Chapter 1

Introduction

1.1 In the beginning

Battle in the mind fields: the characters in this story are, for the most part, a feisty and pugnacious cast. They come prepared for battle, they rarely take prisoners, and they enter the fray defending the faith. These are philosophers, psychologists, linguists, cognitive researchers of all stripes, the inheritors of the great classical questions that may live forever: what is thought? how is it that we are conscious of ourselves? how is it that humans are endowed with the gift of language? Is the multiplicity of languages in the world an indication that there are many ways of viewing the world, or are all the languages of mankind cut from a common cloth?

The great questions that have arisen over the ages by thoughtful people and the equally great answers that have been offered to them have been astonishingly stable over time. The great opposition between Plato's idealism and Aristotle's empiricism continues to this day to structure and to form intellectual debates.

This book describes a history of ideas, and of people, with the goal of understanding the present, and with the certainty that the only way to understand the present is to understand where it has come from. The reader may take that as a promissory note, to be repaid with the chapters that follow in this book. If our certainty derives from our own personal experience, it is also true that we have been struck by the way in which Pierre Bourdieu, for example, made the case for what he called anamnesis (with a slight nod towards Plato). He argued that a necessary condition for scientific progress was understanding explicitly the conditions (not to mention the context and the constraints) under which dominant scientific ideas had emerged. Why? Because the sine qua non of scientific progress is what we might call the *dis-*

enchantment of the scientific world. The student discovers a scientific world, ready-made and virtually magical; the scholar is on a first-name basis with that world; the scientist must eventually become their master, and in most cases, that means knowing how we got to where we are. Know where you came from, and you will know where you are going! [11] [12]. And so we will have to begin in the past: not as far back as we might — in ancient Greece, say— but with a rapid introduction to the minds of Descartes and Leibniz, and then on to the more recent times that are almost modern: the second half of the 19th century, when it seems that we can find the odd character here and there who is already contemporary, and many others who are almost there.

And everywhere people are responding and reacting to what they read, what they hear, and what they are told. That's only human nature. No one locks himself in a closet and refuses to be influenced by other people. Yet it is not at all rare to encounter brilliant thinkers who try to wipe the historical slate clean—*tabula rasa!*, as they say—and start over, afresh. Of course they *themselves* never do start over afresh, *themselves* unaffected by all the ideas and scholarship of the past, that would be impossible— but they send forth the message that the work of the past is unimportant. This seems very odd—and, well, it is. But it is not all that rare, and we will want to understand why this should be so.

The second half of the twentieth century saw the development of an overarching new view of mind that will be the focus of this work. Despite the importance of this perspective, it has no simple name. Calling this new view "cognitive science" is a good first try, but though the term seems broad in its scope, it is, we will see, too narrow and too particular. This new view is tightly bound to the machine that has changed our lives—the computer. But the connection is not a simple one. Computers, the real thing, first appeared during World War II, largely as part of the war effort, in the United States, in England, and in Germany. Computers were needed, at first to solve differential equations rapidly so that artillery could be more accurately aimed, and then in order to break enemy codes and encryption systems, and eventually to help in the development of the atomic bomb. But computers were not the simple source of the new ideas about mind: people were able to invent and create computers because these new ideas about logic and computation were already being developed. Technology, philosophy, logic, mathematics: all these fields were tied together in a complex unity that is no less real today than it was in the beginning of the twentieth century.

A second theme will play a major role, but it is one that plays at a slower rhythm, and it is the rise of modern science. To identify modern science with Galileo, Bacon, and Newton is undoubtedly too simple, but never mind: at

least it reminds us that we are talking now about a wave that had already begun by the early 17th century. The physical sciences over the past four centuries have been extraordinarily successful, and no thinking person could fail to see that. Like a sharp investor looking for a place to put his money, many thoughtful people have looked to the physical sciences to try to figure out what they are doing so right, and to see if there are lessons to be learned that could be applied elsewhere. The crass might call this physics envy; others will see it as prudence and good common sense.[1] [2]

In the end, the purpose of this book is to show that it is simply impossible to understand any of the three fields — linguistics, philosophy, psychology — over the last 100 years without seeing how they constantly interacted with the other two. Each field influenced, and was in turn, influenced by, the others. This interaction is rarely discussed, and when it is discussed, it is presented as a quaint corner of dusty history. We will try to show how wrong this view is, and how much these disciplines have suffered from being unaware of the origins of many of the most important ideas and values that have shaped them. And an important part of this intimate relation between the fields derives directly from the fact that these disciplines share deep historical roots, and in many ways were once one.

In fact, we will have occasion quite frequently to see an idea appear in one discipline as if it were new, when it was no more than a mole that had dug under a fence between disciplines and popped up on the other side. Disciplines are real, and they organize the reality of people's work, and they may at times act as barriers, but at every moment there are individuals who are passionately interested in issues that transcend any given discipline and whose work therefore becomes multidisciplinary. While it is possible to write a history of only a single discipline, it is not possible to research a history of a discipline and restrict oneself to that discipline: the reality, the boots on the ground, has always seen thinkers read and write across the disciplinary boundaries.

And our thinkers live in a political context as well, something that we will neither forget nor overlook. Our primary concern is with the ideas, and if we were to place ourselves on an internalist-externalist spectrum, we lean to the internalist side. But the impact of the political world in which these characters lived is simply too important for us to overlook.

Here is a snappy summary of what we will argue: a bibliography which

[1] Logic, information, and control: these are three important themes that will tie together many of the events and disagreements that we will look at.

[2] The debate that we will focus on over the course of this book involves the relationship between thought and the brain, between the brain and the computer, and between reasoning and logic.

goes back no more than five years is either unscientific or dishonest. On the central questions of the mind, the giants of human thought have preceded us, and we must remember that if we often disagree with them, we never leave them behind. It is critical that we remind ourselves that part of the essence of scientific work consists of confronting a vast library of ideas. When we know a field thoroughly, we find that 9 times out of 10 we can summarize and even evaluate a book by doing nothing more than reading the bibliography carefully.

We will try to accomplish several things in this book. One of our goals is to do what Saussure said needed to be done: we need to show the linguist what it is that he does—which is to say, to figure out what linguistics is.[3] Along the way, we will explore the ways in which linguistics, philosophy, and psychology have shared ideas, sent ideas back and forth over the porous lines that demarcate the boundaries between these fields. It is really quite astonishing to see an idea popping up in one of the fields, having burrowed over from another, often without a clear signal being sent that the idea actually has a history, both deep and rich.

The history of our professions, that is, has not generally been of much interest to the mainstream worker. We don't think that is a good thing, and we hope that the interest that we can arouse in this book will help, just a bit, to offset that apathy.

Needless to say, we have our own views on a number of subjects that we will discuss in this book, and we would not be unhappy if, as the result of reading it, some of our readers become convinced of our views. Still, that is not our primary aim, which is rather to show that among the great questions and ideas that have been central to the mind sciences over the last several centuries, there is more than one way to look at things. No matter how convinced you are of whatever you are convinced of, there is a good case to be made for other points of view. Progress generally comes from finding a new synthesis that brings together older ideas that seemed—but only *seemed*—to be in conflict.

---

[3] As Saussure wrote:

> Il y aura un jour un livre très spécial et très intéressant à écrire sur le rôle du mot comme principal perturbateur de la science des mots 127.

and in a letter to Meillet:

> l'immensité du travail qu'il faudrait pour montrer au linguiste ce qu'il fait [...] l'ineptie de la terminologie courante, la nécessité de la réformer et de montrer pour cela quelle espèce d'objet est la langue en général 14.

Our friends have warned us that this will not be an easy book to read. There are parts that are a bit dramatic, and there might even be some humor, but there are more that are difficult. Despite the tone, we do not offer a simplification of the issues. The reader who does not already have at least a smattering of knowledge of linguistics, philosophy, and psychology is going to be introduced to quite a number of unfamiliar characters and ideas. The reader who *does* have some knowledge of these fields is likely to have his assumptions challenged. We think, on the whole, that these issues have not been treated very well in the literature for the most part, and it has taken us decades to get to the point where we have been able to see some of these things.

Before getting down to serious business, there are several large generalizations in the dynamics which we observe and which we would like to point out, and develop a bit.

The first is that new ideas that catch on are always perceived by the catchers-on to be liberating them from not just a set of ideas, but verily from a *dogma* of an earlier generation. Each successful new way of looking at mind, language, and reasoning is viewed as a *notional liberation movement*. This way of putting it captures both the heady revolutionary fervor that comes along with a new scientific perspective, and the sensation that a new perspective brings out explicitly what was wrong with the old conventional wisdom—and now, being explicit, we can drop it, put it behind us, and move forward with new vigor. One of the ideas we will try to spell out is that we never completely drop old ideas: they remain with us, often getting harder and harder to see consciously, which is generally not a good thing. But one of the constants we will hear in the stories retold by participants in the academic "revolutions" we will look at is that that each, individually and together, felt that they had had a great weight lifted from their shoulders, and that weight was the weight of a heavy past tradition. The behaviorists felt that way (p. 394); the cognitive psychologists (p. 331); the early generative grammarians (**??**); then the later generative grammarians [4] and the list could go on and

[4]Chomsky expressed this emotion very well at one point, when he wrote:

> The whole history of grammar, for thousands of years, had been a history of rules and constructions, and transformational grammar in the early days, generative grammar, just took that over. So the early generative grammar had a very traditional flair. There is a section on the Passive in German, and another section on the VP in Japanese, and so on: it essentially took over the traditional framework, tried to make it precise, asked new questions and so on. What happened in the Pisa discussions was that the whole framework was turned upside down. So, from that point of view, there is nothing left of the whole traditional approach to the structure of language, other than

on.

It follows from this that if you do not understand how it is that a once dominant idea could have captured the imagination of smart, young people, then you simply do not understand it. All new ideas that grab the imagination of new people in a field do so because they are perceived as liberations from some kind of orthodoxy of the past.

[We have not yet defined or explained doxa, I think.] Sociology reminds us that a scientific construction worthy of its name becomes a doxa, in the sense that Bourdieu expounded: and dogma is someone else's doxa. All new thought is in some fashion or other heterodoxy.

And sociology also reminds us that it is not always best to focus too much on the individual: as Bourdieu [10] put it, it is not so much that heir inherits the inheritence, in the world of ideas, as it is the inheritence which inherits the heir! We should not be too shocked to discover that systems of positions and dispositions are reborn in each individual in each new generation of scholars. Ce sont les constructions idéologiques, les systèmes de postions et les dispositions qui trouvent à s'incarner à nouveau dans un individu. Comme le disait Marx dans la préface à l'Idéologie allemande, reprenant la vieille maxime des juges et des notaires : "c'est le mort qui saisit le vif"

Here's another way to put pretty much the same point. The *force* that we can feel when we read the work of giants of the past who have preceded us is their ability to *make us think today*. At the same time, the most profound contributions have always been the result of a thorough knowledge of orthodoxy and its dogma mixed together with a passion for heterodoxy. There is no deep mystery why this should be so. It is the simple result of the fact that no one thinks alone, or starts over from scratch, from zero. (Yes, many seem to think that they do—Descartes being an obvious example—but from anyone's perspective today, Descartes was as thoroughly embedded in

---

> taxonomic artifacts, and that's a radical change, and it was a very liberating one. The principles that were suggested were of course wrong, parametric choices were unclear, and so on, but the way of looking at things was totally different from anything that had come before, and it opened the way to an enormous explosion of research in all sorts of areas, typologically very varied. It initiated a period of great excitement in the field. In fact I think it is fair to say that more has been learned about language in the last 20 years than in the preceding 2000 years.

The last sentence is certainly a show-stopper: you either believe it—or you are stunned by its scientific immodesty. But that is not our concern here, which is just the lightheaded feeling of liberation that so often comes along with being part of a movement that takes itself to be revolutionary. Martin Joos, one of the ornery members of the post-Bloomfieldian generation, must have had this in mind when he wrote that "[l]inguistics has been preeminnently a young man's pursuit ever since the 1920's." Source?

his intellectual and political time as anyone has ever been.) All thinking takes place in a dialog: when when we argue against someone else's position, that other person is present, there, in the dialog. Show us a paper whose bibliography goes back no more than five years, and we will show you a paper whose conception and expression both stutters and limps.

1.1.1 The New Science

The second generalization is closely linked to the first. Every generation wants to make its field be scientific—and to be the first generation that has succeeded at that. Within the mind sciences (linguistics, psychology, and philosophy), each generation rebukes the previous generation for having wrongly thought that it had its hands on a legitimate scientific method and framework, and then it goes on to offer what it takes to be a *truly* scientific vision.[5] Seeing this happen over and over again has given us not only reason to be more cautious with our claims, but opened our eyes to the ways in which the study of the history of our disciplines, done honestly, is a fine tonic for encouraging the development of a long-lasting sense of intellectual modesty.

Here is a modest beginning of some citations in which linguists observe that *finally* linguistics has become a science (italics have been added in each case below):

- Josiah Willard Gibbs 1838 [31] [republished in a book in 1857] *Philological Studies*: Since the commencement of the present century, and especially within the last fifteen years, the philosophy of language has been pursued with great ardor, and the learned on the continent of Europe, *by following the grand Baconian principle of induction, have placed this science on a solid basis*, and are in the way of most important discoveries. These discoveries are modifying the grammars and lexicons of every language.... [p. 3; dated 1857] The new method of

[5]Observers of science tend to focus on how science progresses, and what it gets right. We will be just as interested in what leads people to get things wrong about the mind. Not that we will presume that we always know what is right; but sometimes we do, and we can often learn as much (if not more) by seeing how people virtually prevented themselves from seeing the truth about something. Closely related to this point is another: we will pay attention to how movements in the study of mind offered their own theories for why it was that people did not always agree with them. Usually the reason offered was some variant of the message that the unconvinced were stuck in the ways of the past, with varying chances of being able to extract themselves from their benighted tradition.

The overarching theme of philosophy, psychology, and linguistics over the last 150 years has been the effort to find a common means to explain thought: not the fleeting passage of thoughts through consciousness, but the solid paths of valid inference.

grammar has a thorough and proper unity, because it commences with the proposition, as the central point. The value of every word and of every form is made to depend on its relation to the proposition. This develops the organic relations of language, and gives to the new method a scientific form.... The new method...of course is the same for all languages. Different languages may all be analyzed in the same way.[p. 5; dated 1847]

- William Whewell 1858. Another science, cultated with great zeal and success in modern times, compares the languages of different countries and nations, and by an examination of their materials and structure, endeavours to determine their descent from one another: this science has been termed *Comparative Philology*, or *Ethnography*; and by the French, *Linguistique*, a word which we might imitate in order to have a single name for the science, but the Greek derivative *Glossology* appears to be more convenient in its form. [89], Volume 2, p. 258. (emphasis in the original)

- Benjamin W. Dwight 1859 In old classical usage, [philology] meant the love of literature; afterwards the scholastic mastery and exposition of language; more recently a sort of general amateur study of language, as a matter of mere pleasant curiousity; and last of all, the scientific exploration and comprehension of its interior mechanism, in relation both to its original elements, and also to their varied transformations, through a wide range of comparative analysis. p. 215 [**?**]

- Max Mueller 1861 *The science of language* presented as lectures in 1861: *The science of language is a science of very modern date.* We cannot trace its lineage much beyond the beginning of our century, and it is scarcely received as yet on a footing of equality by the elder branches of learning. We hear it spoken of as comparative philology, scientific etymology, phonology, and glossology. In France it has received the convenient, but somwhat barbarous, name of *Linguistique*....p. 23: We do not want to know languages, we want to know language; what language is, how it can form a vehicle or an organ of thought; we want to know its origin, its nature, its laws; and it is only in order to arrive at that knowledge that we collect, arrange, and classify all the facts of language that are within our reach.

- Max Mueller 1863 : In a course of lectures which I had the honour to deliver in this Institution two years ago, I endeavored to show that the language which we speak, and the languages that are and that

have been spoken in every part of our globe since the first dawn of human life and human thought, *supply materials capable of scientific treatment*....we can treat them, in fact, in exactly the same spirit in which the geologist treats his stones and petrications, nay in which the botanist treats the flowers of the field, and the astronomer the stars of heaven. there *is* [emphasis in original] a science of language, as there is a science of the earth, of its flowers and its stars; and though, as a young science, it is very far as yet from that perfection which...has been reached in astronomy, botany, and even in geology, it is, perhaps, for that very reason all the more fascinating. Macmillan's magazine march 1863, vol VII, no .l 41, p. 337-349.

- 1864 (published in 1867) Whitney: *The Study of Language*. p. 1: *Those who are engaged in the investigation of language have but recently begun to claim for their study the rank and title of a science.* Its development as such has been wholly the work of the present century, although its germs go back to a much more ancient date. It has had a history, in fact, not unlike that of the other sciences of observation and induction—for example, geology, chemistry, astronomy, physics—which the intellectual activity of modern times has built up upon the scanty observations and crude inductions of other days....But to draw out in detail the history of growth of linguistic science down to the present time, with particular notice of its successive stages, and with due mention fo the scholars who have helped it on, does not lie within the plan of these lectures....its execution would require more time than we can spare.

- 1871 August Schleicher, Introduction to his *Compendium*. This begins with a passage so modern that we cannot present less than the first two paragraphs:[6]

> Grammar forms one part of *the science of language*: *this science* is itself a part of the natural history of Man. Its *method is in substance that of natural science generally;* it consists in accurate investigation of our object and in conclusions founded upon that investigation. One of the chief problems of *the science of language* is the inquiry into, and description of the classes of languages or speech-stems, that is, of the languages which are derived from one and the same

[6]Translated by Winfred Lehmann, in A Reader in Nineteenth Century Historical Indo-European Linguistics, on internet

> original tongue, and the arrangement of these classes according to a natural system. In proportion to the remainder but few speech-stems have hitherto been accurately investigated, so that the solution of this chief problem of the science must be looked for only in the future.
>
> By grammar we mean *the scientific comprehension and explanation of the sound, the form, the function of words and their parts, and the construction of sentences*. Grammar therefore treats of the knowledge of sounds, or phonology; of forms, or morphology; of functions, or the science of meaning and relation, and syntax. The subject of grammar may be language in general, or one particular language or group of languages; grammar may be universal or special: it will in most cases be concerned in explaining the language as a product of growth, and will thus have to investigate and lay down the development of the language according to its laws. This is its exclusive province, and therefore its subject is the laying-down of the 'life of language,' generally called historical grammar, or history of language, but more correctly 'science of the life of a language' (of sound, form, function, and sentence), and this again may be likewise as well general as more or less special.

- Ferdinand de Saussure wrote, around 1891:

> Ici se présente cette objection plus ou moins fondée selon nous : vous transformez l'étude des langues en l'étude du langage, du langage considéré comme faculté de l'homme, comme un des signes distinctifs de son espèce, comme caractère anthropologique ou pour ainsi dire zoologique. [...] les plus élémentaires phénomènes du langage ne seront soupçonnés, ou clairement aperçus, classés et compris , si l'on ne recourt en première et dernière instance à l'étude des langues. Langue et langage ne sont qu'une même chose ; l'un est la généralisation de l'autre. Vouloir étudier le langage sans se donner la peine d'en étudier les diverses manifestations qu'évidemment sont les langues est une entreprise absolument vaine et chimérique; d'un autre côté vouloir étudier les langues en oubliant que ces langues sont primordialement régies par certains principes qui sont résumés dans l'idée de langage est un travail encore plus dénué de toute signification

sérieuse, de toute base scientifique véritable.

115 (circa 1891-1894) [26] *Ecrits de linguistique générale* paris Gallimard 2002

- Leonard Bloomfield wrote in 1924, reviewing Saussure 1922 *Modern Language Journal* 8 p. 319:

> The essential point...is...that de Saussure has here first mapped out the world in which historical Indo-European grammar (the great achievement of the past century) is merely a single province; *he has given us the theoretical basis for a science of human speech.*

- 1924: Hermann Collitz (in "The Scope and Aims of Linguistic Science") turns directly to the question at hand. Is linguistics a science? He writes,

> In order to ascertain whether and to what extent linguistics is entitled to the name of a science, we must remember that in Modern English the term 'science' may be understood in two different ways, viz: (1) in a broad sense. . . i.e., scholarly knowledge; (2) in a more modern and more technical sense, so as to be applied exclusively to branches of learning concerned with permanent and invariable relations, such as mathematics, chemistry, physics. These and similar sciences, it is claimed, are able to make predictions for the future. If interpreted in this way, the term would not be applicable even to the evolutionary branches of natural science, such as geology and biology....*The science of linguistics is...concerned with uniformities and permanent or steading recurring conditions in human speech generally.* We may count here, e.g., topics like the relation between language and dialects, the causes of phonetic change, the anture of phonetic laws, the mutual relation bewteen appellatives and proper names, the variosu systems of counting, etc....As branches of linguistics concerned with permanent conditions, we may claim, above all, general phonetics and general grammar. Phonetics nowadays has assumed such proportions as almost to constitute a science by itself...In general or 'philosophical' grammar, on the contrary, stress is laid principally on the relation between grammatical forms and mental categories.

- 1925: Leonard Bloomfield wrote, on the first page of the first issue of the journal (*Language* 1:1, p.1!)

> The layman—natural scientist, philologian, or man in the street—does not know that there is a science of language. *Such a science, however, exists*; its aims are so well defined, its methods so well developed, and its past results so copious, that students of language feels as much need for a professional society as do adherents of any other science.

- Bloomfield 1933 *Language* [7] It is only within the last century or so that language has been studied in a scientific way, by careful and comprehensive observation.... p. 3.

- George Zipf 1936 (George Zipf did not like the term "linguistics," because he thought linguistics in the United States was too uninterested in broad scientific questions) Dynamic philology [Zipf's approach] has the ultimate goal of brining the study of language more into line with the exact sciences. To this end it views speech-production as a natural psychological and biological phenomenon to be investigated in the objective spirit of the exact sciences from which its methods have been taken. (p. 3)

- Charles Hockett 1941 [**?**] *Linguistics is a classificatory science.* The starting-point in such a science is to define (1) the universe of discourse and (2) the criteria which are used in making the classifications.

- Bloomfield 1943 Language 19 p. 198, speaking of Boas: The native languages of our country had been studied by some very gifted men, but none had succeeded [before Boas] in putting this sutdy upon a scientific basis.

- CC Fries 1962 "The Bloomfield School" in *Trends in European and American Linguistics*: All those who knew Bloomfield best seem to agree that his chief professional concern was to develop *linguistics as a science.* [emphasis in original]...There has been considerable difference of opinion from time to time as to the demands of 'scientifi' linguistics, but concerning the label fo rthe ultimate goal itself there has been unanimity. (198)

- Bloch, obituary of Bloomfield, p. 92 There can be no doubt that Bloomfield's greatest contribution to the study of language was to make a science of it. Others before him had worked scientifically in linguistics;

but no one had so uncompromisingly rejected all prescientific methods, or had been so consistently careful, in writing about language, to use terms that would imply no tactic reliance on factors beyond the range of observation."

- 2007: Boekcx and Piatelli-Palmarini in press [70] "We are among those who are persuaded, on solid grounds we think, that *in the past 50 years* linguistics has progressively established itself as a genuinely scientific discipline."

It is all too easy to brush off comments like, "linguists in the 1860s thought that they were engaged in science, too," but when one sees the same message published non-stop over more than a decade, anyone with a bit of common sense has to admit that there is enough reason to stop and think about why it should be that scientists of the mind think, again and again, that they are the first ones to approach the problem scientifically.

As linguists who came of age in the late 1960s, we ourselves saw the two effects we mentioned above among the generative grammarians we were proud to be a part of. We prided ourselves on our liberation from the shackles of behaviorism and other forms of empiricism, and we felt that generative grammar finally brought linguistics into the same playing field as other sciences. *Finally*, we thought, linguistics had developed formal theories that were both worthy of the complex data that emerges from a careful study of the data, and worthy of the efforts of scholars who understood the power of formal mathematical models. We forgot that we were ourselves the children of a revolution which itself had been equally forgetful: the structuralist revolution which drew a blank slate, and reinvented the human and social sciences during the half-century stretching from 1910 to 1960 by setting up the abstract notion of structure as the king in the kingdom of concepts that could explain everything.

We haven't forgotten the feeling, but we have come to realize that we are not the only ones to feel this way. We were preceded by any number of generations of researchers who felt just the way we did—and we have been followed by younger scholars who feel that linguistics is finally about to make it as a scientific field, for the very first time. We do appreciate the irony. When we go back and read the early publications of such benighted predecessors as John B. Watson, the father of behaviorism, or the followers of Ernst Mach, the godfather of the Vienna logical positivists, or the structuralist linguists whose work forms the basis of our field today, we don't find stupid statements— we find people trying to cast off the chains of an official orthodoxy that they are certain is superannuated and simply standing in the way of scientific progress. And yet in the versions of intellectual history that

have been written by later generations of victors of the battles in the mind fields, time and time again, the earlier approaches are described in a way that is so simple-minded that we can hardly take seriously anyone who went down that particular road. And yet, those folks, our intellectual ancestors, were no more stupid than we are today. Something must be wrong with the history books.[7]

Well, maybe it's *not* the history books that are at fault. Maybe it's the simplistic conception of history that is wrong. There is nothing wrong with seeing history as a linear sequence of events, marked with dates and places—but that's not the whole story, and it's not even a large part of the whole story. We cannot see the whole story unless we succeed in seeing the shifting tectonic plates of our history, composed of and populated by ideas and ideologies, including many global visions of the world that continued to have an impact on how scientists constructed and viewed the objects of their investigations.

Let's take an example. For the vast majority of contemporary linguists, regardless of the school to which they adhere, structuralist linguistics as it was practiced between 1925 and 1965 is as foreign as blast of light that still reaches us from the Big Bang, a blast that is now reduced to a background buzz in the sky. Even contemporary views that recognize a debt to structuralism seem to view it as a dried up well that was once the inspiration of a golden age.[8].

In the history of science and that of ideas, the *thickness* of time is not uniform. Forty years separates us from the publication of *The Sound Pattern of English*, the manifesto of generative phonology published by Noam Chomsky and Morris Halle in 1968. And still it seems to be alive, living among its contemporaries in generative phonology. On the other hand, if forty years separated Leonard Bloomfield's set of postulates for linguistic theory from Chomsky and Halle's *opus*, linguists in the 1970s could conceive of classical structuralism only as an obscure theory from an obscure time, a time that was almost literally pre-historic. It is almost as if space-time had been warped to such a degree that neither light nor information could reach us from that time. And this is all the more surprising in that any linguist over the age of forty, to say nothing of the founding fathers of our current schools, were all trained in the methods and concepts of structuralism, whether we knew it or

[7]For the impatient reader who wants an example to chew on, take the case of the theoretical object known as the syllable. During the 1890s, Saussure developed a theory of what he called "sonants," which took high vowels to be not flesh and blood phonemes, but as abstract voiced coefficients which were interpreted by virtue of the phonotactics of the "sound chain." [25]. For two recent discussions, see Tifrit [83] and [34].

[8]Langacker, Talmy, Lamb, West Coast linguistics

not. There isn't a student in linguistics from that generation who didn't burn the midnight oil trying to solve problems built from data torn from the pages of IJAL, the *International Journal of American Linguistics*, to say nothing of problems xeroxed out of the standard textbooks of the 1950s: Gleason 1955, Hockett 1958, Joos 1957. [33], [**?**], [55]. We know whereof we speak: we were there ourselves, we burned that midnight oil.

What is the cure for this selective amnesia that leaves us blind to our own origins? This is the work that we alluded to above under the rubric of Bourdieu's anamnesia, the first goal of any study of the history or the epistemolgoy of a discipline. If it sounds suspiciously like psychotherapy, then so be it! We will need to bring out into the light of day the hidden linkages among ideas, sometimes denied because they show connections to ideas that seem embarrassing or honteuses ou d'evalorisantes. And just that: we will also need to bring out the underground ruptures that were never publicly acknowledged.

We are not ready to think about intellectuals as a spontaneous product of a virgin birth, or as creative powerhouses free of any and all external influences. We cannot understand theoretical frameworks without understanding the linkages and influences that helped to meld and form them. To speak today of intellectual *genealogies* is a bit loaded today, as the phrase comes with many thoughts of Foucault and his take on the history of thought, much of which finds no resonance in our account. But genealogy is important: it is important for understanding a patrimony passed down in ways both conscious and un-; it is important when trying to unravel the conflicts and tensions which sometimes are passed down more as that-about-which-nothing-should-be-said than as any sort of explicit inheritance. One of the themes that promises to teach us a lot about ourselves is the deathly silence that has for so long hovered over the question of how the work of Bloomfield, Sapir, and their students has been a fundamental component of all subsequent American linguistics, including the most dominant perspective, generative grammar.

Our discussion in this book is also at times a struggle against a popular, romantic, and largely indefensible interpretation of Thomas Kuhn's view of the history of science, an interpretation which glorifies the rupture of a scientific paradigm with respect to its predecessors. We will return to Kuhn at the moment when his *Revolutions* fits into our story, but we won't be revealing any secrets before their time if we remind the reader that Kuhn's greatest impact on his readership was the way in which he brought to life the degree to which scientific research was not plodding and ahistorical. [more]

Tout au contraire, Kuhn insère aussi son concept de révolution scientifique comme changement de paradigme dans une conception cumulative de la sci-

ence de ses problématiques et de ses empiries. C'est au sein du paradigme ancien, à partir de ses problématiques et en réorganisant ses propositions que se dégage le paradigme nouveau. Comprendre la modernité de la linguistique c'est donc aussi nous réapproprier son histoire et montrer tout ce que le nouveau doit à l'ancien

This book is our way of broadening the questions that lie behind these observations, so that we can all learn these lessons from our history. And we hope we can pass on to the reader as well the pleasure that comes from tapping into the deep intellectual currents of the writings that form our patrimony.

Looking ahead

Over the course of this first chapter, we will give a quick introduction and overview of the themes that will concern us through the rest of the book. We will first look at the nature of rupture and continuity in disciplinary settings, and after that, take a step back and take a look at the very fundamental question: why do we believe the things we believe? And then we will look at some of the essential characteristics of science: this is, after all, a book about the mind sciences. Finally, we will take an initial look at the rupture and continuity that binds the scientific and philosophical perspectives known as empiricism and rationalism.

In the following chapters, we will take a roughly chronological perspective on the mind sciences, beginning with a closer look at the origins of modern science in the 17th and 18th centuries. [...]

1.2 Rupture and continuity, both seen and unseen: ideas and positions

The history of the mind sciences is one of both rupture and continuities, and one of our main tasks will be to figure out how this can be so. The main answer is simple enough, though. When we focus on the *ideas* in this story, what we see is a braid of ideas that interconnect and develop over time, and our story is one of continuity. When we focus on the *intellectual positions* taken by the individuals in the story, we find bold statements that separate rival camps, and ruptures of various sorts. Both of these perspectives are real, but neither of them, taken individually, is the whole story: the whole story is found only in seeing both, together, at the same time.

In his brilliant book on Galileo, Kepler, and Newton, I. Bernard Cohen found himself trapped by the conflict of these two regimes. In Chapter 5,

he surveys the evolution of the ideas of motion, impetus, and inertia, and the development of these notions in the centuries before Galileo. One thing is perfectly clear: the world did not jump directly from Aristotle's view of motion to Galileo's, even if Galileo, and more modern scholars, would like to give that impression. Cohen writes,

> Galileo's originality was therefore different from what he boastfully declared. No longer need we believe anything so absurd as that there had been no progress in understanding motion between the time of Aristotle and Galileo. And we may ignore the many accoutns that make it appear that Galileo invited the modern science of motion in complete ignorance of any medieval or ancient predecessor.p. 105

If you actually *read* the physics literature in the centuries preceding Galileo's work—the work of many thinkers, including Nicole Oresme and XX—you cannot fail to appreciate the continuous conceptual development during these centuries, and that is Cohen's discipline, after all. And Cohen clearly senses that there is a conflict at some level or other between demonstrating the continuity in the development of the ideas, on the one hand, and the desire to point out how brilliant, creative, and special was the character and work of such men as Galileo. And so he writes,

> By making precise exactly how Galileo advanced beyond his predecessors, we may delineate more accurately his own heroic proportions. p. 105.

Heroic—that says it all. When we focus on individuals and their life stories, we build heroes, and occasionally vilains, and certainly buffoons. We explore the jealousy, we wonder at the rages, but the more we learn about the actual life of the ideas, the more we grow to distinguish the personal strengths and flaws from the relentless advance of ideas.

Needless to say, perhaps, Cohen is one of the greatest historians of science of the 20th century, and we do take his perspective seriously, even if we do not agree with it. He insists on the importance of great leaps of individual minds:

> We do not fully understand why or under what conditions, a few hardy individuals are from time to time led to think in wholly new directions, but the fact is that they do. [FN 6] [22] p. 506.

though he adds,

> New ideas are rarely creations unrelated to the general background of ideas (506)

Here is another way in which the conflict between the regime of ideas and the regime of people has been treated. Claude Allegre [**?**] *L'écume de la Terre*, p. 21, introduced the history of the development of the notion of tectonic plates, first suggested, he says, by Antonio Snider-Pellegrini in 1868, and developed in the following years by others, including Elisée Reclus, and Frank Taylor. But it was Alfred Wegener, writing in the second decade of th 20th century, who is generally given credit for the idea. As Allegre notes,

> Il a défendu sa théorie avec fermeté, mais sans pugnacité excessive, jusqu'à son dernier jour. Aussi doit-il être considéré comme le père de la théorie de la dérive des continents. Comme l'expriment clairement Georges Duby (footnote reference to *Les Trois Ordres ou l'imaginaire du féodalisme*, Gallimard, 1978) il faut, en matière de référence et d'antériorité, se fixer une règle simple: c'est celle qui distingue clairement entre une opinion émise parmi d'autres, de manière plus ou moins furtive, et une oeuvre construite, argumentée, développée autour d'une idée. "La référence à l'une est anecdotique, à l'autre elle est centrale et obligatoire."

Allegre is trying to solve a problem that simply does not exist, which is to say, he is trying to resolve the inevitable conflict between the continuity that inheres in the world of ideas and the rupture that we insist must exist in the world of actors so that we can fairly and justly apportion credit for originality. Perhaps that is too crude a formulation: of course there is *a* problem, a problem of credit assignment, because that is how our modern world today works: we expect there to be an answer to the question: who deserves the credit for the idea of continental drift, the idea that continents are floating on tectonic plates? But this credit problem is not one which aligns sharply with any significant, or even meaningful, question in the history of ideas. In the world of ideas, continuity is the dominant characteristic, and the ruptures that we would need to find in order to justify the simple statements that Smith or Jones was the true originator of a certain idea are simply not to be found.

Let's be clear on this, then: the distinction between the ideas and the intellectual positions that we will study is an artificial one, in the sense that you cannot have one without the other. There is no history of ideas to study if there are no scientists around to develop the ideas, and there are no scientists

to make bold claims if there are no ideas. Nonetheless, the difference is both useful and important if we are to get a better understanding of much of what happens in the history of ideas, and in particular, if we are to understand how the history of the mind sciences could be simultaneously a story of rupture and of continuity.

There is another, very important point that will emerge over the course of our discussion that gives us rather good reason to be skeptical about some of the rupture that is more apparent than real in the intellectual lives that we will look at. We do not live in a time when we are called upon often to respect the great traditions of the past. Instead we live in a time where the early bird gets the worm, and the first horse out of the box has the best chance, and the Devil take the hindmost. There is an effect of *perspective* (borrowing a notion from Bourdon) which plays a major role in the writing and the transmission of scientific knowledge, and one of the effects of this perspective is to emphasize and magnify the perceived differences between what one is doing, oneself, compared with what others have been doing up to now. Ancient history, as one wag put it, ends around the time one is born—or maybe in our case, around the time one goes to college. That would put most of this book in the realm of ancient history for both of its authors—for us, ancient history ended some time around 1970. But that means that we were taught by people for whom a good deal of what we look at in this book was *not* ancient history: it was very much the lived past. But when we look back at the mind fields, and look at the unfolding and the development of the ideas we will explore in this book, we see a great deal more continuity and connection than did the people who lived through the period and knew it, who really knew it and lived it. Who is right?

To each his own opinion.

1.3 Why do we believe the things we do?

> Si une théorie peut être vraie ou fausse, elle peut aussi nous paraître intéressante ou inintéressante sans que son intérêt soit nécessairement lié à sa validité ni qu'il se réduise à son utilité. Plus précisément, si nous avons tendance à exiger d'une théorie que nous jugeons intéressante qu'elle soit vraie, c'est par des moyens différents que nous lui accordions chacun de ces deux attributs. Bien souvent, nous nous reposons sur le principe d'autorité pour accepter telle théorie pour vraie. Mais c'est par nous-mêmes que nous la jugeons intéressante ou non. Nous consentons aisément à nous en remettre à autrui s'agissant de la vérité de telle

> théorie, mais nous nous faisons plutôt confiance à nous-mêmes s'agissant de son intérêt.
>
> Supposons maintenant une théorie fausse ou douteuse. Supposons en outre que cette théorie émane d'une communauté de chercheurs investie d'une autorité scientifique, qu'elle soit naturellement traitée comme une boite noire par tel groupe de personnes et que, par un effet de position ou de disposition, ce groupe soit porté a juger la théorie intéressante. Dan ce cas, il y a toutes chances pour que cette théorie soit perçue comme vraie par le group en question.
>
> Raymond Boudon, *l'Idéologie*, p. 172-3.

In the chapters that follow, we have chosen to take a historical perspective on how the mind has been treated not just because the story is an interesting one, but because if we take it seriously, it challenges us to face the most difficult question of all—why do we believe the things we do? We are going to look at perspectives in the past, and observe new ideas come on the scene and capture and fire up the imaginations of linguists, philosophers, and psychologists. Some people adopt the new ideas, and some don't. Why the difference? Why do some people believe the new ideas? Why are others unconvinced? The question is ultimately one which we face all the time: if there are two different opinions in one of these fields, why should rational people not simply all agree? How can there be two different opinions held at any given time? What accounts for changes in belief, especially scientific beliefs—which is to say, beliefs in scientific theory? Many people have pointed out, with varying degrees of graciousness, that the older one is, the harder it is to change one's views about basic scientific questions, but even that (no doubt correct) observation stands in need of explanation: is it to be explained by hormones and brain deterioration, or by rational risk-aversion, or because the older scientists understand better than the younger ones the reasons why the current orthodoxy came to be dominant? Whichever account turns out to be correct makes a difference for the conclusions that we draw from it.

A slightly different version of this same question is this: why have people *ever* had false beliefs about scientific questions? Is it simply that in the absence of a good theory, people adopt the simplest account, and the history of science is the history of explanations of ever-growing complexity? If not (and the answer to that question is certainly No), where do false hypotheses come from?

Needless to say, the question of why we believe what we do is an enormous one, and we do not think we can settle it with a few remarks, or

even a few chapters. But we can at the very least bring out into the open the fact that we have many ways at our disposal to account for why a person believes something—and this book is all about people believing various things.[9] These different ways sort themselves out roughly on a scale from perfectly respectable to the not respectable at all. At the respectable end of the spectrum, we can account for someone's belief by showing that the belief follows a familiar and unobjectionable logical path from a set of well-founded assumptions to a conclusion, which is the belief we are discussing. That's fine, but that is also rarely the case, in situations that we will care about. More often, we will deal with cases where a conclusion is drawn based on some abstract principles whose application in any given case is a bit murky, and whose certainty is imperfect. A perfect example of this is appealing to Occam's Razor, as many of the subjects of our story do. It's fine to say that multiplying objects needlessly is to be avoided, but it's often not clear how to count the number of objects or what "needless" means in any given case.[10]

When the social character of knowledge—scientific or otherwise—comes into the picture, things get more difficult still.[11] Many of us get a bit uncomfortable when the discussion comes round to social forces in the development of science. Science, some of us like to tell ourselves, should perfectly well be able to get along without social forces. That's fine, but for science to work in this modern age, individuals must trust others. But trust how far? None of us can perform every experiment; we have to rely on refereed journals and their reports. But it's not that simple either. Most of us cannot, and do not, establish the mathematical validity of every formula we use. We need to trust established authority. But how far? To some extent, the practice of science requires individual scientists to be skeptical, but obviously the value of skepticism and the need for trust not only *can* but *must* come into conflict on a regular basis. It's only rational for a scholar to assign a label with a trust-worthiness rating to established people in a field: if Bloomfield says something, then it's probably right—or it's probably wrong, depending on what rating you have put on Mr. Bloomfield's pronouncements.

All of which is to say that the line between between utterly rational thinking and band-wagon effects in a discipline can be difficult to draw, and

[9] We are endebted to the discussion of this issue in Bourdon, Ideologie and XX.

[10] *Entia non sunt multiplicanda praeter necessitatem*: mais tout d'abord la phrase n'est pas d'occam mais de l'un de ses élèves; Occam écrit : *Pluralitas non est ponenda sine neccesitate*" ou bien *Frustra fit per plura quod potest fieri per pauciora*; en tout cas le rasoir d'occam ne sont confound pas comme on le fait souvent avec le principe du tiers exclu.

[11] In the anglo-saxon literature, Merton [61] draws our attention to the early statements such as Francis Bacon's on the Idols, and Voltaire's on the "priestly lie".

can very much depend on what position one looks at things from. A relatively neutral term to use to refer to this would be to describe it as the problem of *authority* in science. No one would object to the statement that there are, indeed, people who are authorities in various scientific subjects, and their authority is recognized not just by people outside of their fields, but also by people within their fields, not to mention those people who have just one or two toes in their fields. What do we do when authorities do not agree? How far do we trust an authority?

At the outset, let's just take note of the fact that there are quite a number of different ways of accounting for why people may disagree, and each of these ways based on a slightly different understanding of why it is that we believe things in the first place. It is true, too, that we tend to give different accounts of why we ourselves believe things and why other people believe things, especially when those other people are living in a different place or time.

There is, first of all, a major divide between two kinds of explanations that we offer along these lines. Explanations of the first kind are pretty simple. They take the form that Smith believes something (which we'll call Smith's belief) because he observed it, or because Smith already knows enough things which, taken together, allow anyone with a basic knowledge of logical inference, to draw Smith's belief as a conclusion. These are the easy cases.

The harder cases are those where we want to offer an account like the following. At the top of the list we put the first, the easy case we just mentioned.

1. Smith concluded X because he could logically deduce X from the conjunction of some general principles he knew, plus some observations.

2. Smith concluded X because it was the simplest hypothesis, given the data available to him.

3. Smith concluded X because his philosophy of science urged him to do so.

4. Smith believed X because *everyone* believed it at the time.

5. Smith believed X because the leading figures in his field believed it at the time.

6. Smith believed X because it was an exciting idea.

7. Smith believed X because everyone he talked to was excited about it.

These accounts get less and less appealing as we go from (1) to (7). We have already observed that no one has a problem with Smith's conclusions if we can describe him as acting as in (1). A scientist could hardly be faulted for following an account as in (2): simplicity is, all other things being equal, a good quality to seek out and pursue. We might even raise that to a principle of philosophy of science, in which case, (2) might fall under (3) as a special case. But more generally, scientists tend to be skeptical of an argument that is explicitly based on a principle of philosophy of science. When we get to (4), and we explain that Smith (or Descartes, or Newton) believed something because everyone else did, our first reaction is likely to be something along the lines of: it's fine in the real world to account for someone's belief (that the Earth is flat, that there are angels watching over us, that God performs miracles on a daily basis) on the grounds that everyone else believed it, but isn't that a rather weak defense with regard to a false scientific belief? Still, there is an answer to that: nowadays we *don't* take science to be a method that guarantees us protection from all false claims, and if it's literally (or almost literally) true that *everyone* believes something, then scientist or not, Smith is very, very likely to keep on believing it.

Things begin to get very sticky when we get to (5)—but now we are getting to the kind of case that arises all the time in battles in the mind fields. What do we say if all of the leading lights in linguistics (or the psychology of verbal memory, or you name it) agree to some principle? Working scientists find themselves on a regular basis confronted with proposals and suggestions that are new and that go against the grain of what established leaders in a field say and believe. Why? Because this is exactly the paradigmatic case of how science advances: young, or not so young, scholars challenge an accepted position, and try to replace it with an alternative. It would amount to a complete betrayal of the notion of science if we could justify an ongoing belief in something just because the leading lights believed it. (We could imagine someone saying to himself, though: I'll let the leading lights decide for themselves, and if they start changing their minds, then I'll do so also. Frankly, we don't like people who take positions like that very much. We will come back to that.)[12]

Benjamin Franklin (no mean scientist himself), wrote in a letter to Ebenezer Kinnersley, a fellow Philadelphian who was corresponding with him on the nature of electricity:

[12]Merton [61] discusses Max Scheler (ed.) Versuche zu einer Soziologie des Wissens. Munich: Duncker and Humblot 1924, who suggests that the evolution of political structures and forces can lead to a situation in which "in the sphere of knowledge to a demand for an 'established truth' and in the political realm to the drive for the abolition of an antiquated parliamentarism, to a readiness for dictatorship, from the right or left."

> But this you jokingly call "electrical orthodoxy." It is so with some at present, but not with all; and perhaps, it may not always be orthodoxy with any body. Opinions are constantly varying, where we cannot have mathematical evbidence of the nature of things; and they must vary. Nor is that variation without its use, since it occasions a more thorough discussion, whereby error is often dissipated, true knowledge is increased, and its principles become better understood and more firmly established.[13].

I. Bernard Cohen (from whom we draw the citation) remarks,

> Franklin's statement to Kinnersley may seem to imply that the progress of science can be viewed as a series of changing "orthodoxies." Yet, we are then faced with a paradox, since a canon of "orthodoxy" would seem to imply a certain measure of hostility to major innovations, and we are all familiar with the sentiment that scientists thrive on the replacement of their old and cherished theories or beliefs by new ones. [22].

Cohen notes,

> The kind of behavior to which reference has just been made can, I believe, be legimitately described by the use of the word "orthodox." It comes from two Greek words ortho??, *straight* or *right*, and doxy, *opinion*. The "correct opinion" in science may actually be no more than "what most of the influential scientists heliere." and a discussion of this question involves group psychology (p. 508)

What do we make of Smith as a scientist if (see (6)) it turns out he believes X just because it was an exciting (intriguing, suggestive) idea? Perhaps it depends on what Smith means when he says that an idea is "exciting". If it is exciting because of its elegance and simplicity, then we're back to position (2), which we have already said is reasonable, as well as traditional. But if that's not what is at stake, then we are likely to decide that Smith is perfectly within his rights to *consider* or *develop* a hypothesis because he finds it exciting or intriguing, but he is overstepping the bounds of rational science when he starts believing it because it is intriguing.

And finally, what of a scientist who believes something just because everyone he talked to was excited about it? This is not as odd a circumstance

[13]Benjamin Franklin to Ebenezer Kinnersley, 20 February 1762, in Benjamin Franklin's experiements, edited by I. B. Cohen, p. 368; cited in [22]

as it might sound: it corresponds in many cases to the situation that students becoming professionalized in a discipline find themselves in. Their teachers, and the senior researchers in their field, are the authorities; and if authorities all find an idea intriguing and exciting, who is a young student to disagree with them?

By the time we get to position (7), we have arrived at a point where there is little science left, and just a lot of belief. If science is a particular kind of good (trustworthy, reliable) belief, let us turn now to the embattled term *ideology* in order to consider some aspects of what is universally agreed to be an unreliable kind of belief.

1.3.1 Ideology

We move from scientific knowledge to ideology.[14] This may very well appear to be moving from one extreme to the other: from the knowledge that we have of the universe which is the most highly supported and regarded, we move all the way to the other extreme, that of ideology, the self-delusion that virtually guarantees that one will be kept from the truth.

This may be—or may not. Much depends on how we understand the term ideology, one which has been used by many schools and over-used, perhaps, by Marxists. Sociologists have used the term more than members of any other discipline. We will find the term useful, especially if we can agree on a clear meaning for the term. Most writers who use the term agree on this much: that which is ideological is less than scientifically well-founded, and the person who is possessed by some ideology or other is, by that very fact, unable to adhere to the highest possible standards of scientific rationality.

There is an enormous academic literature on ideology, most of it the work of sociologists, and much of it valuable and informative. If we turn to it, it is because this is an area where the question of error has been taken seriously, and that is one of our areas of interest, too.

We will begin with some conditions on how we want to use the term ideology. First of all, we reject the notion that any ideologically founded belief is necessarily *false*. Ideology is not simply a particular subset of the class of false statements (though it may, indeed, include many false statements, as do textbooks, journals, and lectures also!). For a belief to form part of an ideology, it must be linked in a particular way to the person or people who believe it. To speak of an ideology, one must have a specific group of people in mind, all of whom share some common beliefs, which include the ideology in question. Finally, we will declare that these beliefs are ideological

[14] 1 Boudon 1986 [**?**].

if interested parties outside of the group perceive that the holders of these beliefs hold to the beliefs with a strong clutch and a fierce sense of certainty that goes well beyond what is scientifically reasonable. [15] [16][17]

1.3.2 Knowledge

Let's begin with some high-level discussion of what knowledge is. This is central to every work that we look at throughout this book, but it's a slippery subject.

The account summarizes our contemporary *doxa*, to employ Bourdieu's

[15](From Furet, *Le passé d'une illusion*:

> ...j'entends ici par "idéologies" des systèmes d'explication du monde à travers lesquels l'action politique des hommes a un caractère providential, à l'exclusion de tout divinité. (516).

[16]Mannheim: "groups can in their thinking become so intensively interest-bound to a situation that they are simply no longer able to see certain facts which would undermine their sense of domination."p. 36. It is interesting to see what Merton [61] is uncomfortable with, as he watches Mannheim try to apply notions of ideology to science. He writes,

> The function of political controversy, in constrast to scientific criticism and discussion, is personal or party aggrandizement at the expense of 'opposing', 'competing' persons or parties. Hence, the objective of discrediting one's opponent *à tout prix*. In science, the " opponent"... is "ignorance" or the " resistance of nature to the uncovering of its secrets." To be sure, because of social factors which are extraneous to the pursuit of science itself, the same elemnts of personal aggrandizement and loyalty to a "school" or faction may intrude themselves into scientific pursuits. But these are considered to represent unfortuante deviations from the governing norm of impersonality....it is the essential function of this norm of impersonality to preclude these emotional involvements of scientsits with certain of "their" theories, so as to leave them willing and ready to foresake these theories when new facts demonstrate their inadequacy. [Note that he says facts, not new theories!]

[17]Koerner, Revolution (internet version) cites Maurice Cranston's characterization of ideology (1874:196):

> It is characteristic of ideology both to exalt action and to regard action in terms of a military analogy. Some observers have pointed out that one has only to consider the prose style of the founders of most ideologies to be struck by the military and warlike language that they habitually use, including words like struggle, resist, march, victory and overcome; the literature of ideology is replete with martial expressions. In such a view, commitment to an ideology becomes a form of enlistment so that become the adherent of an ideology is to become a combatant or partisan.

term: a widely accepted view which may be seen as factually largely correct but which at the same time fails to shed light on a range of important questions which we will consider in this paper—and which also fails to illuminate the more significant factors in the development of a field, the respective positions of researchers which account for how their proposals are received, evaluated, propagated, or ignored. In short, an account which reduces the history and sociology of science to something which may be not much more than a fairy tale for grown-up children.

As Bourdieu often remarked, the sociology of science (and sociology in general) has as its vocation to destroy the enchanted vision of the social and scientific world that is driven by what we have referred to as doxa, and bring back into our line of sight the conflicts, the relations of force, and the struggles that are at the heart of scientific life even when it cloaks itself with the garb of love of truth, of science, or of the advance of knowledge for the greater good of mankind. [18] [19]

[18] Un exemple de cette doxa, ni totalement vraie ni totalement fausse est l'acceptation assez générale de la formule qui voit l'histoire de la phonologie comme l'alternance des modèles à règles et des modèles à représentations. Cette conception circulante et communément reçue affaibli considérablement la force de l'analyse originelle de Stephen Anderson et masque totalement les enjeux sociaux et scientifiques des affontements de cette période. Elle oublie le troisième terme : les dynamiques phonologiques et réduit le débat à un affrontement de deux modèles, l'un caricaturé (les règles et l'algorithme) qui en deviennent ridicules et l'autre nouvelle bible qui permet de taire tout les conflits : nous sommes tous des représentationnalistes. What does it mean today to be representionalist or configurationalist?

[19] From Randall Collins: *A Global Theory of Intellectual Change.*

> For almost 400 years, one or another faction in the intellectual wars has declared philosophy to be nothing more than embryonic science, whether natural or social, perhaps with a residue of outworn religious beliefs. What needs emphasis is that philosophy since 1600 has been as creative on its own turf as at any time during world history. The conditions which produced the ideology of the death of philosophy have given philosophy new materials with which to re-create itself. The generations from 1600 to 1665 believed that their new philosophy, which consisted of natural science, would completely replace the old; yet the last generation of the 1600s saw the greatest outburst of metaphysical system-building in history, Berkeley's. The Enlightenment Encyclopedists recapitulated the attack in the more secularizing mode, to be immediately upstarted by the German Idealists. Science and secularization rearranged but did not replace a core philosophical network that had carried down its own concepts and problems from earlier periods. (pp 525-6). [23]

1.3.3 Jehovah's problem

> Nothing is more usual and more natural for those, who pretend to discover anything new to the world in philosophy and the sciences, than to insinuate the praises of their own systems, by decrying all those, which have been advanced before them.[20]

In this section, we turn from what people think and believe, and turn to what they ignore. We would like to describe an odd and curious phenomenon that occurs and recurs in the history that we will tell. We call it "Jehovah's problem."

The reader will recall the state that Jehovah found Himself in, early in Genesis, just before the Flood. He looked at the sorry mess that the human race had made for itself and for the rest of the world, and decided that He had had enough. He was going to eliminate it all, and start all over again, but do it right, the next time. After a bit of reflection, He realized that Noah was not at all half bad, and it would not be fair to eliminate him, or his family; He would spare them, and the world would start all over again, but this time with Noah and his closest kin. Noah built the ark; Jehovah sent the rain. Forty days later it was all over, and the only ones left were those who had made it onto Noah's ark.[21]

Noah was indeed a lucky man. He, and his offspring, did not have to contend with any competition from any of Noah's former friends, enemies, or teachers. All of his contemporaries were highly beholden to him. The book of history was henceforth rather short, too, because it consisted of everything that Noah wanted it to, and nothing else at all.

We will find many a mover and thinker in the mind sciences over the course of this book who felt himself to be both in Jehovah's shoes, and in Noah's. This is somewho who looks out on what he sees, who looks back on what he has been taught, and does not like it, not one bit. This is someone ready to chuck it all, and start over. Someone who would like to be able to call down 40 days of rain and a huge flood, to wash away the competition, someone who is sure he could ride it out in an ark of their own design. Alas, no one can do that; no one has heavenly powers. Still, we find characters who do the best they can, characters who send forth the message that everything that is being done today is a worthless waste of time. They have a *new* story to tell, a new way to study the mind, and we can do it right this time.

---

[20] David Hume, *Treatise on Human Nature*, Introduction.

[21] Noah's story is based on what we know as the Gilgamesh epic; Gilgamesh/Noah is told to "Démolis ta maison pour te faire un bateau ! Renonce à tes richesses pour te sauver la vie ! Détourne-toi de tes biens pour te garder sain et sauf !" This message comes to him from Ea, who, not insignificantly, is the God of Wisdom.

We call this Jehovah's problem — and obviously, it is not a "problem" in the usual sense; it's more of a mind-set and a marketing strategy, and a particular interpretation of one's one work with respect to the scholarship that has preceded one. But it is very common in the mind sciences, and coming to understand it, in all of its nuances, is one of the challenges that we will face. Most often, this mind-set goes hand in hand with the view that everything that has preceded has failed to be scientific, and now we can go forth and be scientific—a pattern we have already discussed briefly. We will see this in psychology, first when John Watson introduced behaviorism in 1913, and again when behaviorism was overthrown (note the metaphor!) by cognitivism in the 1950s. We see it in linguistics when Bloomfield declares (with his students' proud acclaim) that linguistics has finally become a science, in the 1920s, and again in the 1960s when Chomsky declares (with his students' proud acclaim) that linguistics is finally a science.

We see this in philosophy over and over again, in any number of different guises. The most famous philosopher who invited down upon himself a flood to wash away all assumptions and all former teachings was René Descartes, in the 17th century: he declared that he would doubt all things, wash away all certainty, and try to build up his beliefs and his knowledge from scratch. But while some philosophers have been content to build upon the work of their predecessors, many have called for a complete washing away of what preceded them, on the grounds that it was all spoiled and rotten and worthless. The most ambitious of these philosophical groups was the logical positivists of the Vienna Circle, who urged a program that would relegate almost all of the work of their philosophical predecessors to the dustbin of history, where hopefully no one would ever read it again.

Sometimes a scholar will just get so mad about this that he just out and says what he really thinks, frustration and all. Hermann Cloeren[22] expresses his frustration a few years ago. First he noted that Alfred J. Ayer had the nerve to give an important series of lectures at Harvard University on "the analytical heritagë" and deal only with Russell and Moore.

> But the line of philosophical forefathers of analytical thought reaches back beyond Russell and Moore, and even the Vienna Circle, not especially-history minded, acknowledged other thinkers in their manifesto.

Even *they* were not so historically short-sighted!

[22]The Neglected Analytical Heritage, Journal of the History of ideas, 36:3 513-29, 1975)

> The amazing fact, however, is that after more than forty years have elapsed, the picture of the history of philosophical analysis remains basically unchanged.

And not only that, Cloeren goes on immediately to note that even :

> Georg Henrik von Wright's statement that "The author of the *Philosophical Investigations* has no ancestors in philosophy" is certainly erroneous and may accordingly be regarded primarily as evidence of von Wright's ignorance of Wittgenstein's forerunners. (513)

Oh, the frustration of seeing established and lauded scholars speak with such ignorance about the lack of intellectual antecedents: it is simply galling, isn't it?

It will be one of our main tasks in this book to document this pattern, and to try to come to grips with what is wrong with it, what is, occasionally, right about it, and why it is that it is so common. It is not just a personal problem; we are not interested in a psychological analysis of anybody, and certainly not of the people who have helped move these fields forward. It is rather that we see a pattern over and over again, and not only that, we fail to see it reported in the literature as a generalization. When it is remarked upon, it is always as someone's personal failing—usually that of the would-be Jehovah (or Noah). But clearly there is more to it than that. The proof that there is more to it is simple, and it is this: in the real world, there is no Flood. There is nothing that washes away the books and the publications of the earlier scholars. And yet—and yet Watson was able to convince people not to read literature from before behaviorism, and Skinner's students were delighted to never have to read anything before Skinner. Chomsky's students did not have to read what had been published before 1957, and so it goes. Why did the world of scholars permit itself to become dumb?—that is the question! Anyone can tell you not to read something. But what is it that makes you willing to follow their advice?[23]

1.4 Science, and the mind sciences

One of the greatest challenges that linguistics and psychology — and to some extent, philosophy — is the question as to whether they live up to the

[23]Try reading something which was once well respected and which today is forgotten or despised. It may be the beginning of your intellectual liberation.

standards expected of a science. Lurking behind the question, it should not be forgotten, is another question: just what good is it to have it confirmed that linguistics (or psychology, or economics) really is a science? Eventually, we will have to address that question, too: certainly philosophers may buy out of even wanting to be a science, and some linguists might, as well; but for the most part, scholars in all three of these domains have taken it for granted that aspiring to the logical status of a science is very much a good thing for their discipline.

What is a science? Ah, there's the rub. We will see that there is wide variation in the answers offered to this question, affected by time, by theoretical orientation, and by a realization (clear in some people, less clear in others—and even contested by some) that there are narrow and broad senses of the term *science*. That is, for some, we could specify physics as a science in a narrow sense, since it embodies work that can be easily formulated in mathematical terms, and it can be tested in laboratory conditions, and easily replicated; linguistics and economics are sciences in a broader sense.

We are going to see philosophers, linguists, and psychologists hold up what they see as the important and abiding features of science, and proclaim that this is what we must do in order to be scientific. The problem is: what they hold up for us to look at is not always the same. Views about what science is change over time, and vary greatly from person to person at any given time. So we will have to do our own thinking about what it is that makes a science, and we will have to do our own thinking about the extent to which the nature of a science may vary from field to field. There is no guarantee that the best notion of science in physics is the best in linguistics: it may be, it may not.

Let's establish an inventory of characteristics that have played a role in our collective minds in deciding whether a discipline should be considered a science or not.

1.4.1 Linguists citing great scientists

The Minimalist Gamble (author?)

> Minimalism's *why*-questions are strongly reminiscent of general questions that are asked in modern theoretical physics.

1.4.2 Major characteristics

As we will see below, linguists have been trying to establish linguistics as a science for nearly a century and a half. Why? There's no one simple answer,

but this is surely behindt he move: the resutls we can obtain through (a) scientific method are surely more trust-worthy, and thus easier to *sell*, than results from a less reliable method.

And what does a typical science *do*?

1. A science collects and organizes, data,
2. often using high tech methods,
3. in laboratories;
4. Those who perform the work, the scientists, are thoroughly trained in their disciplines,
5. and share much scientific culture, both explicitly and tacitly;
6. The science which they pursue is composed of generalizations of varying levels of abstracftness and breadth of coverage,
7. expressing its generalizations in mathematical expressions.
8. The system of "laws" comprising the science is organized a long a number of dimensions:
    - one is the breadth of predictive coverage;
    - another is direction of implication;
    - degree of core-ness, and hence protection from disconfirmation;
    - relation to "laws" in other sciences;
    - elegance and simplicity.
9. the scientists and their laboratories submit their work to critical outsdie review;
10. and they ruefully admit that everything that they belive today will someday be proven false. Hence, nothing is sacred; all is subject to critical review.

- Knowledge is social, not individual
- Scientists are disinterested
- Scientists observe nature
- The value of science lies in its generalizations, and in their denseness

- Scientific knowledge has powerful applications
- There is one more point which has been lurking, but which needs to be emphasized. First, scientists are very often motivated by the search for the "aha!" feeling.

  Second, science tells us what exists, going well beyond what our senses can tell us. Not all scientists agree with, but probably most do.
- Replicable methods for collection of data in laboratories. (versus anything goes in the goal of persuasion.)
- Using mathematical and using formal models to describe laws and other generalizations.

  One of the hallmarks of modern science has been the use of mathematics in formulating laws and in applying laws to particular cases. Galileo is often taken to be the first great modern scientist, and he urged his reader that the language in which the book of Nature was written was the language of mathematics. Now, it's true that in Galileo's day, mathematics tended to be geometry more than it tended to be algebra; things began to change in that regard fifty years later, under the influence of Descartes, but the general point remains the same.[24] Philosophy has not been untouched by this, but it has been affected in different ways from linguistics and psychology. Linguistics and psychology have both seen considerable enthusiasm about the prospect of developing a mathematical approach to their problems, while philosophers have rarely gone in for mathematical formulations of their systems. What mathematics has done for philosophy, though, is to set a high standard for establishing certainty, in the sense that a mathematical demonstration of a statement is one of the strongest arguments that can be offered, and philosophers have always looked for maximally strong support for their position.

[24] As I. Bernard Cohen pointed out, in his discussion of Galileo's experience in the early years of the 17th century, looking at the heavens–the Moon, the Milky Way, the planets, the Sun — :

> Not only did Galileo describe the appearance of mountains on the moon; he also measured their height. It is characteristic of Galileo as a scientist of the modern school that as soon as he found any kind of phenomenon he wanted to measure it. It is very well to be told that the telescope discloses that there are mountains on the moon, just as there are mountains on the earth. But how much more extraordinary it is, and how much more convincing, to be told that there are mountains on the moon and that they are exactly four miles high! [**?**] p. 60

- A search for beautiful patterns—if need be, despite the data. The notion that the great scientist is one who finds patterns of great beauty is a notion that will not go away, despite how difficult it is to get clear on what that could possibly mean. Werner Heisenberg attributed to Einstein a beautiful statement of this point of view:

> It is quite wrong to try founding a theory on observable magnitudes alone. In reality the very opposite happens. It is the theory which decides what we can observe. You must appreciate that observation is a very complicated process. The phenomenon under observation produces certain events in our measuring apparatus. As a result, further processes take place in the apparatus, which eventually and by complicated paths produce sense impressions and help us to fix the effects in our consciousness. Along this whole path—from the phenomenon to its fixation in our consciousness—we must be able to tell how nature functions, must know the natural laws at least in practical terms, before we can claim to have observed anything at all. Only theory, that is, knowledge of natural laws, enables us to deduce the underlying phenomena from our sense impressions. When we claim that we can observe something new, we ought really to be saying that, although we are about to formulate new natural laws that do not agree with the old ones, we nevertheless assume that the existing laws—covering the whole path from the phenomenon to our consciousness—function in such a way that we can rely upon them and hence speak of "observation". [25]

Chomsky offers a similar point of view ([21], Interview on minimalism):

> [T]ake the phrase "Galilean style". The phrase was used by nuclear physicist Steven Weinberg, borrowed from Husserl, but not just with regard to the attempt to improve theories. He was referring to the fact that physicists "give a higher degree of reality" to the mathematical models of the universe that they construct than to "the ordinary world of sensation." [4] What was striking about Galileo, and was considered very offensive at that time, was that he dismissed a lot of data; he was willing to say "Look, if the data refute

[25] Heisenberg, Werner, Physics and Beyond, Encounters and Conversations, A.J. Pomerans (trans.), Harper and Row, New York, NY 1971, pp. 63–64. cited on the internet.

> the theory, the data are probably wrong". And the data that he threw out were not minor. For example he was defending the Copernican thesis, but he was unable to explain why bodies didn't fly off the earth; if the earth is rotating why isn't everything flying off into space? Also, if you look through a Galilean telescope, you don't really see the four moons of Jupiter, you see some horrible mess and you have to be willing to be rather charitable to agree that you are seeing the four moons. He was subjected to considerable criticism at that time, in a sort of data-oriented period, which happens to be our period for just about every field except the core natural sciences.

We interrupt this extended quotation just to draw your attention to the fact that Chomsky is now chastising all of the non-natural sciences, which is to say, the social and human sciences, including linguistics. [26]

> But the Galilean style, what Steve Weinberg was referring to, is the recognition that it is the abstract systems that you are constructing that are really the truth; the array of phenomena are some distortion of the truth because of too many factors, all sort of things. And so, it often makes good sense to disregard phenomena and search for principles that really seem to give some deep insight into why some of them are that way, recognizing that there are others that you can't pay attention to. Physicists, for example, even today can't explain in detail how water flows out of the faucet, or the structure of helium, or other things that seem too complicated.

---

[26]

> We're familiar with the same criticism in linguistics. I remember the first talk I gave at Harvard (just to bring in a personal example), ...it was in the mid 1950s, I was a graduate student and I was talking about something related to generative grammar. The main Harvard Professor Joshua Whatmough, a rather pompous character, got up, interrupted after 10 minutes or so: "How would you handle..." and then he mentioned some obscure fact in Latin. I said I didn't know and tried to go on, but we got diverted and that's what we talked about for the rest of the time. You know, that's very typical and that's what science had to face in its early stages and still has to face.

It would perhaps not be unfair to paraphrase this as: physics, the best example of science, had to face this in its early Galilean stages; linguistics, a young science, has to face it now.

If physicists are allowed a certain room to maneuver, and they do not have to account for all observed motion, then surely linguists should be allowed at least as much wiggle room.

> Physics is in a situation in which something like 90% of the matter in the Universe is what is called dark matter – it's called dark because they don't know what it is, they can't find it, but it has to be there or the physical laws don't work. So people happily go on with the assumption that we're somehow missing 90% of the matter in the Universe. That's by now considered normal, but in Galileo's time it was considered outrageous.

It is not clear what "normal" means in this context; many astrophysicists may view the situation as outrageous as well, and outrage might encourage one to find a new and better theory.

> And the Galilean style referred to that major change in the way of looking at the world: you're trying to understand how it works, not just describe a lot of phenomena, and that's quite a shift.

- Skepticism about the word of authorities Meyer (1933) p. 420:

> Other disciplines have passed through a process of rigorous scrutiny of handed-down conceptions in moving towards scientific maturity, and there seems to be no legitimate reason why economics or any other social study should be regarded as an exception. There is still too much dependence upon the authority of past utterances in economics, whereas no such attitude maintains today in the physical or biological sciences. Theories once seriously held by Pythagoras, Aristotle, Euclid, Ptolemy, Galen, Linnaeus, Newton, and other outstanding thinkers in recognized scientific fields, have been utterly discarded, or greatly modified without disturbing the places their proponents occupy in history. In fact, science would seem to have advanced in proportion as it has learned to profit by the mistakes of its most important benefactors. It was only as this lesson was generally appreciated after the Middle Ages that the scientific renaissance became possible.

- Disinterested parties at the helm

- Statement of clear hypotheses and the attempt to test or disconfirm them.
- Study of objects in observable space and time.
- Predictive ability
- Connection to technology
- Coherence and unity of science;
- Publication in refereed journals

It's a fair guess that anyone who looked at that list would think fewer than half of the items on the list are reasonable, and at least half of them describe some kinds of sciences, but focus on aspects that are not essential to science, and may quite will be irrelevant to science, in the long run. But if people largely agree to that, they do not agree on what ought to be the next step, which is to agree to which are the essential characteristics of science and which are the irrelevant, or inessential, ones.

There is no simple way to divide up the range of views regarding the nature of science. Nonetheless, at the risk of oversimplifying, we can roughly divide the views into three sorts: those that focus on defining the abstract method of science, those that focus on the concrete activities of scientists, and those who focus on the language, concepts or statements of science.[27]

1.5 Empiricism and rationalism

The terms *empiricism*[28] and *rationalism* have covered a wide variety of views regarding the nature of mind and the ways in which we can best learn about the natural world around us. In its early days, empiricism was associated with the views of John Locke and David Hume, and during the 17th and

[27] "To construct science means to construct a system of propositions which stand in certain fundamental coherence with one another. The logic of science is thus the logical analysis of this system, of its elements and of the method of tying these elements." Carnap, 1934 [15] p. 8. And page 9: philosophy is "the logical syntax of the language of science."

[28] E. McCann, "History: philosophy of mind in the seventeenth and eighteenth centuries," in Guttenplan, 1994, p. 338, cited in Andler et al volume 1, p. 604.

> It is worth remarking that there is more than a little bit of anachronism to be found in the understanding many contemporary writers have of positions and arguments drawn from the early Modern period, and that anachronism is not always entirely benign. —

18th centuries, empiricists differed in some important ways with their rationalist colleagues on the Continent with regard to the origin of human knowledge. Empiricists and rationalists were largely on the same side of the important intellectual battles of the age, which involved the importance of scientific investigation as the most important way to understand the world. Both empiricists and rationalists were in agreement that science was a better authority than either the Aristotelian scholastics or the established churches of their time were.

Empiricists and rationalists differed, though, with regard to whether the careful study of the information that is presented to the human mind is sufficient to account for all of human knowledge. Empiricists took the strongest possible stand: all knowledge comes through the senses. Rationalists disagreed, and argued either that the empiricist position was *necessarily* wrong, because the learning process cannot start from nothing—or that the empiricist position was *simply* wrong, because we do know things that the senses alone could not convince us of.

Empiricists' central example of what one knows is one's own report of what one sees, tastes, or hears, while rationalists' central example of what one knows is a mathematical statement of which one knows well the proof (my own favorite example is that a group of prime order has no proper subgroups, but if that does not appeal to the reader, then one could take a different example, like the statement that the only even prime number is 2). For their part, rationalists weren't entirely certain that we really *know* what we think we perceive; we could be mistaken, while empiricists believed that rationalists were ignoring and overlooking some uncertainties with regard to mathematics that simply couldn't be held against sensory reports.

Bear in mind the following: in the 17th and 18th century, there was no distinction to be drawn between philosophy and psychology, and this lack of distinction can be treacherous for us, as we look back on the issues of those days, because we, in the 21st century, make a distinction that seems to us rather evident. On the one hand, we can study what it is for *us* to know something, a question that today calls upon the disciplines of philosophy, history, and sociology, but on the other hand, we can also study what it is for *humans* to know something, a question that we typically hand over to psychologists, neuroscientists, and their colleagues. Never mind, at least for now, that there is a great overlap between being "us" and being "human"; I want merely to draw the distinction in how we ask about knowledge—its foundations, its origins, its trustworthiness. (There is a tradition that tries, as best it can, to erase the distinction that I am trying to clarify, a tradition that is often referred to as cognitive naturalism, or the like.) I will refer to the question of what it is for us to know something as the *philosopher's*

question of knowledge (or the *epistemological* question), and to the question of what it means for a human being to know something as the *psychologist's* question of knowledge.[29]

The philosopher's question of knowledge is posed starkly by Descartes: how can I establish my knowledge of anything if I start by doubting everything, including what my senses tell me? Descartes' own answer is based on a search from inside his mind, and the early modern tradition of the 17th and 18th century followed that path as well. By the 19th century, answers to this question began to come from without: from studies of the history of ideas, the history of the material world, and the history of the political world.

The psychologist's question of knowledge only became clear when it became possible to ask questions of interest about the knowledge of other people by methods that were not different in kind from methods used to study other things in the natural world, and this moment was the beginning of the discipline of psychology. Its roots, as we have said, lay in philosophical work, but the methods that created a modern psychology came into their own in the middle of the 19th century. This disciplinary evolution was also the result of the impact of Darwin's theory of evolution, and the conclusion that human beings were, from a biological point of view, part of the same natural world as apes, rats, and pigeons.

We move forward to the 20th century. Virtually all of the issues that separated rationalists and empircists have been so morphed by the passage of time and the advances of science that the two traditional terms are largely archaisms. But not entirely: the term *empiricism* came back into vogue early in the 20th century, largely due to its use in Berlin by a group that called itself logical empiricists, and the term's association with the influential Vienna Circle, which called its views logical positivism.[30] The term *rationalism* came back into vogue later in the century largely due to its use by Chomsky and other cognitive scientists, beginning in the 1960s. I will refer to these as *modern* empiricism and rationalism, unless context makes it so clear that we

[29]If you are wondering how we can make such a distinction—since we really do know, individually, that we are human beings—we will certainly return to this question, but for the moment, simply consider what conclusion you would draw if (to take a not unreasonable example) it was shown to you that psychological methods established that you had no awareness of yourself, or were not able to draw rational conclusions—in short, some conclusion which you had personal, but not scientific, reasons to dismiss. You would, in such a case, compartmentalize the scientific conclusion from the principles you employ during your normal life.

[30]The development of the formal tools used to characterize syntax—and thus the modern generative framework—were the work of the Vienna Circle, and others directly influenced by them, such was Willard Quine.

may omit the term *modern*.

Modern empiricism's principal target was the nature of scientific knowledge and knowledge *tout court*—an epistemological question. Empiricism held close to the positivist movement in seeing scientific knowledge as the highest model of knowledge. In some respects, empiricism was a wipe-the-slate-clean philosophy, much like Descartes's general doubting, or Socrates's "I only know that I know nothing." In its most extravagent form, psychological behaviorism, it made strong claims of dubious merit (though we should bear in mind that behaviorism predates modern empiricism by two decades). In its constructive form, it offered a philosophical justification for a freedom of spirit and thought which allowed young scientists to overthrow the heavy weight of a moribund tradition, and to replace it with such new frameworks as the theories of relativity and of quantum mechanics.

Modern rationalism is not primarily a theory of epistemology; it is an approach to understanding how human beings think and know. It is a child of the mathematical breakthroughs of the 1930s, 1940s, and 1950s involving a new conception of logical inference, of mathematical proof, and ultimately of how something resembling thought could be embodied in what we today call a computer. [...]

Modern rationalism has seen fit to emphasize the question as to how *malleable* the human mind is, and to just what extent variety in the world of human experience can lead to differences in mature humans. Modern empiricism focuses on how it is possible for humans (or "beings," if we want to sound philosophical) to learn; modern rationalists, who are uncomfortable with this approach and even this interest, sometimes refer to the ability to learn as being "malleable."[31] Even though it is hard to see what the difference would be between being malleable and being able to learn, it certainly seems less dignified to be committed to a perspective that sees people as endlessly malleable than to one that sees them as being capable of limitless learning.

[31]One spot, chosen at random, is Jan Koster's reflections on the study of the mind and of language, "Linguistics, Historicism and the Humanities." Koster remarks, for example, that

> Ideas of an initially empty and malleable human mind have always been considered progressive and were all too obviously ideologically motivated, as was pointd out by Harry Bracken in several books and articles (for instance, Bracken 1984).

1.5.1 The problem of induction

The problem of induction is usually understood to be a problem of philosophy. It asks us what the justification is for drawing generalizations from a set of specifics, like a set of observations. How do we know which sorts of events will continue to re-occur, and which will not? We expect the Sun to come up tomorrow; that is the classic example of a reasonable conclusion to draw after having seen it come up in the morning on so many days. But a man falling from a tall building may see floor after floor go by him, and still he would not be justified in concluding that he will never hit the ground. How do we know when it is reasonable to say: on the basis of N examples of a generalization, I conjecture that the generalization is always going to be true?

This problem came onto center stage in the philosophical world with David Hume in his *Enquiry Concerning Human Understanding*, and many philosophers have tried their hands at answering it. But for our purposes, it is much more than a philosophical question: it is also a question for any theory of mind that is concerned with how the mind deals with experience, and it is certainly a question for linguistics.[32] There: all of the sciences of the mind must have a go at it. The linguist who analyzes a finite amount of data that she has collected, just like the baby who has heard a finite sample of her new language, can only do her job by drawing generalizations from the data. But which generalizations are the right ones? Is there a general way of writing descriptions of languages that gives us some insight into this problem? This last question is the one that Noam Chomsky brought to the marketplace of linguistic ideas in the 1950s, and it is one that we will return to frequently.

Hume's answer to the problem of justifying generalizations that have been induced from data was in two parts. The first was logical, and negative: he argued that there was no logical foundation for the inference of generalizations. But the second was positive; it was an answer to the question, how and why do people draw general conclusions on the basis of a finite sample of experiences? Hume's answer to this question was to appeal to people's abilities to observe similarity between ideas and perceptions, and also contiguity in space and in time.

[32]From the philosopher's point of view, any such inference is tricky business, since it is reasonably clear that there is a serious danger of making a mistake and drawing the *wrong* conclusion from the observations. From the psychologist's point of view, understanding how people (or for that matter, rats) draw inferences is a difficult task, but one which holds the promise of telling us something about otherwise scientifically inaccessible reaches of the human psyche.

The problem of induction has become, in a sense, the central question of linguistics. A linguistic theory is a statement about the kinds of generalizations that a language learner can make, and thus indirectly, about the kind of generalizations that she cannot make.[33]

There is another connection to make, as well: the link between the problem of induction and the characterization of laws of nature.[34] Not every empirical generalization is reasonably considered a law of nature, and Nelson Goodman, an American philosopher who will play an important role in our story, is credited with having invented a whole genre of generalizations, each of which is reasonable and may well be true, but which none of us would take to be plausible candidates to be laws—the generalizations about the coins in your pocket. You might note in the morning, and again at that night, that every single coin in your pocket is a nickel (or a 1 euro piece, if you prefer). But you will not take that to be the sound basis for concluding that the same thing will be true again the next day. Why not? Somehow—and the question as to how is not at all trivial!—we are able to distinguish between generalizations that have a chance of being lawlike and those that are at best accidents (and at worst, inobservant and mistaken observations).

Reasonable people differ, though, as to what a potential law of nature is—and even on what sort of language laws of nature can be specified in. One of the biggest disagreements concerns whether statements specified in the language of probability are possible candidates for lawlike statements. We are not even asking whether a particular probabilistic statement is true: we are just asking whether a statement that assigns a probability to an event is a statement like "all the coins in my pocket are pennies" or like "water molecules are composed of hydrogen and oxygen".

Our understanding of what it means to infer structure from data has changed dramatically over the last several decades, all due to the rise of superfast and superpowerful computers.

Probablility, statistics, and information theory

One of the important strands that we will follow throughout this book is the theme that links probability, statistics, and information theory. Empiricism is deeply connected to the theory of probability: as we will see, and as empiricists have long recognized, empiricism is obliged to develop a

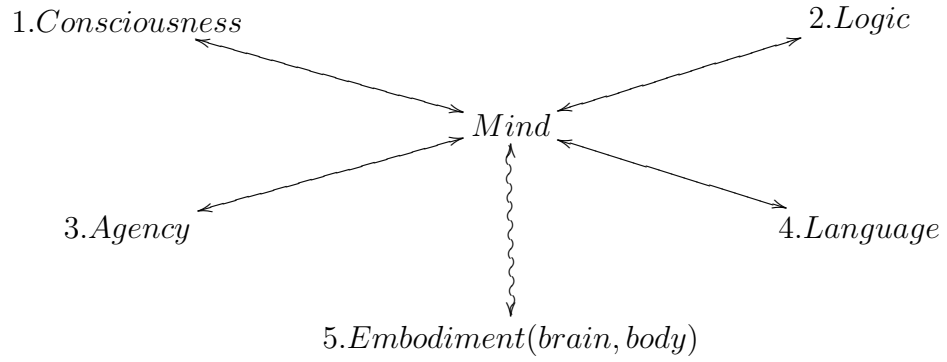
[33]From either point of view, empiricist perspectives look for methods to infer structure from sensory data, while rationalist perspectives look for ways in which unlearned or even unlearnable principles provide a satisfying account of how various given languages are learned.

[34]Chapitre 4, Barberousse, Kistler and Ludwig 2000 is useful in this regard.

clear and coherent theory of probability and induction, because it recognizes these as the central and most important ways in which human knowledge arises. The cybernetics revolution in the late 1940s introduced the world to information theory, and engineers and linguists were quick to draw the connection between the analysis of electronic signals and the analysis of linguistic "codes"—whether the codes embodies sounds or more abstract symbols.

1.6 What is the mind?

Our story is the story of scientific approaches to the mind. And what is the mind? There are five aspects of the mind which will be important to us: self-awareness, inference, agency, and language, and embodiment. Let's say a bit about each.



The first aspect of mind is *awareness* or *consciousness*. Of the four areas of mind, this is the toughest to say something clear about. We are aware that we ourselves exist; we can lose track of ourselves when we get thoroughly involved in something, but we never completely lose track that we, ourselves exist. We have a special relationship to our subjective feelings, whether these feelings are pains (and our own pains hurt worse, most of the time, than anyone else's, and certainly our own pains hurt in a very special and different way than anyone else's, including our children's), or moods, or anything else.

The second is logic, or rational inference: the ability to draw reasonable conclusions. I see broken glass on the sidewalk; I can see that someone dropped (or ran over) a beer bottle. I hear that the weather report says it will rain; I conclude I should bring the sheets that are drying in the breeze in the back yard. Someone who could once draw these conclusions but no longer can is someone we might well say is losing his mind. He is: he is losing his ability to draw inferences.

The third is agency, or the sense that we ourselves have control over what we do and say. I am typing these words: they come out on my computer's screen quite as I expected them to, and if I choose to, I can hit backspace and select a different word instead. I am responsible for each word I write, and for the things I do. I may not always know exactly why I do things (I have enough experience with myself to know that I am not always transparent to myself), but that is the exception and not the rule.

And fourth is language: it is hard to be sure whether language is or is not an essential ingredient of mind, but one thing is certain: it is very hard to think (and harder yet to speak) about the nature of mind without using language, and without considering sides of the mind that in some fashion or other involve human language. We use language to communicate with other people, and we seem to use it to organize our thoughts: at times, we feel like we couldn't organize our thoughts without it.

Are these four categories four different aspects of one and the same thing? Are they four quite separable faculties of human mind? Can we find, imagine, or build creatures that have some but not all of these features? We have no simple answer to these questions, but we will find that many (if not all) of the answers that others have given to questions about the nature of mind can often be better understood once we get clear on which of the four aspects of mind are being addressed — and which ones are being ignored.

The fifth aspect of mind is a bit different from the first four. It is the observation that minds that we know of are all embodied in two senses: first, these minds are all implemented in neuro-wetware, which is to say, brains; and second, we learn a lot of what we know by virtue of our interaction with the world around us, including our interactions with other people.

Let's say a little bit more about each of them: awareness, rational inference, agency, and language.

- Consciousness and awareness

  Rational inference. Traditionally, the word logic has been used quite simply and generally to mean whatever it is that allows one to make a valid inference, which is to say, to pass from one statement (or a set of statements) to a conclusion.

  In modern times, it has been traditional to divide logic, or rational inference, into two very different types, often called deductive and inductive logic (or inference). Deductive logic is certain; it is often exemplified by examples like the following. If we know that all bats are blind, and also that my pet Alex is a bat, then we can infer with certainty that Alex is blind. Inductive inference is always less than certain:

it consists of drawing a conclusion about a class of statements from a more limited set of premises. On the other hand, if notice that I have slept poorly every time that I drank coffee after dinner, and from this draw the conclusion that if I drink coffee after dinner tomorrow night then I will sleep badly, that is an inductive inference, and it goes beyond the premises from which it started. Some writers, like Charles Peirce, have suggested that this two-way division is not quite right, and that what we have called "inductive inference" needs to be divided into two different sorts; for now, we will just note that in passing, and come back to it later.1[35]

John Searle writes:

> By 'mind' I mean just the sequences of thoughts, feelings and experiences, whether conscious or unconscious, that go to make up our mental life. (*Minds, Brains and Science*, p. 10f).

- Logic and inference

  ...There is a long tradition that links logic (or inference) and grammar. The Port-Royal Grammar of 16xx carried as its subtitle: logic, or the art of correct thinking. Boileau wrote: "Ce qui se conçoit bien s'énonce clairement et les mots pour le dire viennent aisément". L'Art poétique.

- Agency Searle says that consciousness is a necessary condition for agency:

> The first thing to notice about our conception of human freedom is that it is essentially tied to consciousness. We only attribute freedom to conscious beings. If, for example, somebody built a robot which we believed to be totally unconscious, we would never feel any inclination to call it free. Even if we found its behavior random and unpredictable, we would not say that it was acting freely in the senses that we think of ourselves as acting freely. If on the other hand somebody built a robot that we became convinced had consciousness, in the same sense that we do, then it would at least be an open question whether or not that robot had freedom of the will. (p. 94) Minds, Brains and Science.

[35]1 An excellent review can be found in Grattan-Guinness 1981.

- Language

  A position that says language is necessary for ideas:

  > To enter into the physical brain (or inside the computer) is precisely the wrong way to perceive ideas; for ideas are in the process of communication between one thinker and another, and we perceive the ideas of another brain only by having them communicated to us. It is the same with oneself: one perceives one's own ideas only insofar as one is in a communicative model. There is no thinking except as aftermath or preparation of communication. Thinkers do not antedate communication, and the communicative process creates the thinkers as nodes of the process. [**?**] p. 2.

  Marvin Minsky (In "Matter, mind, and Models," wrote:

  > Our everyday intuitive models of higher human activity are quite incomplete, and many notions in our informal explanations do not tolerate close examination. Free will or volition is one such notion: people are incapable of explaining how it differs from stochastic caprice but feel strongly that it does. I conjecture that this idea has its genesis in a strong primitive defense mechanism. Briefly, in childhood we learn to recognize various forms of aggression and compulsion and to dislike then, whether we submit or resist. Older, when told that our behavior is "controlled' by such-and-such a set of laws, we insert this fact in our model along with other recognizers of compulsion. We resist "compulsion," no matter from "whom." Although resistance is logically futile, the resentment persists and is rationalized by defective explanations, since the alternative is emotionally unacceptable.

1.7 On studying ideas historically

A few words on the organization of this book. As you may have noticed, most of it is organized historically, chronologically. This was a choice which we made; it was not imposed on us by the very nature of the material. This order might suggest to a casual reader that we think that there is a natural cumulativity to the knowledge and belief systems that we encounter. But if that is so—and it is, because there is, indeed, a natural cumulativity in some

important ways—this is not the only kind of structure or of interaction that a chronological narrative can help to bring out. It is *always* the case that the thinkers of a given moment are responding to questions and issues that their contemporaries are grappling with, and it is always the case that creative thinkers are engaged in exploiting new ideas and methods in whatever domain they may pop up in. All creative thinking is dialogue, but not all creative thinkers feel that it is in their best interest to demonstrate just how true that statement is.

As we will see in some detail in Chapter 3, there was a strong bias on the part of 19th century thinkers to present their ideas in a historical fashion, much as we have done in this book. One apparent exception to this was Auguste Comte, who struggled, as we have, with the question of relating the historical and in the logical relationship of ideas. In the end, he chose to write in a fashion more logically than historically than we have, but he well described the conflicting pressures that the writer feels. In fact, he presents extremely well the reasons *for* presenting this material in a historical way. He wrote,

> The only fundamental imperfection that we could hold against the [logical] mode of presentation is that it leaves out the way in which the various domains of human knowledge were formed, something which is—though distinct from the actual acquisition of this knowledge—inherently of the greatest interest for philosophical concerns. In my opinion, this consideration would be very important if it really were a reason in favor of the historical approach. But it is easy to see that there is only an apparent relationship between studying a science by following the so-called historical mode, and really knowing the actual history of this science.
>
> Indeed, it is not only true that the various parts of each science (which we would separate from a logical point of view) developed simultaneously and each under the influence of the others, which would tend to make us prefer the historical order; considering in its totality the development of the human spirit, we also see that the various sciences have in fact been developed at the same time and with mutual influence; we even see that the progress of the sciences and the arts have depended on each other, with numerous reciprocal influences; and all have been tightly linked to the general development of human society. This vast interconnection is so real that often, in considering the real rise of a scientific theory, the mind is drawn to considering the perfection of some

> art that has no rational connection to it, or even some particular step forward in social organization, without which this discovery could not have taken place....The consequence of this is thus that one can only know the true history of each science, which is to say the real formation of the discoveries that it contains, by studying, in a general and direct fasion, the history of humanity.[24].[36]

Thus for Comte, the *reductio ad absurdum* follows from the fact that to develop the historical mode in all its glory, we need to present an entire history of the entire race. But there we part company: any study, any book is by its nature limited in size and scope, and ours is no different. With everything we discuss, there is a point beyond which we do not go, and we often regret the decision to go no further. But the reader may occasionally agree with Comte: and if we could, we probably would have written an entire history of the human race. [37]

---

[36]La seule imperfection fondamentale qu'on pourrait reprocher au mode dogmatique, c'est de laisser ignorer la manière dont se sont formées les diverses connaissances humaines ce qui, quoique distinct de l'acquisition même de ces connaissances, est, en soi du plus haut intérêt pour tout esprit philosophique. Cette considération aurait à mes yeux, beaucoup de poids, si elle était réellement un motif en faveur de l'ordre historique. Mais il est aisé de voir qu'il n'y a qu'une relation apparente entre étudier une science en suivant le mode dit historique, et connaître véritablement l'histoire effective de cette science.

En effet, non seulement les diverses parties de chaque science, qu'on est conduit à séparer dans l'ordre dogmatique, se sont, en réalité, développées simultanément et sous l'influence les unes des autres, ce qui tendrait à faire préférer l'ordre historique; mais en considérant, dans son ensemble, le développement effectif de l'esprit humain, on voit de plus que les différentes sciences ont été, dans le fait, perfectionnées en même temps et mutuellement; on voit même que les progrès des sciences et ceux des arts ont dépendu les uns des autres, par d'innombrables influences réciproques, et enfin que tous ont été étroitement liés au développement général de la société humaine. Ce vaste enchaînement est tellement réel, que souvent, pour concevoir la génération effective d'une théorie scientifique, l'esprit est conduit à considérer le perfectionnement de quelque art qui n'a avec elle aucune liaison rationnelle, ou même quelque progrès particulier dans l'organisation sociale, sans lequel cette découverte n'eût pu avoir lieu.... Il résulte donc de là que l'on ne peut connaître la véritable histoire de chaque science, c'est-à-dire la formation réelle des découvertes dont elle se compose, qu'en étudiant, d'une manière générale et directe, l'histoire de l'humanité.

[37]a famous quote from Max Planck (1949, 33-34):

> A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.