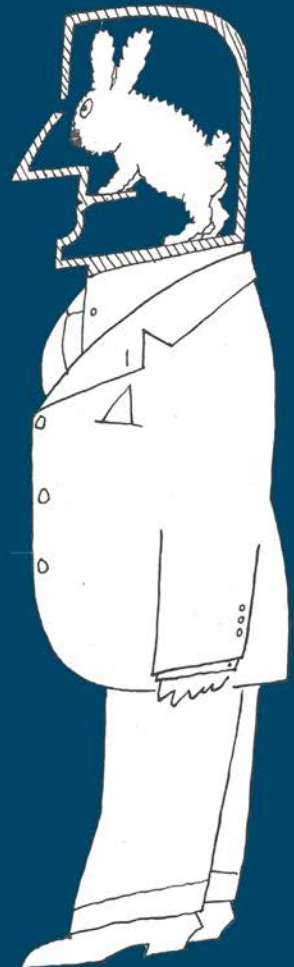


Manfred Velden

Psychology - a Study of a Masquerade



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Preface

The term *psychology*, as meant in the title of this book, refers to that part of psychology which claims to proceed along natural scientific lines and which alleges to produce “fundamentals” which can be successfully applied, quite like the fundamentals an engineer has learned in the school of engineering. Much of what is taught about psychology at university is of that kind. It must be stressed that the term does not refer to the actual work done by psychologists in diverse fields of application, most of which is rightfully respected by a wide public. The relationship between those alleged fundamentals and what is actually happening in applied fields is treated in the chapter “Applied vs “fundamental” psychology”.

The book wants to evidence that psychology’s attempt to emulate the natural sciences by using natural scientific methods and procedures and thus trying to *be* a natural science is actually an ill-fated one. In this attempt, psychology has made ample use of one of the most powerful instruments of the natural sciences, mathematics. As will be seen, psychology has made a rather specific kind of use of mathematics. In order to characterize this kind of use, actually a rather garbled one, it has been inevitable to go into some concrete applications of mathematics. Rather than to describe them in detail it would have sufficed for some of them to just point to the fact that the application does not make sense in the context, for example if the preconditions for the application are not satisfied. But I still do present the mathematics in order to demonstrate the amazing psychological effect arising from the use of mathematics all by itself, i. e. regardless of the adequacy of its use in the given context. The mere presence of mathematics in a line of reasoning makes that reasoning appear more convincing, more scientific, more exact by mere association. For that effect to be felt the reader need not *understand* the mathematics. It is enough to just look at it to be impressed. This may sound paradoxical, but it is exactly what happens. Much of psychology’s success in presenting itself as a real science is based on that effect.

There are two other instances, however, where it is essential to understand the mathematics, because that understanding alone allows us to see why its appli-

cation makes no sense or is even misleading in the particular context. These instances are the “null hypothesis test”, usually termed the “significance test” universally applied in psychology, and the heritability coefficient, which in psychology supposedly tells us to what degree mental traits are genetically determined. For both the significance test and the heritability coefficient, all that is necessary *mathematically* to understand is the statistical concept of variance, which for its part just requires knowledge of elementary mathematics. If there is anything demanding in trying to understand what’s wrong with the significance test and the heritability coefficient in psychology, it is not the mathematics, but the grasp of the *context* in which they are applied. That context, on the basis of which alone the adequacy of applying mathematics in science can be judged, will be supplied in detail.

Introduction

Seeing the title of this book, few readers will recall the imposing six-volume *Psychology: A Study of a Science*, edited by Sigmund Koch and published between 1959 and 1963. With this series, to which about every influential psychologist of the time contributed, Koch intended to document the scientific state of psychology. But while doing the editing he already developed grave doubts about that state, indeed, even about whether psychology was a science in the first place. Thus he had originally planned to write a 7th volume of *Psychology: A Study of a Science*, in which he would comment on the status of psychology as a science. In the process of editing the earlier volumes his attitude as to that status changed fundamentally. As he put it: "... between 1942 ... and the early 1950s, the scales fell gradually from my eyes" (Koch, 1999, p. 7). Koch did not finish that 7th volume, but a collection of essays, written from the 1960s to the 1990s, was published posthumously¹ in 1999. It may be seen as equivalent to volume 7, a "surrogate volume 7" as he put it, containing a "critique of the modern psychological enterprise" (Koch, 1999, p. 1). With the scales fallen from his eyes, Koch had come to a view of psychology as a science that differed totally from the one held by most of his contemporaries and even by himself for a long time. In his critique he did not only flatly deny psychology the status of a systematic science, but also deemed the results produced by psychology to be rather worthless, both theoretically and practically. Of psychology as a science he wrote: "If psychology is a science, it is a "science" of a strange kind" (p. 128). Koch saw psychology as a "discipline of deceit", "as the institutionalization of a delusion" (p. 6), an "imitation science" that effected "... a progressive obfuscation of what man already knows about his own condition" (p. 304) and which has been "flagrantly and vulgarly oversold" (p. 307). Clearly Koch could well have given the title *Psychology: A Study of a Masquerade* to his surrogate volume 7.

We must therefore face the irritating fact that one of the most distinguished

¹ Koch died in 1996.

psychologists of his time², after decades of active research in his field, started thinking radically about it and came to the conclusion that the bulk of its results was useless. And we must accept the equally irritating fact (once you think about it) that the large majority of psychological scientists today do not know about that fact in the first place.

How can that be? Was Koch eventually proven wrong and thus rightfully forgotten? Quite to the contrary! Koch's critique is as valid today as it ever was. Since the time the scales fell from Koch's eyes, psychology has even intensified its masquerade, partly with old masks, like complicated mathematics and statistics, partly with new ones like brain scans, evolution theory or molecular genetics, impressing a wide public with all that sophisticated methodology and expensive machinery and making believe that the science behind it all is as solid as the tools used seem to suggest.

The question must be asked how such an absurd situation, a large branch of science going awry for about a century and hardly anyone caring or even noticing, could have come about. How can it happen that in *science*, where a rational and critical mindset is supposed to rule, a whole field just *postures* as a science? The scheme is so unbelievable that most readers will tend to doubt the sanity of the (very few) radical critics rather than the scientific status of the field. If the anomaly of a fact borders on the absurd, we simply tend to deny it. In order for that anomaly to still become acknowledged, a good case needs to be made for it, a *really* good one. But if Sigmund Koch with his comprehensive education, profound knowledge and brilliance of mind could not convince his colleagues, let alone a broader public, how dare I try the same again? I don't know. I just feel very strongly that the nonsense produced by psychology as an alleged systematic science must not be allowed to stand. And, as Koch has pointed out, it is not just the scientific nonsense as such that we are dealing with, but also, and more importantly, the loss of human context in psychology. Man's psychological functions viewed as subject to strict laws, the knowledge of which would allow the prediction and control of behavior (goal of psychology according to John Watson's behaviorist program), denies psychology the only sensible approach, namely that of seeking to understand those functions in a *meaningful* psychological context, as is done in the humanities.³ After 100 years the prevailing approach has not produced one single such strict law (or anything deserving the

2 He not only led that monumental enterprise *Psychology: A Study of a Science*, was coeditor of *A Century of Psychology as Science* (1992), but also served as president of three divisions of the American Psychological Association and was director of the Ford Foundation Program in the Humanities and the Arts (1964–1967).

3 Deviations from that approach within the humanities, prompted by biological explanations of behavior in psychology and the preposterous nonsense resulting from it, I have dealt with elsewhere. (Velden, 2012).

term “law” in the first place) but has left us with a heap of results with expiration dates of just a few years after publication. What it feels like to study this kind of science, Koch (1999) describes as follows:

“Our students are asked to read and memorize a literature consisting of an endless set of advertisements for the emptiest concepts, the most inflated theories, the most trivial “findings”, and the most fetishistic yet heuristically self-defeating methods in scholarly history – and all of it conveyed in the dreariest and most turgid prose that ever met the printed page.” (p. 136).

A science institutionalized and respected worldwide being criticized in this devastating fashion by one of its best and most renowned scholars constitutes an absurdity, the causes for which must run deep. In order to see what these causes are, the fundamental problems of psychology as a science must be addressed. These problems have been known for centuries⁴ (if not for more than 2000 years), but they in no way have been solved. The most fundamental of them addresses the question of what psychology is all about.

⁴ They, for example, prompted Galileo Galilei (1564–1642), seen by many as the founder of modern natural science, to exclude psychological processes from the natural world he was investigating, not because he found them somehow supernatural, but due to their subjective nature (Watson, 1979).

Psychology – a field of its own

Psychology is about the soul (psyche), of course, and, in contrast to us today, ancient Greek Mythology knew exactly what that is and created one of its typical, colourful stories about it. Psyche was a woman so beautiful that the god of love, Eros, fell in love with her. After dramatic interludes the story ended happily with Psyche being (re)united with Eros forever and made immortal. Seen from a scientific standpoint the story is not such a happy one because the impersonation of the soul is a form of reification, the transformation of something conceptual, abstract, into something concrete. In science it must be seen as a misleading and logically untenable step which has plagued psychology ever since. But apart from its mythology, Greek antiquity came quite close to today's psychology in the sense that Aristotle wrote about *mental functions* like perception, memory, or learning, subjects still studied in modern psychology. To see the "soul" as composed of diverse mental functions is the most influential idea in the history of psychology. It has undoubtedly contributed to scientific progress but, as we shall see, has a serious downside to it, too.

Mental processes like perception, learning, or thinking are familiar things to us, the terms denoting them being in daily use and known to everyone, such that at first glance there appears to be no problem in making them the objects of study in a science called psychology.

But unlike in daily language, where those terms have an obvious communicative usefulness, they suffer from grave definitional problems as objects of scientific study. There are properties or aspects to them that make psychology quite an exceptional science, "a "science" of a strange kind", as Koch put it (Koch, 1999, p. 128), and by which he meant that it is not really a science in the first place.

The first fundamental problem researchers trying to make a science out of the study of the soul were confronted with was the fact that their subject of study, mental functions, cannot be observed objectively but only subjectively, i. e. by self-observation (introspection). You cannot see what someone else is thinking, just, with some luck, "observe" what you are thinking yourself, and this pro-

cedure is a rather unreliable one, particularly because there is no standard form in which thoughts exist. They will often come about in a verbalized form but in no way need to. The form in which ideas exist before they are transformed into language we do not know, such that, if we experience our own thinking in the form of language, that does not mean that the words are identical to the actual thinking that occurred before verbalization set in. If ideas are *not* transformed into language they may be difficult to recall as there is an infinite variety of possible forms they may have had. With the visual sense generally dominant, the form may often be a visual one, yet need not be. Mathematical problems, for example, may be solved by some kind of visual (geometrical) representation, but also in a rather non-visual, abstract form.⁵ In principle all kinds of mental modes may be involved in our form of thinking, possibly including even a purely abstract one.

In order for the study of mental events to become a science, i. e. for establishing general rules or laws about them, the contents of introspection must be communicated, commonly by the use of language, which implies a second source of uncertainty as the verbalisation (or other form of communication) need not be an exact representation of what has been subjectively experienced. As everyone knows, the communication may furthermore be biased, in the extreme case by the subject lying about his experiences. But even if the communication is one to the best of the subject's knowledge, it may still be biased, namely by unconscious processes, making "response bias" one of the most intriguing problems of psychological measurement.

As if these problems were not enough, there is a further fundamental one that exacerbates the uncertainties in the study of mental processes. As pointed out by William James but consistently ignored in psychological research, the mental functions studied by psychology cannot be classified in the orderly fashion suggested by books about general psychology, where we find functions like perception, thinking, feeling, learning etc. In the actual process of conscious experience such functions are however mere aspects of that experience which, on top, interact with each other in most complicated ways. During any period of time you live through, you perceive things, recall other things on account of the occasion, think about them, have feelings or sensations emanating from what you perceive, recall or think about, and have many things more happen in your mind. On a purely descriptive level when approaching mental functions we must start with the fact of this extremely complicated interaction of the most diverse mental functions, a fact James called the "stream of consciousness" or "stream of

5 Mathematicians use different forms when thinking about identical mathematical problems, which at times makes it difficult to communicate their ideas to each other (see, for example, Penrose, 1989, pp. 548).

thought” (James 1890, chapter IX). In order to still study *single* functions, psychological researchers have tried to create situations in which only discrete mental functions are represented in consciousness, as, for example, in psychophysics, where the occurrence of simple, unidimensional mental processes, like the sensation of loudness of a tone, appeared plausible. As I will show below in the context of so-called difference thresholds, even this idea turned out to be an illusion (see p. 39).

To make things *still* worse, the mental functions orderly listed in general psychology textbooks are by no means clearly defined, which they would have to be in order to be seen as *the* constituents of conscious experience. What, for example, is “thinking”, a mental function studied extensively in general psychology? It may contain imaging, recalling, sensing, or even feeling, abstract cognition (whatever *that* may be), judging, and many things more. And these mental subfunctions may be limitlessly subdivided further.⁶ Thus the fundamental precondition for establishing a field of study, namely defining the objects of study, is not and cannot really be satisfied in psychology.

Can the conditions for studying mental functions and, if that study is to be a science, for extracting rules or laws about them, still be worse than described so far?

They are a *lot* worse!

Not only since Freud *unconscious* mental processes have been (and had to be) assumed. Even the hard nosed physicist and physiologist von Helmholtz (1821–1894) resorted to them when explaining perceptual phenomena (the so-called constancies) by “unconscious inference”.⁷ Even if we do not, like Freud, attribute overwhelming importance to unconscious mental processes, there can be no doubt that they may at times massively influence the conscious ones. The effects reach from rather simple mental processes like those occurring with optical illusions, for example, to highly complex ones like, for example, deciding about one’s political affiliation. So if our stream of consciousness, which is only partly accessible anyway, is additionally mixed up with those unconscious mental processes that are, by definition, inaccessible, our capacity for making sense of our mental world, the actual task of psychology, must be severely restricted.

As if all these fundamental problems confronting us when we try to understand mental processes and, as is characteristic of a science, to extract rules about how they proceed, were not enough, there is yet a further one we must acknowledge. Mental processes, even seemingly elementary ones, are in no way uniform. They vastly differ between persons, groups, and times. Not two people

6 As it turned out there is not anything like a mental “element” which would constitute the limit of the subdividing process.

7 Modern perceptual science still postulates such processes (see below, p. 89).

in the world memorize phone numbers in the same way, Italian and American women differ as to such seemingly simple things as the experience of pain, and minorities are perceived differently in today's societies as compared to the ones of fifty years ago.

After all this I think it need not be stressed that a science trying to establish rules about mental functions must be one of a kind. Or, more concretely, it must be asked whether this whole project makes any sense, whether, in other words, psychology can be a science at all in the first place, or whether it would be more appropriate to speak of "psychological studies", as Koch proposes. Obviously aspects of mental life can be meaningfully studied without such studies eventually being integrated into a systematic and coherent body of knowledge called a science. Psychology is not a science in that sense and it need not be one.

Pondering the many problems facing the endeavor to establish a coherent science of the human mind – problems so fundamental that each of them calls into question the very viability of the whole enterprise – it is hard to tell whether the decision to go ahead with it has been a bold or a stupid one. As it often happens when something impossible is tried, it may look bold at the time and stupid in hindsight. In the case of psychology as a science, the verdict depends on *who* is looking back, however. While Koch judged it as being, if not stupid, so at least nonsensical, the vast majority of researchers in the field have seen this by now 150 year-old decision as consistent if not even as logical. History shows that a decision may have been objectively nonsensical, but consistent when the scientific mindset at the time it was made is considered. More often than not in the history of science, the existing mindset rather than objective considerations determined the course of events, not rarely with a retarding effect. As everybody knows, for example, in astronomy the mental disposition underlying the notion that man and the celestial body he inhabits must be at the center of the world severely delayed progress. As hardly anyone knows, the mental disposition leading to the idea that science can solve *any* problem, even the above mentioned fundamental ones in the study of the mind, has delayed rather than furthered our knowledge about the human mind. There actually *are* unsolvable problems! Not acknowledging the fact and trying to solve them is not just a waste of time, it will also produce a lot of nonsense which, if propagated by influential people, may pollute the scientific environment for decades, even centuries.

The origin of psychology as a science of its own

The mental disposition behind the idea that science can solve any problem was prevalent in the 19th century, and for good reasons. The accomplishments of the so-called classical natural sciences, physics, chemistry, and biology, were spectacularly successful in application (think of electricity, the synthesis of new organic molecules, or the detection of microbes) and obviously they were, because they were based on solid theoretical frameworks, such as the law of gravitation in physics (Newton), the periodic table of elements in chemistry (Mendeleyev), or the theory of evolution in biology (Darwin and Wallace), making them the coherent sciences that they are. These sciences were characterized by common methodological principles, above all by objective observation, experimentation, and the use of logic in interpreting results, the latter often implying the use of mathematics. In physics the latter is so obvious that it may invoke the (false) impression “that its apparent preoccupation with numbers and mathematical formulae, actually the most superficial property of physics, is the core that makes it a science” (Weizenbaum, 1976, p. 159). If those methodological principles, so effective in the natural sciences, could be applied to mental phenomena, so the reasoning may have gone, success of a psychological science would be all but inevitable. With the mathematical description of laws of nature proving particularly successful in physics, it was physics (the study of matter and energy), of all sciences, that became the model for psychology as a science.

The project of psychology modelled after physics appeared well under way in sensory physiology (physiology of the sensory systems) in the middle of the 19th century. There a specific subfield, soon known as psychophysics, had developed that dealt with the capacity of sensory systems. The decisive indices for that capacity were so-called sensory thresholds, absolute and difference thresholds, the first defined in terms of the minimal physical energy required to elicit a conscious perception of a stimulus, the second in terms of the minimal *difference* in energy between two stimuli needed to elicit a conscious perception of the two stimuli being different. The ultimate goal of psychophysics was to

mathematically describe the relation between stimulus magnitudes (in physical units) and sensation magnitudes resulting from the physical stimulus hitting the sensory system. As it turned out, however, finding scales (and units) for *sensation* magnitudes is a task not solvable in a satisfactory way.

Two laws came out of these studies, one proposed by Weber and one derived from Weber's law by Fechner. Fechner's law was the decisive one for the history of psychology, stating a specific mathematical relation between stimulus and sensation magnitudes, which, as mentioned, had been the goal of psychophysics. It was published in its final form in 1860 in a book by Fechner which, as he saw it, was a text of the "exact science of the functional relations or relations of dependency between body and mind." (Boring, 1950, p. 281).

It pretty soon turned out that Fechner's law did not hold (and that it must generally be questioned whether the relation between stimulus and sensation magnitudes may be described mathematically, see below), but the damage was done. Fechner's "law" had lastingly instilled into the scientists' mind that, it holding or not, the relation between stimulus and sensation magnitudes can be described mathematically, and, more importantly, that mathematics can be used in psychology as an instrument, quite like in physics. This conviction was so unshakable that psychophysics, rather than accepting failure, became part of the masquerade performed in order to make psychology appear to be an exact science. The problems with Fechner's law in particular and with psychophysics in general will be described in detail below (p. 39).

Weber's studies about thresholds, beyond their role with respect to mind-body relations (see Fechner's above quote), also insinuated that a second methodological characteristic of the classical natural sciences could be meaningfully applied to mental phenomena: experimentation. Those studies, even though not experimental ones in the narrow sense (manipulation of conditions in order to ascertain their effect on some variable), were systematic, quantitative studies about mental processes, i. e. judgments about sensations (e. g. "I have heard a sound", "weight A appears heavier to me than weight B") requested from the subjects. Thus, in the second half of the 19th century (Weber's studies were published – still in Latin – as early as 1834), there was consensus about the possibility of experimentally studying mental processes, as was documented by the founding of the first psychological "laboratory" by the renowned psychologist and philosopher Wilhelm Wundt in 1879. The emancipation of psychology from theology and philosophy and its installment as a science in its own right seemed to be complete.

Mathematics and experimentation notwithstanding, the fundamental problem (among the many others described above) remained: that data about mental events gained through introspection are by definition subjective ones and may thus not really be trusted because there is no control over whether the subject

(self-observer, introspector) really tells what is going on within him. This is not so obvious with data from psychophysics which are based on relatively simple judgments like “I heard a sound”, usually transmitted by saying “yes” or by pressing a button.⁸ To ask a subject to report about the quality of a *feeling*, for example, may be more demanding, however. The report may only inadequately reflect the subject’s experience, particularly because any feeling might be located on several dimensions like, for example, that of pleasantness-unpleasantness or that of intensity. For a valid description of feelings it would therefore be necessary to know how many dimensions there are and *what* these dimensions are. But the psychological study of feelings failed to identify the number and kind of dimensions on which to locate single feelings. After decades of introspective research, for which scientists had to be trained in the procedure of introspection, it had become rather clear to many psychologists around the turn to the 20th century that a science about mental processes such as feeling, thinking, perceiving, or learning cannot be based on introspective data. So while still struggling to become a science, psychology, the study of mental events, was in a dilemma. The direct access to these events was only possible by a procedure that after decades of research had proven to be unreliable. The project of psychology as a science seemed to be trapped. So what was there to do? Give up? “No” John Watson (1913) declared and gave a radical answer to the first question, seen by many as ingenious and bold an answer at the time: “Don’t study mental events in the first place!” The answer is certainly bold, but at the same time, at least at first glance, absurd rather than ingenious. A science originally designed to study mental processes is now supposed to *not* do that.

Watson, who had written a doctoral dissertation about learning processes in rats, suggested that psychology study *behavior* and *nothing but* behavior. In this way he solved the above dilemma (or so it seemed to many at the time), accepting objective (behavioral) data only and *in this respect* at least putting psychology finally on a par with the natural sciences. The solution came at a price, of course, the disregard of mental processes, and it must be asked whether that price was worth paying. Even if you do not but want to predict and control behavior, as was supposed to be the goal of psychology according to Watson, it must be doubted that this can be accomplished by studying behavior only. S–R (stimulus-response) psychology, as behaviorism came to be known as, thought that a study of all possible systematic relations between stimuli and responses would, if only perfected, allow a prediction of behavior, and, by manipulating the stimuli in the right way, allow control of behavior without having to study the mental processes occurring in the subject between S and R. These mental processes which

8 At closer inspection, especially if stimuli of magnitudes close to thresholds are presented, those judgments can be anything but simple (see p. 51).

actually determine the response, even in such seemingly simple S–R connections characteristic of a conditioned reflex (see below), must be assumed to follow rather strict rules. If they do not, i.e. if they are unreliable or even absent, behavior cannot be predicted. In the scientific process it may at times be reasonable to proceed according to the motto “Let us for the sake of simplicity assume that ...” (as Watson did with respect to the reliability of S–R relations), but in psychology it is better not to. *Nothing* is simple when dealing with mental processes, even the (seemingly) elementary ones, particularly the ones occurring in humans. And *human* mental processes or behavior, for that matter, should be at the center of interest in a science of psychology. But the behaviorists rarely took humans as their subjects.⁹ Their most preferred species were rats and pigeons, both of them with mental processes well below the ones occurring in cats, dogs, or apes as to complexity. For the sake of simplicity they assumed that the principles of behavior (title of Hull’s (1943) influential book) were essentially the same in man and the animals.

The two decisive settings in which behaviorists studied S–R relations were called “paradigms”, or “paradigms of learning”. The term may lead us to expect the new psychology to deal with mental processes after all, mental contents being the subject of what we learn in school, for example, but the “paradigms of learning” referred to the learning of *bodily* functions. In the case of the so-called “classical” conditioning paradigm these were the functions of smooth muscles and glands, in the “operant” conditioning paradigm they were the functions of the striate muscles, the ones used for moving the body. The first was simply taken from the research performed by the physiologist Ivan Pavlov, the second was largely elaborated on by Burrhus Skinner who in the process became one of the best known scientists in America. Classical conditioning became known in America largely through John Watson, an admirer of Pavlov, who often wrote in popular journals and who through those popularizations of his ideas had become one of the best known scientists in America earlier.¹⁰ I do not go into the details of classical and operant conditioning. I think it suffices to characterize the new psychology by describing the functions subject to the above two learning paradigms, like, for example, salivation (function of a gland), constriction of blood vessels (function of smooth muscle), and bar pressing¹¹ (function of striate muscle). We must still wonder how a psychology based on changes in these functions alone and leaving all mental processes out of consideration, could come to dominate academic psychology (the one taught at

9 Watson, when working with humans, typically experimented with newborns, obviously assuming rather simple S–R relations occurring in them.

10 Pavlov did in no way see himself as a psychologist and refused to be seen as one.

11 Typical behavior in the “Skinner box”.

university) and be accepted among a wide public. In retrospect we may allow that the program had never been tried before and may have looked bold at the time rather than naïve. There was no empirical evidence at the time indicating that the project must fail. But as it turned out there was *logical* evidence for its inevitable failure. That evidence comes from linguistics, the study of language. Trying to explain language, Skinner, the behaviorist, was confronted with an unsolvable task: to explain the transmission of meaning, the obvious function of language, without ever referring to the concept of meaning. The fact that he still followed Watson's marching order "Don't refer to mental functions and strictly stick to bodily ones!" shows us that there actually seem to be no limits to the effects of self indoctrination. The behaviorists, in their firm belief in being able to do without any mental functions turned out to be *unable* to see the absurdity of their undertaking. Their incapacity was a *structural* one, so to speak, i. e. one implied in their scientific belief system.

Behaviorism, a psychology in denial of mental events, has always grappled with the problem of language because language is obviously based on mental contents in that it serves to transmit them from person to person. The behaviorists thus would have liked to avoid the topic altogether, which in fact they largely did. But language so obviously is of such paramount importance for human behavior that even the behaviorists could not avoid it altogether without their theorizing being seen as vastly incomplete. So Skinner *did* turn to the topic, but obviously he was unaware that in doing so he marched into a battle lost from the beginning. The battle was the one with Chomsky's ideas about how mental events (concepts, ideas) are transformed into language.

The first step Skinner took in his approach to language was to call language "verbal behavior". As a first step it helped him maintain the illusion that he was dealing with just one special kind of overt behavior, explainable without any reference to mental processes. As you may expect Skinner used the paradigm of operant conditioning, often called "Skinnerian conditioning", in order to explain language.¹² In his view, the words or phrases of a sentence represent a concatenation in which one word prompts the next one, the prompting being determined by operant conditioning. So a word follows a previous one if in the past it has been reinforced to do so. We need not discuss the implications of this view for understanding language in detail, however, because the approach was nonsensical from the start.¹³ The sequence of words in a sentence may look like a concatenation because they come one after the other, but it is *not* based on a concatenation but on a logical hierarchical structure as described by Chomsky.

12 The paradigm consists in reinforcing (rewarding) behavior (or elements thereof) that is under the control of the somatic nervous system, i. e. in reinforcing bodily movements.

13 Sometimes I think that Skinner himself must at least have suspected that it was.

Taking an example from Koestler (1967), the sentence “The postman kicked the dog” may on a higher level be seen as a noun phrase (The postman) and a verb phrase (kicked the dog), the two of them on a still higher level constituting the yet not articulated idea of the event (postman kicking dog). There may be more levels, but the scheme is always one of an idea (concept, image, thought) being transformed into a sentence (or several sentences). About the idea we just know that it must exist before the verbalization without knowing in what form it does. This scheme, which is generally accepted today (no student in linguistics today learns about Skinner’s “theory”)¹⁴, *could* not be accepted by Skinner, however reasonable, logical, or plausible it was, because it was about meaning and mental processes, thinking about which was derisively termed “mentalism” by behaviorists and rejected for a science based on objective observation. So Skinner was *a priori* precluded from understanding the origination of language. Considering the overwhelming importance of language for the human mind and thus for behavior, he was precluded from saying anything reasonable about mind and behavior. The case nicely exemplifies that, as mentioned, there are no limits as to the effects of self-indoctrination, making highly intelligent people come to hold views that are pure nonsense.

According to Koch, a more basic misconception lies at the base of the delusional idea (dominating psychology for about half a century) that psychology as a science can be based on bodily functions alone, namely that human mental processes and human behavior can be understood without considering them in a meaningful context. Linguistics, a science about the transmission of meaning between people, to be stripped of the concept of meaning and to be established on the basis of laws governing but bodily processes, is perhaps the crassest example of the consequences of that misconception. Koch’s critique will be dealt with in detail below.

Behaviorism in the form described may have dominated academic psychology for about half a century, but not all psychologists adhered to the ideology, particularly not those who worked in fields of applied psychology, where practical results count and where one cannot wait until some distant future when psychology might have become a science capable of supplying effective tools for application. Most psychologists working in applied fields did not believe anyway that that would ever happen. In psychotherapy or educational counselling, for example, mental contents must be communicated in order for the therapist or the counsellor to understand the problems he is asked to solve and it is hard to see how else to proceed. That it is necessary to explicitly point to this trivial fact,

14 In the most recent attempt to understand language (one that actually tries to refute Chomsky’s concept of a universal grammar) Skinner is not even mentioned (Evans, 2014).

tells of the absurdity, even madness, of the misdevelopment psychology has undergone for decades.

It was one mental trait, however, obviously too important as to be dumped for ideological reasons, that resisted behaviorism's determination to purge psychology of mental constructs in general: intelligence. The inventor of the intelligence test as we know it today, Alfred Binet (see below, p. 134), had a purely pragmatic attitude toward his invention. He used his intelligence test scores, among much other information, to identify those children with learning problems in school who needed some form of special schooling. He did not go into any scientific depths about the definition and structure of the concept. That was mainly done in England and the United States where a newly developed mathematical instrument, factor analysis, was applied to supply a scientific basis to the concept. To no avail.

About one hundred years later there is still no agreement about the structure of intelligence – whether it is best described by a single intellectual factor (g (general) factor) or several specific ones (like mathematical and verbal intelligence), with the proponents of the latter in no way agreeing on what those factors are. As to *definition*, all that psychologists agree upon is that intelligence is what is being measured by intelligence tests, which constitutes pure definitional nihilism. It is an “operational” definition (see below, p. 58), which at a closer look is no definition in the first place. You may think that it is meant ironically but it wants to be taken seriously. For the definition allows one to “measure” intelligence objectively, a fact psychologists craving for natural scientific respectability love so much that they have agreed upon it in spite of its obvious nonsensicalness.

Whatever the definition of what intelligence tests measure, one might think that the enormous amount of research that went into the construction of intelligence tests (see, for example, Lord and Novick, 1968) at least yielded tests with a reasonable predictive power. The task force installed by the American Psychological Association in order to report on the scientific state of intelligence research, however, reported that a mere 25 % of the differences in success in school are due to differences in intelligence test scores. Obviously other factors must be taken into account, like, for example, a person's motivation to achieve. In fact, the whole psychological setting of a person must be considered in order to make any predictions about his future achievements, which is exactly what Binet had proposed. Instead, psychologists, insisting on the idea that a single test score will do, “solved” the problem by simply deciding that the predictive power of intelligence tests is not a poor, but a *good* one (see below, p. 135). So the illusion (*delusion*, rather) was upheld that psychology is capable of developing instruments with a predictive power comparable to the one instruments in physics

have, implying the delusion that psychology will succeed as a science if it proceeds according to the principles of the natural sciences.

When the dominance of behaviorism waned in the late 1950s and mental functions were being reintroduced into psychological research, operational “definitions” (see below, p. 58) of mental functions were essential for studying them. “Cognitive psychology” as it now developed, did not want to return to using introspection as a scientific method (due to its unreliability), however, but strictly based its research on objective data meaning that when mental (“cognitive”) functions were to be quantified, a description of the operations performed in measuring them was required. This reliance on objective data now made psychologists feel like natural scientists even though they were studying such elusive things as mental functions. Academic psychology has happily followed this course and felt truly scientific ever since. Its happiness was such that it did not realize that an “operational” definition is no definition at all.

The happy feeling of being able to measure mental functions objectively has in recent decades still been much consolidated by the use of *biological* indicators of mental functions and states like, for example, changes in the electrical properties of the skin, changes in cardiovascular functions such as heart rate or blood pressure, changes in brain electrical activity (EEG), and more recently, blood flow in circumscribed areas of the brain measured by so called “brain imaging techniques”. The hype about these techniques makes today’s scientific psychologists proud to be seen as “brain researchers”, with quite a number of them seeing themselves not as psychologists anymore, but rather as biologists. *This* now appears to be the ultimate solution of the subjectivity problem. That problem seems to have disappeared altogether.

Biologism, the idea that any mental function can be explained in terms of its underlying biological processes, is as dominant today in psychology as behaviorism was some 60 or 70 years ago. Quite like behaviorism then, biologism is today seen as great scientific progress. As it turns out, and as has been argued by Koch and will be further elaborated on in this book, the whole development, rather than constituting progress, is based on a fundamental misconception about the nature of the human mind.

The question must be asked how psychology is getting away with *posturing* as a natural science by presenting itself with objective definitions, experiments, a lot of mathematics and statistics, and an impressive amount of machinery. Unlike Koch, who argued from a highly abstract, theoretical standpoint, I, in this book, want to expose the masquerade played before us (both before the scientific community and a wide public, too) by in detail describing examples of crass discrepancies between impressive procedures and paltry results, discrepancies unknown in any other science. At the same time I want to convey an idea, a *feeling* if you will, of the mental setting behind this kind of science, based on

my observations as an active participant in the scientific process for about half a century, a participant for quite some time suffering himself from the delusion of taking part in a natural scientific enterprise.

But before in detail describing the failures of the science of psychology, let us have a closer look at Koch's critique, which goes to the *root* of the pompous theatre psychology has created, to its "cognitive pathology".