If you ask a psychologist who knows a bit about the history of his discipline when it all began, chances are good that he will say that it all started during the second half of the 19th century, and that psychology emerged out of two fields: philosophy and, especially in Germany, physiology.

There is much truth to that. Nowadays, the opening of Wilhelm Wundt’s experimental laboratory is often taken as an icon symbolizing this shift, and the year 1879, the year when it opened its doors at the University of Leipzig, is taken as a symbolic starting moment for psychology. But there was no one who lived through any particular event in 1879 which made him look up and say that a new discipline had just begun. Quite the contrary: we have already seen Whitney, quite a few years earlier, discussing the relationship of linguistics and psychology.

The tacit agreement that psychology was born when Wundt’s lab was opened is no doubt a reflection of the belief that the methods—and in the case at hand, the laboratory methods—are what comes closest to the heart of psychology as a discipline. We will look at the steps that the field of psychology took over the last decades of the 19th century, in Germany, in the United States, and in France. Despite the creation of much that was new, despite the development of new methods and new questions, the field, as it was viewed by its elder statesmen, had not yet broken away completely from the mother discipline of philosophy. But the younger psychologists were ready to jettison that older tradition.

4.1 Germany, the homeland of psychology in the 19th century

The first chapter in the book of modern psychology was written by Ernst Heinrich Weber and Gustav Fechner at the University of Leipzig, several decades before Wundt opened his laboratory there. Weber was a professor of anatomy and physiology at Leipzig, where he had gone in 1818, and over the course of his career he developed many of the most familiar

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1 On the subject, there is again a large literature. A good overview of the reanalysis of the history of psychology in recent decades can be found in Arens, 1989. Ash, 1995 is an excellent account of the complex rise of psychology. Ben-David and Collins, 1964 has been especially influential in what we write in this section. The artificial and self-conscious nature of the selection of 1879 as the starting point of Wundt’s laboratory is discussed lucidly in Boring, 1963: 22-23. When Wundt arrived in Leipzig in 1875, he was given an old and unused auditorium. Some students, and some non-students, began conducting research there in 1879, some of it published in Wundt’s journal, founded in 1883. Somewhere along the line Wundt and his colleagues started to refer to the space as the Psychologisches Institut. It was officially recognized as such in 1894.
and basic experimental techniques learned today by budding psychologists, like determining how great a sensation has to be to be noticeable to a human being. For many sensations, it was possible to develop a theory of thresholds: for example, if we want to know how close together two pin pricks can be felt, but felt as happening at different places, we can perform experiments on human beings, and determine what physical distances on one's finger, one's leg, or one's lips are needed in order for two sharp, simultaneous sensations to be perceived as two sensations, rather than one. Weber became famous for a series of experiments in which he found that subjects who were asked if two objects were the same in weight would display a threshold of weight-difference that was not absolute, but was rather more complex: if the proportion of the weight of the heavier weight to the lighter weight were greater than a specific threshold, then the weight difference would be perceived. Today, this notion of just noticeable difference is a standard concept in experimental psychology. Weber's discovery was one that fit perfectly with the orientation of the age, with its focus on measurement, and it established for the first time that an unexpected but quantitative law could be established that extended to both sides of the body/mind divide.

Gustav Fechner's book *Psychophysics* came out in 1860, and for some that was a momentous and earth-shaking event. He advanced Weber's discoveries to a mathematical level that seemed astonishing to scientists who had had no inkling that something having to do with the mind could be aptly characterized in quantitative and algebraic terms. Ernst Mach spoke of the liberation he felt it granted him from "the greatest intellectual discomfort of my life." Fechner proposed that the strength of the subjective sensation is proportional to the logarithm of the intensity of the stimulus. He made the argument step by mathematical step: he denoted the stimulus by $\beta$, and its increment by $d\beta$, the sensation $\gamma$ and its increment $d\gamma$, with the terms $d\beta$ and $d\gamma$ each to be considered as referring to an arbitrary unit of their own nature. He then gave Weber's Law as this: $d\gamma = K \frac{d\beta}{\beta}$, and then derives: $\gamma = x\log_2 b$, where $b$ is the threshold of perception, and $x$ is a constant whose value is determined by the nature of the units used (and the base of the logarithm chosen). This was, in its way, as stunning a result as was Kepler's discovery that the planets move in elliptical orbits, in the sense that it demonstrated that once again, a new domain was open to quantitative analysis and mathematics.

### 4.1.1 Wilhelm Wundt

Wilhelm Wundt is the defining figure in the history of modern psychology. Like Ernst Heinrich Weber, he was trained as a medical doctor, and turned his attention toward the question of applying the methods of experimental science to the exploration of human thought and behavior. His book, *Physiological Psychology*, published in 1874, had a good deal of influence, and in 1875 he moved to the University of Leipzig, where he would soon establish a psychology laboratory. As we have seen, Leipzig was where both Fechner and Weber had been professors; they had retired by the time Wundt arrived, but were supportive of his orientation. And Leipzig was the home of the Neogrammarians, whose controversies were at their peak as Wundt joined the faculty there. In the years that followed Wundt's arrival,

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1Friedman, 2000, p. 29.
2Fechner, 1860.
3It is the Weber-Fechner law that accounts for the decision to include the logarithm in the definition of bel and decibel, familiar to students of phonetics today.
he trained an enormous cadre of academic psychologists who took his perspectives to the four corners of the globe.

One important aspect of his modernity lies in the simple fact that his laboratory ran experiments, and it did so at a wholesale, not retail, pace, with a cadre of graduate students and assistants: he had 186 students, of whom 116 earned their degrees in psychology. Wundt had learned how to do that as a student of physiology. An experiment was designed to allow the experimenter to control the stimulus presented to the subject, and ideally the subject is constrained to a response that can easily be characterized. In between the stimulus and the response might be a brief period of time during which the subject, who had been suitably prepared to introspect, would report what he observed. Laboratory methods were appropriate for individual psychology, but this was only half of Wundt’s world. He also saw another side to modern psychology, which he called Völkerpsychology, a phrase that has been translated in various ways into English: there is the flat-footed “folk psychology,” and “ethnic psychology,” and also the more apt “collective psychology.” This was the discipline that would study language, customs, and myth, and other aspects of mind that are socially based. As we noted earlier, Wundt was adopting an idea that came from Herder. Over the last 20 years of his life, he published a ten volume treatise on Völkerpsychology, beginning with a treatment of the nature of language. That very substance and devotion makes clear how important the social was to Wundt’s view of the mind: the individual mind is an abstraction that we make in a world where all minds are in the very first place part of a community.

Wundt saw himself both as following in the tradition of philosophy, and as rejecting important psychological traditions before him—most strenuously rejecting the associationism that followed so easily from empiricism, especially Anglophone empiricism, such as that of John Locke. The issue that separated Wundt from associationism is how the interaction of ideas (in the broadest sense) is to be understood: the extreme associationist position treats ideas (and perceptions, a kind of idea) as thing-like, in that it has enough placement in space and time that we can see two ideas appear together frequently, and in that these ideas take on some sort of connection (think: glue) between them. In this picture, there is no active backdrop playing a role: the activity is playing on a stage on which appear only two disconnected ideas, so to speak, and if we extend the metaphor, the activity is spontaneous, without a script or a director. The opposite view, which Wundt embraced, is active: at the very least, ideas are attended to (and Wundt would have used the word apperceived, but that is a word that carries no meaning today), and they are juxtaposed by the active mind. The world of billiard balls (explained by Newton’s laws of motion) is the inspiration for the

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1 On Wundt, see Espagne, 1998. On Wundt, Kurt Danziger’s “The positivist repudiation of Wundt” (1979) (Danziger, 1979a) is very useful. It opens the discussion of the inadequacy of the Anglophone misunderstanding of Wundt, derived in large measure from E. G. Boring’s description of Wundt. Danziger’s “Wundt and the Temptations of Psychology” is an outstanding statement of Wundt’s perspective on psychology. See also Nerlich and Clarke, 2001 and Rieber and Robinson, 2001. In a dissertation on Wundt (1987), historian David Robinson noted that in 1883, Wundt—at Leipzig, at that point—turned down an offer from Breslau, and was able to negotiate an excellent retention package, including a raise from 5400 to 7500 marks; 1200 of research funds for the coming year; and additional laboratory space. It is interesting to see how academic careers in late 19th century Germany continue to present-day America. See also Blumenthal, 1970; Blumenthal, 1975; Blumenthal, 1979. Levelt, 2014: chapter 6 is a good source on Wundt as well.

2 Danziger, 1983.

3 One might disagree with our formulation here; one might believe that for Wundt, the individual mind was all that exists, and that a culture’s belief system emerges out of the interaction of such individuals. We take these as being two different ways of saying much the same thing.
Fig. 4.1: Wilhelm Wundt
associationists, who thereby saw unity in the physical and psychic world, but Wundt was thoroughly opposed to that, and he emphasized the active side of mind.

Wundt defended a position that he called “voluntarism,” though the connotations of that word are treacherous: he would emphasize that what is psychologically real are events, or occurrences, that occur in time; these events are not ideas that may leave and then come back to consciousness. If we set foot in the metaphor of thought as a river, then for Wundt the water swirling at our feet (and which has flowed downstream already in the moment that we notice it) are the mental occurrences, and nothing will ever bring them back to us again. On this point, Wundt is on the side that others will call psychologism, which we discussed above (page 170). Thoughts are not impressions; they are activities.

It was Wundt’s view that others had fallen into the fallacy of incorporating into their view of ideas certain conclusions that they have reached about the physical world: that objects perdure, most of all. An idea may be of this particular book, but the idea of the book has none of the durability that we believe is true of the book. (Brentano is surely an excellent example of someone who has fallen into what Wundt took to be a fallacy.)

Perhaps the single most important distinction in Germany separating camps of scientists who studied human beings in one fashion or another was the question as to whether all explanation could be reduced to statements about causal relationships among events in space and time. It may seem to us highly reasonable to accept the idea that science can study causal relationships in the physical world! But the issue was not whether such causal relationships exist, but whether there were any relationships that were studied by psychology that obeyed laws that were not fundamentally physical laws—and the most striking case was the case of one belief serving as a reasonable basis for believing another statement.

There is an issue here whose importance is often underplayed. It is easy to accept the affirmative side of the positivist creed: that all elements of reality can be localized in space and in time; but the flip side of that is that nothing else really exists. And the dynamics—that is, the change or evolution over time—of a real system are bound by the laws discovered by the basic physical sciences, which essentially means physics, chemistry, and almost certainly physiology and biology more generally. A positivist psychologist can only propose the existence of some thing if he can explain it in terms allowed by those positivistically more basic sciences.

And ideas may be difficult to define in those terms, but far more difficult to define in physical terms are the relations that we know, or think we know, must exist between ideas, such as the relation of implying, or serves as a grounds for believing.

In Germany, the controversy was placed in the context of the relationship between natural and social sciences. Natural sciences could be defined as those which without undue controversy fall under the positivist’s principles; the objects of study exist in space and time, and they interact in ways ultimately governed by physical laws, all of which reduce to physics. But the human sciences are different; the German refer to these as Geisteswissenschaften. Wissenschaften simply means sciences, while Geist can be translated as mind, spirit or even

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1 See Danziger, [1979a](#), an important paper bringing out the underlying issues. See also Danziger, [1980b](#).
soul. Kusch\textsuperscript{1} emphasizes that Wundt noted four distinctions between causality in the physical and the psychological realms: causality was imputed by a theory in the physical world, but is consciously experienced in psychology; that the psychological notion is “everywhere shot through with value determinations”; the psychological notion cannot be understood without reference to the notion of purpose; and causation in the physical world assumes a substantial foundation—in the classical sense of a perduring substance—whereas this is not the case in the world of psychology.\textsuperscript{2} And it should be noted that the non-substantiality of the psychological world was related to the rejection of a role for the unconscious: “no underlying substrate was assumed, and all processes and events were ‘depictive’ (anschaulich) and thus observable.” As we will see later on, the difference between these two styles of analysis would give rise to the Praguean distinction between phonetics (a study in the physical world) and phonology (a study in the psychological world).

Kurt Danziger has argued that Wundt “is a singularly inappropriate figure to choose as the originator of the modern psychologist’s professional identity.”\textsuperscript{3} Certainly we do not find Wundt making a clear call for the separation of psychology from philosophy (quite the opposite!), or offering a definition of research questions that are independent of speculative philosophy. In a letter to Adolf Meyer in 1918, Titchener, one of those who helped establish Wundt’s reputation, wrote that “Wundt is...the very first large figure in the history of thought who is temperamentally psychological...I believe that Wundt’s generalizations are mostly wrong; I do not at the moment recall any one of the larger ones that I accept today—though I have in my time swallowed most of them; but I still affirm that Wundt’s instinct is psychological, even where it leads him astray...That is his importance: not the special things he teaches.”\textsuperscript{4}

There was a tension in 19th century psychology concerning both the significance and the usefulness of introspection as a source of data for the psychologist; it was a tension that divided roughly along the lines of the British versus the Germans. For the British (not just the English: we include the Scotsmen), introspection remained a privileged source of information about the mind; the mind was, for them as for Descartes, a transparency visible to the user. On the Continent, that was no longer the dominant view, and had not been since the time of Leibniz. Kant had made it especially clear that what is phenomenally accessible to the end-user is only the phenomenal self, which is to say, only the appearances of psychological reality.\textsuperscript{5}

Wundt discussed two processes, both of which have been translated into the English word introspection. The first is Selbstbeobachtung “self-observation,” which he dismissed, while

\textsuperscript{1}Kusch, 2005:132ff.
\textsuperscript{2}The reader will recall our allusion above to the uncertainty regarding whether energy was a substance, like matter but distinct from it; to the extent that energy could be understood as the cause of movement (as potential gravitational energy causes a body to fall), this suggests that energy is a substance. We emphasize this not so much because it sheds light on what was happening in physics, but rather to emphasize that even within physics and chemistry controversy could continue with regard to what was real, and what constituted substance.
\textsuperscript{3}Kusch, 2005, p. 133), citing Wundt 1908b, pages 260-2.
\textsuperscript{4}Danziger, [1979b:31
the second is innere Wahrnehmung “internal perception,” which he held to be important—indeed, foundational. (Recall our earlier discussions of introspection, in connection with Comte (page 142) and with Brentano.) Internal perception was immediate and entirely independent of memory, and the use of practiced observers was encouraged because they were in the habit of responding without reflecting on the answer before responding. Wundtian psychology has achieved a certain degree of suspicion in anglophone countries because of the distorted image of Wundt’s work brought by Wundt’s student Titchener, as we will see below.

Wundt’s view about the inner, subjective experience of the mind was that it was not distinct from the outer, objective experience of the mind: if there are “psychical” and “physical” objects, they are “not different objects at all, but one and the same content of experience.” There are two different ways of looking at human experience, though.

This content is examined in the one case, that is, in the natural sciences, after abstracting from the subject. In the other case, that is, in psychology, it is examined in its immediate character and its complete relation to the subject. All metaphysical hypotheses as to the relation of psychical and physical objects are, when viewed from this position, attempts to solve a problem which never would have existed if the case had been correctly stated.

This view embraces the duality of human experience; it is a rejection of dichotomizing, either/or dualisms. Wundt’s view was that the same experience can be viewed and analyzed in more than one way, and the psychological point of view and the physicist’s point of view are about as distant and complementary as two views can be. But both are among the many ways that humans can talk about and appreciate their experience, and their experiences. Wundt’s view was a version of what has been called psychophysical parallelism, a view that Wundt could point to in Fechner’s work, and which we can still see as a dominant view today.

4.1.2 Oswald Külpe

Oswald Külpe was a student of Wundt’s who took an independent stance on several of the most important questions in psychology of his time. Külpe was born in 1862, and studied psychology during the 1880s: first at Leipzig, where he studied with Wundt, then at Berlin and Gottingen, after which he went back to Leipzig to earn a doctorate with Wundt. He continued to work with Wundt until 1894, when he was appointed professor at Würzburg. He developed a strong laboratory there, perhaps second in Germany only to Wundt’s, and he directed the research of many of the top psychologists of the next generation, include

111, Grundris, English translation, paragraph 2.4
2Wundt’s view is close to one that is described by 20th century philosophers as neutral monism, but that term is generally used to describe a position on what really exists—a metaphysical or ontological view. In this book, we are more concerned with overarching connections among the mind sciences, and focus therefore on perspectives on human experience, rather than ultimate claims on reality. Kusch, 2005. chapter 4 has an excellent discussion of this side of Wundt and others of his generation.
Max Wertheimer and Kurt Koffka, leaders of the Gestalt psychology movement (see below, ??).

Külpe was much more sanguine than Wundt about the notion that experimental methods could be used to study human intelligence, and his Würzburg psychologists were among the first to use subjects' own reports of how their mental process had proceeded as they tried to solve a difficult task given to them. Külpe himself developed a series of experiments which challenged the traditional view that sensation preceded an individual’s conscious intent, and independently of it as well. He presented images briefly, after asking the subjects to identify the color, or the shape, or perhaps what letters were present. When he then asked them about other characteristics that he had not prepared the subjects to attend to, what he found was that subjects did poorly, and the more aspects of an image the subjects were asked to pay attention to, the worse they were on reporting the other aspects. In short, even basic and elemental aspects of sensation had the character of an act performed by the subject. This view of sensation would be central to the development of Gestalt psychology, as we will see in the next chapter. Külpe was the teacher both of Max Wertheimer, who founded the Berlin school of gestalt psychology, and of Karl Bühler, a gestalt psychologist of a different school, working in Vienna, who will also be a significant actor in what follows. Wundt, on the other hand, saw Külpe’s position as a setback for psychology, and would have no part of an attempt to reduce (as he saw it) psychology to physiology. This is an issue that remains with us today. [Wettersten, 1988]
On Külpe’s view, then, sensation was an activity, not a passive process, and higher level cognition was not an amalgam of sensations either. This was the starting point for Max Wertheimer’s gestalt psychology, as we will see in the next chapter.

4.1.3 Carl Stumpf

Carl Stumpf, as we have already seen, was a student of Franz Brentano (and also of Hermann Lotze, who officially directed his dissertation), and like so many of his time, he trained first as a philosopher, and then left his greatest mark on psychology. His career was strikingly successful, as the German-language universities vied with each other to make offers to persuade Stumpf to come and settle down. He taught first at Würzburg (filling the position that Brentano had just left), and then at Prague, where he did important work on the psychology of musical perception and interacted with colleagues Anton Marty, Ernst Mach and Ewald Hering, and then he moved to Halle (where he was a colleague of Georg Cantor)—and then to Munich. Finally, in 1894, at the age of forty-six, he accepted an offer to move to the University of Berlin, to teach both philosophy and psychology, and he spent the rest of his career there, developing the Psychological Institute, and training some of the most important psychologists of the next generation, such as Max Wertheimer, Kurt Koffka, Wolfgang Köhler, and Kurt Lewin, as we will see in the next chapter (see page 248), and they would be the starts of the Berlin school of gestalt psychology. Stumpf would pass on the directorship of the Institute of Psychology to Wolfgang Köhler in 1922, and he was the dissertation advisor to Edmund Husserl, as we will see in Chapter 7.

\[^{1}\text{Ash, 1993.}\]
Stumpf’s work on phonetics, the sounds of language, were extremely influential, and would leave an important mark on the theory of phonology. His work was both a continuation of Helmholtz’s earlier work, and also a strong criticism of it; it was published in a book in 1926, *Die Sprachlaute*. Stumpf’s analysis of the phonetic characteristics of vowels and consonants would be adopted and developed by Roman Jakobson in Prague (see Chapter 9) and later with Morris Halle in Cambridge, Massachusetts (see Volume 2).1

Max Meyer was one of Stumpf’s less famous students. He was not a great success academically but he published a significant book on behaviorism two years before the more famous publication by John B. Watson. This would be of no great consequence to us but for another aspect of his academic career. He had one, just one, doctoral student in his career, which he spent at the University of Missouri, and that one student was Albert Paul Weiss. Weiss was a behaviorist who spent most of his academic career at Ohio State University; he had a weak heart, and died at the age of 51. But Weiss was a close friend of Leonard Bloomfield, the linguist, and during the few years they shared together at OSU, Weiss’s behaviorist orientation would have an enormous impact on Bloomfield, as we will see in Chapter 6 below.

There is another important line of influence of Wundt’s work which flowed to Russia, and then beyond. This stream of influence involves a number of important psychologists whose names are rarely encountered today, but who play an important role when we come to discuss Trubetzkoy and Jakobson, in their lives in Russia and afterwards. Of these students of Wundt, the most important is Georgi Chelbanov, who became professor of psychology at the University of Moscow in 1907, and his student Gustav Shpet. We include this stream of influence in the diagrams, but will leave our discussion of this matter for later in the book—mainly in Chapter 9, when we discuss Trubetzkoy and Jakobson.

### 4.2 Psychology comes to the New World

During the 1880s and 1890s, psychology came to the United States, packed in the trunks and suitcases of the handful of young Americans who traveled to Europe—mainly to Germany—to study what was happening over there. These were largely students of philosophy who were not satisfied with what was being taught in philosophy courses—young Americans such as William James, G. Stanley Hall, and James McKeen Cattell; there was also Edward Titchener, an Englishman who came to the United States after studying in Germany. Of these four, all studied in Europe (which was the normal case for any American seeking an advanced education), and all but William James studied with Wilhelm Wundt.

#### 4.2.1 William James

William James’ greatest contribution to psychology was his book *The Principles of Psychology*, published in 1890. It was a massive book, twelve years in the writing; it was so long that James was obliged to produce a shorter version for the slow of reading. When the book was finally finished, much of James’s interest in psychology was finished along with it. But it was such a magnificent creation that James remained till the end of his life many psychologists’

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1 We will look at “Tense and laxness,” in Volume 2, Jakobson and Halle.
conception of the true American psychologist, the leader of his discipline and the authority of his field. John Dewey, foremost American philosopher of his time, wrote of James, “By common consent he was far and away the greatest of American psychologists—it was a case of James first and no second. Were it not for the unreasoned admiration of men and things German, there would be no question, I think, that he was the greatest psychologist of his time in any country—perhaps of any time.”  

William James was 48 at the time *The Principles of Psychology* was published, and professor of philosophy and psychology at Harvard. His origins were unusual: his grandfather had been an immigrant, but one who made a considerable fortune in upstate New York. William James’s father suffered from being unappreciated by his own very successful father, and this sense of incompleteness was passed down, as can happen in families, to William. The family fortune allowed for private tutoring and travel in Europe—this was the first generation of wealthy Americans, mid-century, who could spend considerable lengths of time in Europe, learning the languages and the culture of the Old World, and James drank deeply of the art he found in Europe, and mastered both French and German—an important step, as we have noted, for a scientist-in-training. James’s siblings were just as extraordinary as he was. His younger brother Henry would become one of the most famous men of letters of his generation, and his sister Alice’s diary has become a significant contribution to our understanding of her day. William’s interests ranged widely, and scarcely would remain fixed; they changed, they evolved. As a young man, he was very interested in the arts, and spent a good deal of time in Europe studying art when he was in his twenties. He came back to Boston, earned a degree in medicine, and was hired to teach anatomy and physiology at Harvard University in 1873. His interests moved towards psychology at that point—he was greatly impressed by Wundt’s book (1874) on physiological psychology, and he wrote a review of it—but James had not studied the subject formally, and learned by teaching, beginning in 1876. Two years later, he signed a book contract and started writing his book, and its publication in 1890 was an iconic moment in early American psychology.

He was ready then to say to himself that he had had enough. *Basta.* He worked hard to persuade Hugo Münsterberg to come to Harvard and take over the psychology lab, and eventually the department. Münsterberg was a generation younger than James, and was German, and had earned his doctorate with Wilhelm Wundt in 1885, so he was the spitten image of a successful academic psychologist. It was not an easy task to persuade Münsterberg that coming to the United States did not constitute academic suicide, and the process of persuasion took quite a few years. In the end, Harvard won out over Freiburg in Münsterberg’s decisions, and this was a beginning of the tipping towards the New World’s academic leadership. But as a step it was hardly noticed at the time.

When he was able to do less psychology, thanks to Münsterberg’s presence and energy, James was able to think more about philosophy, developing the distinctively American school of pragmatism. He had been friends with Charles Sanders Peirce in college at Harvard, and James interacted with the luminaries of American philosophy in the years that followed his engagement with psychology. He developed during this time his own style of pragmatism, distinct from that which Peirce presented and published. Like many of his

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1Dewey, 1910.
generation, James became seriously involved in the study of parapsychology and life after
death. There hardly seemed to be a limit to his interests.\footnote{James maintained a warm relationship of friendship with Ernst Mach throughout his life; he had met Mach in Europe when he was young, and they remained in contact by correspondence. James wrote very glowingly of Mach’s work when it was published in the United States. See Blackmore 2000.}

If James had came to psychology from anatomy, he had never been a great lover of labo-
ratory experiments, certainly not as Wundt and his colleagues conceived of them, and he
was happy to leave all that to Münsterberg. There is a wonderful passage in his book which
reveals much of his opinion about a range of things: he wrote that “psychology is passing
into a less simple phase. Within a few years what one may call a microscopic psychology
has arisen in Germany, carried on by experimental methods, asking of course every moment
for introspective data, but eliminating their uncertainty by operating on a large scale and
taking statistical means.” This sounds very reasonable, does it not? James plows on: “This
method taxes patience to the utmost, and could hardly have arisen in a country whose
natives could be bored.”

We might like to think that this was just good, clean fun, but there was more to it than that. \textit{Could not be bored?} James had continued, “Such Germans as Weber, Fechner, Vierordt, and Wundt obviously cannot; and their success has brought into the field an array of younger experimental psychologists, bent on studying the elements of the mental life…” Maybe Germans could not be bored, but they could read English. Wundt’s laboratory did not ap-
preciate these remarks. Charles Judd, an American working there, recalled that “especially
was there a very pronounced antipathy to James. James had done what was thought to be
quite out of order; not only had he criticized Wundt but in some cases—as, for example in
discussing the innervation theory—he had allowed his criticism to take the form of witty
sarcasm. This was far too much. Not only that, but he indulged in that remark about patient
laboratory work in a land where they did not know what it means to be bored. As a result
diplomatic relations were promptly suspended.\footnote{This kind of wit could fly both east and west. When Whitney’s book was published in 1867, Heymann Steinthal published a review of it, noting that it would likely be a popular book, because it was “easily accessible to the common mind,” and [in Alter’s words:] one should not expect from this sort of book the same depth of treatment one would find in a work written for a German audience! (cited in Alter, 2005: 170.)}

We noted just above that James seemed to have had it with psychology by the time his
book came out. Hear him again, as he wrote, “it would be terrible if even such a dear old
man as [Fechner] could saddle our Science forever with his patient whimsies, and, in a
world so full of more nutritious objects of attention, compel all future students to plough
through the difficulties, not only of his own works, but of the still drier ones written in his
refutation.\footnote{James, [1890] I 549, cited in Boring.}”

There is something in William James’s style of writing, most of the time, which marks
the beginning of a down-to-earth, no-nonsense American style of writing, a style that he
shared with John Dewey, and we will see it more and more—we will shortly see it in John
B. Watson, for example. And that style certainly left open the possibility that one might
bring up German psychology, and say that it couldn’t have arisen in a country where people
could get bored. In another paper, James skewers “scholastic philosophy,” saying that it
is “common sense grown pedantic.” Which is everything that Americans don’t want and
don’t like. There is a culture clash here, a we’re-not-Europeans kind of identity, and it is
one that grows and develops over the course of the following century. We will see a replay of this later on, when a kind of folksy and at times self-deprecating sense of humor runs full tilt into a no-nonsense East Coast earnestness. Self-deprecating? That was a style that William James could adopt, and frankly it would be unimaginable for a German Professor to give a presidential address to his society, and make a remark like the one that James did: “You will agree with me that I have brought no new insight to the subject, and that I have only gossiped to while away this unlucky presidential hour to which the constellations doomed me at my birth. But since gossip we have had to have, let me make the hour more gossipy still by saying a final word about the position taken up in my own Principles of Psychology…” and off he went again.

The Principles of Psychology begins with the sentence, “Psychology is the Science of Mental Life, both of its phenomena and of their conditions.” There is a lot to unpack there: for James, the central fact of psychology was the flow of consciousness, of awareness, and how that flow was accessible to our inner awareness, and how it was related to our place as biological organisms in the larger world we live in. To James we owe the expression of how the baby must encounter the world, “as one great blooming, buzzing confusion.”

There is no better way to get a sense of James’s way of thinking than to listen to him; and he wrote many sensible things about the ways in which a person is constantly engaged in social acts. Here is a perfectly typical example of how William James reminds us of who we are:

Our social self-seeking, in turn, is carried on directly through our amativeness and friendliness, our desire to please and attract notice and admiration, our emulation and jealousy, our love of glory, influence, and power, and indirectly through whichever of the material self-seeking impulses prove serviceable as means to social ends. That the direct social self-seeking impulses are probably pure instincts is easily seen. The noteworthy thing about the desire to be “recognized” by others is that its strength has so little to do with the worth of the recognition computed in sensational or rational terms.

We all have enough insight into ourselves, you can almost hear him say. Surely we can all see ourselves well enough to see how predictable we are, even if we are not entirely rational about it. What follows might almost come from a novel: “We are crazy to get a visiting-list which shall be large, to be able to say when any one is mentioned, ‘Oh! I know him well,’ and to be bowed to in the street by half the people we meet.”

A moment later, James remembered a passage from Thackeray’s Book of Snobs: “Thackeray somewhere asks his readers to confess whether it would not give each of them an exquisite pleasure to be met walking down Pall Mall with a duke on either arm. But in default of dukes and envious salutations almost anything will do for some of us; and there is a whole race of beings to-day whose passion is to keep their names in the newspapers, no matter under what heading…” If the rhythm were a bit different, we might almost be reading Alexander Pope.

1James, Principles: 308.
For James, psychology was not the study of the individual limited to the interactions she might have in our psychology laboratory. It was the study of people, a study that all of us have been engaged in since our youngest years, and we have all reached some of the same conclusions.

Not only the people but the places and things I know enlarge my Self in a sort of metaphoric social way. “Ça me connaît,” as the French workman says of the implement he can use well. So that it comes about that persons for whose opinion we care nothing are nevertheless persons whose notice we woo; and that many a man truly great, many a woman truly fastidious in most respects, will take a deal of trouble to dazzle some insignificant cad whose whole personality they heartily despise.

4.2.2 **G. Stanley Hall**

G. Stanley Hall was the other leading psychologist in the United States in James's day. Recent scholarship has not been kind to Hall's reputation. He worked hard to develop the authority of the leading psychologist in the United States, a status that would place him, had he achieved it, above William James, but despite a range of professional accomplishments, the American world of psychology did not come to view him in that light.

Hall arrived to do doctoral work at Harvard in psychology in 1876, and worked with William James, who was only two years older than he was. A warm relationship of friendship "would eventually prove to cover a good deal of competitive hostility." He was awarded his PhD just two years later (which was unremarkable at the time)—and it was the first PhD awarded by Harvard's department of philosophy, as well as the first awarded in the United States in the field of psychology.

Hall then went to Germany, and spent the first year in Berlin. While there he worked closely with Hugo Kronecker at the Physiology Institute. He wrote back to William James at one point, "I have stood in much the same terms of intimacy and recipiency [to Kronecker] as last year to you, and to [him] I am likely to owe a scarcely smaller debt of gratitude." The next year, in 1879, he went to Leipzig, and took advantage of the opportunity to attend Wundt's lectures.

He was not impressed. "Wundt is more and more exasperating," he wrote to William James.

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1. p. 308-09.
4. Ross, 1972, p. 79, fn. 49.
5. Ross, 1972, p. 81.
more speculation and less valuable observing than any man I know who has had his
career.

It's not clear exactly what that means, but it's not good (and sounds a bit reminiscent of the
disparaging remarks that Whitney made about his German professors). “His experiments,
which I attend,” Hall went on, “I think utterly unreliable and defective in method.” He
returned to the United States, and tried to get a position at Johns Hopkins University, which,
like Harvard, awarded the PhD degree. James wrote a strong letter of recommendation; he
wrote that Hall “is a more learned man than I can ever hope to become...I feel his
exceptional merits, moral as well as intellectual, so strongly that I cannot bear to think of
his being any longer with a place commensurate with them.” Strong praise, but Hopkins
was not ready to hire Hall at that moment.

By 1884, things had changed, and Hall was appointed professor of psychology at Johns
Hopkins (over Charles Sanders Peirce, in the event) and in 1889, he became president at
Clark University, in Massachusetts. He has long been recognized as one of the early aca-
demic leaders who created a German-style, graduate and research oriented university in
the United States—Clark University—which opened its doors in 1887, with a world class
faculty (which included Franz Boas). Sokal noted, “By 1892, however, Hall’s chronic secre-
tiveness and dishonesty had alienated most of his colleagues, and by the end of the year
most of them had left, primarily to go to the University of Chicago.”

4.2.3 Edward Titchener

Edward Titchener studied with Wundt in Leipzig, in 1890, just after getting his degree from
Oxford. He became a professor of psychology at Cornell University in upstate New York
two years later, where he continued Wundt’s goal of studying the character of the content
of consciousness; his approach was known both as structuralism (he coined the term) and
as introspective psychology.

In coming to Cornell when he did, Titchener was in a position to develop an influential
group of researchers trained in his ways of doing psychology, and he did precisely that over
the next several decades, supervising the writing of 56 PhD dissertations in psychology.

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1 Ross, 1972:104.
4 We should emphasize that as far as we can see, there is really no connection between this early psychological
usage of the term structuralism and the later uses of the term in a number of social sciences, most notably in
linguistics. As we will see in Chapter 9, the term structuralism took on a new life in the context of the Prague
Circle of linguists, and many later scholars responded to the Praguean call to interpret Saussure as a leading figure
in the establishment of structuralism. We will come back to this question when we discuss the Prague Circle.
See Percival, no date who directs us to Titchener, 1898. See also Joseph, 2002, p. 54, footnote 1, who points
to Titchener, 1898. Joseph says that Angell (see below) first used the term structuralism, and this was in 1907.
And see Koerner, 1975, 721-722. Koerner’s footnote 4 reads, “Jakobson reports that he had used the expression
’structural method’ at the Congress of Slavists in Prague on October 7, 1929...Recently, O Szemerényi...has
claimed that V. Mathesius was the first to introduce the term “linguistique structurale et fonctionnelle’ TCLP
4:291…”
5 Titchener, 1898. Cf. also the critical-controversial papers of Caldwell, 1895, 187, and Titchener, 1895, p. 290
6 On Titchener: Bjork, 1983, chapter 4, and Danziger; also Boring, Edward Bradford Titchener; Rand B. Evans,
E.B. Titchener and his lost system. See also Titchener, 1898; Hindeland, E.B. titchener; Leahy, 1981.
Titchener viewed himself as bringing Wundt’s work to the United States. One of this students recalled that Titchener was “a brilliant young man who would give us the latest news from Leipzig rather than one to be heard for his own sake.”

By all accounts, his was a dominant—we could say domineering—personality. Two of his students who had married each other decided that they would “accept ‘insults’ and arbitrary control from Titchener in order to retain the stimulus and charm of his sometimes paternal and sometimes patronizing friendship. I never broke with the master and I still feel the credit balance remained on my side.” This was Edwin Boring writing these words, and Boring was to become a highly influential professor of psychology at Harvard; still, years later he recalled the “personalized magnetism” that Titchener generated, as well as the “real kindness [Titchener demonstrated] to those disciples who avoided transgression.”

Eventually, like so many, he began to refer to what younger people did as “fads.” In 1907, he wrote to Robert Yerkes, who was perhaps the first leading American figure in animal learning:

> animal behavior is like the functional standpoint—extremely in fashion. There is a new field opened for mediocrity: That is the real secret. As soon as a novel standpoint is announced or a new region or work opened, the rank and file rush in, because anything that they say or do will for the time pass muster: And it is contrariwise, deucedly difficult to get a hearing where the bulk of past work is great and the methods established. Let us keep our heads: that is the important thing. Work on animal behavior is decidedly important, and the functional standpoint is decidedly worth thinking through—my favorite expression, you see! But they are not the be-all and end-all of “scientific” psychology. Cited in Bjork, 1983, pp. 97-98.

Towards the end of his life, Titchener offered an overview of the kinds of confrontation that modern psychology had encountered when it was first being developed by Wundt. His goal was to show to those most active in psychology that they were likely to be misled about the growth of psychology because of the generational effect: his teachers’ generation was responding to the previous generation, but did not make it their business to say it out loud.

Wundt was responding in everything he wrote to Herbart, but how could someone reading Wundt today have even the slightest idea of that?

Wundt’s psychology “took shape against a background of physics and of a physiology informed by physics. Fechner was a physicist; Helmholtz was physicist and physiologist; We-
ber and Hering and Wundt were physiologists.”

The principal drive, Titchener said, was to establish a psychology which can be housed (as he put it) under one roof with physics and biology—without which, “science can have no stable meaning.” His picture is clear: biology is based on physics, and psychology is based on biology. But the academic environment was hostile, and there were three forces holding psychology back. The first was the influence of Herbartian psychology; the second was what he called “empirical psychology,” and the third was quite simply philosophy.

**Herbartian psychology?** A modern psychologist who could identify who Herbart was and what he did would be a *rara avis* indeed. Nor was his a household name when Titchener was writing, either: he wrote, “To most of us, I suppose, Herbart is little more than an historical name; to Wundt in the seventies Herbart was an all-pervading institutional opponent.”

He listed nearly a dozen Herbartian psychologists who were prominent before Wundt, and then wrote, “Bonitz and Exner; Waitz was at Marburg; Stoy was at Jena. To us, truly, these men are just a list of names and dates; to a nascent experimental psychology they represented a highly formidable opposition.” Then Titchener emphasized what he suspected the current reader was in no position to perceive: that “all through the second part of the *Physiological Psychology* Wundt has Herbart steadily in mind; that the improvised doctrine of apperception is meant as a counterblast to Herbart; that the whole of Wundt’s psychology beyond the chapter on perception is shaped with polemical reference to Herbart.”

Titchener was telling his reader that when they read Wundt, they may have heard and understood the *answer*, but they did not understand the question: they did not even realize that there was a question that they were not understanding. He went on:

> That, you see, is the sort of trick that history plays upon us; a good many of the later Wundtians have been anti-Herbartians without realising it. So that the effect of Herbartianism upon experimental psychology was in reality twofold, internal and external. The Herbartians were in possession; they must be dislodged, superseded, discredited, if experimental psychology was to grow to power; that was the external side of things. Internally, meantime, the very fact of having to combat a well-rounded and critically tested system tinged and moulded the doctrines of experimental psychology itself.” (315)

Titchener hit the nail clean on the head: this is a tremendously important phenomenon, and one that is not always easy to note in real time. Titchener could hope that his students would understand him when he wrote that, but we will never know.

Let us take a look at what was on Titchener’s mind as the 19th century came to a close. William Caldwell, a philosopher at Northwestern University, published a critique of some of Titchener’s work in the *Psychology Review*. Caldwell’s concern was that Titchener’s sole

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1 Titchener, 1925, p. 313.  
2 Recall our discussion of teleology above, p. 57 and p. 67. Bjork notes that Titchener “was conscious of engaging in “a battle between the principles of science, as they were built up by the men of the nineteenth-century, Darwin included... and the principles of teleology, which have been adopted... by the biologizing psychologists.” And he told Adolf Meyer something he might have said to James: “I shall use every weapon at my disposal to resist your revolutionary aims: I have not the faintest intention of allowing myself to be killed.” p. 87, Bjork gives citations.  
3 Caldwell, 1898.
focus was on the consciousness of sensation and affection, and he argued that this leaves out some very important things: first of all, it leaves out will or intention, and in addition it leaves out the integrative act of consciousness, the pulling together of all the sensations and affects into a perceived unity. Titchener, he wrote, “explains the highest psychical formations from their lowest elements; it is equally important for psychology to explain the lowest elements from the point of view of the highest psychical formation, such as control and conduct and self-affirmation.” The critical point to emphasize, he wrote, was the “active, unifying, synthetic self.”

Titchener’s response begins with a off-hand remark, in a footnote, that is nonetheless worth noting. If Caldwell had presented twelve numbered arguments against Titchener, Titchener notes that at least three of them “rest on technical errors… Such lapses are hardly to be avoided by anyone who travels out of his own special field into that of another discipline; they do not at all impair the value of Professor Caldwell’s contentions regarded as a whole.”

Here is a chide, gentle or not, that philosophers do not have all the chops necessary to do psychology, or even to understand it in a sound, professional way.

When a biologist studies an organism, Titchener wrote, he may study form, or structure, and this is anatomy or “morphology”; he may study function (physiology); or he may study development (ontogeny). He may do any of these not only from the point of view of an individual; he may also do this at the level of life as a whole. Its parts are its species, its physiology is its ecology, and the historical version of ontogeny is phylogeny, the development of species over time.

The psychologist’s task runs parallel to that of the biologist, he continued. The study of form is the attempt to “ravel out the elemental processes from the tangle of consciousness.” But if we dissect a frog, we will not discover the functions of the organs we find, and likewise discovering the elements of consciousness is not the same thing as discovering the functions of these elements.

Yes, there is function, he wrote. In the biological world, this includes digestion, locomotion, secretion, excretion, and so on. In the psychical world, this includes memory, recognition, imagination, conception, judgment, attention, apperception, and volition—to mention just a few. If we want to understand such things as recognition and judgment, we have to engage in functional psychology, and that is simply not what the experimental psychologist, engaged in Titchenerian psychology, is doing: he is doing structural psychology, akin to the task of student of biological anatomy—not to that of the student of physiology.

“It cannot be said that this functional psychology… has been worked out either with as much patient enthusiasm or with as much scientific accuracy as has the psychology of mind structure,” he went on. But the main interest has been in morphological analysis, not in function. And why is that? “The reasons [are] not far to seek: We must remember that experimental psychology arose by way of reaction against the faculty psychology of the last century. This was a metaphysical, not a scientific, psychology.” If we focus on function before we get structure clearly worked out, we run the great risk of allowing for

\[^1\] op cit. 408.
\[^2\] op. cit. 449.
\[^3\] op.cit. 450
pseudo-explanations that things are as they are just so that they can do what they in fact do. If we did allow that kind of account, then psychology would just become philosophy again (a slight dig at the philosophers). “In a word, the historical conditions of psychology rendered it inevitable that, when the time came for the transformation from philosophy to science, problems should be formulated explicitly or implicitly, as static rather than dynamic, structural rather than functional.”

Now that is an interesting remark that we should pick up on: Titchener was associating dynamic with functional, and also with less scientific. This is a linkage that we will see again later, most notably in discussions of linguistics in the second quarter of the twentieth century.

E.G. Boring, in his own memoirs, remembered studying psychology at Cornell, from 1910 to 1918, which “revolved around and was kept in its orbit by the personality of E. B. Titchener. What a man! To me he has always seemed the nearest approach to genius of anyone with whom I have been closely associated.” And this was after a long career in which Boring had been a professor at Harvard for decades. “I used to watch my conversations with him, hoping I might gain an insight into why his thinking was so much better than mine. I decided presently that his superiority lay in his easy command of memory traces, his ready entertainment of novel relationships, his equally ready abandonment of unprofitable hypotheses, and his avidity in the pursuit of goals.” And he goes on in the same vein for quite a while. “Seldom did he distinguish between his wisdom and his convictions and he never hid either.”

Recent scholarship has not treated well Titchener’s (or Boring’s) presentation to American scholars. Kurt Danziger drew scholars’ attention to the fact that “Titchener practically made a career out of interpreting Wundt in his own highly idiosyncratic fashion,” and many Americans got their first and only taste of Wundt from reading Boring’s classic history of psychology—and “it is apparent that Boring took his admired teacher, E.B. Titchener, as a guide in these matters.”

Danziger echos a point we raised in Chapter One: we must not lose sight of the fact that questions, once settled, become no longer understood, and this effect is amplified in the passage to the next generation. Danziger wrote

Boring was himself deeply committed to the positivist philosophy of science whose influence on the early development of psychology is at issue here. But his is the commitment of the second generation: What had been for his teachers conclusions carefully arrived at and boldly asserted, have now become matters to be taken for granted, implicit certainties not open to debate or even worthy of mention. For the historiography of psychology the major consequence of this stance is that the dependence of psychological theory and method on prior philosophical commitments is lost from view.

1 op.cit. 453.
2 Boring, 1952:32.
3 Danziger, 1979a:206.
4 Danziger, 1979a:206.
Danziger is being perhaps too modest, or in any event underselling his point: the consequence that he identifies is not simply a consequence for the historiography of psychology; the historians of psychology can take care of themselves. The more important consequence is the loss of consciousness among the research psychologists themselves.

Because only one kind of philosophy of science is regarded as legitimate (or even conceivable), differences on scientific issues are not seen as the consequence of philosophical differences. This is a comforting attitude for those who have no wish to question fundamental assumptions, and that usually includes the conservative majority. When a particular philosophical commitment becomes characteristic of the mainstream of development in a certain field, it is usually so much taken for granted that it is not even identified by its practitioners. (206)

4.2.4 Functionalism: the University of Chicago

The University of Chicago was founded in 1891 with money provided by John D. Rockefeller and a vision that came from William Rainey Harper, a teacher of Hebrew, a man of boundless energies—and a scholar with a PhD in linguistics, a former student of William Dwight Whitney's at Yale. One of Chicago's early priorities was to establish an important psychology department, and Harper brought John Dewey in from the University of Michigan in 1894.

Under the leadership of Dewey and of James Rowland Angell, a school of psychology arose which came to be known as functionalism, a perspective interested not so much in the structure of consciousness as in the way the mind functions to meet goals in a given context or environment. Early in his career, John Dewey was deeply involved in both disciplines, psychology and philosophy (in this respect following the path of William James), but pragmatism, as a disciplinary label, largely kept itself to philosophy. Was functionalism, in psychology and as developed at the University of Chicago, just the interpretation of Deweyan pragmatism in a psychological context? As we will see just below, this was a touchy issue.

4.2.5 John Dewey and functionalism

John Dewey was the American intellectual par excellence. He was born in Burlington, Vermont: could there be a more American place to be born, in the year of our Lord 1859? This was the year in which Darwin's first, great book was published, and it would influence Dewey intellectually when he first encountered it at the University of Vermont. Dewey taught school for two years after college, then headed for graduate study at Johns Hopkins University, where he studied philosophy with Charles Sanders Peirce and G. S. Morris, and experimental psychology with G. Stanley Hall. He spent ten years as a faculty member at the University of Michigan, and then in 1894, he moved to the University of Chicago, 

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1This functionalism had nothing to do with the functionalism that the linguist Martinet would propose several decades later.
which had only been in existence for two years, and there he was actively involved in both questions of philosophy and psychology. Dewey also set up the Laboratory School of the University of Chicago, because his pragmatism was a philosophy that needed to work in the wild if it was to work anywhere at all. In 1904, he moved to Columbia University.

Early on, before there was something that would be called functionalist psychology, Dewey cast his lot with the new kind of psychologist, the one who no longer spent his hours introspecting but rather would run subjects in experiments. Already by 1884, he felt that there had been a revolution in psychology:

What can be meant, then, by saying that the rise of this physiological psychology has produced a revolution in psychology? This: that it has given a new instrument, introduced a new method,– that of experiment, which has supplemented and corrected the old method of introspection. Psychical facts still remain psychical, and are to be explained through psychical conditions; but our means of ascertaining what these facts are and how they are conditioned have been indefinitely widened.

What was characteristic of the new methods was the use of experiments under controlled conditions, and the commitment to quantitative measurement, both of which were inconsistent with the older style of introspection.

Dewey also made some remarks in this early paper on the character of the relationship between those who embrace a new revolution in science and those who have come before. It is so unusual and so mature that it bears both citation and reflection. We have no need to castigate those who came before us, Dewey wrote:

There is no need to cast stones at those who, having a work to do, did that work well and departed. With Sir William Hamilton and J. Stuart Mill the school passed away. It is true that many psychologists still use their language and follow their respective fashions. Their influence, no doubt, is yet everywhere felt. But changed conditions are upon us, and thought, no more than revolution, goes backward. Psychology can live no better in the past than physiology or physics; but there is no more need for us to revile Hume and Reid for not giving birth to a full and complete science, than there is for complaining that Newton did not anticipate the physical knowledge of to-day, or Harvey the physiological.

Yesterday's psychologists did the work that needed to be done when they did it; and we can be grateful to them for doing it. And their work was not just solving scientific problems: it was identifying the problems, it was asking the questions for the first time.

The history of all science demonstrates that much of its progress consists in bringing to light problems. Lack of consciousness of problems, even more than lack of ability to solve them, is the characteristic of the non-scientific mind. Problems cannot be solved

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1The following citations are from Dewey: “The new psychology” Dewey, 1884
till they are seen and stated, and the work of the earlier psychologists consisted largely in this sort of work. Further, they were filled with the Zeitgeist of their age, the age of the eighteenth century and the Aufklärung, which found nothing difficult, which hated mystery and complexity, which believed with all its heart in principles, the simpler and more abstract the better, and which had the passion of completion. By this spirit, the psychologists as well as the other thinkers of the day were mastered, and under its influence they thought and wrote.

And then Dewey called upon all of his contemporaries to set down their arms that had been raised against scientists of the past, and to live in the present:

Thus their work was conditioned by the nature of science itself, and by the age in which they lived. This work they did, and left to us a heritage of problems, of terminology, and of principles which we are to solve, reject, or employ as best we may. And the best we can do is to thank them, and then go about our own work; the worst is to make them the dividing lines of schools, or settle in hostile camps according to their banners. We are not called upon to defend them, for their work is in the past; we are not called upon to attack them, for our work is in the future.

It is hard to imagine an expression of a nobler attitude towards one's discipline than that which Dewey expressed in these words.

4.2.6 James R. Angell

James R. Angell was ten years younger than John Dewey. He had been a student of Dewey's as an undergraduate, and then master's student, at the University of Michigan (where Angell's father was president). Angell went to graduate school at Harvard, where he was influenced by William James as well. Angell seems to have had an amiable soul; towards the end of his life, he wrote that he thought highly of his Harvard teachers, William James, Josiah Royce, and George Herbert Palmer: “all three remained my warm friends as long as they lived and two of them, James and Palmer, I am sure considerably overestimated my abilities.” Together, Dewey and Angell established functional psychology at the University of Chicago, and after Dewey left for Columbia, it was Angell who remained at Chicago as the doyen of the functional psychologists there.

Functionalism

Functionalism took its cue first from Darwin: it put the emphasis on the primal fact that all people (and all animals, for that matter) have evolved in a biosphere in which individuals survive if they are able to formulate goals—finding food, avoiding predators—and establish behaviors that allow them to achieve those goals. *The central fact is that people act,* and

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1Angell, 1936, p. 10. This is another example of the self-deprecating tone we observed in James, and will see again, an expression that is unimaginable in one of his German colleagues.
the goal of psychology should be to understand how people carry out activities in order to achieve the ends that they have defined; consciousness may play a role in this, but it may not, and it was therefore a part of the story, but not the central part.

James Rowland Angell explained in a lecture in 1906 how functionalism differed from earlier views. First he pointed out how functionalism distinguished itself from structuralism. Functionalism involves...[1] the effort to discern and portray the typical operations of consciousness under actual life conditions, as over against the attempt to analyze and describe its elementary and complex contents. The structural psychology of sensation, e.g., undertakes to determine the number and character of the various unanalyzable sensory materials, such as the varieties of color, tone, taste, etc. The functional psychology of sensation would on the other hand find its appropriate sphere of interest in the determination of the character of the various sense activities as differing in their modus operandi from one another and from other mental processes such as judging, conceiving, willing and the like.

The structuralist might sit in a quiet room and introspect; the functionalist would rather know how a person accomplished a task in an environment that resembled a real life situation. This difference on method would emerge again and again; in late 20th century linguistics, linguists differed with regard to whether linguists' judgments about specific sentences, out of context, were of any value: some thought these judgments were the rock-bottom empirical basis of the science, while others thought they were poor substitutes for evidence about how sentences were “really” used, in real contexts by real people.[2]

[Functionalism’s] fundamental intellectual prepossessions are often revealed by the classifications of mental process adopted from time to time. Witness the Aristotelian bipartite division of intellect and will and the modern tripartite division of mental activities. What are cognition, feeling and will but three basally distinct modes of mental action? To be sure this classification has often carried with it the assertion, or at least the implication, that these fundamental attributes of mental life were based upon the presence in the mind of corresponding and ultimately distinct mental elements.

One of the consequences of the Darwinian revolution was the realization that one of the means for better understanding people was by studying the behavior of animals, in the hope that such studies, conducted under controlled laboratory conditions, would reveal general laws about learning and behavior that were true across all mammals, or perhaps even across all animals. Psychologists were now encouraged to open laboratories to the study of a range of animals: rats, pigeons, and dogs, for example. Angell remarked on[3]

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1 Angell, 1907: 63ff.
2 Angell, 1907.
3 Angell, 1907.
the rejuvenation of interest in the quasi-biological field which we designate animal psychology. This movement is surely among the most pregnant with which we meet in our own generation. Its problems are in no sense of the merely theoretical and speculative kind, although, like all scientific endeavor, it possesses an intellectual and methodological background on which such problems loom large. But the frontier upon which it is pushing forward its explorations is a region of definite, concrete fact, tangled and confused and often most difficult of access, but nevertheless a region of fact, accessible like all other facts to persistent and intelligent interrogation.

By the way, Angell will come back to our story twice more—first, when his student, John B. Watson, rebels against Angell’s functionalism and establishes a new framework that he calls behaviorism (see page 215), and then later when he goes to Yale University as its president, and succeeds in hiring Edward Sapir, the superstar professor of linguistics, away from the University of Chicago.

4.3 Psychology in France

A good deal of our attention has been focused on actors and events in Germany and in the United States, but what was happening in France throughout this period can give us a sense of some general patterns as well. France was closer to Germany in every sense than the United States was, but France also had a culture and a tradition that was quite different from that in Germany.

In the second half of the 19th century, France enthusiastically embraced the methods and goals of experimental psychology that were being developed at a brisker rate in Germany. When we juxtapose the developments in neighboring and competing countries, as we do throughout this book, we find three strategies (in a broad sense of the term)\footnote{The history of psychology in France and England is an area in which considerable work is being done at present. We have profited from the work of Serge Nicolas: \url{https://sites.google.com/site/prsergenicolas/home/publications} and our information draws on his sources, notably Nicolas. 2013.}

The first strategy was one of the development of a distinct approach, well-informed about the progress being realized elsewhere but conducted according to the logic and the dynamic that is specific to a particular country. This is basically what we find in Russia during this period, and to a large extent what we see in Great Britain.

The second strategy was one of massive importation and the licensing of a foreign trademark, so to speak, which would eventually lead to a transfer of the requisite competencies and ultimately an autonomy which could lead to a transfer of leadership. This is the model that we see very strikingly throughout this book, as the United States engages in a transfer of knowledge and competence from Germany, supplemented by the equally important flight of intellectuals during the Nazi period to the United States. The second volume will discuss the rise to dominance of the United States in the period after World War II.
The third strategy, which was the one that France adopted with regard to its long-time competitor across the Rhine, is straight-forward competition. That was just what had happened in linguistics, when the French linguistic scene had gotten behind the creation of a Paris School that could stand up to the comparative linguists in Germany, and to the young Neogrammarians in particular. But that strategy demands a leader who has the intellectual stature of a young Saussure. It was less clear in psychology that France could come up with a leader who had the breadth, the depth, and the energy of a Wilhelm Wundt. There were a few possible candidates who we will mention shortly, but for reasons both personal and structural, an equally powerful French school did not arise.

We should not lose sight of the fact that France had another more structural disadvantage compared to Germany: France was still suffering from the organization of the university that Napoleon had instituted, one which kept professional training apart from research. The importance of research seminars of the sort that were developed in Germany, and the laboratories that were there as well cannot be overestimated. As a result, the sheer number of graduate students, of dissertations, and of ongoing research projects in the German world of psychology led to the domination of the German world of psychology, and helped the field become distinct from that of philosophy and from the work done in medical schools.

What France did have during the 19th century was a national educational system that put a great deal of support into formulating a standardized curriculum for philosophy that was uniform across the entire country, and which was taught to students in their last year of high school, and which set the stage for the training of advanced students who would then, after earning their advanced degrees, go off and become high school (lycée) teachers. A small number of those 600 or so teachers might hope to move up the academic ladder to a university appointment after a decade or so, but the professional goal of a university student studying philosophy was typically to become a high school teacher, and to teach philosophy as a capstone course in the final year of the high school curriculum.

This powerful machine was in large measure the lifework of one man, Victor Cousin, about whom a historian has recently written, “So indelibly did that pedagogical experiment bear the stamp of one Victor Cousin, philosopher and educational administrator, and of the innovations of the early 1830s by which he raised philosophy from its 'hitherto rather humble role in our system of education,' [as Emile Durkheim later put it] that Cousin's predecessors had been all but obliterated from the collective memory.” Cousin instituted a composite philosophy in the early 1830s for a country that was on the rebound from a massive social and political revolution in 1789, followed by a Napoleonic empire, and then fifteen years of uncertain monarchy. Cousin's philosophy allowed room for an important role to be played by sensation and experience—sensation and experience were often emphasized in philosophies that were sympathetic to social and political revolution—but also for reason, will, and introspection, three faculties that tended to feel more comfortable in the eyes of those who were not so revolutionary.

This “eclectic philosophy” (as it called itself) was the state-sanctioned philosophy, and thus nearly the official philosophy of the nation. This eclectic philosophy was also devoted to

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the cultivation both of “spiritualism” (what we would prefer to call today “mentalism”) and of science in the study of the mind. This final side would naturally lead to the presence of an important component of scientific psychology in the French curriculum even at a time when psychology in Germany had barely begun to emerge.

4.3.1 Theodule Ribot

In some respects, Theodule Ribot was the closest thing that France came to producing a Wundt of its own, and he is often considered the father of psychology in France. Ribot was trained as a philosopher, and he taught philosophy at the lycée level for a number of years before moving up to the higher academic world in Paris. He not only followed closely what was going on in the developments in psychology in Great Britain and in Germany, he published translations and overviews of that work into French for his fellow francophones to read.

Ribot published an influential book in 1870 on English psychology in which he joined his strong criticism of French spiritual eclecticism with a defense of the possibility of a scientific approach to psychology, which meant a defense against the criticisms offered by Auguste Comte against that very position. Ribot defended the view of *psycho-physiological parallelism*, by which “every psychichal state is invariably associated with a nervous state,” which

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1 See Nicolas and Murray, 1999.
3 Ribot, 1870, *La psychologie anglaise contemporaine.*
not only allowed, but obliged Ribot to study both external and physiological regularities and introspective reports.

He founded the *Revue philosophique de France et de l’étranger* in 1876, which served as an effective method of bringing work from around the European continent to the attention of the French intelligentsia, and for bringing his work, and that of other French psychologists such as Jean-Martin Charcot, to a wider audience.

A year later, when he was 37, he published an article in the inaugural issue of the British journal of philosophy *Mind*, a journal which was devoted to exploring the scientific status of psychology. This article provides a perfect statement of how a generation feels when it looks back at the previous generation with disdain. Ribot’s generation was the one with the new understanding of what makes something *science*, and the preceding generation was Cousin’s, one that did not understand science, and did not understand that psychology did not want to stand on a foundation of principles offered by speculative philosophy: psychology wanted to make claims about observations, and let itself be challenged by the results of empirical tests:

Undisputed master of a legion of disciples, Cousin watched over and strictly maintained a philosophical orthodoxy...It was a doctrine without originality, and standing absolutely aloof from the discoveries of science. Its fundamental principle was this: In philosophy, everything has been said; the age of systems is past; all we have to do is to question history, to take what is true out of each system, and from all these elements to form a *perenniss philosophia*. Without letting himself be stopped by the fundamental objection that, in order to choose, a criterion must first be determined, Victor Cousin fixed on Spiritualism, which seemed to him more congenial than any other doctrine to the political opinions and religious beliefs of the period and to the French mind. He leaned, above all, on Descartes, that he might give a patriotic and national character to his philosophy. The foundation was to be psychology, disclosing everything to man by mere reflection—his nature, the laws of his mind, morals, aesthetics, the nature and attributes of God... The psychology of Eclecticism was, however, very superficial; it was only a literary expansion of the truths of common sense; the few facts to be met with in it were borrowed from the Scotch... the result [of his eclecticism] might be shortly described as ‘Christianity without miracles.’

If that was not bad enough, there was worse to come.

Eclecticism had always a single criterion—common sense, a single aim—to maintain itself in power by a succession of skilful manoeuvres, especially in regard to the clergy... Obedient to one impulse, and participating in the force which, thanks to centralisation the State possesses in France, the professional body was a real power, and formed a kind of lay clergy. Outside, there were but two classes dissenting from it: the

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1Ribot 1879 p. 6, cited by Guillin 2004.
Catholics...and the socialist, communist, and humanitarian schools, who were never weary of denouncing the bizarre invention of a State-philosophy.

It was a moment that called for the familiar metaphor of sweeping the stables clean: as two historians of the field have written recently regarding Ribot’s task at this moment, “Throughout academia, there was a need to sweep away metaphysical speculations and verbose explanations.”

We discussed the importance of the Ecole Pratique des Hautes Etudes (EPHE) earlier in our discussion of Saussure’s appointment there in 1881 (see page [121] above). A chair was created there at the very same time in the history of psychological theory, and while Ribot was interested in the position, it was not offered to him. He did have the opportunity to teach some classes there beginning in 1885, and three years later, Ribot was offered a chair at the prestigious Collège de France. Ribot himself was not really an experimentalist at heart; in this respect he was much like William James. But there was an expectation that he would develop a laboratory, and so Ribot became engaged in an effort to establish a laboratory at the EPHE, and the government agreed to this in 1889. As we will see below, Henry Beaunis was appointed director of this experimental laboratory.

Ribot’s career was deeply rooted in the clinical treatment of patients, and in the early development of what came to be known as psychopathology. He noted that in cases of amnesia, there was a strong tendency for recent memories to be lost first, and earlier memories to be lost only later, if at all. He called this the Law of Regression, but the world has come to know it as Ribot’s Law. We will see an echo of this in Chapter 9 when Roman Jakobson came to study aphasia some sixty years later; Jakobson’s observation is better known in linguistics than Ribot’s original statement is. Ribot’s professional linkage between medicine and psychology was typical of the situation for psychologists in France at this time, and it was in medical schools that research laboratories developed in France for psychological research, and of course the need for clinical aid for patients led to an orientation towards questions of psychopathology.

### 4.3.2 Alfred Binet

Alfred Binet (1857-1911) is best remembered today for developing the first practically useful examination for measuring intelligence, and his name is still found in what we now call the Stanford-Binet examination. He worked in a range of areas of psychology during the period of psychology’s efflorescence, but the impact of his work was less than it would have been if psychology had developed in France in the context of a university structure as Wundt and his colleagues had in Germany at the same time.

Binet did not have traditional academic training, either in philosophy or in psychology. In the late 1870s, he studied psychology and philosophy in books and journals, and was very much taken with John Stuart Mill’s point of view. Théodule Ribot encouraged him to write papers, and he published them in his Revue Philosophique. He worked for seven with the

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1Nicolas and Murray, 1995, p. 281.

2Ribot T., Les maladies de la mémoire (1881), Les maladies de la volonté (1883)

3Wolf, 1973
world-renowned experimentalist and pathologist Jean-Martin Charcot at the major hospital in Paris called Salpêtrière, but this period ended very badly for Binet. One of the projects that Binet worked on involved hypnosis (Charcot’s specialty), hysteria, and magnetism (the sort created by magnets). Over a period of years, Binet (along with Charcot and another, younger, colleague, Charles Fére) was involved with claims about the extraordinary effects that they could produce on hypnotized subjects with their magnets. Other laboratories in France were unable to reproduce the results, but it was a researcher from Liège, in Belgium, who with a deft pen proceeded to argue that the results from the Charcot lab were almost certainly the result of suggestions made by the experimenters in the presence of the hypnotized subjects, or else were the result of the experimenter being trained by the subject. After several years of published barbs in the journals, Binet was obliged to admit that their results were largely the result of suggestion, and he left Charcot’s laboratory with a renewed sense of how a person could be led to believe things by virtue of wanting to believe them.

We noted above that Théodule Ribot succeeded in establishing a psychology laboratory in 1879 as part of the Ecole pratique des Hautes Etudes, with Henri Beaunis as its the first director. Beaunis asked Binet to come on board with the project, making him associate director in 1892, and when Beaunis retired in 1894, Binet became director. The laboratory was never a great success in the way that Wundt’s was in Leipzig. Looking back, a number of suggestions have been made for this, ranging from a domineering side of Binet’s personality—but when was that a flaw in a laboratory director?—to the fact that there was no official degree that a student could obtain after spending time in his laboratory, something that they could take home and use if they were not French. Worse yet if they were French: there were hardly any positions to be had teaching psychology.

Binet remained director of this laboratory for the rest of his career, and he was not able to obtain a chair as a university professor. He was a close friend of Paul Passy, an important linguist who was awarded a new chair in phonetics at the Ecole Pratique des Hautes Etudes (EPHE) in 1894, and we have a letter that Binet wrote to Passy in 1901, when Ribot had resigned his position at the Collège de France. Binet wrote, “You know perhaps that Ribot has just resigned, and that I am presenting myself against [Pierre Janet] to replace him. It will be a rough campaign, in which I am happily supported in the most vigorous manner, and if I lose, it will not be my fault... It is over twenty years that I have been active in psychology, as you know; I educated myself all alone, without any teacher [maître]; and I have arrived at my present scientific situation by the sole force of my fists; no one, you understand well, no one has ever helped me. I have done experimental psychology—the title of Ribot’s chair—and I am really the only one in France who has done so.” But Janet was given the chair. When Binet applied for Janet’s previous position, it was given to George Dumas. Both Janet and Dumas had been professionally close to Charcot in a way that Binet had not been; Janet had been Charcot’s “substitute professor” for a number of years at the Collège de France.

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1If the experimenter latches onto a behavior that he observes in the subject on the first experiment, and then continues to use that behavior as an indicator with some significance associated with it, then who has trained who? The subject has trained the experimenter, with neither of them aware of it.


Binet remained committed to the research we remember him for, the measure of intelligence. Early in his work, he tried to use the ideas of Paul Broca on phrenology, which offered the hope (if that is the right word) that a person’s mental traits could be measured off of their skull. Eventually he convinced himself that this did not work, and he went on to develop other sorts of tests. He had two daughters, and he learned a great deal by observing them in a naturalistic context, as Jean Piaget would do years later, and as Gestalt psychologists would do as well.

Working together with Théodore Simon, a colleague from his days at Charcot’s lab, Binet created tests for children that would allow a measurement of intelligence in an experimental context. As education passed from the Catholic church to the French government, the usefulness of these tests became clear. Binet studied the nature of the learning deficits of the mentally handicapped, and in 1905 at the World Congress of Psychology, he presented, with his colleague Simon, a set of psychometric tests and a scale to mark the mental stage of a child as indicated by these tests. This was perhaps the high water mark of the development of psychometric psychology, and Binet would work on this Binet-Simon scale for the rest of his life. Binet and Simon were clear on the point that they were not measuring the intellectual age, but rather the intellectual level, but this nicety was often lost, and just a few years later, the notion of Intellectual Quotient (IQ) was proposed (by Wilhelm Stern), and we all know how widely that notion has been adopted.

Stephen Jay Gould has recently dramatically brought out the uses to which intelligence measurements have been put, and he recognized Binet’s realistic qualms. He wrote,

Binet also had a social motive for his reticence. He greatly feared that his practical device, if reified as an entity, could be perverted and used as an indelible label, rather than as a guide for identifying children who needed help.

The importation of Binet’s tests to the United States would also open a veritable industry of testing, one which was of great use to educators, to employers, and to the military in a time of vast conscriptions during the world wars. As Gould documents, Binet’s tests were of use to what he called “pioneers of hereditarianism”, but that was “a home-grown American product,” not a part of the picture that lay behind Binet’s life work.

4.3.3 Paul Broca

We have seen certain parallels between the development of physical anthropology in the 19th century and the classification of languages and their morphological systems, which turn led to a hierarchical view of linguistic systems. This is not the only important influence that physical anthropology would have on the history of linguistics. In France, one of the important proponents of physical anthropology was Paul Broca, who we have already encountered in connection with the cephalic index. Broca was a physiologist, and an anthropologist, and also a medical doctor, and he was naturally very influenced by the positivist, empiricist, and scientific atmosphere of the period—he was what was often called at the
time a “free thinker,” and he opposed conservative Catholic tendencies. He founded the Society of Freethinkers in 1848, and was denounced as a dangerous revolutionary agitator! Broca began his career as a physiologist working on hybridization in the animal kingdom—thus close in some ways to Darwin, and in other ways opposed to him. He was banished by the conservative Biology Society, and founded in 1859 the Société d'Anthropologie de Paris, which brought together progressives and atheists opposed to the Ethnological Society of Paris, which was itself under the control of conservative Catholics—and which in 1863 saw a subgroup split off to become an organization we have already encountered: the Société Linguistique de Paris. The Société Linguistique de Paris progressively moved away from its founding conservatism, and eventually became more generally the home for all French linguists.

As an anthropologist and doctor, Broca was not interested in typologies or hierarchical arrangements of languages of the Schleicherian sort. As a craniologist, he was interested in language as a species-specific characteristic of the human mind. He was associated as a doctor to the Bicêtre hospital in Paris, where there was a patient, a certain Monsieur Leborgne, who had been there for twenty years. Leborgne’s right leg was paralyzed, and while he understood what was said to him, he was able to utter only the syllable “tan”—which earned for him the nickname Tantan. He died on April 17, 1861, and when Broca removed his brain (which is still preserved in Paris), he discovered a cerebral lesion on the third left frontal convolution.

Broca drew the conclusion that the damage to this zone, which is now known as Broca’s area, was responsible for Tantan’s aphasia, and six months later, he presented a paper to the Société d’Anthropologie de Paris on his discovery. He presented the material again before the Société d’Anatomie de Paris, and this unleashed an enormous polemical discussion. The notion that cognitive functions were localized in the brain now had strong scientific support.

We discussed the impact of Gall’s theory of phrenology earlier, in connection with Auguste Comte’s general positive theory of mind and society. Gall’s perspective represented one extreme end of a controversy that continues to the present time—with the goal of analyzing the powers of the mind into separate and autonomous faculties, a view that fit well with the spirit of 19th century analysis, and which would appeal to Paul Broca in his effort to understand the effects of lesions on the brain.

The polemics between the two views was fierce, and reach its apogee at the International Congress of Medicine in London (1881), where the German anatomist Friedrich Goltz presented surgical experiments he had performed on animals in support of his holistic and integrated view of the functioning of the brain, and where the Scotsman David Ferrier responded by presenting other surgical experiments supporting a strong localist position. Ferrier’s position carried the day, and localism became the standard view for a good while after that.

But counterbalancing the importance of the case of “Tan-tan” for Broca’s localism was the case of Phineas Gage. Gage was a foreman working on the construction of railroads, and he was injured in September of 1848 on a worksite in Vermont. An iron bar went entirely through his left frontal lobe, without significant damage to his cognitive or motor abilities—
though he lost an eye. Yet curiously, his personality underwent significant changes. His case was important one for the antilocalists, both for those espousing a totally holistic view and those advancing a thoroughly associationist view.

In 1874, Carl Wernicke presented the case of a patient with damage to another area in the brain, one which would be known in the future as “Wernicke’s area.” This kind of damage is associated with aphasias of comprehension and reception, and the discovery appeared to support the localist position, since a specific new area had been established. But the defenders of the holistic position showed the importance of the arcuate fasciculus, a bundle of axons that connects Broca’s area and Wernicke’s area, playing an important role in linguistic functioning. Wernicke himself did not defend the localist position, arguing rather for an associationism involving a number of areas involved in the functioning of language, both in production and in reception.

The debate over localism, associationism, and holism would continue over the years to come, and we will see it next in the controversies surrounding Karl Lashley’s holist view during the heyday of behaviorism.

4.4 The unity of mankind—and the differentiation of types of humans

Here is a question that 19th century people asked themselves: What makes us different?

We noted earlier that the turn towards India in 19th century linguistic thinking, along with its consequent fascination with things Aryan, was an impulse that permitted an alternative conception of how the human beings across the globe were related to one another, a historical alternative that was most notably different from the conception offered by the Bible. The 19th century was a century in which the differentiation of mankind was explored and accounted for in a range of ways, often rejecting both traditional Western religion and the Enlightenment tradition that all men are one. Once again, the interest of this development for us is not antiquarian, but rather flows from the fact that we are still engaged today in trying to come to grips with conflicting systems of beliefs in this area. Whether it is in the context of a casual conversation, or a political speech, or an academic debate, people turn to the question, Who are we? And what makes us different?

In linguistics, one major school of thought today, the one associated with Noam Chomsky, sees the broadest horizon of explanation as deriving from Universal Grammar, a faculty of language shared by all humans, and this is an image that would feel quite comfortable for an 18th century scholar, someone who was part of the Enlightenment, if he were to wake up today. Whether or not one adopts that particular ahistoricism—and many today, especially in the social sciences that are distant from linguistics, do not—it is impossible for a science of humankind to avoid the question: what makes us different from one another? It is impossible to avoid that question because people will not stop asking the question even if a prevailing theoretical framework has little to say about it; certainly different theoretical frameworks are equipped to different degrees to give meaningful answers to the basic questions: in what ways are we all alike, and what makes us different?
The great comparative linguistics of the 19th century that we have reviewed in an earlier chapter very naturally led to a sense that two peoples who have languages that were once one and the same must share a good deal in common, culturally and (if we accept the notion) spiritually. But that can leave the nagging feeling that peoples whose languages share no recognizable roots in common with ours are different.

In linguistics today, the question of who we are is largely understood with the interpretation that we means the entire human race, and the other, the one who does not have language, is constituted by the now extinct lines of descent from the original genus Homo who emerged on the scene two million years ago, whose descendents are no longer alive, and who probably died off because they did not have language as we know it.

Of course, any thoughtful scientist must also recognize that over time, the tacit understanding changes too of just who counts as us, when we ask about what makes us different. And when we ask what makes us different, there is always a tacit phrase in there: who is it that we are different from?

Social forces of all sorts were responsible for confrontations that led people to think about us and them in the 19th century. Slavery was rapidly abolished—in 1830 in England, in 1840 in France, and in the mid 1860s in the United States. But colonial empires continued to thrive and grow, and the growth of nation-states only made more stark the confrontations of differences and inequalities that were found in cities. Everywhere the question was asked: is this man my equal, this man whose skin color is different, whose language, culture, religion, and everyday mode of existence are so dissimilar from my own?

The fact is that over the 19th century we observe a confrontation between the universalism of the Enlightenment and its opposite, a sort of differentialism, which ranged all the way from an interest in how individual characteristics varied within social groups, to an interest in how they varied across groups, to an instrument that could be used in support of a political or social view that disenfranchises those whose skin color is different, or whose great grandparents were Jewish. As we will see over the next chapters, linguists have long weighed in on these issues, and usually—not always, but usually—in ways that seem judicious to us today.

Anyone who spends a lifetime studying cultures and societies that are not their own is passionately interested in the differences that they encounter, but the differentialism that this natural enthusiasm engenders can cross the spectrum from value-free typologies and rich ethnographies to the kind of heavy-handed judgmentalism that surfaces as speculation regarding the ways in which a society’s language prevents its speakers from thinking abstract thoughts. Linguistics took major steps away from providing support for that latter way of conceiving of differences among cultures with the changes associated with the third generation of linguists, in the 1870s, as we noted in Chapter 2. But we have been struck by the ways in which valid and responsible studies of societies and cultures can be recruited in the service of more sordid ends, as has been described by such scientists as Stephen Jay Gould in the United States, and Jean-Paul Demoule more recently in France.  

The strong inclination to measure and to classify during the 19th century also left its mark on the development of sociology and anthropology, two fields that are very close to the mind sciences we have focused on in this book. Anthropology arose as a science that took as its aim the development of a classification and an understanding of the physical and cultural characteristics of human groups, and their development over time, with attention to physical, cultural, and social characteristics. James George Frazer published in 1890 a monumental book, *The Golden Bough: A Study in Magic and Religion*, a book which would stimulate further interest in the study of myths, rituals, and religions. Anthropologists such as Bronislaw Malinowski and Rudolf Virchow would have tremendous influence on linguists who followed them, such as J. R. Firth in England, greatly influenced by Malinowski, and Franz Boas in the United States, great influenced by Virchow; Boas will be an important figure in Chapter 6 below.

Anthropologists and biologists have not infrequently felt called upon to stand in judgment on the number and nature of the races of mankind. Charles Darwin expressed his dissatisfaction with the question, never mind the answer offered to it:

1 The question whether mankind consists of one or several species has of late years been much agitated by anthropologists, who are divided into two schools of monogenists and polygenists. Those who do not admit the principle of evolution, must look at species either as separate creations or as in some manner distinct entities; and they must decide what forms to rank as species by the analogy of other organic beings which are commonly thus received. But it is a hopeless endeavour to decide this point on sound grounds, until some definition of the term “species” is generally accepted; and the definition must not include an element which cannot possibly be ascertained, such as an act of creation. We might as well attempt without any definition to decide whether a certain number of houses should be called a village, or town, or city. . . . Those naturalists, on the other hand, who admit the principle of evolution . . . will feel no doubt that all the races of man are descended from a single primitive stock; whether or not they think fit to designate them as distinct species, for the sake of expressing their amount of difference.

Darwin was skeptical whether the speciation that he observed, and applied in his account of the gradual unfolding of the tree of life over the course of countless generations, could be meaningfully applied to understanding the humans who are alive today:

1 Charles Darwin 1871 The descent of man vol 1 P 228
other, and that it is hardly possible to discover clear distinctive characters between them.

4.5 The era of machines

A historian such as Eric Hobsbawm reminds us that at the heart of the 19th century are found revolution and great growth of capital, both of which played out on a stage that included the rapid growth of cities and the workers who moved to those cities looking for work. The metaphorical engine that drove this industrial revolution was, quite literally, the physical engine. Civilization had known machines for two thousand years, and had effectively used wheels, pulleys, and screws for all that time, making the strains of humans and animals more effective. Leonardo da Vinci dazzles us even today with the mechanical devices his imagination created. But now in the 19th century, new sources of energy were being tapped for making bigger, faster, and more powerful devices. There were steam engines, and then there were internal combustion engines, and while they changed the face of the world, they are not the ones that we wish to focus on here. They were very important—but the ones we want to focus on are the machines that began to display intelligence, just a little bit of intelligence.

It was two hundred years earlier, and more, that rapid changes had begun in how the world was seen by thinking people. The scientific revolution moved into high gear when at the end of the 16th century Galileo challenged the way that space, time, and motion were conceived. Gradually the view took hold that things in the world interacted mechanically, by bumping into each other in various ways. Look closely, these early scientists said, and you will see that things move until they are stopped by something else; objects interact and then move on. The gears that make up a clock provide a clean example of this: watches work because all of the gears interact locally and immediately with each other, mostly two at a time. Strings and chains can be attached to gears and then pull other things: we knew that water mills worked that way, and maybe human muscles did also. All these objects interacted with each other in mechanical ways, which means based on local interactions of contact between the two objects. And objects that can be understood and explained in such terms and nothing more were mechanical objects, or “machines.”

It is not difficult for the reader today to understand what René Descartes was getting at when he said that man’s body was very much like a machine, nor when de la Mettrie famous declared that man was a machine. But what is significantly more difficult for the reader today is to recognize that we are still engaged in figuring out what we mean by machines and what aspects of the universe (include us ourselves) naturally fall under that label of being a “machine.” As our understanding of the physical world has expanded and matured, we are able to account for more aspects of the universe by means of the intellectual tools that evolved out of Descartes’s and Newton’s mathematics, and we are all inclined to accept the view that a process is mechanical if it can be thoroughly accounted

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We oversimplify if we limit the scientific revolution to a view of the world as a mechanism. While the view of the world as machine was vital, another view was every bit as important, which was the notion that mathematics could explain the world, an idea that in the western world has come down to us from Plato and Pythagoras. We have become so accustomed today to viewing mechanism and local collisions, on the one hand, and on the other, mathematical formulas that predict trajectories and gravitational forces that it is not at all obvious how far apart these two traditions are.
for by such mathematical methods. But the science, the mathematics, and the logic that underlies what we mean when we declare a process to be mechanical has radically shifted over the past few centuries, and so has the engineering capacities of modern science to build tools to our own design. What counts, then, as a machine and as a mechanical process has evolved and changed, and the changes in how we view mind and language that we study in this book are part and parcel of those changes.

For Descartes, an animal was a self-moving machine—and an observer from a distant planet might be excused for not seeing, at first blush, that man was nothing but a self-moving machine too. The most advanced machines of the age were the exemplars of machines which thinkers used to express their opinions about the human condition. If there could be an intelligent machine, it would have to be something like a clock, a watch, a chronometer, built from springs, gears, chains and some internal source of energy. Unlike water clocks and sundials, clocks with gears kept their internal structure hidden from all but the initiated; this kind of mechanism, a mechanical structure built by humans, lent itself naturally to a perspective in which our universe was the handiwork of a divine watchmaker who built clocks whose face was visible to us but whose inner workings were at least for now hidden.

The middle of the 17th century was the moment when new ideas in this area exploded; it was the time of Descartes, of Newton, and of Blaise Pascal, all three philosophers and imaginative mathematicians. Pascal was particularly interested in building a machine to carry out arithmetic operations in a way that would be practically useful for people who needed to carry out many arithmetic operations quickly and accurately. The machine was later called Pascaline, in his honor, and its existence spurred on others to set the bar higher and higher. The philosopher and mathematician Gottfried Leibniz saw Pascal’s machine, and decided to go one better, and build a machine that would calculate multiplications as well. Over the course of the next century, watchmakers and inventors built successively more complex and impressive machines that imitated human intelligence and behavior. In retrospect, we can identify an important thread in this development in which the pattern of successive acts that the machine needed to perform could be built into a simple device whose sole function was retaining the pattern, just like we see today when we open up a music box and watch a cylinder go round as the box plays a familiar tune, which has been transferred to a set of bumps on a rotating cylinder. The same technology was applied to the task of weaving inside of a loom, and Joseph-Marie Jacquard, later in the 18th century, went so far as to create a system in which punched cards were used to control the pattern of weaving, with each card used to define the pattern of one single row. By the middle of the 20th century and well into the 1960s, this idea was used as a standard method to enter data into a digital computer. We used to call them “punch cards.”

It was the Englishman Charles Babbage who deepened the theoretical reflections on how machines could in real and practical terms revolutionize how mathematical tables and formulas could be calculated far better and faster than mere humans, and it was Ada Lovelace, a talented mathematician and child of the poet Lord Byron, who was able to clearly see the mathematical character of what Babbage’s machine could eventually accomplish.

Someone today wishing to get a sense of how Babbage and his contemporaries viewed this machine has no choice but to go back to an article published in 1842, in Switzerland and in
French. It was written by an Italian engineer from Turin, Count Federico Luigi Menabrea, who had attended a lecture that Babbage had given two years earlier. Menabrea's paper was in turn translated by Ada Lovelace into English, and she and Babbage added copious notes to Menabrea's original. How much of the addition was due to Lovelace and how much to Babbage remains a point of contention.

Let us look at the beginning of this paper, because it gives such a clear sketch of the ideas in the background:

Those labours which belong to the various branches of the mathematical sciences, although on first consideration they seem to be the exclusive province of intellect, may, nevertheless, be divided into two distinct sections; one of which may be called the mechanical, because it is subjected to precise and invariable laws, that are capable of being expressed by means of the operations of matter [physiquement]; while the other, demanding the intervention of reasoning, belongs more specially to the domain of the understanding[ pensée]. This admitted, we may propose to execute, by means of machinery, the mechanical branch of these labours, reserving for pure intellect that which depends on the reasoning faculties [singular, not plural, in the original]. Thus the rigid exactness [rigor, in the original] of those laws which regulate numerical calculations must frequently have suggested the employment of material instruments, either for executing the whole of such calculations or for abridging them; and thence have arisen several inventions having this object in view, but which have in general but partially attained it.

This clear division of mathematical thought into a part that could called “mechanical” and a part that could not would become a more and more important step, and will constitute the focus of our attention in Chapter 8 below.

There is much that is strikingly modern in the discussion of the way in which the Analytic Engine is designed in order to be as general as possible. Lovelace explained the way in which values of numbers could be held in a large number of distinct places in the machine, but when they were to be used in order to calculate a new value, they would be copied in the central calculating unit, which Babbage called the mill. The cards on which the sequence of intended operations have been punched are called “operation cards,” and “the operation-cards merely determine the succession of operations in a general manner. They in fact throw all that portion of the mechanism included in the mill into a series of different states, which we may call the adding state, or the multiplying state, etc. respectively.” This is precisely what we say today, when talking about a finite-state device, or a Turing machine: it enters into a state, which consists precisely of the readiness to perform an operation.

1Menabrea, 1842.

2Menabrea wrote years later (1884) that “this translation was accompanied by notes of the greatest interest, which develop most clearly what I was only able to express in an incomplete manner.” Menabrea, 1884 Juillet-Décembre 1884, Paris, Gautier-Villars, p 181.

The article and Lovelace’s notes were published by Richard Taylor in his collection of Scientific Memoirs, volume 3, 1843. (Richard Taylor Scientific memoirs, selected from the transactions of foreign academies of science, and from foreign journals. 1843 ). Most of these materials are available online [http://www.fourmilab.ch/babbage/sketch.html](http://www.fourmilab.ch/babbage/sketch.html) but without the author indicated, no doubt because it was a woman, and it would not be until 50 years later, and following Menabrea’s later remark, that the work was attributed to Ada Lovelace, a fact that Babbage had never concealed from Menabrea.
(addition, multiplication, etc., according to which state it is on) on variables that exist in
the mill—or as we say today, in a register. “In each of these states the mechanism is ready
to act in the way peculiar to that state, on any pair of numbers which may be permitted to
come within its sphere of action.”

Lovelace was just as clear in explaining the importance of conceptualizing the mathematical
operation as a set of recurring operations forming a cycle (we would say a “loop” today), a
relation that can be extended to cycles of cycles, and so on:

Wherever a general term exists, there will be a recurring group of operations, as in
the above example. Both for brevity and for distinctness, a recurring group is called a
cycle. A cycle of operations, then, must be understood to signify any set of operations
which is repeated more than once. It is equally a cycle, whether it be repeated twice
only, or an indefinite number of times; for it is the fact of a repetition occurring at all
that constitutes it such. In many cases of analysis there is a recurring group of one or
more cycles; that is, a cycle of a cycle, or a cycle of cycles.

At the same time she emphasized the fact that the design of a set of operations for the
Engine made clear the abstract character of each operation as a higher order entity:

In studying the action of the Analytical Engine, we find that the peculiar and inde-
dependent nature of the considerations which in all mathematical analysis belong to
operations, as distinguished from the objects operated upon and from the results of
the operations performed upon those objects, is very strikingly defined and separated.

But the science of operations, as derived from mathematics more especially, is a science
of itself, and has its own abstract truth and value; just as logic has its own peculiar
truth and value, independently of the subjects to which we may apply its reasonings
and processes. Those who are accustomed to some of the more modern views of the
above subject, will know that a few fundamental relations being true, certain other
combinations of relations must of necessity follow; combinations unlimited in variety
and extent if the deductions from the primary relations be carried on far enough. They
will also be aware that one main reason why the separate nature of the science of
operations has been little felt, and in general little dwelt on, is the shifting meaning of
many of the symbols used in mathematical notation.

Lovelace left no doubt that Babbage’s Engine was a machine that reasons—that embodies
reason—and that this machine, once it was made real out of disks and gears of metal, would
change the way we understand thought and reasoning:
In enabling mechanism to combine together general symbols in successions of unlimited variety and extent, a uniting link is established between the operations of matter and the abstract mental processes of the most abstract branch of mathematical science. A new, a vast, and a powerful language is developed for the future use of analysis, in which to wield its truths so that these may become of more speedy and accurate practical application for the purposes of mankind than the means hitherto in our possession have rendered possible. Thus not only the mental and the material, but the theoretical and the practical in the mathematical world, are brought into more intimate and effective connexion with each other. We are not aware of its being on record that anything partaking in the nature of what is so well designated the Analytical Engine has been hitherto proposed, or even thought of, as a practical possibility, any more than the idea of a thinking or of a reasoning machine.

In the end, Babbage was not able to build his Analytical Engine during his lifetime. We would have to wait for the skills of the engineer to grow and for the pursestrings of governments to open, and it was not Babbage's machine that was first built, but those of others working in the 1930s and 1940s. But Babbage's and Lovelace's ideas fired the imaginations of generations to come. And today's computers are a direct result of their ideas in the first half of the 19th century.

We will return to this story in Chapter 8, when we discuss the origins of the Turing machine (and its close relatives) in the 1930s; see page 421 below.

4.6 Moving on

The last three chapters have had as their goal a preparation for engaging with the development of the mind sciences in the 20th century. We have looked just a bit at the political and social changes going on in Europe and the United States during the 19th century, but we have seen in quite some detail the ways in which thinkers of all sorts began to create new sciences: sciences of humankind, sciences of the mind, sciences of language. No one knew for sure the right way to create a new science: no one ever does. Some thought that doing science meant finding new questions to ask, and that decision often went along with dropping some useless old questions. Others thought that embracing science meant the development of new methods, and those methods frequently embraced measurements and other sorts of quantitative innovations. Some thought, perhaps darkly, that science was at its best when it could be pursued for its own sake, without any practical end in sight, while others were caught up in intellectual movements that loudly proclaimed their relevance for both the present and for the future.

In the next five chapters, we will do our best to pull apart the major trends in psychology, linguistics, philosophy, and logic, with a focus on those themes that were shared among these disciplines, themes that passed back and forth across, or beneath, the disciplinary fences. The anxiety over the nature of science, we will see, remained a major preoccupation throughout this period.
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