example, Patel 2003). It is its interaction with the caudate nucleus that builds a system, a net that is dedicated to syntax. How extended is this net and how is it built? These questions not only go beyond the currently available methods but also the current comprehension of neurobiological mechanisms. *It could turn out that Broca's area is just like a hub, the crossroads of several different circuits. Maybe nothing "happens" there. It could just be an area where many networks converge, for purely neuroanatomical reasons.*

2.3 Possible Grammars versus Impossible Grammars

Let's go back to the boundaries of Babel once again, this time looking for a new kind of clue. We can now rely on the results of the first experiment: syntax is autonomously represented in the brain.

In "Limits of Variation" we saw how modern linguists' theoretical model shows that not all possible combinations of structures are actually realized. For instance, in languages such an English, where the verb precedes the direct object (*eating a pear*), the preposition precedes the noun (*after dinner*); conversely, in languages such as Japanies, where the verb

23. Only the second result of this experiment completely converges with the results in Embick et al. (2000). They managed to isolate syntax with an fMRI study without, however, seeing activation of the left caudate nucleus. Since they did not use an artificial language, it is dubious whether the cortical activity related to syntactic errors in their experiment could really be separated from the cortical activity related to semantics.

follows the direct object (*nashi-o taberu,* "a pear eats"), the preposition follows the noun, thus *yuushoku go* means literally "dinner after" (hence the preposition is actually the "postposition"). Languages with a hybrid situation where the verb follows the direct object while the preposition precedes the noun are extremely rare, although they are possible from the purely combinatory point of view. Some limits of variation are of course due to structure dependency. For instance, the hierarchical combination of phrases or locality principles prevents pronominal coreference or phrasal movement in some structural conditions, as we saw in chapter 1. And grammars can also be universally limited in the format of their rules in a much more general way.

A particularly important and general fact about the architecture of natural grammars that we observed in detail in chapter 1 is that *linear order plays no role in syntactic rules; the only thing that matters is hierarchical structure,* which builds complex trees according to a recursive procedure of progressive "nesting" of a certain phrase XP within the same type of phrase XP. A most important question naturally arises here: What is the nature of these limits? Are the empirical generalizations that linguists have come up with over centuries of language investigation isomorphous,[24] or at least compatible with the neurobiological data? Why aren't all languages that one can think of realized? In other words, what is the nature of the boundaries of Babel? Are these boundaries historical and conventional or are they biologically driven? This is what we wanted to probe with our second experiment.

As usual, however, there was a preliminary methodological step to take: we needed to make this question simple enough to allow us to design an experiment. Scientific investigation always requires simplification. In the first chapter, we approached the problem of the boundaries of Babel on the basis of purely linguistic data derived from comparative syntax and language acquisition, and concluded that the most likely hypothesis is that there is a biologically determined guide that imposes common structural limits on all languages (the "tabula praeparata"). In order to account for the rich diversity among human languages, we further

24. *Isomorphism* is a term that comes from abstract algebra. It indicates a structure preserving map between two sets containing the same number of elements (*homomorphism* instead is when the map is from a larger set to a smaller one). More generally, we say that the two structures of any kind are isomorphic when the operations in one structure are fully preserved in the other, disregarding the nature of the elements that enter into this operation.

assumed that the system contains some "open points," or parameters. By identifying the parameters, the linguist can derive the class of all possible human syntaxes. Children acquire a language by setting the parameters of the language they are exposed to on the basis of their experience; the net of principles that guides children to organize linguistic input must somehow be predetermined with respect to experience, otherwise one should observe a random trial-and-error path toward language acquisition, a fact that is never observed. Clearly, any empirical evidence in favor of the existence of a neurobiological correlate in language acquisition would provide empirical evidence in favor of the hypothesis that the boundaries of Babel are neither historical nor conventional. How could we find a simple way to test this hypothesis by means of neuroimaging techniques?

The results of the first experiment, as we said, were crucial. They provided the first input needed to proceed in the research: we reasonably concluded that syntax activates a dedicated brain net. Relying on this result, then, we approached the problem of determining the boundaries of Babel from a neuropshychological point of view. When it comes to syntax, in fact, we knew what net to look for in the brain, in particular Broca's area. This result, however, was not enough to immediately design a feasible experiment; as in the previous case study, the limits and advantages of neuroimaging techniques must to be taken into account. More specifically, in a neuroimaging experiment we always need to compare a baseline with a variation, which in practice almost always translates into a comparison of at least two distinct tasks. This amounts to saying that there is no *direct* answer to any of these questions concerning the boundaries of Babel, since what can be measured—brain hemodynamic flow—is always differential. Therefore, our experimental question must be: What pair of tasks could shed light on the hypothesis of the biological nature of the boundaries of Babel?

We can eliminate one possibility right away. We cannot compare the brain activation regarding the knowledge of language of two subjects whose native language is different. The result would be unsurprising—at most we would see in both subjects the activation of Broca's area and other related areas, which would add very little to what we already know. We are not interested in knowing that a grammar, in particular a syntax, activates a certain neural net: this is now established. What we want to know if whether there is evidence for the neurobiological nature of the *limits* of the syntaxes of human languages in the functional architecture of the brain, that is, for the boundaries of Babel.

Let's review the reasoning that led us to our second experiment. We started from the conviction that on the basis of linguistic observations, the syntax of all human languages has specific properties that are autonomous with respect to cognitive, motor, and other components: it is discrete, recursive, hierarchically organized, it has a class of specific dependences (agreement, movement, pronominal coreference), and a filter that limits them that we called "locality" and to which universal abstract generalizations apply. For the sake of convenience, we have grouped the properties of hierarchy and locality of syntax under the label "structure dependence." Summing up, then, the starting point is this: All the syntaxes of human languages are structure dependent.

We can now sharpen the focus of our experimental question concerning the boundaries of Babel: Does structure dependence correlate with a dedicated neurological activity? There are different ways to approach this issue and no one can say *a priori* which one is right. The path we decided to take was a simple one, perhaps the simplest possible: *to test whether a syntactic rule that violates structure dependence activates the same neural net that is typically activated when the syntax of a natural language is used*. If the brain response is different, it would indicate that the restrictions on the syntaxes of natural languages have a determined biological matrix or at least that these restrictions are sensitive to it. Of course, we need a reasonably simple implementation of structure dependence to proceed in building the experiment—not an easy task). Chomsky (1991) effectively summarizes the idea that we can build grammars that violate structure dependence rules: "Knowing something about [universal grammar], we can easily design 'languages' that will be unattainable by the language faculty" (40). In a sense, the use of impossible grammars as tool for discovery partly resembles the use of impossible sentences that has characterized the development of generative syntax as compared to classical structuralism, as we saw in the "Error as Omission" section of chapter 1. In fact, *in the same way as impossible sentences have led to significant advancement in the understanding of the formal mechanisms behind linguistic competence, impossible languages may lead to significant advancement in the understanding of the neurobiological nature of the limits of variation across languages.* This will be the guideline of our second experiment.

Chomsky's conjecture concerning impossible languages was in fact at the origin of an experiment from a different field that I mentioned in the prologue. This made us confident that we were on the right track. Neil

Smith, professor of linguistics at University College London, in studying the behavior of an autistic boy whose only passion was to learn grammars of foreign languages, noticed that the autistic boy could not learn artificial languages that violated structure dependence (Smith and Tsimpli 1995). Autism is a very delicate, complex, and tricky pathology and not everyone has the same opinion about it. In particular there is no general agreement on the neuropsychological nature of autism. Moreover, a conclusion drawn from the study of a pathological condition cannot be used as straightforward support for a general theory of human language in normal subjects. Nevertheless, the boy's resistance to learning the artificial language that violated structure dependence was an important discovery and gave us a good feeling about the path that we were about to take.

Once we adopted this strategy, the most difficult issue was the design of the differential tasks that could give us measurable data about cortical blood flow, as required by the methods of neuroimaging. The central idea of the experiment was to have adult subjects learn foreign languages and to "hide" among the actual rules of the language rules that violate structure dependence. The ideal experiment would have been to have this task performed by subjects who did not already know any language—that is, babies—and follow their language acquisition by keeping them in an fMRI language all the time. Beyond any technical limit, obviously, such an experiment would have violated the most fundamental ethical principles and was not even taken into consideration.

Shifting to adults was not without theoretical risks. It has been known, at least since Eric Lenneberg's seminal work (1967), that adults learn a second language in a completely different way than children learn a first language: they never do it spontaneously but rather decide to do it, they rarely lose their foreign accents, and they follow a path toward the mastering of the grammar that is different from children's. Puberty is commonly assumed to be the line of demarcation between adults and children for language acquisition, and second-language acquisition proceeds differently before and after puberty. (The same is true for first-language acquisition if there has been a lesion that damaged grammatical competence). Thus, there is a window of opportunity within which the complete and spontaneous learning of a language can occur. Later, those acquiring a language must follow special conscious strategies, as any adult who has ever learned a second language knows very well (There is a vast literature on bilingualism: see Lenneberg 1967; Caplan 1992; Denes and Pizzamiglio 1999; Fava 2002; Birdsong 1999; Abutalebi, Cappa, and

Perani 2001; Wartenburger et al. 2003; Van Dongen, Loonen; Van Dongen 1985).

The risk for our experiment was that the neural nets dedicated to the acquisition of syntax would already be "frozen" in adult subjects and therefore would not show any activity because of the loss of neuronal plasticity that has been assumed to be typical of postpubescent subjects starting from Lenneberg's (1967) work. At this point, a very complex and uncertain issue arises which must take into account the results of our previous experiment. That experiment showed that processing syntactic data activates a complex net that involves cortical and subcortical areas. Anticipating the results of our second experiment, we will see that Broca's area in adults turns out to still be sensitive during language acquisition; in particular, it is indeed sensitive to the difference between rules that follow the principles of universal grammar and rules that do not. This will inevitably lead us to raise a series of new questions, including the problem of the "window period" hypothesis itself, at least to the point of not tying it to the loss of sensitivity in Broca's area. Let's now leave these questions for the conclusion and move to a step-by-step examination of our second experiment.

The central point was to design syntactic rules that violated structure dependence and to verify if the cortical activation while learning them was different with respect to natural rules, that is, rules that do not violate structure dependence. Of course, to prevent any bias, subjects had to be ignorant of this strategy: in fact, these rules were hidden among rules that did not violate structure dependence. The first empirical challenge was to build rules that violated structure dependence without making them too complicated, which would otherwise make them suspicious. The simplicity of the rules was in fact a crucial aspect of the experimental design. The task was itself already quite articulate: subjects first had to learn rules and then were taken to noisy neuroimaging machines where they had to lie down on a table inside a tight empty cylinder and look at a monitor by means of a little mirror very close to their eyes. Learn complex structures in those conditions would have been quite a venture. Also, everything had to happen quickly so that we could monitor the subjects' learning of a second language in real time. Moreover, the quest for simplicity contrasted with the very fact that structure dependence is an inherently complex phenomenon since it involves all the different structural aspects we have seen (recursion, dependence and locality). Structure dependence is manifest in agreement, movement, coreference, and locality; in fact, it is a pervasive trait of syntax in all languages. Designing sentences that violate all these principles might have yielded sentences too complex to be used as inputs to test our subjects. It was not complexity that was at stake, it was syntax.

The first issue, then, was to try to see if structure dependence could be formulated in a simple way. *The key step was to reflect on the most general property of structure dependence: no structure dependence phenomenon depends on linear order. This was the ideal property to rely on for the experimental design and it served as the foundation for designing the impossible syntactic rules of our experiment.* It is a simple view of structure dependence that does not make reference to the complex principles of hierarchy or locality, but it captures their essence. Research requires simplification, as we have already seen, and our version of structure dependence is a clear example of simplification but, needless to say, only experimental results could tell us whether it was well designed for our purpose.

Principle of structure dependence, simplified version No syntactic rule can make reference to the linear order of words.

This principle is very powerful and has pervasive effects on syntax. For example, it establishes that the rules of the syntax of natural languages cannot be arithmetic, meaning that the rules cannot be based on the number of a certain position within a sequence of words (for example, the first, second, or *n*th word), nor can they ignore phrase structure by targeting the whole string (for example, by inverting the whole sequence word by word), nor can they establish dependences between fixed positions in the string (for instance, the first and the last words in a string), nor on the number of words in a string, assuming that to know the number of words you must count them in line (that is, you must apply the notion of successor in line). Some clarification is needed, though. You surely do not expect to buy a grammar text of a foreign language and find a chapter about two-word sentences, another one for three-word sentences, for five-word sentences, and so on. But you may be misled by the format used to express linguistic rules in some natural languages, such as the famous rule of German syntax, the "verb second" or "V2" rule—according to which the inflected verb in main clauses always occurs in the second position. If the V2 rule is formulates such that the second position means the second word of the clause, this phenomenon could look problematic with respect to our principle. A more accurate formulation of the rule, and in fact the only correct one, would be to say that the inflected verb in main clauses is always moved to the head of the first phrase of the clause (CP, or complementizer phrase). In fact, the inflected verb can be the second, third, or fourth word, depending on how many words are in the specifier of the first phrase. If you say *gestern hat Johann ein Buch gelesen* ("John read a book yesterday," literally, Yesterday has Johann a book read), *hat* (third-person singular of the German equivalent of the helping verb *have*) is the second word that you run into. But, if *in Berlin* (in Berlin) replaces *gestern* (yesterday) in the specifier of the first phrase, the result is *In Berlin hat Johann ein Buch gelesen* and the inflected verb *hat* is now no longer the second word but the third. If the verb were always the second word, the sentence would be **In hat Berlin Johann ein Buch gelesen*, which is completely ungrammatical. In conclusion, this example from German shows us that we need to be careful with simplified formulations of structure dependence.

Let's go back to our experiment. It was performed with a 1.5 tesla fMRI machine; there were eight subjects—four women and four men, all righthanded and without neurological problems. Their average age was twenty-three and they had never been exposed to any language but German. They attended short lectures about a foreign language, and each subject went through ten testing sessions. Each session was preceded by a behavioral exam.[25]

The first experiment was performed by teaching the subjects Italian grammar. Their task was to decide whether the sentences that they saw on the monitor were correct or not, according to the rules they had learned, and then to push a button with their left hand. They had to learn six rules; three were actual rules of Italian grammar while the other three violated structure dependence.

25. For the sake of brevity, I will not illustrate another experiment that was done on impossible languages and was built on pseudo-sentences like those in the previous experiment, rather than on actual languages, as we are doing now. In some sense, this experiment is complementary to the one that we are describing here and is quite important for completing the experimental design on acquisition of possible versus impossible rules. In this other experiment, the subjects were not *taught* the rules, but had to *discover them by themselves*. The neuropsychological results converge with those of the experiment that we are describing here. In particular, together with the activation of the opercular part of Broca's area (common to both experiments) and of its homologue in the right hemisphere, we found that the dorsal premotor area, the left ventral area, and the left angular gyrus were all selectively involved. The only difference—and it was significant—was in the behavioral data: it did not take subjects longer to judge impossible rules. See Tettamanti et al. (2002); for a detailed critical analysis see Tettamanti (2003).

Let's look at these six rules, starting from the rules that follow structure dependence. They are all rules that are well known to native speakers of Italian and sharply distinguish Italian syntax from German syntax. The first rule stipulates that, unlike in German, the subject does not need to be expressed; this phenomenon is often called the "understood subject" in traditional grammar. In Italian you can say *Mangio la pera,* "(I) eat the pear," whereas in German you must say *Ich esse die Birne,* "I eat the pear." If this German sentence did not contain *Ich* (I), it would be ungrammatical.

This rule divides languages of the world into two equivalence classes: those that allow for subject omission (the majority of languages, including, for example, languages of different families, such as Italian, Japanese, Turkish, and American Sign Language) and those that do not (for example, English, German, and French). In fact, it was the correlation of this simple fact with a large class of apparently unrelated phenomena by Richard Kayne (1980), Tarald Taraldsen (1978), and Luigi Rizzi (1980/1982) in their pioneering comparative studies in the 1970s that led to the notion of parameters, established in Chomsky (1981). As stated in the first chapter, parameters constitute one of the two pillars of the most advanced grammatical system in contemporary linguistics, the "principles and parameters" system, formulated in the now-famous lectures that Chomsky gave at the Scuola Normale di Pisa in 1979 (see Chomsky 1981). Based on these works, linguists realized that some cross-linguistic differences could not be reduced to a universal system of principles. Therefore, it was assumed that the system of Universal Grammar kept degrees of freedom—the parameters—which were the only syntactic elements that experience could affect. Once the existence of parameters was recognized, a new world opened for research concerning the nature, format, and restrictions on the kinds of possible parameters.

The second rule concerned the structure of passive sentences. A passive sentence results from a transformation of an active sentence containing a transitive verb whereby the object or person who is acted upon becomes the subject of the new sentence (therefore triggering verbal agreement) while the person who performs the action becomes an optional element that is often preceded by a preposition. Italian and German vary with respect to the position of the main verb in their past-participle forms. Take the active sentence *Paul eats the pear*—in German *Paul isst die Birne,* in Italian *Paolo mangia la pera* (that both have the same word order as the corresponding English sentence). In German, in a passive sentence the main verb comes at the end of the sentence: *Die Birne wird von Paul gegessen* (literally, The pear is by Paul eaten). In Italian and in English), instead, you say *The pear is eaten by Paolo, La pera è mangiata da Paolo* with the same word order (Paolo mangiala pera).

The third and last possible rule that subjects had to learn concerned the structure of embedded clauses. In Italian and English, the word order in an embedded clause is the same as in the main clause, so when a matrix sentence such as *Paolo mangia la pera* is embedded under a verb like *dire* (to say), the embedded sentence will have exactly the same word order as the corresponding matrix sentence: *Pia dice che Paolo mangia la pera* (Pia says that Paolo is eating the pear). In German, by contrast, the inflected verb of the embedded clause must go at the end of the clause while, as we saw, the inflected verb of the main clause is the head of the first phrase: so when a matrix sentence *Paul isst die Birne* (Paul eats the pear) is embedded under a verb such as *sagen* (to say), the sequence is altered: *Pia sagt, dass Paul die Birne isst* (literally, Pia says that Paul the pear eats). In our experiment, our German-speaking subjects had to learn that, contrary to German, the order of the verb in an embedded clause in Italian does *not* vary with respect to the main clause.

Let's now move to the most delicate point: the "impossible rules"—the name we gave to rules that are never found in any human language—that we will use to build sentences that violate structure dependence. The first impossible Italian rule concerned negative sentences. Our German-speaking subjects were taught that negation in Italian is formed by always inserting the word *no* in the fourth position in a declarative string. Therefore, the negative of the sentence *Paolo mangia la pera* (Paul eats the pear) would be *Paolo mangia la no pera* (literally, Paolo eats the no pear); the sentence *Un amico di Paolo mangia la pera* ("A friend of Paolo's is-eating the pear"; literally, a friend of Paolo eats the pear) would be *Un amico di no Paolo mangia la pera* (literally, a friend of no Paolo eats the pear).

The second impossible rule was about interrogative sentences. It was very simple: subjects had to learn that in Italian interrogative sentences are formed by reversing the order of the whole sequence of words in declarative sentences. According to this rule, the interrogative version of the sentence *Paolo mangia la pera* (Paul eats the pear) would be *Pera la mangia Paolo?* (literally, Pear the eats Paolo?). Actually, changing the word order of *some* of the words in an affirmative sentence can yield an interrogative sentence in certain languages, such as in English sentences with helping or linking verbs or the copula. Thus *John has eaten the pear* becomes *Has John eaten the pear? John is fat* becomes *Is John fat?* But there is no known language where inverting the sequence of *all* words of a sentence can *always* lead to a well-formed different construction.

The third and last of the impossible rules concerned the indefinite article. Subjects were taught that the first indefinite article in an Italian sentence always agrees with the last noun in the very same sentence. Therefore, they learned that they should say *Una gnomo mangia una pera* (a.FEM.SG gnome.MASC.SG eats a.FEM.SG pear.FEM.SG; the agreeing morphemes are underlined here), in which the first indefinite article *un-a* agrees with the last noun *per-a* in gender and number (both are feminine and singular), rather than the correct form *Uno gnomo mangia una pera* (a.MASC.SG gnome.MASC.SG eats a.FEM.SG pear.FEM.SG), in which the first indefinite article *un-o* agrees with the immediately following noun *gnomo-o* (both are masculine and singular).

These were the rules that the subjects had to learn and then use to judge the grammaticality of the sentences that appeared on the monitor. Before looking at the results, I would like to complete the experimental picture by adding a second group of tests that were needed in order to disprove a possible objection that could have been raised if we had tested the learning of Italian only with-German speaking subjects. Imagine that we found differences in cortical activation between possible and impossible rules in Italian and in German. How could we exclude the possibility that the coincidence in activation between possible rules does not depend on the fact that Italian and German are members of the same language family, that is, Indo-European? (actually, this point was raised by an anonymous peer reviewer prior to the paper's acceptance; a residue of a historical explanation to language diversity).

Therefore, our second step was to repeat the same experimental design with a "far language," Japanese. I will briefly describe the rules that were used—first the three possible rules and then the three impossible rules, as before.

The first possible rule that subjects had to learn concerned the way in which main clauses are built. In German, the "non-marked"[26] order in the simple sentence *Paul is eating the pear* would be *Paul isst die Birne* (Paul eats the pear), where the object follows the verb. In Japanese, by contrast, the complement of a phrase precedes its head, and therefore, the

26. By non-marked I simply mean the most common one. In German, in main sentences phrases might undergo a partial rearrangement, so, for example, the word order might be OVS.

Japanese equivalent of our sentence would be *Paul wa nashi o taberu* (literally, Paul the pear eats, where *wa* and *o* are "syntactic markers" of the subject—or topic—and the object, respectively), where the object precedes the verb. In conclusion, the German-speaking subjects had to learn to reorganize the sentence in a way that was different from their native language.

The second possible rule concerned passive sentences. We already saw the German example. The Japanese equivalent sentence is *Nashi wa Paul ni taberareru* (literally, The pear Paul eaten, where *wa* and *ni* are syntactic markers of the subject and the prepositional phrase expressing the agent *Paul*, and the suffix *-areru* is added to the verb to form the passive form).

The third possible rule concerned embedded clauses. In Japanese, the sentence *Pia says that Paul eats a pear* becomes *Pia wa Paul ga nashi o taberu to iu* (literally, Pia Paul a pear eats that says). This "unnatural" order, as discussed in chapter 1) is the result of the fact that in Japanese, the head-complement parameter for phrases has the opposite value than it has in languages such as English and Italian (discussed in chapter 1).

Let's move to the impossible rules. The first two are similar to those we used for Italian and concern negative and interrogative sentences, respectively. We used the same strategies. The negative version of the sentence *Paul wa nashi o taberu* (literally, Paul the pear eats) would be *Paul wa nashi nai o taberu,* where the negation *nai* always occupies the fourth position in the sentence. The interrogative version of the same declarative sentence *Paul wa nashi o taberu* would have the reverse word order: *Taberu o nashi wa Paul?*

The third impossible rule was different from the one we used in Italian and similar to the rule for negative sentences because Japanese does not have articles. Subjects had to learn that to have a sentence in the past tense, the suffix *-ta* always had to be added to the fourth word in the sentence, no matter what the word was. Therefore, the past form of the declarative sentence we are using would be *Paul wa nashi o-ta taberu.*

Recall that the simplified version of the principle of structure dependence we are adopting here is that no syntactic rule can refer to the linear order of words. Our subjects had to deal with these grammars without knowing that some of the syntactic rules they had to learn violated this principle. Box 2.1 summarizes the possible and impossible rules in Italian and Japanese that our subjects—native speakers of German—had to learn.

Box 2.1
Possible and Impossible Rules in Italian and Japanese

I. Possible rules in Italian
1. Subject omission
2. Verb position in passive sentences
3. Verb position in embedded clauses

II. Impossible rules in Italian
1. Fixed position of negation in the sentence as the fourth word
2. Interrogative sentences invert the word order of declarative sentences
3. The first indefinite article agrees with the last noun

III. Possible rules in Japanese
1. Word order in the main clause
2. Word order in passive sentences
3. Embedded clauses

IV. Impossible rules in Japanese
1. Fixed position of negation in the sentence as the fourth word
2. Interrogative sentences invert the word order of declarative sentences
3. Past is formed by adding a suffix to the fourth word

The experiment was actually performed as follows.[27] First, subjects were taught a minimal vocabulary of one language or the other in order to exclude lexical learning from our learning task. For Italian, we taught thirty-three nouns, articles, and six first-conjugation verbs with their helping verbs. For Japanese, we taught twenty-one nouns and four verbs. Subjects were not given any information about the phonology of the words (essentially, they read them with their own German accent). Before we started the learning tasks, we verified that the subjects had actually learned their vocabulary. Once the experiment started, they had to push a button with a finger of the left hand after deciding whether the sentence followed the given rule or not. They were taught a new rule after a three-minute pause between sessions. Stimuli were presented on a screen for thirty seconds while subjects were lying on the fMRI table. The first image described the rule with some examples and each of the following images showed one sentence. Subjects had to judge the grammaticality of each subject on the basis of the rule they had just learned. Preliminarily, the experiment was performed on twenty other subjects without fMRI

27. The experiment on Italian was performed at the Institute of Diagnostic and Interventional Radiology, Friedrich-Schiller University, Jena, and the experiment on Japanese in the Department of Neurology at the University of Hamburg.

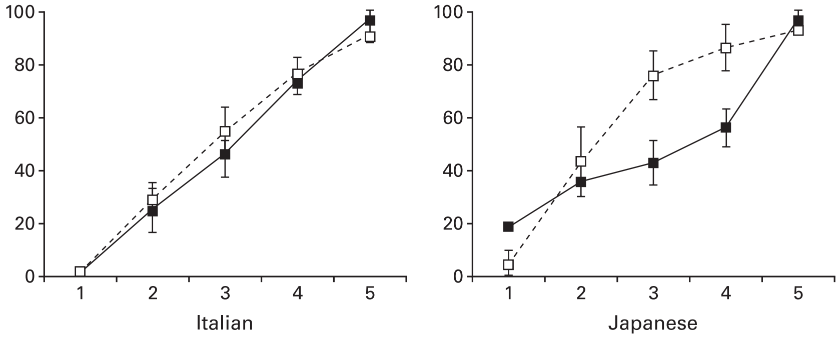


Figure 2.8
Percentage of correct answers to learned-rules questions about Italian and Japanese sentences. In these graphs, the percentage of correct answers is on the vertical axis and the number of the testing session is on the horizontal axis. The dark dots indicate answers related to the learning of possible rules and the white dots indicate answers related to the learning of impossible rules.

measures to verify the effectiveness of the learning and judging process. Let's see how subjects behaved and what measures we obtained.

For each grammaticality judgment, we first measured reaction time and accuracy. This kind of preliminary data is quite important because it helps in evaluating the effect of cortical activations. For instance, it is useful to make sure that a specific area is activated not by the level of difficulty of a task as compared to another but by the intrinsic difference among the tasks. The graphs in figure 2.8 show the percentage of correct answers according to the learned rules and distinguish between Italian and Japanese and between possible and impossible rules.

The first fundamental piece of data: The percentage of subjects' correct answers was identical for both possible and impossible rules in both languages, as clearly shown by the convergence of the two lines at the end of the fifth testing session on both graphs. *Following or violating structure dependence is, therefore, irrelevant when it comes to learning accuracy: subjects reached the same level of mastery with both possible and impossible rules.*

A second piece of data is that in Japanese the number of correct answers with impossible rules seems to increase more quickly than the number of correct answers with possible rules. We do not have a convincing explanation of this difference, but it does not affect the final converging result. It is not implausible to think that the "phonological" and morphological component may have been a disturbing factor in Japanese but not in Italian. In any case, there was full convergence in the end.

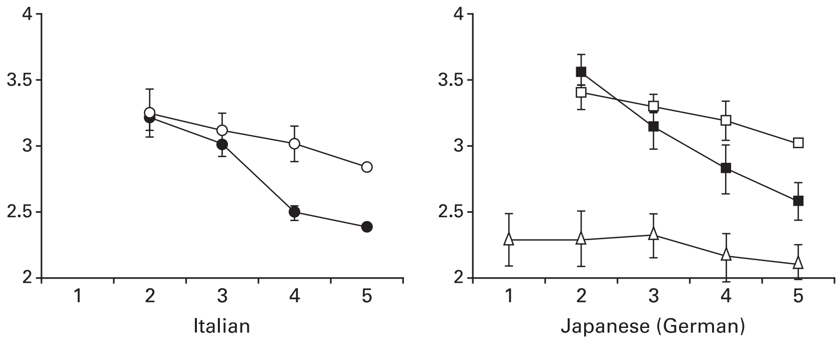


Figure 2.9
Reaction times to judge grammaticality of a sentence. Testing session progression over time is reported on the horizontal axis, while reaction times (in seconds) are on the vertical axis. Dark and white dots refer to possible and impossible rules, respectively.

Figure 2.9 shows reaction times—the speed at which subjects judged the grammaticality of a sentence on the basis of a given rule as compared to subjects' reaction times in their native language, German.

We need to evaluate these graphs carefully. The subjects' reaction time to judge German sentences, shown in the lower part of the graph on the right, does not vary significantly. Of course, it was no surprise that subjects' reactions to native language do not vary much, since they did not have to learn anything other than getting used to the task. Just getting used to pushing the button might have helped a bit during the last sessions.

In the cases of Japanese and Italian sentences, subjects' performance improved with each session and the time they needed to decide whether or not a sentence followed the rule that they just learned decreased progressively. However, it is immediately evident that subjects took a little longer to react in the case of impossible rules, though the differences were always less than a second. The reason for this difference is not obvious. We will speculate on this point after presenting the neuroimaging data. It is important, however, that in all cases the reaction times decreased significantly.

We have finally arrived at the "executive" part of the experiment—when brain blood flow was measured in order to deduce aspects of the functional architecture of the cortex (though within the limits that we have already discussed) while learning rules that followed structure dependence with respect to rules that do not. *For the sake of clarity, let's summarize the central assumptions constituting the experimental hypothesis: first, structure dependence is biologically determined; second, Broca's area is selectively activated when processing natural language syntax (within a complex neural network).* If these assumptions are true, one expects them to converge, that is, Broca's area to behave in a different way when the subject is exposed to possible versus impossible rules. More specifically, in the best of all possible worlds, Broca's area will be activated with rules that follow structure dependence and remain inactive with rules that do not. This is all that the experiment can tell us. Now let's look at the details.

Figure 2.10 constitutes a synthesis of the central results: it shows three fMRI images (sagittal, axial, and coronal views) of the part of the cortex that was activated during the task of judging the grammaticality of sentences in Italian according to the rules the subjects were taught. Next to the images are eight graphs, one for each subject. The horizontal axis depicts the statistically normalized accuracy of the answers: the farther away from the intersection of the two axes the value of the answer is, the more accurate it is. The vertical axis represents the variation in metabolic activity in Broca's area, measured as the variation of oxygen in the blood flow, called the BOLD (blood oxygen level dependent) effect. The higher the BOLD effect, the more metabolic activity was observed, and the greater the use of energy and thus neuronal activity on the cortex at Broca's area.[28] The black dots correspond to the answers given to sentences on the basis of possible rules and the white dots correspond to the answers given to sentences on the basis of impossible rules. The lines between the dots in each graph are the interpolation of the values represented by the black and white dots and give us an immediate visual idea of the variation of blood flow in Broca's area in relation to the accuracy of the answers: dashed lines refer to impossible rules, continuous lines refer to possible rules. Let's start with the data that the eight native speakers of German produced when they learned Italian.

Looking first at the three cortex images, we see that the activated area is, in fact, Broca's area. This fact immediately gives us the first important piece of information: Broca's area is also activated in adults who are learning a foreign language. In other words, the relevant cortical activity

28. It is reasonable to assume that variation of the oxygen level is related to variation in the quantity of regional cerebral blood flow (rCBF).

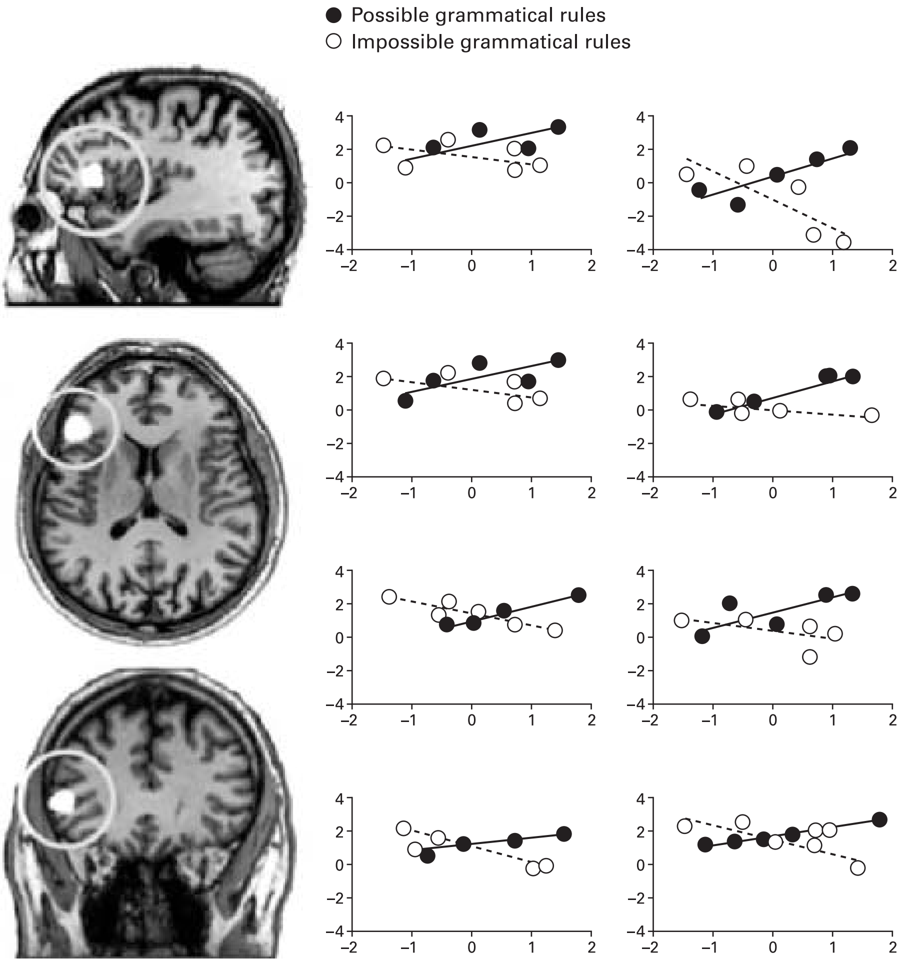


Figure 2.10
The graphs show that, when subjects are judging the grammaticality of Italian sentences, the more accurate the answers are the more Broca's area is activated with possible rules and the less it is activated with impossible rules.

triggered by an unknown language coincides with the activity that is expected in performing syntactic tasks in a known (not necessarily native) language. Let us now consider the other graphs.

By looking at the lines, which interpolate the values of the dots, we realize that the more accurate the grammaticality judgments (the horizontal axis) are, the more Broca's area (on the vertical axis) is activated with rules that follow structure dependence and the less it is activated with rules that do not. *The brain has "sorted out" the syntactic data, without the subjects' realizing it: Broca's area, which is included in the network that is naturally predisposed for syntactic tasks, has been progressively activated when processing rules that respected structure dependency while it has been progressively deactivated when processing sentences that did not.*

A natural question at this point is: What area of the neuronal net is tasked with dealing with those rules that do not follow structure dependence, those we called *impossible* rules? Our experiment does not give clear results, and this in itself is interesting: There does not seem to be an area that is dedicated to processing rules that do not follow structure dependence. A work on artificial languages (Tettamanti et al. 2002, note 25) that borrowed the methodology of the first experiment described in this book, based on pseudo-sentences, has given significant insights on this rather murky and delicate question. The learning of possible and impossible rules based on pseudo-sentences involved largely overlapping neuronal nets that include front parietal areas in both hemispheres, although Broca's area is activated only with possible rules, converging with the data we obtained from testing real languages. Therefore, it is not implausible to imagine that the neuronal net that processes impossible rules in the experiments on Italian and Japanese involve the very same front parietal lobe but without activating Broca's area, which would align with the results of Tettamenti et al. (2002).

We repeated the same experiment with the learning of possible and impossible rules in Japanese by German-speaking subjects. The results were the same: as the accuracy of the answers increased, the more Broca's area was activated with rules that follow structure dependence and the less it was activated with rules that do not (see figure 2.11). The data from this experiment make moot the objection that German and Italian could activate the same cortical areas due to "historical" similarities between the two languages, which are both members of the Indo-European language family, and not because of the distinction based on the principle of structure dependence which was discovered by means of comparative analysis

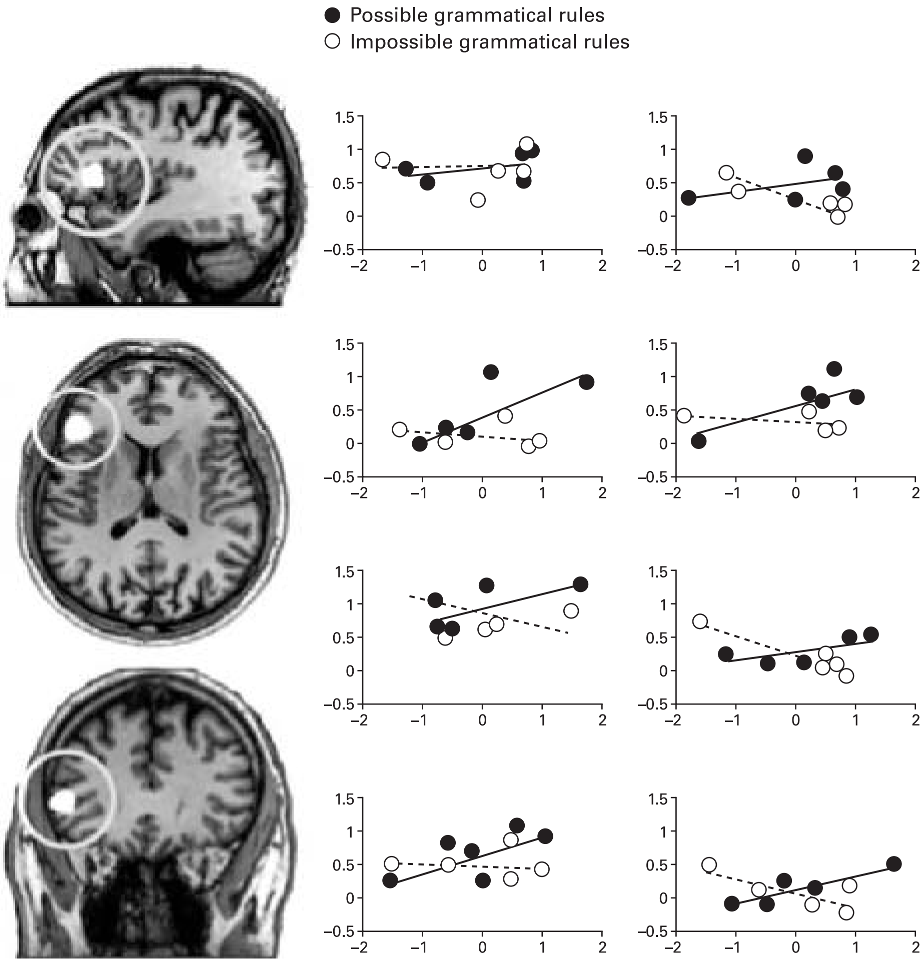


Figure 2.11
The graphs show that, when subjects are judging the grammaticality of Japanese sentences, the more accurate the answers are the more Broca's area is activated by possible rules and the less it is activated with impossible rules.

within generative grammar. Interestingly enough, there is a 12-millimeter distance between the two areas that were activated for the two different languages (see Abutalebi, Cappa, and Perani 2001 and Abutalebi et al. 2007 for bilingualism and related neuroimaging results).[29]

At least two significant inferences can be made on the basis of this data. First, the variation in Broca's area activity in relation to the possibility or impossibility of the rule supports the hypothesis of a biologically determined guide to the learning of syntax. Otherwise why should cortical activity decrease with rules that violate structure dependence if this principle were just a generalization resulting from historical accidents? In other words, why should cortical activity be affected at all if the fact that no language has rules that violate structure dependence was just by chance given that no difference in complexity is involved? In fact, this first consequence of the experiment has a twofold impact that is both epistemological and empirical. First, the convergence of the results from the study of comparative linguistics and the study of functional architecture of the brain is once more empirically confirmed. Second, we have unexpected evidence that the adult brain is still able to react to learning with a certain amount of cortical differentiation that involves Broca's area in a significant way. Interestingly, this fact cannot be easily reconciled with the well-established so-called "window of opportunity" hypothesis of language acquisition, which we referred to before by referring to Lenneberg's studies. In addition, when we examined blood flow to the brain we found that the learning of impossible rules was not associated with any specific cortical activity. Although it is difficult to fully understand the significance of this fact, it is not implausible to think that the absence of specific cortical activity for impossible rules may have played a role in shaping the grammars of human languages, by eliminating entire groups of conceivable grammars. One possibility to be explored is that the neuronal net that is involved in processing impossible languages is already employed for other cognitive activities, resulting in an overloading of neural activity in a certain brain network. It goes without saying that this is speculation; the only robust result here is that the rules that do not follow structure dependence are not processed by the same natural network that normally processes syntactic structures, getting rid of historical, sociological, or "conventionalist" explanations.

29. In the original images obtained through the fMRI process, the results from Japanese were indicated together with those of the preceding measure on Italian sentences: for technical reasons, this comparison is not shown in figure 2.11.

To summarize, we started with an important result from theoretical linguistics, based on the comparison of a sufficiently large number of languages: the principle of structure dependence, according to which no rule can be based on the linear order of a string of words, is universally valid. What matters is hierarchical organization, not linear order. Then, we wondered whether this universal property had a predetermined biological nature. Relying on the experimental methodology of fMRI neuroimaging, we built two different tasks to compare cortical activities while the tasks were performed. Rules that do not follow the principle of structure dependence (suitably designed in a simplified format) were built by manipulating the grammar of Italian and Japanese. These impossible rules were then added to a set of possible rules that the subjects, native speakers of German, had to learn but without being given any knowledge of the difference between the two types of rules. Though subjects did not show any relevant behavioral difference in learning the two different kinds of rules, their brains automatically sorted them out by activating Broca's area only for the rules that followed the principle of structure dependence (see Marcus, Vouloumanos, and Sag 2003b for a critical comment on this experiment).

As so often happens in science, this result may generate more questions than it answers. But it would be hard to deny that the convergence of linguistic models and functional brain architecture is surprising: there was no a priori guarantee that these data should ever lead to a convergence. Notice that I am just talking about "convergence." It would be quite different to talk about "unification." At this stage in the research—due to theoretical and experimental limits—talking about unification would be not only premature, but even dangerous, in a sense. We can't know if we will ever arrive at unification. It is not even clear whether and how the two disciplines will need to be radically redesigned before closer comparisons can be made. Similarly, at the beginning of the twentieth century, the unification of chemistry and physics occurred only after classical physics moved to quantum physics. The difference in complexity between the million of billions of synaptic contacts and the observation of blood flow in a cortical area is such that it is impossible to know what research in this field will look like in the future, or whether linguistics can ever be turned into a branch of neurobiology. We may need to go through radical changes in order to look at both language and the brain in a way that unifies them. Although it is unclear whether this will ever be conceivable, the very fact that new questions can be raised is, I believe, rather encouraging.

2.4 Why Isn't There a Mendelian Linguistics (Yet)?

The reader may have noticed by now that throughout this discussion I have not talked about genetics at all, except for a cursory mention of the *EMX* gene family. This omission was not by chance. Reading about linguistics, it is easy to come across claims such as that the generativist program is actually a research program on the "genetic endowment" of our species with respect to language. Nothing more premature could be said, unless we add a clarification.[30] Peter Medawar, the 1969 winner of the Nobel Prize for Medicine or Physiology, conveyed this delicate point concerning genetics and linguistics much better than I ever could when he said (Medawar and Medawar 1983, 9),

> One of the gravest and most widespread aberrations of geneticism is embodied in the belief that if any characteristic is enjoyed by *all* individuals of the community, it must be genetically underwritten. Thus, if it should turn out that a certain basic linguistic form such as the Aristotelian subject/predicate form is an element of all languages of the world, then its usage must be genetically programmed. (Some of Noam Chomsky's writings are not guiltless of this assumption, which is also a disfigurement of sociobiology as it steers its precarious course between the twin perils of geneticism and historicism.) It may be well to repeat in this context the reason why the supreme canon of geneticism is not satisfactory: if any trait is to be judged "inborn" or genetically programmed, then there must be some people who lack it. The ability to taste phenylthiocarbamide, for instance, is known to be genetically programmed because there are those who lack it.

It is now clear how we should reconsider the attempt to investigate the genetically determined capacities of the human species with respect to language. At the present stage of linguistic research, there are no data that allow us to identify aspects of syntactic competence that satisfy the requirements that Medawar was talking about: aspects that are absent in groups of individuals or whose statistical behavior does not satisfy Mendelian criteria. We are quite far from having a "Mendelian linguistics," although in principle nobody can exclude a priori the possibility that we will eventually reach this point. It is true that syntactic research has illuminated systematic differences among linguistic groups. Nevertheless, as we saw, not only are these differences reducible to few parameters (at least potentially), but, even more important, no one has yet found an individual within a given linguistic group who does *not* exhibit a syntactic

30. Another example of this overeagerness is the controversial interpretation of Myrna Gopnik's data, which we will discuss later, which stirred up much enthusiasm at the time of publication in 1990.

feature that can be genetically transmitted among the individuals of the same family. A parameter is definitely a difference, but it is certainly not a significant difference in a Mendelian sense. Actually, it is the opposite by definition: it is a difference that is systematically *shared* by all members of a family if they speak the same language.

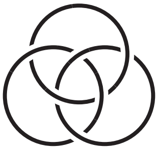
Certainly, no one could reasonably deny that the ability of *Homo sapiens* to acquire a language by instinct has a genetic base: Every organism trait that is nondependent from the experience is ultimately a consequence of gene regulation or of the impact that environment has on the degrees of freedom that the genes themselves allow for; it's almost tautological. Simply put, the situation concerning grammar and in particular syntax is at this point far too complex to give scientifically relevant results in a Mendelianly significant way. As far as the genes that play a role in the structure that underlies grammar are concerned, we must remain at the level of pure speculation and perhaps constrain ourselves to speak more generically of a biologically rather than genetically determined guide. Of course, it is well known that there are linguistic pathologies within families, but they mainly concern language components that are completely peripheral, such as the pronunciation of certain sounds and stuttering, which epidemiological data suggest have a genetic component.

On the other hand, we cannot ignore the fact that among all linguistic pathologies, purely syntactic linguistic pathologies of syntax—as syntax is defined in chapter 1—are basically absent (see, among others, Caramazza 1994; Denes and Pizzamiglio 1999; Fava 2002). Certainly there is agrammatism, mainly due to focal lesions, which causes the omission of many functional words (articles, helping verbs, copula, and so forth) and radical morphological simplification (on the latter point see Kean 1985). However, to the best of my knowledge, no one has isolated specific damage to aspects of the syntactic system that would be expected if a model like the generativist one is adopted: there are no people who do not apply the principles of recursion or locality, mix the constitutive elements of phrases, or randomly move structural elements in sentences (running against locality). Data in the literature suggest syntactic damage, but it is never completely independent of the semantic component (see Grodzinsky 2000, and the commentary on this work in Cappa et al. 2000; see also Cotelli et al. 2007 for a recent approach to syntax in neurological patients). Syntax, then, seems really hard to destroy. Not even the passage of time seems to touch syntax, in spite of the ravages aging inflicts on the body and on some cognitive skills such as memory. The principle of structure dependence that characterizes all the rules of human language survives throughout an individual's life. What, then, can we say about the absence of selective pathology in syntax? More specifically, how can we interpret this fact if we think that human language results from the execution of a biologically determined project, that is ultimately of genes?

One interesting possibility is to assume that the genes that contribute to the formation of the cortical structures that are involved in language acquisition are at the same time expressed in building other organs (pleiotropy) and—which is crucial—in building vital organs. The inactivation of even just one gene would have disastrous consequences on the embryonic development, resulting in miscarriage, and survival of an individual physiologically, although these consequences would not be disastrous at all for the species as a whole.[31] Rather, such an interlacing of grammar and vital organs would bring an important advantage: it would be impossible to have human mutants lacking language—individuals who are identical in every way to nonmutant human beings except for their inability to use syntactic rules to express themselves. Notice that I am not referring to the pathologies that concern "peripheral" portions of language such as muteness and deafness. It is now well known that people who are deaf

31. Independently from the hypothesis that language genes might be expressed in vital organs, a further complication should be considered. More specifically, it is also reasonable to assume that genes do not cooperate in a purely "associative" way like addenda in a sum: if one of them is missing (or inactive) the entire result may be compromised.

A figuative way to think of it is offered by the so-called Borromean rings, from the coat of arms of the aristocratic Borromeo family (counts of Arona on Lake Maggiore in Italy since the mid-fifteen century), probably coming from Francesco Sforza, Duke of Milan, who got it from Cremona, his wife's city symbol. These rings have a curious property that has been studied in mathematics: the three rings cannot be separated from one another, but if any one of the three is taken away the other two would be separated:



Genes may cooperate like Borromean rings: taking one away might compromise the entire bond (see Fox 1962 for a mathematical treatise on Borromean rings).

and mute from birth express themselves, by signs rather than words, with exactly the same linguistic complexity and richness as people who are able to use voice to communicate. Also, the differences between one sign language and another are no smaller than the differences among spoken languages—an important advance over what was believed in the past (see, among others, Neidle et al. 1999; on Italian sign language see Geraci 2002). Rather I am referring to the core aspect of grammar, crucially including syntax in the way it was illustrated it in chapter 1, that is, a structure dependent recursive component.

There is, however, another option to consider, when it comes to genetic foundations of language. We need to be aware that research on language genes could somehow be misled by some tacit assumptions. In fact, there are at least two distinct assumptions that need to be spelled out: one is that language corresponds to a unitary phenomenon; the other is that each phenotypic trait of an organism is the result of a dedicated set of genetic instructions selected by evolution for some function. Neither of these perspectives may be correct. The latter one was argued against in the seminal work by Gould and Lewontin 1979 (see also Gould 1997). In Gould 1997, it is said that they borrowed "the architectural term 'spandrel' (using the pendentives of San Marco in Venice as an example [figure 2.12]) to designate the class of forms and spaces that arise as necessary byproducts of another decision in design, and not as adaptations for direct utility in themselves" (10750).

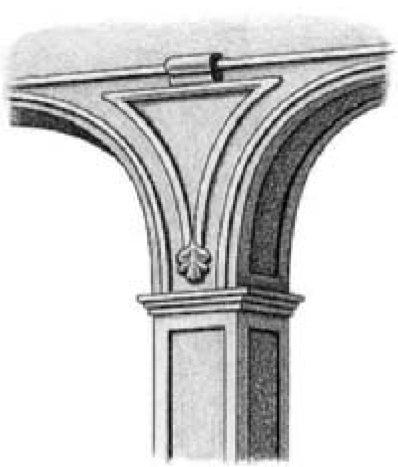


Figure 2.12
The spandrel is the space between the two arches.

In biology, we can for example think of an armpit as a "spandrel": there are no armpit genes; this biological feature is just the effect of the combination of independent genes dedicated to other features.

The neuropsychological structure that allows the existence of language in the brain could just be like a spandrel, or an "armpit of the brain"; it could have arisen as a necessary consequence of other selected features, rather than assembled directly, piece by piece, by natural selection. The genes underlying the neuropsychological structure that makes human language possible might be involved in the expression of traits that have nothing to do with language (and communication).

But there is an even more radical possibility to be considered here. To do so, I would like to resort to another image, a famous figure that is often referred to in psychology: the Kanizsa triangle (see figure 2.13), named after the Italian psychologist Gaetano Kanizsa (see Kanizsa 1955, 1979). In the following picture, we perceive a triangle even if none has been drawn. The triangle emerges in our mind from the way our brain "completes" the picture.

This triangle is often cited as an extreme example of the basic tenet of the Gestalt psychology, namely that we perceive objects as a result of their emergent properties.

The idea that I would like to suggest by referring to this figure—although counterintuitive and radically far from the deep-rooted assumption that language is what human nature is essentially built on—is that *language itself could just be like the Kanizsa triangle: we perceive it as an "object," that is, a unitary and real phenomenon, but in a sense it is not even there—it is a perceptual artifact.* As in the case of the spandrel, it might well be that what we perceived is in fact the "byproduct of another

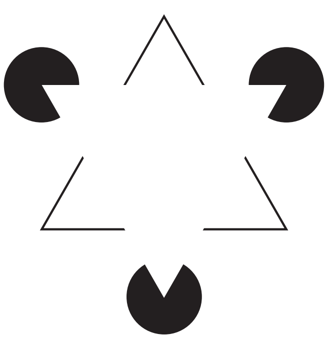


Figure 2.13
Kanizsa triangle.

decision in design," but there is a crucial difference here: in that case, the object—the spandrel—exists independently of our perception; in this case, instead, it does not. It is as if the spandrel itself, so to speak, were just a mirage emerging from the way we perceive things. Of course, this hypothesis makes sense only if we do not identify language with communication: it would be hard to deny the existence of communication independently from our perception. Rather, I am referring to language as a formal object, the intricate net of regularities that we call grammar. Communication exists in fact even without grammar and outside mankind.

This could be the reason for the absence of Mendelian, statistically relevant evidence for language genes: unlike other phenotypical traits, language may lie hopelessly outside the reach of scientific inquiry when it comes to genetics because (part of) it is construed—or completed—by the human mind. At the moment, there are no strong empirical reasons, I believe, to choose one scenario over the other (that is, language-specific genes, pleiotropic genes, the "spandrel effect" or the "Kanizsa effect" (KE) for language).

Some attempts have been made to study the genetic basis of human language, although the results have not been particularly encouraging. Among all the studies in genetics and linguistics, it is worth recalling those that concern the gene called FOXP2 (see Enard et al. 2002; Lai et al. 2001; Lai et al. 2003; for a critical comment see Marcus and Fisher 2003a). The story of the presentation of the linguistic role of this gene is particularly interesting. In the early studies the authors talked cautiously about the effects on this gene on speech. Later, though, language and grammar were also invoked. The data are crucially based on the examination of three generations of a family, called the KE family by convention, in which half of the members (about fifteen individuals) were affected by severe speech and language problems whereas the other half did not exhibit any of these symptoms. For instance, half the family could not pluralize invented nouns. They were able to complete a sentence like *Many boy_ run* by adding the suffix *-s* to *boy* to get the plural form *boys*. But they were unable to do the same with an invented noun such as *wug*. When faced with the sentence *Many wug_ run*, they were not able to add the plural suffix and get *Many wugs run*. It looked as if they learned regular plural forms in the same way as they learned singular nouns (*child*) or irregular plural forms (*children*). These data, originally collected by Myrna Gopnik (1990), were immediately interpreted as a form of genetic "feature-blindness" and were attributed, without discussion, to exclusively grammatical damage. Subsequently, the data were associated to a portion of a nonsexual chromosome of the genome that was called FOXP2 (Lai et al. 2001) and led some scholars to believe that the first "grammar gene" had been found.

A more accurate reanalysis of the data has led to the conclusion that the pathological subjects did not exhibit just a selective grammar deficit but also a severe impairment of motor coordination of facial muscles (oral-facial dyspraxia) and language impairments that were much more severe that just the inability to recognize morphemes. For instance, they could not distinguish between words and pseudo-words, they had trouble with complex sentences, and they were unable to correctly produce even nonlinguistic sounds. People started to wonder whether the oral-facial dyspraxia was the actual cause of the language deficit and speculated that the altered gene was not selectively connected with grammar but that its expression was much more general (see Vargha-Khadem et al. 1995).

Gary Marcus and colleagues (2003a), in a critical review of works related to FOXP2 and on the expectations for it within the scientific community, commented on the hasty shift from thinking of the damaged FOXP2 gene as causing damage to language and grammar to assuming that it caused a more general damage: "FOXP2 cannot be called 'the gene for speech' or 'the gene for language.' It is just one element of a complex pathway involving multiple genes, and it is too early to tell whether its role within that pathway is special." Clearly, we are, unfortunately, still quite far from a "Mendelian linguistics" and progress may require a radical rethinking of the linguistic model that we are adopting.

Let's summarize this chapter. Although we have not reached the unification between linguistics and neuroscience—something that might require a radical rethinking of both disciplines—we have reached a surprising convergence between them: linguistic theories, based on the comparison of grammatical regularities across different languages, turned out to be compatible with neurobiological results in a systematic way. The central point is that speakers' brains selectively show sensitivity in Broca's area for syntactic rules that respect the principle of structure dependence and not for rules of any conceivable format, although the speakers themselves are unaware of any theoretical distinction. This leads to significant conclusions, some of which we have highlighted. The impact of these findings on our representation of the way the brain works in general is not negligible. The brain has often been compared to a piece of computer hardware, and grammar to a software program, as if the brain were neutral with respect to a program that can be learned (see Johnson-Laird 1988; for a critical discussion see Searle 1980, 1990). At this point, no one can guarantee that this view is correct. Actually, the data seem to lead in the opposite direction, at least as far as natural language acquisition is concerned: when it comes to grammar, *structure-dependent syntax is not just one of the possible softwares that this hardware, the human brain, may run, rather it turns out to be the only software that it construes*—making the hardware-software metaphor itself meaningless when it comes to the relationship between the physical brain and syntax.[32]

What can we conclude from this tortuous journey toward the boundaries of Babel? We have taken just a small step by providing evidence that the brain is sensitive to the format of syntactic rules, thus suggesting that a mere historical, sociological or in conventionalist explanation for the absence of non-structure-dependent grammar is not tenable. This step may be very small, but we can now confidently face the boundaries of Babel from quite a different perspective, the neurobiological one. *The enigma of impossible languages has not been solved, it has been so to speak naturalized.*

The results presented here raise many hard and quite murky questions, at different levels: empirical and theoretical. A first question surely touches on the main issue raised in this book, although from quite a new perspective. We started by asking whether the limits of Babel are historical or neurobiological, and we found that those languages that are outside the boundaries of Babel are also outside the normal neural network that the brain uses for language processing. But in studying this, we also discovered that subjects were indeed able to learn all types of syntaxes, albeit by using different networks in the brain. We face then a new enigma: what blocks the activation of different networks to process language? Other questions have been raised by the results of our experiments, for example those concerning the limit to spontaneous language acquisition:

32. In principle, there is no reason why this cannot apply to all cognitive processes that involve structures that are organized hierarchically and recursively, as is the syntax of human languages. Natural candidates are music, counting skills, temporal and spatial orientation and representation (perspective), and perhaps others (See Hauser, Chomsky, and Fitch 2002, and for music Patel 2003). Actually, if this proves to be true, we could even bring ourselves to look at this capacity as a pervasive and *supramodal* one, meaning that it is present in many different functions. For a discussion that develops the line of research presented in Moro et al. (2001) and Tettamanti et al. (2002), see Tettamanti (2003).

what blocks it in adults if Broca's area stays active in them while learning a foreign grammar, as we discovered while testing German native speakers?

I will just stop here. The experiments described above may eventually raise more questions than they answer, but this should not stop research. Actually, I share with many the impression that what really matters in science is not having the right answers so much as it is having the right questions: that is, the questions that, in principle, make us think of a way to obtain an answer.

In the next chapter, we will cautiously step into some of these new and controversial fields.

3 The Form of Grammar

In the previous two chapters, we saw that the grammars of human languages cannot vary freely, particularly with respect to syntax. The class of possible human languages is dramatically constrained by complex general principles such as structure dependence, which does not have an immediate counterpart in other cognitive domains. We also verified that these boundaries are not just generalizations of accidental, that is, purely historical, conventional linguistic regularities, but that they correlate with specific neuronal activities. These facts raise new questions that were unthinkable just a century ago when the notion of limit of a grammar was virtually inconceivable, aside from philosophical speculations. Why are there constraints on the kind of syntactic structures that the human mind can compute? Why, for instance, are there not syntactic rules based on the linear order of words? Why, instead, do all the rules of all languages have to follow structure dependence? Are there any advantages or disadvantages that these constraints give to our species? Why are the constraints exactly *these* and not others? Can some aspects of these constraints be reduced to human biological structure?

In this last chapter of the book, we will focus on these questions. Unlike in the first two chapters, however, where I summarized, by means of an essential "sample," some fundamental results of the research in syntax in the past fifty years and described two neuroimaging experiments, here I will make hesitant steps over the quicksand of speculation, where the boundaries of Babel are even more uncertain. I will not refer to well-established data. Instead, I will discuss some critical aspects of current research and illustrate some perspectives on research that may become more relevant in the next few years. Only time will tell whether these observations will be of any value.

This chapter is divided into two sections, corresponding to the two core issues that underlie the questions I just raised. *In the first section I will deal with the reasons for the existence of any constraints on the syntax of* *human languages. In the second, I will focus on why syntax has these specific constraints—a conceptually different issue.* In order to address this issue in concrete terms, I will briefly illustrate a still-debated theoretical model on the linear nature of the human linguistic code and on its impact on syntax. On this path, therefore, I will no longer deal with the direct relationship between linguistics and the neurosciences. Instead, I will stay within linguistics, but without forgetting the biological perspective supported by the new data coming from neurosciences.

3.1 Logical versus Learnable, or Why Do Languages Have Rules?

Let's return to the first two questions. Why are there constraints on computable syntactic structures? Why are there no grammars that do not follow structure dependence? These questions raise an even more radical one: Why is there syntax at all? Could we not communicate with a grammar that assigns a distinct meaning to *any* possible sequences of words?

In order to better understand this question, let us return to a concrete example from chapter 1, where we took a look at a sample of syntax: *I think that Mom wants to talk [with this nurse] before meeting [with the chief doctor].* We observed that only one of the two bracketed phrases produces a grammatical sentence when it is moved. While it is possible to say *[With which nurse] do you think that Mom wants to talk before meeting [with the chief doctor]?*, the following is certainly ungrammatical: **[With which chief doctor] do you think that Mom wants to talk [with this nurse] before meeting?* Would it not be easier, more "logical," to be able to move both phrases? For sure, it would not be obviously nonsensical because, although ungrammatical, we can imagine what a hypothetical speaker would mean with the second question. From the ease-of-communication point of view, one could reasonably argue that the more structures you can generate, the more options you have for conveying information. Nevertheless, there is a constraint, and it is pretty clear. To repeat, why then are there constraints to computable syntactic structures?

It could be that the constraints are just accidental, that they do not serve any purpose. This happens continuously in nature. Let's take an example from a field other than linguistics, such as anatomy: Why can't we turn our heads 360 degrees?[1] Wouldn't it be easier, more "logical," for a human being to have a panoramic view without moving the whole body?

1. I owe Orin Percus this specific example for illustrating the idea that grammar is a theory of the constraints of conditions which are similar to anatomophysiological conditions.

We could avoid attacks from behind, for example. It does not take much to realize that this question is nonsensical from a biological point of view. The structure of the cervical vertebrae—the complex system of bones, muscles, and nerves that holds the head—is not a perfect structure designed for a purpose, but just *a* solution, perhaps the best solution, that owes its existence to the structural conditions imposed by the physical world—one of these being gravity—and by the limits and the degrees of freedom of the structure of the bones, muscles, and nerves of the human body. These limitations in turn are imposed by our genome and have accumulated, for different reasons, including accidents over thousands of years of evolution (on the impact of natural laws on the shape of organisms, see the classical work by Thompson 1917 and the comments by Gould 1977a, 1977b). One could object by saying that these limits can still be genetically selected and that, therefore, it would be expected that new mutations may improve the human body so much that, some time in the future, the head will actually be able to completely turn or look back more easily. No serious biologist would subscribe to this expectation: it would be much like expecting that a certain species will evolve wheels instead of legs to run faster. We know that evolution does not proceed according to a project of "global improvement." Evolution is completely random and local; it proceeds through local mutations that may give rise to advantageous or nonadvantageous phenotypic differences. It is often said that environment will select the most advantageous mutations. But the issue is even more complicated. Some genes are expressed in different parts of the organism, a phenomenon we already mentioned called *genetic pleiotropy*. Therefore, a mutation that is favorable in one respect can easily co-occur with a mutation that is unfavorable in another respect. The balance between an advantage and a disadvantage is what determines whether the descendents will carry that mutation. Our idea of mutation, and therefore of evolution, must incorporate the fact that it is precisely when the disadvantage is not too damaging that the mutation is preserved, tolerated (see Gould 1977a for an in-depth discussion of this and related issues).

To these considerations we need to add a further element of complexity that we cannot disregard. A phenotypic feature of a biological organism that now performs a certain function may not necessarily have been selected for that specific function at the very beginning. Insect wings are a famous example that biologists often mention (see, however, Wesson 1991 for its controversial status). When insect wings first appeared, they were not big enough to support body weight and allow for flight. Why, then, has evolution increased wing surface up to the point that wings turned into an effective tool for locomotion? Obviously, we cannot think that random, local mutations could foresee this future function: selection acts here and now and does not look ahead. In fact, wings developed as heat exchangers; their vibrations were used to fan the surface of the insect, and only when they *by chance* reached a sufficiently wide surface by successive mutations could their vibrations also be used for flight. Two different evolutionary steps involving the same biological structure, one following the other, were guided by two different functions that partially overlapped. The old Lamarckian adage according to which the function modifies the organ is in a sense turned upside down: a function develops when the organ is modified by chance.

From the theory of this phenomenon, which the biologist Stephen Jay Gould called *exaptation,* and from other similar theories, we learn that an element of the body of an organism that performs a function may not have been selected for that function initially and only later started performing it. Darwin (1862) himself noticed this crucial issue: "When this or that part has been spoken of as adapted for some special purpose, it must not be supposed that it was originally always formed for this sole purpose. The regular course of events seems to be, that a part which originally served for one purpose, becomes adapted by slow changes for widely different purposes" (282). How, then, can we be sure that the neuronal base that has allowed humans to communicate with a syntactically constrained grammar has been selected for reasons of communication? Even today, I do not think it is possible to answer this question, in part because "linguistic paleontology" lacks an equivalent to fossils. Without neuronal-anatomic correlates of linguistic capacities detectable in the skull, the only evidence that humans had language in a certain historical period can come from archeological discoveries of writings.[2] But traces of

2. In principle, if no writing has been found, we could accept as indirect proofs of the human capacity to communicate with a complex language the presence of very complex socioeconomic structures, or the memory of facts for which it is not reasonable to assume a pictographic tradition, or magical or religious rituals that require verbalization. As a matter of fact, there are no such cases. But more generally, the very fact that, when it comes to culture in general, each man does not rerun the history of mankind from scratch in his or her life, should be considered as a sign of the effect of language. A spider starts to build its web on the base of the very same knowledge as its ancestor. A child, instead, does not discover, say, fire at the age of three, the wheel at six, and the atom at puberty. A child has potential access to all the knowledge accumulated by his or her ancestors; the transmission of ideas and knowledge are in fact unique among humans and they appear to be inherently related to language.

writings go back only to the fourth millennium B.C., when cuneiform and Egyptian hieroglyphic scripts were used, and the second millennium B.C. (Hittite and Linear B scripts), just to mention two examples. Six thousand years of history of mankind are definitely not enough for important biological effects on the evolutionary scale—certainly not for a significant genetic evolution in such a complex area. Let's not forget that we do not even know how exactly genes contribute to form the neurological basis of linguistic capacity and that at least one third of all the genes in our genome are believed to be expressed in the brain. In other words, *we are lacking certain empirical traces on which we could found a phylogenetic history of the language faculty.*[3]

Edoardo Boncinelli (2003) effectively summarizes a critical position about the issue of the appearance of language in humans: "It is unlikely that the faculty of language communication has undergone significant changes since it first appeared. This faculty is likely to have suddenly appeared as it is today, like many other biological faculties and structures.... This does not imply that it has not been prepared, so to speak, by many changes that had occurred in our genome before, under the push of natural selection and purely random events" (22).

Talking about insect wings and grammar should not be qualitatively different then, at least from the point of view of evolution. Nonetheless, it is not easy to get comfortable thinking about grammar as being similar to a motor system: for centuries, grammar was seen as a cultural expression of our species, perhaps the highest cultural expression, almost a trace of the "spirit" of human essence. Still, unless we consider human language faculty as unique in the trajectory of evolution, we need to resign ourselves seeing human languages not as "a perfect communication system" but as the result of a complex genetic history that has produced language from a non-disadvantageous-point mutation, which probably did not have any specific purpose. And if it did have a purpose, it was probably not communication.[4] If human language characteristics were just the result of selective pressure with respect to communication needs, it would be hard to justify the fact that in other living species language is clearly much less expressive, in terms of richness of structures. In particular, it

3. In 1866, the Société Linguistique de Paris declared in its bylaws (*Statut*, art. 2) that it would not accept any communication dealing with the question of the origin of language (or the creation of a universal language). A century and a half later, it seems we are not too far from the same embarrassing uncertainty.

4. On the concept of language as a perfect (nonredundant) system, see Chomsky (1995, 2004) and Hauser, Chomsky, and Fitch (2002).

lacks syntax—which means, among other things, it lacks discreteness, recursion, hierarchy, locality, and syntactic movement. After all, the other living beings are under selective pressure as well. Therefore, the different response between humans and other animals cannot simply be just an environmental fact. *More specifically, environment may have selected some form of communication code in all species (or more generally, among all individuals in organized groups, including cells); what environment is very unlikely to have selected is the specific structure that human language has, in particular syntax.*

It is legitimate to think that the structures that underlie grammars could have expressed themselves in other domains, for other functions. Noam Chomsky wrote, in an essay in 1993: "The information provided by lexical items and other expressions yields perspectives for thinking and speaking about the world by virtue of the way their elements are interpreted 'at the interface'; embedded in different performance systems in some hypothetical (perhaps biologically impossible) organism they could serve for some other activity, say, locomotion" (Chomsky 1993, 48). These words show how far we have come from Wittgenstein's idea of language as a "linguistic game," language as convention. And less than thirty years before Chomsky's comment, the cultural environment was such that Lenneberg (1967) had to justify his study of the biological foundations of language. But it is not the first time in the history of scientific thought that a cognitive faculty has in some sense been "relativized" to the actual channel through which the nervous stimuli are interpreted. Although sometimes forgotten, Emil du Bois-Reymond, a Prussian doctor of French origins living in Berlin in the nineteenth century and the father of experimental electrophysiology, had a view of vision and hearing that was equally provocative and radically reductionist:

> It is universally concealed that the sense-organs and the sense-nerves carry to their appropriate cerebral regions or, as Johannes Müller calls them, 'sense-substances,' a motion that is in all cases ultimately identical. As in the experiment suggested by Bidder and successfully made by Vulpian on the nerves of taste, and those of the muscles of the tongue, the sensory and motor nerves, on being cut across, so heal together that excitation of the one class of fibers is transmitted by the cicatrix to the other class: in like manner, were the experiment possible, fibers from different sets of nerves would blend perfectly together. With the nerves of vision and of hearing severed, and then crossed with each other, we should with the eye hear the lightning-flash as a thunder-clap, and with the ear we should see the thunder as a series of luminous impressions" (du Bois-Reymond 1874, 19).

In conclusion, grammar is like our cervical vertebrae: it is *an* environmentally compatible answer among the many that our genome allows. In this sense, but only in this sense, language can be defined as a "perfect" solution to certain structural and environmental conditions. But if understood in this way, language is no more perfect than any other biological, and maybe physical, characteristic of the natural world. The term *perfection* becomes synonymous with *biologically acceptable* and ends up losing any teleological connotation. But should we completely exclude the idea that the constraints on grammar do not give our species any advantage? Of course not.

All adult humans possess a grammar, along with various levels of lexical knowledge. It could certainly be advantageous to be able to use more powerful grammars, that is, grammars with fewer rules, grammars that allow even more sentences than those that follow the principle of structure dependence. In fact, with the very same words, we would be able to build an even bigger repertoire of possible structures, dramatically augmenting our expressive capacity. Going back to the example considered before, it would be useful if both *[With which nurse] do you think that Mom wants to talk before meeting [with the chief doctor]?* and **[With which chief doctor] do you think that Mom wants to talk [with this nurse] before meeting?* were grammatical sentences. Two questions stemming from a single affirmative sentence. But what happens to children who have to acquire their native language? Children who have a linear string of sounds as their only input? This is the crucial factor that may provide perspective on the possible disadvantages of restrictions on the class of possible human languages.

Consider a concrete example: the structure of phrases. As we saw earlier, given any string of words, there are many ways to build phrases, ways to hierarchically group the words without changing the order of the string. For instance, we notice that given four words, a, b, c, d, there are eleven possible combinations: abcd, (ab)cd, a(bc)d, ab(cd), (ab)(cd), (abc)d, a(bdc), ((ab)c)d, (a(bc))d, a((bc)d), a(b(cd)). If children are born with a system that assigns to each lexical head a specifier and a complement that are hierarchically organized, leaving linear order only to be fixed by experience, it is easy to understand how syntactic constraints significantly reduce the number of combinations that should be taken into consideration with a string of *n* words. When children hear a sentence like *The director of the play brought the video back*, they will not have to go through all the possible phrasal combinations and, say, consider the string *brought the* as a phrase: the innate "grid" of allowed geometric structures will act as a sieve and guide them to eliminate some combinations right away. In doing this, they will of course be helped in segmenting and structuring the flow of speech by other component of grammar such as morphology, or the lexicon they have already learned, or prosody, or phonology (on the influence of the lexicon, see Fisher and Gleitman 2001 and Gleitman and Gleitman 1997; for the role that phonology plays, see Peña et al. 2002, Nespor, Guasti, and Christophe 1996). In fact, children must be guided by the pre-experiential "hypothesis" that the linear string contains phrases and that phrases consist of a head, a specifier, and a complement, in a way that is asymmetric, recursive, and homogeneous, as in the case of *the director of the play* or *the video*. We do not know precisely how children apply this knowledge or whether it can be traced back to some more primitive recursive procedure guiding words (and morphemes) assemblage. Actually, this is an open research question. What we can reasonably assume is that such a syntactic filter not only makes it easier to understand how fast the children's acquisition of their native language occurs but also explains why some mistakes are never found in their language production (see Fisher and Gleitman 2001; Gleitman and Gleitman 1997; Yang 2003; Guasti 2002).

Locality principles are another paradigmatic case. In the first chapter, we described them as *antidependence* to highlight their role as a filter on possible dependences. When children start to learn the use of the pronominal system, the locality principles in their syntax make it much easier for them to avoid taking into consideration all the possible pronominal coreferences and converge toward the correct interpretation. The number of coreferences that are a priori possible is drastically reduced. Thus, children do not need to learn by experience that the pronoun *he* and the noun *Dad* can refer to the same person in the sentence *Dad said that he was tired* but not in the sentence *He said that Dad was tired*. The potential ambiguity of the second sentence—which would be hard to detect by another speaker unless of course the sentence were immediately tested in context—is automatically eliminated by the locality principle on pronominal coreference, which is based on the hierarchical structure of phrases, as we saw in our sample of syntax in chapter 1. Hardly an experience-driven phenomenon.

Language, of course, can still be ambiguous, even in very simple cases, such as the famous sentence *They are flying planes*, which can either mean: these people are making planes fly or these things are planes that are flying. This just tells us that language has *some* ambiguities (and redundancies), like the system of cervical vertebrae, is not perfect: it is simply a possible answer, maybe the only one, to specific genetic, structural, and environmental conditions. But in spite of a certain level of ambiguity, the constraints on the syntax of human languages such as locality and the hierarchical asymmetric structure of phrases surely make it easier for children to decipher and manipulate the linguistic structures they are exposed to. The conclusion is that it is hard not to see this aspect as an advantage when it comes to language acquisition; the same can be said for other components of grammar, like phonology, semantics and morphology. In other words, *the price to pay in order to have attainable languages is that not all conceivable grammars are compatible with the biological guide each human is born with.*

But what is the class of attainable grammars? This is a conceptually different problem, one that is tied to the neuropsychological structure of the brain and of the organism in which it is located. We have found that the neural network which is normally activated for language purposes selects only grammars that are sensitive to hierarchical organizations, and not to linear organization. But from just the perspective of language learning, it is not implausible to imagine a possible world with exactly the opposite situation: the only possible grammars would be those that are sensitive to just linear organization, and not to hierarchical organization. Whether either class of grammars has an intrinsic (perhaps computational) advantage in the domain of language or in some other cognitive domain remains a fascinating question that so far is completely unexplored, I believe. The only sure thing is that restricting the potential classes of grammar to a subset helps children to acquire language.

To better understand the issue, however, a comparison with a different cognitive domain such as vision may be useful. *The fact that a child's brain is sensitive to only a certain range of conceivable syntaxes can, in some sense, be compared to the fact that the human eye is sensitive to only a restricted range of the electromagnetic spectrum, that is, only to the continuum of colors between ultraviolet and infrared such as one finds in a rainbow.* If humans were also sensitive to infrared waves, they would have many advantages in this cognitive domain: they could see warm-blooded organisms in the dark, recognize hot surfaces that could burn them, and so forth. On the other hand, an important advantage of not being able to see infrared is that, as our eyes are less sensitive, our brains need to process less visual information and consequently the reaction to stimuli can be faster. Imagine what it would be like if we were sensitive to all the possible electromagnetic frequencies. Sitting in my kitchen on a sunny day with the radio and TV on while using the microwave would generate an unbearable chaos, a visual Babel. Reality would just contain too much information. I would be flooded with signals, similar to watching a dead channel on a TV: no information at all, just a swarm of pixels, similar to a thick fog that prevents you from making out any stable shapes.

Similarly, if children could not discriminate between linguistic and nonlinguistic sounds—for example, if they could not distinguish the rhythmic ticking of raindrops on a wooden canopy from a meaningful linguistic code—or if they had to compute all possible combinations of words, not to speak of phonemes and morphemes, they might well be flooded with an unbearable amount of information. I am not saying that humans would not survive and develop if they were able to see additional wavelengths or use other kinds of grammars; I am saying that it is not necessarily true that increasing the degree of sensitivity is necessarily a welcome result. As is always the case in biology, we must assume that the "rainbow of grammars" we are sensitive to at present is just a state of acceptable equilibrium, that is, an equilibrium that is not too disadvantageous in its totality among all possible states. But we should stop here. This kind of thinking is merely speculative. Biology, like history, is not built with *ifs*.[5]

Let's conclude this section dedicated to the existence of constraints in grammars, to the fact that not all conceivable grammars are realized, by noting how it is completely implausible that children can derive these kinds of limits from other cognitive domains, especially those of sensory perception, such as vision, movement, or spatial organization. *If anything, the opposite is true: the fact that every syntactic structure depends on hierarchy and not on linear order is misleading for children, for whom the linear order of sounds that form sentences is the only available physical evidence*. Hierarchical organization is, then, a psychological phenomenon that must be grounded solely in human neuropsychology, not in the way the physical world outside our mind/brain is structured.[6] On the other

5. Though similar, vision and language are also different in important respects. For instance, there are parameters of variations among the syntaxes of different languages, whereas vision and other cognitive domains do not exhibit any variation of that kind. We speak in "different" ways, but we all see in the same way, despite assigning different names to colors (see Regier, Kay, and Cook 2005 and Lindsey and Brown 2006).

6. If this conclusion is confirmed, the impact of mirror neurons on the learning of syntax can only be peripheral (see chapter 2, note 6) since mirror neurons are by definition sensitive to physical, i.e., perceptible movement (and the correlated intentional action), whereas the only physical aspects of syntax are the linear sequence of words. As we saw, linear sequence is irrelevant: hierarchical structure, which is the only thing that matters when it comes to syntax, is compressed and hidden in the flat sequence of words. Thus mirror neurons would be, by definition, incapable to detect syntactic rules.

hand, structural conditions such as prominence, which is so important in establishing grammatical relations, do not seem to play any plausible role in cognitive domains other than linguistics, as far as I know.[7] In conclusion, everything converges to show that there are no extralinguistic cognitive domains that can help a child, or a linguist to reduce the class of possible human languages. Once again, we are obliged to admit that if this reduction occurs, it must mean that it is part of our biologically determined innate knowledge and is language-specific. As we will see in the second part of this chapter, this does not mean that extralinguistic conditions cannot play a role in determining some characteristic properties of language. These conditions, though, do not depend on some known cognitive component other than linguistics, but on the structural fact of the linearity of the linguistic code.

In summary, we have no way to show that linguistic regularities and their neuropsychological correlates result from an evolution aimed at facilitating communication. Actually, it is plausible to think that they are completely unrelated to the selective pressure for the improvement of communication. Nevertheless, it is true that the constraints on possible combinations—"the boundaries of Babel"—certainly have an advantageous effect on children's spontaneous acquisition of their native language. In fact, these constraints significantly reduce the computation and interpretation of the possible combinations of linguistic elements and therefore allow for a much more rapid convergence toward the grammar of the language children are exposed to, a result that might otherwise actually be unattainable. Evidently, the disadvantage for humans in having grammars with filters such as structure dependence that constrain possible combinations and thus limit informational structures must have been compensated by the advantage in terms of learning.[8]

7. This conclusion might be too strong if it aimed at excluding mathematical abilities. It could very well be that (some) recursive functions, such as those associated to Peano's axioms, that characterize the "number sense"—to put it in Dehaene's (1999) words—are isomorphous to prominence. After all, it is not unreasonable to make the hypothesis that mathematics and grammar can only coexist in a brain, disregarding pathologies.

8. Another language mystery is why languages are not all mutually comprehensible. At a certain abstract level humans just speak one single language, but obviously we do not perceive this unity at all. In many countries, dialect variation can be so dramatic that it is sufficient to move from one village to another not to be able to speak the other dialect. The impact that this fact has had on civilization is surely enormous. Evidently, the equilibrium between the disadvantages and the

3.2 On the Linear Nature of the Linguistic Signal, or Why Do languages Have Exactly These Rules?

The first question that we asked in this speculative chapter was "Why are there constraints on the syntactic structures?" That question could be more precisely framed: "Which advantages do constraints on syntactic structures bring, or which disadvantages do they not bring?" Another question can be asked, which is conceptually independent: "Why do *these* exact constraints on syntactic structures exist and not others?" It may not be by chance that a variant of this question characterizes current linguistic discussion, thanks to Noam Chomsky, who played a crucial role in research on the biological foundations of language in the 1950s.[9] This further turn is almost a logical consequence of the first turn: if language has a predetermined biological base, it should be possible to reduce some of its specific characteristics to the biological and physical structure of humans and the natural world we live in.

In this section we will explore this aspect of the research at the border of linguistics and the biological and physical nature of the world. We will do this in our usual way: we will not go into a systematic theoretical discussion, but we will choose a case study and try to see how this case can shed some light on our understanding of the general issue. A premise is needed: we do not yet have an answer to the question concerning the effects of the biological and physical nature of the world on grammar. I think that at the present stage, we can only attempt to formulate a question that is sufficiently simple to suggest an experiment, or at least to help us to see some empirical consequences. There is still a long journey in front of us. Here I would like to offer a possible suggestion in favor of

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advantages of having more than one language has not been so negative that it extinguished our species. Perhaps if we had only one language it would have been more difficult for urban civilization to make any progress: in fact, coordinating a small village rather than an immense megalopolis is certainly easier. Different languages might have helped in keeping people separated during the preindustrial epochs.

9. In the current scientific debates, *minimalist program* is the label usually assigned to the research program that addresses this challenge. The minimalist program is in no way an alternative to the principles and parameters model, but rather is just a research perspective within the same general model that focuses on the biological impact on language structures. For the origins of the minimalist program, see Chomsky (1995); an updated critical version can be found in Chomsky (2005).

this new trend in the research by focusing, once more, on the syntaxes of natural languages.

At a famous conference for the bicentennial of Columbia University in New York City in 1954, Willard Van Orman Quine (1957/1966) started his philosophical discussion by saying:

> I am a physical object sitting in a physical world. Some of the forces of this physical world impinge on my surface. Light rays strike my retina; molecules bombard my eardrums and fingertips. I strike back, emanating concentric air-waves. These waves take the form of a torrent of discourse about this table, people, molecules, light rays, retinas, air-waves, prime numbers, infinite classes, joy and sorrow, good and evil.
>
> My ability to strike back in this elaborate way consists in my having assimilated a good part of the culture of my community, and perhaps modified and elaborated it a bit on my own account. All this training consisted in turn of an impinging of physical forces, largely other people's utterances, upon my surface, and of gradual changes in my own constitution consequent upon these physical forces. All I am or ever hope to be is due to irritation of my surface, together with such latent tendencies to response as may have been present in my original germ plasm. And the lore of the ages is due to irritation of the surfaces of a succession of persons, together, again, with the internal initial conditions of the several individuals (1 ff).

This quotation touches on many issues, some more controversial than others. I want to focus on just one: our organism, including our thoughts and sentences, is immersed in a physical world, made of physical laws, and conditioned by our germ plasm. (Quine gave this address in 1954, only one year after the discovery of the structure of DNA; arguably, he might well have spoken of DNA rather than germ plasm if the theory had already been widely known.) If this is true, the question that linguists or neuroscientists have to ask themselves is this: What are the effects that the biological and physical components of the natural world have on the structure of human language? Formulated like this, however, the question, although fascinating, does not take us anywhere. There are too many physical variables, too little is known about the neuronal system that oversees linguistic functions, and we are still too far from a "Mendelian linguistics" that, directly or indirectly, could perhaps bring us to the genes that control the faculty of human language.

Nevertheless, there is at least one type of linguistic data that is undoubtedly subjected to the physical conditions in which we are immersed, and to which our organism has to respond in order to produce the communication code. We encountered this type of data in the first chapter: the linear structure of the linguistic signal, the fact that when we speak we *have to* put words one after the other (technically, we say that the linguistic signal has to be "linearized" or "compressed"). *There is no alternative to this biological and physical constraint: humans communicate by producing sounds over time, time is linear and monodimensional, and the communication code has to follow this linearity.*[10] If we had two mouths,[11] or the ability to communicate by telepathy, we might not be subject to this constraint. But we are not this way, and everything else is sci-fi linguistics (or "ling-fi").[12] Ferdinand de Saussure, at the beginning of the nineteenth century, noticed the fundamental importance of this aspect of language and wrote about it as the "third principle" of the nature of linguistic signs, in his *Cours de linguistique general,* saying: "The signifier, being auditory, is unfolded solely in time from which it gets the following characteristics: (a) *it represents a span*, and (b) *the span is measurable in a*

10. The code of sign language also has to be linear over time. The fact that signs can be performed simultaneously should not mislead us; in phonology, "distinctive features" are also simultaneous. What matters is that coding is subject to the laws of time. Actually, even a sentence that has been only thought but not pronounced, or maybe pronounced just in dreams, is subject to this requirement, which is far from insignificant. On the scientific issue of linearization and hierarchical structure in sign language, see Cecchetto and Zucchi (2004).

11. Strictly speaking, having two mouths would not be enough, for if two persons would simultaneously utter two different words, say, *John* and *arrived*, I would not be able to naturally interpret it as *John arrived*. More generally, linguistic structures can never be parsed simultaneously. This makes language completely different from other cognitive domains such as music. If I hear a person playing the flute and another the oboe, or someone playing the harpsichord with two hands, I can "interpret" the combination of the two sounds simultaneously according to the laws of harmony, but it is never the case that I can hear two persons simultaneously playing two distinct monologues of Shakespeare and combine them into a single text. There is no such a thing as a "verbal symphony."

12. In the history of linguistic thought, there are many examples of discussions on perfect or imaginary languages (for a detailed discussion see Eco 1993/1995). They are often useful for understanding the boundaries within which natural human languages are constrained. A typical example is the hypotheses regarding the languages of angels, which are mentioned in book I of Dante's *De vulgari eloquentia*, or the invention of languages with mathematical regularities such as Giuseppe Peano's *algebra de grammatica* (and the previously mentioned *latino sine flexione*) (see Peano 1930). This issue is also deeply interlaced with the question of the first language ever spoken, such as in the ancient Egyptian culture (the famous episode of the Pharaoh Psammeticus narrated by Herodotus, for example) or in the great Jewish tradition, as in Abraham Abulafia in the thirteenth century and his reflections on the Qabbalah). The issue of the first language is also an object of novels such as John R. R. Tolkien's famous saga, *The Silmarillion*.

single dimension; it is a line. While [this principle] is obvious, apparently linguists have always neglected to state it, doubtless because they found it too simple; nevertheless, it is fundamental and its consequences are incalculable" (emphasis added; Saussure 1922/1974, part 1, chapter 1, paragraph 3).

I would like to spend a little more time on the linearity of the linguistic signal to try to contribute to the formulation of a question on why the syntax of human languages has the constraints it has—*this* structure—and not others. First, I will briefly present a new syntactic theory, called antisymmetry theory, on the linearization of the linguistic signal, which was proposed by Richard Kayne in the beginning of the 1990s. Then, I will show how a "weak" variant of this theory, called Dynamic Antisymmetry theory, allows us to look at the phenomenon of syntactic movement (discussed in chapter 1) as a consequence of an extragrammatical constraint such as linearization.

Before illustrating antisymmetry, it is useful to recall a fundamental feature of the syntax of human languages. In the first chapter we examined some examples of syntactic dependence such as agreement and pronominal coreference, and concluded that syntactic dependence does not rely on linear order in any crucial way. Think once more about agreement. The string **a mathematician tell many stories* is clearly ungrammatical in Standard American English. On the other hand, the sentence *The boys who know a mathematician tell many stories* is fully acceptable, even though it contains exactly the same string that was just judged ungrammatical. Therefore, the linear order of the words in the string *a mathematician tell* does not matter at all. What matters is the fact that the subject of the grammatical sentence, *the boys,* is hierarchically more "prominent" (in the technical sense) in the tree structure that underlies the grammatical sentence: the verb "selects" that subject to agree with *because* of this prominence. Similarly, we saw that linear order does not matter with pronominal coreference either. The contrast between *Peter says that he is tired* and *he says that Peter is tired* may look as though it is due to linearity (pronouns always have to follow the noun they refer to), but it was not hard to see that the linearity explanation is incorrect. The sentence *When the teacher says that he didn't do the assignment, Peter always comes up with lame excuses* shows once more that it is the hierarchical structure that matters: a pronoun cannot corefer with a noun in the same "domain"—and domain is a notion that is crucially based on phrase structure and, therefore, on the hierarchical structure of the sentence. Many more examples could be given; the point remains the same:

within syntax, it is always the hierarchical structure that matters—never just the linear order.

At this point it may seem that we have gotten off track. Earlier, we concluded that the linear structure of the linguistic signal is the only irrefutable physical fact in syntax. And now, we just claimed that linearity does not matter for syntax at all, all that matters is hierarchical structure, clearly a non physical fact. We seem to be far from our initial goal of connecting physical aspects of the world to syntactic constraints. *But this is exactly the point: linearity is imposed on syntax by the biological and physical structure of the world. It does not have any other reason to be.* Therefore, a way to investigate the effects that the physical and biological organization has on the world is to ask how you go from hierarchical to linear structure. How are the syntactic trees compressed, flattened into strings of words? What are the mechanisms that turn hierarchical structures into linear strings? If anything, this research would be a necessary preliminary step for isolating the proper structure of grammar, or "narrow syntax," as Hauser, Chomsky, and Fitch (2005) put it.

Richard Kayne (1994) suggested an answer to the question of linearization at the beginning of the 1990s when he formulated his theory of antisymmetry. I will focus on the central aspects of this theory. Readers who are interested in a detailed account can look at the various works that have been published (see, in particular, Kayne 1994; Moro 2000, chapter 2, for a simplified version; and Cinque 1996, for a critical overview).

The term *antisymmetry* refers to a specific property of "linear orderings," which are special binary relations defined over sets of elements. A relation is a fundamental mathematical object, but it can also be grasped intuitively. For instance, "to-be-the-father-of" is a binary relation. Imagine meeting a group of four men, Carlo, Aldo, Angelo, and Andrea and finding out that Carlo is the father of Aldo, Aldo is the father of Angelo, and Angelo is the father of Andrea. The "to-be-father-of" relation consists of ordered pairs of people. For instance, Carlo and Aldo, or Angelo and Andrea. The relation does not contain the pair Carlo and Andrea or the pair Andrea and Angelo because the first element of each of these pairs is not the father of the second element. "To-be-an-ancestor-of" is another relation. If you consider the same group of men, the pair Carlo and Andrea is now part of this relation because Carlo is an ancestor of Andrea (Carlo is the father of Aldo who is the father of Angelo who is the father of Andrea). These differences among binary relations can be expressed in a precise, formal way.

With a bit of simplification, a binary relation is called a "linear ordering" if it is total (it is defined over all the elements of the set), transitive (if it holds between x and y, and between y and z, then it also holds between x and z), and antisymmetric (it cannot hold between x and y and between y and x at the same time).[13] Returning to our example, the "to-be-an-ancestor-of" relation is a linear ordering defined over the set of the four people.

There are at least two linear orderings in syntax: word precedence of the string that constitutes a sentence, *linear order,* and prominence in the hierarchical structure. The standard terminology is confusing here, because linear order is a type of *linear ordering,* but is not the only one. Confusion arises from the fact that linear order may be the linear ordering *par excellence*. It can be easily shown that locally, both syntactic relations—linear order and prominence—are total, transitive, and antisymmetric. With a bit of simplification, we can say that the core of the theory of antisymmetry consists of the following axiom that connects the two linear orderings in a nonambiguous way:

Linear Correspondence Axiom (LCA): For each syntactic tree, a word W precedes a word W′ in the linear string if and only if there is a phrasal category X that contains W and a phrasal category Y that contains W′ such that X is prominent on Y.[14]

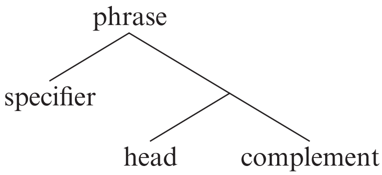
How does the LCA explain the way the linguistic hierarchical structure is flattened into a linear string? The format of the LCA may look complex, but the central idea is simple: just one linear sequence can be unambiguously derived from a given hierarchical structure—a syntactic tree. The empirical hypothesis is very clear: *The linear sequence and the hierarchical order are, therefore, the manifestation of the very same linear ordering—the two sides of the same tapestry*, to return to a metaphor we used in the first chapter.

The LCA has an extremely deep impact on all syntactic theory. In Kayne's theory of antisymmetry, this axiom applies pervasively to any level of syntactic representation and compresses all the syntactic trees

13. For a precise definition of the notion of relation and linear order, see Partee, ter Meulen, and Wall (1990).

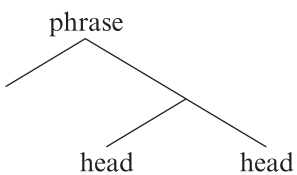
14. A syntactic tree is the hierarchical bidimensional representation of phrases; an element X is prominent on Y if and only if Y is contained in a node that is adjacent to X. *Phrasal category* refers to a head of a phrase.

into linear sequences. It can be clearly shown that the LCA can derive all the properties of phrase structure that we discussed in the first chapter. In particular, it can derive the lack of symmetric structures in syntactic representations and the fact that only trees like the following one can be derived:



In the tree above, the specifier is prominent on the head and on the complement, and as a consequence, it precedes them. On its turn, the head is prominent on the elements in its complement and as a consequence, it precedes them. Therefore, the head follows the specifier and precedes the complement. Incidentally, since we are representing trees on a two-dimensional page, we are forced to choose a linear order for the hierarchically organized elements, and this could be misleading. If we could represent this same hierarchical structure in a three-dimensional space, we would leave the specifier, the head, and the complement unspecified as to their reciprocal linear order. On the other hand, as we just noticed, the linear sequence resulting from this tree is surely unambiguous under the LCA: the specifier precedes the head, which precedes the complement.[15]

Now let's look at an example of a structure that is *not* compatible with the LCA, to better understand the general mechanism (for a more detailed account the interested reader can see Moro 2000 and Cinque 1995). Imagine a phrase that contains two adjacent heads in the hierarchical structure, as in the following syntactic tree.



Why does this tree violate the LCA? Because there is no prominence relation between the two heads and therefore the LCA cannot determine

15. This is equivalent to saying that the four possible variants that we will soon discuss, in which hierarchical relations do not change, are just *notational* variants of the same syntactic tree, in which the specifier precedes the head, which in turn precedes the complement.

the linear order that should be assigned to the words contained in the two heads.

With similar arguments, by means of a complex formal apparatus based on the notion of prominence, we can ensure that a head does not have two or more complements or two or more specifiers. In conclusion, we can derive the entire asymmetric structure of phrases that we saw in our sample of syntax from just one axiom.

In the case of complements and specifiers, though, the reason why the LCA would rule the structure out would be different from the case of two adjacent heads. For instance, if a head had two complements, the structure could not be linearized—not because of a *lack* of prominence relations, but because there would be *too many* prominence relations. The first complement would be prominent on the words *inside* the second complement; and the second complement would be prominent on the words *inside* the first complement. The LCA would produce two contradictory linear orders: the words in the first complement would have to precede those in the second, and vice versa. For similar reasons, two phrases that are conjoined without an "intermediary" head would be impossible. In general, then, the LCA excludes structures where there are too few prominence relations (leading to "head-head" constructions) or too many prominence relations (leading to "double complement," "multiple spec," and "phrase-phrase" constructions), as if only one equilibrium were possible.

What are the reasons for this axiom? There are two conceptually distinct motivations, theoretically and empirically distinct, for adopting the LCA. The first motivation is related to the simplification that the LCA brings to different levels of syntactic theory.

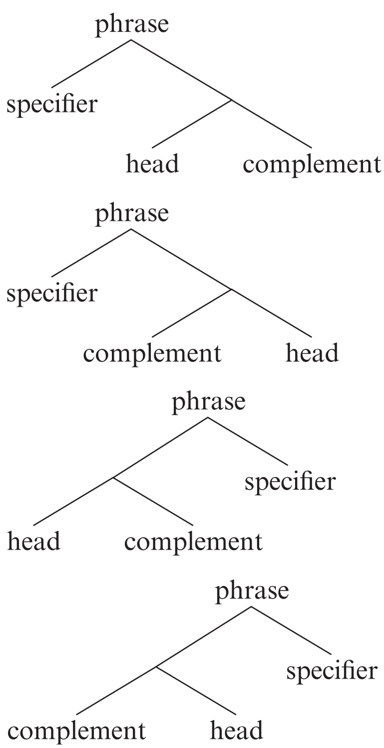
First, as we said, all the architectural properties of phrases (uniqueness of the head, complement, specifier, and so forth) can be derived from just one axiom, which makes the theory formally simpler. For this reason, LCA also substantially (although indirectly) corroborates the theory of child language acquisition that is based on a biologically determined component: children do not need to be endowed with a list of many specific rules ("no double head," "no double complement," "no double specifier," "no double phrase," and so on recursively, that is "no triple head," "no triple complement," etc.), in order to build phrases; they just need to be equipped with a single axiom, the LCA, in order to derive all the structures as if they were theorems.

The second motivation for adopting the LCA is comparative and is based on considerations involving the exploration of different languages.

Before illustrating the impact of the LCA on comparative syntax, it is worth citing Kayne himself (1994):

> If languages were allowed the option of having complements precede heads and heads precede specifiers, then we would expect to find languages that were the mirror image of Germanic with respect to verb-second phenomena (that is, the finite verb would move to second-from-last position in root sentences). I do not know of any such languages. If that gap is not accidental, it supports the idea that S-H-C [specifier-head-complement] is the only available order of constituents (50).

As we saw in the first chapter, before the theory of antisymmetry was proposed it was assumed that by varying just the linear order of the specifier, head, and complement, and by keeping the hierarchical structure fixed (that is, keeping the fact that the head and complement are adjacent while the specifier is at a higher level), the following four different syntactic trees could be obtained. Starting from the first tree, we can vary first the linear order of the head and the complement and get the second; then vary the specifier only and get the third; finally, vary both the linear order of the specifier and the head-complement complex and get the fourth, which turns out to be the mirror image of the first tree.



Throughout the 1980s, researchers held the hypothesis that all the trees of all languages in the world would correspond to variations in these tree schemas: in all languages, the hierarchical organization of the elements that form phrases (specifier, head, and complement) would be identical, though their linear sequences could parametrically vary.

Faith in the heuristic force of formalism was absolute; the four possible forms *had* to correspond to the four kinds of phrases that were found in the languages of the world. For a long time this hypothesis went challenged, but not without encountering important empirical problems such as the existence of apparently "mixed" languages such as German, whose tense phrase (TP) seems to follow the complement-head schema, while all the other phrases (CP, AP, VP and NP) are of the head-complement kind. Nevertheless, this hypothesis was widely accepted by the scientific community. The first tree schema applies to English, French, and Italian and languages like them; the second works for Japanese and Turkish; the third one—much more rare—seems to apply to Malagasy, spoken in Madagascar. This theory, however, faces a problem that is not yet resolved: languages of the fourth kind, whose phrasal structure is the mirror image of the first tree schema, have practically never been found.

There have been many cases in the history of science where a combinatory theory predicted the existence of an object that had not yet been found. One example is the completion of Mendeleev's Periodic Table of Elements (Mendeleev 1869). Between 1868 and 1870, while he was building a table in which he wanted to place sixty known elements in order according to their atomic weights, Mendeleev noticed that some recurrent schemas appeared, but only if empty positions were left that corresponded to values that had not yet been associated to any known element. In spite of a certain amount of skepticism from the scientific community, Mendeleev not only stood by his theoretical construct but also considered it a predictive matrix for the discovery of new elements that would fill in those empty blocks. The table was so "elegant" that the missing elements *had to* exist. History showed that his intuition was correct. The discoveries of Gallium in 1875, Scandium in 1879, and Germanium in 1886 by French, Scandinavian, and German scientists, respectively, for whom these elements were named, showed that not only could the table be completed, but also that the properties of the elements could be exactly predicted on the basis of their position in the table. The theory precisely indicated what to look for.

In linguistics, we are in a similar spot: should we wait and hope that the hole in the taxonomy will be filled (and admit that we are dealing with an accidental omission) or should we radically rethink the theory so that it no longer contains the hole? Should we accept a predictive schema as Mendeleev did, or should we revolutionize the schema, eliminating the hole? There is no guarantee that one road is better than the other.

Kayne has taken the latter road by proposing the theory of antisymmetry based on the LCA. Kayne's reasoning is as follows: The fourth kind of tree surely does not exist because it is the mirror image of the first. Since prominence relations are identical between the two trees (the specifier is always prominent on the head and the complement, while the head is prominent on the complement), it does not make sense to imagine that both these trees exist, or, more precisely, that these two trees have the same linear order. In fact, the two can be considered as notational variants resulting from the fact that we represent trees in the two-dimensional space of a sheet of paper. From this premise, the hole, then, is no longer a hole. The fact that the fourth structure is not attested in the languages of the world is derived as a theorem from the LCA.

There is, however, a price to pay: if the mirror image of the tree which is immediately derived by the LCA (the type where the complement precedes the head and the head precedes the specifier) can be correctly excluded, one now needs to justify the existence of languages which seem to show the residual "intermediate" types (where the specifier precedes the complement and the complement precede the head, and where the head precedes the complement and the complement precedes the specifier). In fact, the LCA not only says that the fourth kind of hierarchical structure produces the same linear order as the first, but also that the other three hierarchical structures are equivalent to each other with respect to linear order, since the prominence relations between specifier, head, and complement in each tree are identical: in fact, all four trees are just notational variants of a single unlinearized phrase structure. If the LCA allows us to derive only structures whose linear order is specifier-head-complement, what can we say then about the second and third trees, which seem to fit some attested languages? The debate on this issue is still so lively that no firm conclusions can be given at this point. For many, the LCA is not tenable, exactly for this reason. For others, the LCA is plausible and the second and third structures can be derived from the first by means of syntactic movement. For instance, those who accept the LCA say that the direct object in Japanese—and SOV language—moves from the right of the verb to reach a position that is prominent with respect to the verb, which then translates into linear precedence (see Kayne 2002). Many linguists, however, do not agree with deducing the second and third structures from the first by means of syntactic movement. They prefer to keep the old schema and allow for linear parametric variations among the elements that form phrases and tolerate the embarrassing hole in the theory. If this model based on the LCA is not firm yet, what is the use of introducing antisymmetry in this context?

The reason is that I would like to show how a "weak" variant of the theory of antisymmetry not only can lead toward the unification of two areas of syntax, phrase theory, and syntactic movement, but also—which is relevant here—it can make the linearity of the linguistic code play a significant role in the shaping of grammar. Linearity, as we noticed, is an extragrammatical property, which is exclusively due to the organization of the physical and biological world. I would like to suggest a possible path to explore in order to answer our fundamental question: Why does the syntax of natural languages have *these* characteristics and not others? And how can these characteristics be related to extragrammatical conditions? Syntactic movement is the specific characteristic of the syntax of human languages that we will focus on trying to relate it to extragrammatical conditions imposed on language.

Why do natural languages have syntactic movement? Syntactic movement is the phenomenon according to which a phrase is pronounced at a point along the linear sequence that is different from the point in which it receives part of its interpretation. If you say *Which story do you think that Simon knows that Andy tells?* the phrase *which story* is the direct object of the verb *to tell*, although it is far away from this verb and closer to the verb *to know*. Technically, we say that a copy of the phrase *which story* is inserted in the new position, while the original is not pronounced but still holds the direct object position, as shown by the ungrammaticality of the sentence **Which story do you that Simon knows that Andy tells a fairytale?*

The property of syntactic movement, like the asymmetric structure of phrases, is neither logical nor necessary. In the first chapter, we noticed that no artificial language, mathematical system or programming language is designed with this property: only natural languages have it and all natural languages have it. The standard theory, canonized in Chomsky (1995) and accepted by most of the scientific community, gives a morphological explanation for the triggering of syntactic movement. In short, phrases are assumed to move in the syntactic tree to reach a local context in which they can interact with other words that share the same morphological features. What are morphological features? And why do they sometimes have to move?

Morphological features can be thought of as systematic characteristics that distinguish words from each other. For instance, the contrast between *table* and *tables* and *book* and *books* is due to the addition of one phoneme—/s/—as a suffix, the change in the morphological feature of number: singular with no suffix, plural with the suffix.

To understand the standard theory of syntactic movement, it is useful to focus on interrogative morphological features. Observe the following characteristic of the complementizer phrase, which constitutes the top part of the clause structure, as we saw in the first chapter. Among other functions, the head of the complementizer phrase connects a matrix clause with its embedded clause. The head of this phrase can be characterized by declarative or interrogative morphological features. Think of the sentences *Paul tells me <u>that</u> Peter will read this book tomorrow* and *Paul tells me <u>whether</u> Peter will read this book tomorrow*. The complementizer *that* is declarative, whereas the complementizer *whether* is interrogative. Since the two complementizers are morphologically different, we can say that *whether* contains interrogative morphological features, whereas *that* contains declarative morphological features. Those who support the standard theory of syntactic movement think that interrogative elements move closer to an interrogative complementizer. Consider, for example, the following two sentences: *Paul tells me that Peter will read this book tomorrow* and **Paul tells me that Peter will read this book when*. The second sentence is ungrammatical because the temporal adverb *when* needs to be paired with an interrogative complementizer in a local configuration, unlike *tomorrow*, which does not contain interrogative morphological features. Now, assuming that interrogative complementizers can also be realized by unpronounced heads—much as declarative complementizers are in cases like *Albert says Isaac was right*—we get the following sentences: *Paul tells me when Peter will read the book*, where *when* has been moved to the specifier position of the unpronounced interrogative complementizer, the required local configuration. Notice that the fact that the interrogative complementizer must not be pronounced when an interrogative element is moved to their specifier position is not a universal restriction. Even in some dialects of English, such as the one spoken in Belfast (Henry 1995), the complementizer is pronounced in the relevant configuration and gets the same form as the declarative complementizer, namely *that*, such as for example in the case of movement of an interrogative object phrase *John wonders which novel that he read*. The same happens in many other languages across the world, like Dutch (as in *Ik vraag me af <u>wie of</u> er morgen komt?*; literally, I wonder <u>who if</u> there tomorrow comes; "I wonder who comes tomorrow?"), Quebec French (as in *Qui que tu as vu?*; literally, who that you have seen; "who have you seen?"), some Northern Italian Dialects, like Veneziano (as in *Cossa che la magna?*; literally, what that she eats; "what does she eat?") (cf. Rizzi (1990) and references cited there).

In conclusion, standard theory accounts for the fact that a copy of an interrogative temporal element such as *when* has been moved to the specifier of the complementizer in terms of the principle that requires similar morphological features to be paired in a local configuration. The local relations where pairings take place are often called *checking relations. The central proposal of the standard theory of syntactic movement is that all movements are triggered by morphological reasons.* An element moves only to reach a position where it can enter into a checking relation with an element that contains similar features. I gave the example of interrogative clauses, but other examples could be made, including, for example, passive clauses. Still, this should be enough to understand the core of the standard theory of movement. But we are left with a crucial question: Why do morphological features, like the interrogative ones, have to be paired, or checked? The answer—systematized in Chomsky (1995) and further developed in subsequent works—is based on two conceptually distinct hypotheses. First, not all morphological features are semantically interpretable—interrogative features, for instance, are not. Second, the checking of two uninterpretable features would delete the uninterpretable features themselves and make the sentences interpretable. Therefore, the deep reason that motivates movement would be the "principle of Full Interpretation," according to which all the features that constitute a linguistic expression must be interpreted in natural languages, regardless of whether they are aspects concerning pronunciation or perception of an acoustic signal or, as in our case, semantics.[16] If interrogative morphological features are assumed to be uninterpretable at the semantic level, grammar must have a way to get rid of them. This, in the standard minimalist framework, is the role of syntactic movement.

Standard theory sees syntactic movement as a label that is associated with a complex phenomenon: an element that contains an uninterpretable

16. The principle of Full Interpretation plays a central role in the minimalist model. All linguistic representations have to be readable by the two components that necessarily characterize the language faculty: the semantic-conceptual-intentional interface and the sensory-motor interface. In the radical version of minimalism, these are the only two components: there are no other levels of syntactic representation (see Chomsky 1995).

feature is copied in a position close to a feature of the same kind, while the original is not pronounced. This theory leaves movement to the semantic-morphological component entirely and raises questions that are not resolved. Why do uninterpretable features exist? What forces deletion of phonological features? How does in general the principle of feature deletion work? In my opinion, many of these questions currently allow only for answers that are based on ad hoc hypotheses. Instead of trying to answer these questions, we can wonder whether this is really the only option for explaining what triggers syntactic movement or whether a radically different path could be taken.

In Moro (1997b),[17] an alternative theory is proposed: unlike standard theory, movement is not triggered by morphology but by the linearization requirement that the LCA imposes on the linguistic signal. The central point is a "weak" version of the theory of antisymmetry. I shall try to illustrate the core of this alternative theory while keeping in mind the reason for this apparent digression: to show how an extragrammatical requirement can play a role in accounting for a central aspect of the syntax of human languages—movement.

In the original version of the theory of antisymmetry, the LCA can *never* be violated: the correspondence between linear order and hierarchical structure is constant and defined at any level of representation of the linguistic structures. The weak version of this system argues instead that grammar is more parsimonious than thought before: Why should we assume that linear order is already decided before the sentence is actually pronounced? In the end, hierarchical structure is the only thing that matters for grammatical relations, as we have said many times.

The alternative hypothesis is, therefore, to assume that the LCA can be violated *before* the hierarchical structures are to be interpreted at the sensory-motor interface and the linear sequence pronounced by linearizing the words (technically, producing an utterance is called "spell-out"). Put another way, the LCA establishes a correspondence between hierarchical order and linear order, but if linear order is necessary only at the time of spell-out, why should the LCA also be active before then? And if it is not, how can the linear order of the words be determined at spell-out, if the structure violates the LCA? This tension between the necessity of linearization and the unbounded capacity of hierarchical composition is at the core of the alternative theory of movement and the specific empirical hypothesis that implements it. Let's then assume the less restrictive

17. The theory was subsequently developed in Moro (2000, 2004).

theory of syntactic combination, namely one where before spell-out, words and phrases are free to combine in hierarchical structures that violate the LCA producing symmetrical structures. The alternative theory of movement is the following: if two overt syntactic items violate the LCA, then movement intervenes at spell-out by deleting the phonological features of either one and merging a copy of that element in a suitable prominent position. In this way, the precedence order can be established among phonologically realized items and the sentence can be pronounced. To emphasize the converging role of antisymmetry and movement, this alternative theory is called "Dynamic Antisymmetry."

Thus, movement is motivated by the principle of Full Interpretation in Dynamic Antisymmetry as well. This principle establishes that all the structural aspects that constitute a linguistic expression must be interpreted at the relevant level in natural languages: all linguistic structures are instructions for the interfaces, all instructions must be interpretable. The standard theory and Dynamic Antisymmetry are identical in this regard. The difference is that standard theory is based on the hypothesis of the deletion of morphological features (under the checking process) *and* the successive deletion of phonological features. In contrast, Dynamic Antisymmetry does not require any semantic adjustment for interpretation of morphological features: it is just the deletion of the phonological features after copying that matters for linearization purposes. In either case, movement is not "teleological": simply, if it did not occur, the structure would not be readable by the interfaces.

A major significant difference between the two competing theories must be highlighted here. Just like in the standard theory, in a Dynamic Antisymmetry framework, movement can also be seen as a complex operation that can be divided into two separate factors. First, one copy of the moved element is merged in a prominent position. Second, the phonological features of the other copies are canceled. The crucial difference between the two proposals is that in Dynamic Antisymmetry, deletion of phonological features is no longer to be accounted for independently from copying. For Dynamic Antisymmetry, the deletion of the phonological content of the original *is* the reason why there is movement, that is, it is the way the grammar deals with a substructure of the tree where linearization would not be possible.

The substructures where the LCA is violated, called "points of symmetry" of the structure, can be of various kinds. In general, a point of symmetry is characterized by three definining properties: it is made of two elements at the same hierarchical level (one is not prominent on the other); the two elements have phonological content that must be expressed in a linear sequence; the two elements are of the same syntactic nature: they are either both heads (X) or both phrases (XP).

According to Dynamic Antisymmetry, syntactic movement is triggered not by morphology but by the geometry of the syntactic tree and, in the end, by the need to compress hierarchical structure into linear sequences for purely physical-biological reasons. The function of syntactic movement is to "break the symmetry" of syntactic structures—to "neutralize" a point of symmetry. Practically, a principle like the one informally stated below is at work ("linearization" is a shorthand way of referring to the "compression or flattening of a hierarchy into a string of words"): Syntactic movement breaks the symmetry of the structure in order to allow for the linearization of the linguistic signal.

Of course, in Dynamic Antisymmetry, morphology is still fundamental for the theory of movement, but only because—among all the possible solutions for breaking symmetry—it chooses those that are compatible with the morphological characteristics of the point an element moves to. Not every element can be moved to any position: morphology now works as a sifter that sorts for the accessible structural positions, *pace* locality principles. By doing this, morphology limits the kinds of possible dependences and therefore is, not unexpectedly, still relevant for a theory of locality of movement.

We are dealing with two competing theories: on the one hand, standard theory, according to which *movement is triggered by morphology*, by the need to delete uninterpretable features; on the other, Dynamic Antisymmetry, according to which *movement is triggered by geometry*, by the need to flatten the structure into a linear sequence.[18] How can you choose between two competing theories? Scientific research often requires you to make decisions of this kind. Sometimes you can look for a contradiction in a theory in order to refute it. But in most cases you have to make a decision on empirical grounds based on the theory's capacity to highlight new facts or simplify facts that are already known. As far as I know, there is currently no way to make a final decision about which of the two theory better accounts for the facts. Nevertheless, there is at least one class of phenomena (aside from the deletion of the phonological

18. In principle these two accounts may be unified: the key step is to admit that whenever a point of symmetry is created by merging two syntactic elements, the computation cannot proceed because the checking procedure cannot be established (the impossibility of linearization being a correlate of this). Thus, movement of either element to a proper higher position would make computation possible, and linearization would follow. I will not pursue this possibility here.

content of the original) that Dynamic Antisymmetry can capture quite naturally—phenomena that on the other hand stand as potential counterexamples to standard theory.[19]

One of the fundamental empirical advantages of Dynamic Antisymmetry is that it predicts that languages may exhibit "mirror structures," structures that are characteristically distinguished by movement of either element from a base generated point of symmetry. Standard theory cannot easily account for this kind of structure without resorting to ad hoc assumptions. In fact, they rather stand out as problematic cases. According to standard theory, an element moves *only if* it is forced to: if it can avoid movement, then it stays in the position in the syntactic tree in which it was merged. This is intrinsically related to the central idea of standard theory: movement is guided by the need to delete uninterpretable morphological features. If a feature has to be deleted, its movement cannot be substituted by the movement of another element. But if an element does not move, it means that it does not have features that need to be deleted. Clearly, the very existence of mirror structures does not easily fit within this theory. If we adopt Dynamic Antisymmetry, instead, the existence of mirror structures is not only easy to explain, it is predicted: it is a consequence of the fact that a point of symmetry is made of *two* phonologically realized elements and that therefore the violation of the LCA can be neutralized by moving one element or the other (in accordance with independent morphological restrictions).

Across languages, there are many examples of mirror structures, according to our technical definition. Copular sentences constitute one of the most striking examples. Copular sentences are built around the copular verb *to be*, linking the subject and the predicate in prototypically simple sentences such as *John is the cook* (the term *copula* was chosen to highlights its role in "linking"—from *copulare* in Latin). The copula is a characteristic element of Indo-European languages, but many languages have it.[20] A copular sentence is any sentence with of the kind NP copula

19. For an updated list of empirical and general consequences of Dynamic Antisymmetry, see Moro (2004).

20. Sometimes elements other than verbs are called "copulas." For an overview of copular constructions across languages, see the monumental collection edited by Verhaar (1967–1973). See also the appendix of Moro (1997a) for a short history of the term *copula* that goes from Aristotle's Ancient Greek texts to the modern theories by Meillet, Russell, Montague, Jespersen, and Chomsky, passing through the medieval treatise on *Dialectica* by Abelard and the *Grammaire générale et raisonnée* by the school of Port-Royal.

XP, or—in a more detailed form—[NP [copula XP]], where the inner phrase is a VP, i.e., NP VP. Besides *John is the cook* (NP copula NP), other such sentences are *John is fat* (NP copula AP), *John is on the beach* (NP copula PP) and *John is eating* (NP copula VP). NP copula NP sentences (called nominal copular sentences) make up an interesting subfield of research for they are the only example where the both the subject and the predicate are realized by the same lexical category (NP), and these types of sentences have been constantly analyzed throughout the development of linguistics.[21] I will illustrate here some crucial aspects of the syntax of nominal copular sentences.

Consider for example the sentence *This picture of the wall is the cause of the riot*. The sentence can be intuitively associated to another one where the order of the two NPs in inverted: *The cause of the riot is this picture of the wall*. Such pairs of copular sentences are very common in languages like Italian and English and the majority of Indo-European languages. It can be proved that these pairs of sentences show surprising anomalies with respect to any other [NP [V NP]] structure, that is to any choice of V other than the copula. This can be shown by admitting that the two copular constructions result from a movement transformation that involves the two NPs.[22] Although we will not go into a detailed analysis of copular sentences, I would like to illustrate at least two unexpected phenomena that justify the special analysis of nominal copular sentences.

21. In particular, it is easy to notice that these are the only sentences where the predicate and the subject are realized by the same lexical categories, that is, a noun phrase. For example, the sentence *John caused the riot* can be rendered as *John was the cause of the riot,* where the predicate, like the subject, is an NP. For this reason, they were central in the field of logic, especially medieval logic, to the study of syllogism. In a syllogism, a certain NP behaves like a predicate or like a subject, depending on the premise in which it occurs (for example, *my best friend* in *Joshua is my best friend*; *My best friend is mortal*; *Joshua is mortal*). It has often been suggested that the copula expresses identity in NP V NP sentences: for a detailed argument contra this hypothesis see Moro (1997a), including Jespersen (1924).

22. See Moro (1997a) for a detailed analysis of copular sentences originally published in Moro (1988). The former also deals with so-called existential sentences, such as *C'è un teorema affascinante* (There's a fascinating theorem), sentences with the verb *sembrare* (to seem)—which is also called a "quasi-copula"—and the so-called "unaccusative constructions" (see the seminal work by Burzio 1986 and the advanced analysis in Hale and Keyser 2002) and tries to provide a unified account of these different issues. For an extension of the theory of inverse copular sentences, see also den Dikken (2006).

I will concentrate on Italian, because the special properties of copular structures are easy to detect in this language. Let me just remind that the reason for this digression is to provide evidence in favor of Dynamic Antisymmetry, that is, in favor of a theory that makes a physical factor like linearization a core engine of grammar, motivating syntactic movement. The very existence of mirror structures like the one illustrated here is the direct reflex of the interplay between symmetry and movement.

The first anomaly pertains to verbal agreement. In general, in NP V NP structures in Italian, the verb agrees with the NP on its *left;* this is also true for many other copular sentences, but—significantly—not all. Let's take the plural of *picture* and construct the corresponding Italian sentences: *La causa della rivolta* *sono* *queste foto del muro* (literally, The cause of the riot are these pictures of the wall). Surprisingly, the verb *sono* (are), agrees with the plural NP on its *right.* If the verb agreed with the singular NP on its left, it would be the singular form *è* (is), and the sentence would be sharply ungrammatical. If the order of the two NPs is reversed, there is nothing special about the sentence, since the verb would appear to agree with the NP on its *left*, as it does in nearly all NP V NP sentences: *Queste foto del muro* *sono* *la causa della rivolta* (These pictures of the wall are the cause of the riot). As far as agreement is concerned, then, the way this pair of Italian copular sentences differs is not the same as how other pairs of NP V NP sentences differ. Take, for example, the verb *rivelare* (to reveal). Agreement would unselectively be on the left; in other words, the following sentence is ungrammatical: **La causa della rivolta rivelano alcune foto del muro* (literally, The cause of the riot reveal some pictures of the wall). The verb should be *rivela* (reveals), which is singular and agrees with the preverbal noun phrase *la causa della rivolta,* which is also singular. If we want to maintain the hypothesis that in Italian the verb always agrees with the NP on the left, then we are forced to admit that in a subset of NP copula NP sentences, it is the postverbal NP which is the subject and that the sentence has undergone a movement transformation which displaced the two NPs in this tricky order. This would require a radical revision of clause structure, allowing for the subject to be trapped within the VP when other elements have been moved out, such as the predicative noun phrase in the case of copular sentences. (See Moro 1997a for a detailed proposal and the analysis of NP V NP copular sentences.)

The second anomaly pertains to movement of the NP, more specifically, to movement of the pronominal clitic *ne* (of-it/him/her/them), which I mentioned in chapter 1. Consider once more *La causa della* *rivolta sono alcune foto del muro*. The clitic *ne* cannot replace the phrase *del muro* (of-the wall) and be extracted from the noun phrase that follows the verb: **La causa della rivolta ne sono alcune foto* (literally, The cause of the riot of-it are some pictures). This is surprising. In general, *ne* can be extracted from a noun phrase that follows the verb in NP V NP sentences in Italian, *pace* independent restrictions on the type of noun phrase involved (see Cinque 1980): *Gianni ha visto alcune foto del muro* (John has seen some pictures of the wall) becomes *Gianni ne ha visto alcune foto* (John of-it has seen some pictures). On the other hand, the noun phrase that follows the copula in the sentence *La causa della rivolta sono alcune foto del muro* (literally, The cause of the riot are some picture of the wall) seems to behave like a so-called "postverbal subject." Postverbal subject constructions are not found in English but are common in Italian. For instance, in Italian, the combination of a subject like the NP *Molti amici di Carlo* (many friends of Carlo) and a predicate like the VP *hanno telefonato* (have telephoned) yields two possible combinations: either *Molti amici di Carlo hanno telefonato* (literally, Many friends of Carlo have telephoned) or *Hanno telefonato molti amici di Carlo* (literally, Have telephoned many friends of Carlo). The latter is an example of a postverbal subject construction. In this type of subject, movement of *ne* from the subject is not allowed, just as we saw in the copular construction: **Ne hanno telefonato molti amici* (literally, Of-him/his have called many friends). Paralleling the conclusion derived from the first anomaly, this construction suggests that in a subset of NP copula NP sentences, the postverbal NP is the subject of the sentence and the string has undergone a rearrangement via movement—trapping the NP subject within the VP.

These anomalies, and many more, suggest that NP copula NP sentences cannot be analyzed like any other kind of NP V NP sentence. The basic idea is that copular sentences result from merging a symmetric NP-NP structure with a copula (the NP-NP is the point of symmetry). The instability of this symmetric structure forces movement of either NP higher up to the preverbal position (technically, it is "raised"). This movement results in two totally different structures that have been hard to recognize, because the surface order of the element is the same, and—which makes the situation even harder—because the final sequence is exactly like that of transitive sentences, namely NP V NP. Technically, when the subject NP is moved, the resulting structure is called a *canonical copular sentence*. When the predicative NP is moved higher up in the tree structure, instead, the resulting structure is called an *inverse copular* *sentence*—the quite peculiar structure where the subject, as we noticed earlier, is "trapped" in the VP and the predicate is in the preverbal position canonically reserved to subjects. This latter type of sentence has many interesting properties that differentiate it from any other NP V NP structure, some of which are sketched out here, including the far-reaching fact that the rigid canonical long-standing clausal skeleton associating the subject to the NP and the predicate to the VP in NP VP structures must be abandoned.[23] For our purposes, however, what matters here is that there are empirical reasons to assume that canonical and inverse copular sentences are mirror structures, resulting from movement of either NP constituting a point of symmetry. And mirror structures, as we just noticed, are predicted by Dynamic Antisymmetry contra the standard theory, which can hardly capture these data without undermining some of its basic tenets.[24]

Clauses are not the only mirror structures; they are also found in other domains of syntax, including noun phrases. Let's look at an example from English where the relevant property is easy to detect. In the sentence *Martin reads books*, the direct object is *books*. The sentence can be expanded into *Martin reads books of these kinds,* where the direct object is still *books.* But think about the following sentence: *Martin reads these kinds of books*. What is the direct object? It is still *books*, though it is no longer right next to the verb *reads* in the sequences of words. Noun phrases such as *books of these kinds* and *these kinds of books* show that there are good reasons to assume that mirror structures also exist among noun phrases. What matters here is that the interpretive relation between *reads* and *books* is the same in both cases. Thus, it must be the case that it is syntactic movement that has changed the linear order, and that there are two options because movement is triggered by a point of symmetry, which by definition is made of two elements. This is exactly the same as

23. For example, linguists are no longer forced to assume that elements like the preverbal *there* are dummy subjects; since predicates may also occur in this position (as in *The cause of the riot is a picture of the wall*). It is not unreasonable to explore the possibility that the preverbal *there* in sentences such as *There is a picture of the wall* is a dummy predicate, that is, that *there*-sentences are inverse copular sentences. This is in fact the line of reasoning adopted in Moro (1997a).

24. The fact that either NP must be moved in Italian nominal copular sentences also constitutes a sharp departure from a long-standing assumption that a lexical NP is not necessarily required in this language, as a prototypical language where subject can be omitted. This fact strongly supports Dynamic Antisymmetry and undermines the standard theory.

the two options that we saw with the sentences *Alcune foto del muro sono la causa della rivolta* and *La causa della rivolta sono alcune foto del muro.* The idea here is that this type of complex noun phrase results from merging a symmetric NP-NP structure with an element such as *of:* the instability of this symmetric structure forces either NP to move to the specifier position of the element *of,* which would play the role of a nominal copula, instantiating another case of mirror structure.[25]

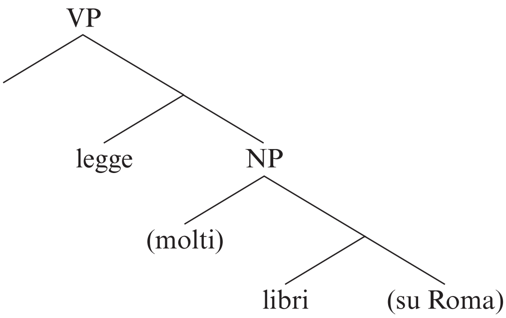
In conclusion, these case studies support the theory of Dynamic Antisymmetry in that they provide evidence for the existence of mirror structures which would be hard to explain within standard theory. The existence of mirror structures, in turn, supports the idea that movement is inherently connected with symmetry and—after all—with the necessity of meeting the extragrammatical requirements imposed by linearization on syntactic structures.

But, aside from these specific empirical cases, how does Dynamic Antisymmetry actually work? From a methodological point of view, you should not expect that a syntactic theory automatically derives *all* the instances of movement; like all the other components of grammar, syntactic movement cannot be straightforwardly *deduced* from the data, as it always happens in the empirical sciences. Linguists can only hypothesize that there is syntactic movement in a certain structure and verify that this hypothesis leads to satisfactory general empirical results, in terms of simplicity, as defined in the chapter 1. Therefore, in evaluating standard theory versus Dynamic Antisymmetry theory, we can only look at specific data. And there are data in favor of one theory and against the other, and vice versa. I think that at the moment we cannot conclude that one theory should be abandoned in favor of the other, and perhaps a unification could be proposed based on symmetry. Let's look at a simple example that may help us better understand this issue. We need to go back to Italian, because English does not exhibit the structure we are about to discuss and any other structure would require too long a digression.

Like English, Italian is a language in which the direct object usually follows the verb. Technically, Italian and English are called VO (verb-object) languages, unlike Japanese, which, as we saw in the first chapter, is an OV (object-verb) language. *Paolo legge libri* (Paul reads books) is grammatical in Italian, whereas **Paolo libri legge* (literally, Paul books

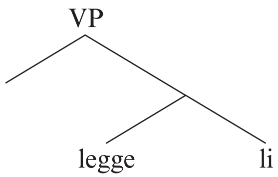
25. For a study of nominal structures that exhibit structural properties similar to those of inverse copular sentences, see Kayne (1994) and den Dikken (2006) and, again, Moro (1997, 2000).

read) is not. Assuming the LCA, we must conclude that the reason why the verbal head *legge,* reads, precedes the nominal head *libri,* books, is that it is prominent in the syntactic tree in spite of the fact that they are both heads. Why doesn't *legge libri* constitute a point of symmetry? Indeed, there is syntactic evidence in favor of the idea that *legge* is prominent on *libri*. As shown in the tree below (tree elements in parenthesis are optional), the direct object *libri* can be expanded by adding *molti* (many) as its specifier and *su Roma* (about Rome) as its complement and become *molti libri su Roma* (many books about Rome). A natural hypothesis is that this "syntactic space" that comes with *libri* is present even when *libri* does not co-occur with overt specifiers or complements, preventing a violation of the LCA.



But it is not true that the direct object *always* follows the verb in Italian. As you may remember from chapter 1, section 1.2.5., whenever the direct object is a clitic pronoun, it moves from its "natural place" to a position preceding the verb. For instance, *Paolo li legge* ("Paul reads them," literally, Paul them reads) is grammatical only with the clitic pronoun *li* (them) preceding the verb *legge,* while the other word order option is ungrammatical: **Paolo legge li*. In this case, Italian becomes an OV language and behaves like Japanese: the direct object has to precede the verb. Of course, it would be completely ad hoc to assume that the availability of both the VO and the OV order in Italian is due to a parametric variation *within* the very same language (call it "infraparameter" as opposed to the usual parameters *tout court* or interparameters). Assuming that the same language includes both values of a given parameter would not be an explanation and should not even be considered as a viable solution; moreover, no other phenomena would correlate with this parameter within Italian, as far as I can see.

Here is a simple and direct empirical question: Why does the direct object not move when it is *libri* while it has to move when it is *li*? To be consistent with the hypothesis we are assuming here, we are lead to assume that *li,* as opposed to *libri*, is just a head, witness the fact that it cannot be modified by a specifier or a complement, as shown by the ungrammaticality of **Paolo legge molti li su Roma* (literally, Paul reads many them about Rome). As illustrated in the tree below, there is no "syntactic space" between *legge* and *li,* in contrast to the case involving *libri*.



The *li* element, then, is a head, on a par with *legge*, but it cannot occur with its own complement or specifier: it is a naked head, metaphorically. Following Dynamic Antisymmetry, this very fact explains movement: it is just the fact that *li* is a head that triggers it. If a head is adjacent to another head, no linear order can be established. The head *li* cannot be at the same level as the head *legge*. The symmetry must be broken and *li* must move to a prominent position that would allow linear order to be established when the sentence is said.[26] Notably, *li,* in the sequence, does not need to be next to *legge* but can move even farther away—that is, many steps higher up the tree, arguably to a position where no symmetry is created, like an empty phrasal head similar to an unpronounced complementizer, *pace* morphological restrictions. In the sentence *Paolo li ha in ogni caso sempre letti* ("Paul always read them in any case," literally, Paul them has in any case always read) the direct object *li* has moved five words away from the main verb *letti.*[27]

26. When structural conditions are different, the point of symmetry can be neutralized by moving the verbal head, as in *leggendo-lo* (reading-it). The fact that both options are available supports an analysis according to which points of symmetry need to be neutralized by movement, although the analysis of nonfinite verbal forms (infinitival, gerundive, and participial forms) has not been fully developed yet.

27. In addition ot clitic pronouns, Italian has full (stressed) pronouns, which cannot be moved. For instance, *lui* (he/him), is the full pronoun corresponding to the clitic pronoun *lo*: *Maria ama lui* (Mary loves him.fullPronoun) versus **Maria lui ama* (literally, Maria him.fullPronoun loves), compared to **Maria ama lo* (literally, Maria loves him.cliticPronoun) versus *Maria lo ama* ("Maria loves him"; literally, Maria him.cliticPronoun loves). Interestingly, the full pronoun *lo,* unlike its clitic equivalent, allows for an element to intervene between it and the verb. Like a noun (*un tavolo*; a table), it can take the indefinite article *un*

One important observation before concluding. As we just saw, the verb-object order in Italian is not rigidly VO when the direct object is a clitic pronoun; in fact, it is OV for inflected verbs. This variation has been accounted for by means of structural reasons (the neutralization of a point of symmetry) rather than by means of the notion of parameter. If this analysis is confirmed, we may just wonder whether all parametric variations are actually forced by structural reasons and are not due to the degrees of freedom left open by universal grammar. This conjecture is compatible with the fact that these variations do not need to occur only between *two different languages* but can also be observed within *the same language,* whenever the relevant structural conditions are met, as in the case of the verb-object order in Italian. Of course, a lot of work needs to be done to show that, for example, the OV order in Japanese is due the need to neutralize a point of symmetry in order to allow for linearization.[28] This is at best just a conjecture. However, if it turns out to be true, it would make sense for us to start wondering whether all the parameters can be reduced to structural reasons and to specific lexical properties—for instance, the fact that a certain lexical category is or is not realized as a head in a certain language, or the fact that the lexicon of a certain language contains syntactic categories that do not need to be linearized because they are inherently phonologically null. *This would mean that we really no longer need the notion of parameters as "degrees of freedom of grammar" that need to be fixed by the learning child via experience: in a sense, there would be no freedom, as there is none when it comes to the position of clitics or head nouns in the Italian example just discussed.* But we have gone too far. Other pieces of evidence need to be found in order for this conjecture to be further developed.[29] What can we conclude with adequate certainty, then?

(a) as its specifier: *un lui* ("some guy," literally, a he/him). For a detailed proposal about clitic and full pronouns, see Moro (2000). What matters here is that the distinct position where the two pronoun types, namely *lo* and *lui,* occur only depends on their structural capacity to project a full phrase or serve as just a head. This is exactly what Dynamic Antisymmetry predicts.

28. For example, if in a certain language heads where allowed to be specifiers, then in that language the specifier of an object would constitute a point of symmetry with the verb selecting it, requiring movement of the object to a prominent position.

29. Thanks to Marina Nespor and Jacques Mehler for a critical discussion of these issues.

According to Dynamic Antisymmetry, syntactic movement within a hierarchical structure—one of the fundamental exclusive properties of the syntax of human languages—is a consequence of an intrinsically nongrammatical restriction: the physical or biological need to linearize the linguistic signal. If we were not subjected to this restriction—if we communicated telepathically, for example—hierarchical structure alone would suffice to establish the grammatical relations between the words in a sentence and (if this theory is correct) there would not be syntactic movement. Moreover, if some version of Dynamic Antisymmetry turns out to be correct, syntactic movement and phrasal structure would be directly related in a "causal" way; movement would depend on the need for phrasal structures—that is, hierarchical structures—to be compressed into linear sequences. The "causal" connection between these two properties, syntactic movement and phrasal structures, is welcome, since only the syntax of all human languages exhibit them, among all animal communication systems. If these two properties were independent, this coincidence should be explained in another way.

In my opinion, it is still unclear which of the two theories is correct.[30] My primary goal here is not to argue for the alternative theory and against the standard one. Rather, it is to show that it is possible to find a way to approach the central issue addresses here—why does syntax have precisely *these* limits?—from an extralinguistic point of view. *In fact, on the basis of the example of an intrinsically extragrammatical factor such as linearization, this alternative theory of syntactic movement shows that such a factor can have a deep structural impact in determining the "shape of grammar."*

Of course this does not exhaust the relation between grammar and the natural world. The neuroimaging experiments discussed in chapter 2 help

30. The interested reader may approach Dynamic Antisymmetry from the cited sources. Many issues are still unexplored (for the role of the notion of symmetry in domains other than syntax, in particular morphology, see Di Sciullo 2005). *Wh*-movement, on the other hand, is not: the analysis would suggest that *wh*-phrases are more complex than thought, and that they contain a point of symmetry. Thus, two simple sentences, *Who left* and *Lo vedo* (him saw), can be treated on a par as a way to solve the problem related to linearization. Moreover, constructions like the so-called *was-für* split constructions in German and some other languages, *Was für Bücher hast du gelesen?* ("What books have you read?," literally, What for books have you read?) would turn out to be analogous to inverse copular sentences where a predicative element (the bare *wh*-phrase) is raised and the subject (the residual nominal part) is left in situ.

us to understand the depth of the connection between grammar and neuropsychological aspects of the brain, which is still largely unexplored. Once again, I share the feeling with many that the appeal of scientific research is to open our minds to the amazement generated by progressive knowledge rather than to close our minds with an ephemeral sense of certainty.

We are at the end of our journey exploring grammar and the brain in search of the boundaries of Babel, an enigma that contemporary linguistics has highlighted by means of the question, Why aren't all conceivable grammars realized? Whatever the complete answer will be—if there will ever be one—we can now quite confidently state that the explanation cannot be purely historical, conventionalist or sociological; rather, it must be the effect of the neuropsychological architecture of the brain. It would otherwise be hard to explain why only those rules that are based on hierarchy activate the neural network which is selectively dedicated to language. On this journey I have tried to convey the same amazement and curiosity that I experienced while I was moving along the path that brought me to these observations about the coincidences between linguistic theory and the neuropsychological architecture of the brain.

We saw that language is acquired by experience, but only within the boundaries that are established by a biologically determined guide, although there is as yet no available evidence as to how this guide is linked to gene regulation. We saw that this hypothesis, which was initially formulated on the basis of purely comparative data, converges with neuropsychological data obtained with neuroimaging techniques. We saw that the fact that not all possible languages are realized can reasonably be considered the price to pay so that human languages can be acquired by children. Finally, we saw that it is empirically reasonable to pursue the hypothesis that some syntactic constraints imposed on natural languages depend on extragrammatical factors, that is, on physical and biological factors of human organisms, like the linear organization of the linguistic signal.

We may never be able to unify linguistics and neuropsychological models (more generally, physical and biological models). Still, what we already know shows that the two fields can interact in a fruitful way.

Epilogue

Jorge Luis Borges used to say that ultimately, books can only talk about other books. I conclude, therefore, with a quote of a quote, and no further comments. The original is from Leibniz's *New Essays on Human Understanding,* and Chomsky's *Aspects of the theory of syntax* led me to it.[1] If you recognize in this quote something of what I have said in this book, then I will have been at least partially successful in my task. If the book also made you curious to know more, even about just one of the issues it explored, then I will have succeeded in passing to you the baton that I received from my intellectual mentors.

I have also use the analogy of a veined block of marble, as opposed to an entirely homogeneous block of marble, or to a blank tablet—what the philosophers call a *tabula rasa*. For if the soul were like such a blank tablet then truths would be in us as the shape of Hercules is in a piece of marble when the marble is entirely neutral as to whether it assumes this shape or some other. However, if there were veins in the block which marked out the shape of Hercules rather than other shapes, then that block would be more determined to that shape and Hercules would be innate in it, in a way, even though labour would be required to expose the veins and to polish them into clarity, removing everything that prevents their being seen. This is how ideas and truths are innate in us—as inclinations, dispositions, tendencies, or natural potentialities, and not as actualities, often insensible ones, which correspond to them.

1. Leibniz (1765/1969, 52) and Chomsky (1965).

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