SOFTWARE AND MIND

Andrei Sorin

EXTRACT

Chapter 3: *Pseudoscience*
Sections *The Problem of Pseudoscience,*
Popper's Principles of Demarcation

This extract includes the book's front matter and part of chapter 3.

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These sections discuss the concept of pseudoscience and the principles of demarcation between science and pseudoscience developed by Karl Popper, used in the book to show how mechanistic fallacies lead to pseudoscientific thinking.

The entire book, each chapter separately, and also selected sections, can be viewed and downloaded at the book's website.

www.softwareandmind.com

SOFTWARE AND MIND

The Mechanistic Myth and Its Consequences

Andrei Sorin

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Don't you see that the whole aim of Newspeak is to narrow the range of thought?… Has it ever occurred to you … that by the year 2050, at the very latest, not a single human being will be alive who could understand such a conversation as we are having now?

George Orwell, *Nineteen Eighty-Four*

Disclaimer

This book attacks the mechanistic myth, not persons. Myths, however, manifest themselves through the acts of persons, so it is impossible to discuss the mechanistic myth without also referring to the persons affected by it. Thus, all references to individuals, groups of individuals, corporations, institutions, or other organizations are intended solely as examples of mechanistic beliefs, ideas, claims, or practices. To repeat, they do not constitute an attack on those individuals or organizations, but on the mechanistic myth.

Except where supported with citations, the discussions in this book reflect the author's personal views, and the author does not claim or suggest that anyone else holds these views.

The arguments advanced in this book are founded, ultimately, on the principles of demarcation between science and pseudoscience developed by philosopher Karl Popper (as explained in "Popper's Principles of Demarcation" in chapter 3). In particular, the author maintains that theories which attempt to explain non-mechanistic phenomena mechanistically are pseudoscientific. Consequently, terms like "ignorance," "incompetence," "dishonesty," "fraud," "corruption," "charlatanism," and "irresponsibility," in reference to individuals, groups of individuals, corporations, institutions, or other organizations, are used in a precise, technical sense; namely, to indicate beliefs, ideas, claims, or practices that are mechanistic though applied to non-mechanistic phenomena, and hence pseudoscientific according to Popper's principles of demarcation. In other words, these derogatory terms are used solely in order to contrast our world to a hypothetical, ideal world, where the mechanistic myth and the pseudoscientific notions it engenders would not exist. The meaning of these terms, therefore, must not be confused with their informal meaning in general discourse, nor with their formal meaning in various moral, professional, or legal definitions. Moreover, the use of these terms expresses strictly the personal opinion of the author – an opinion based, as already stated, on the principles of demarcation.

This book aims to expose the corruptive effect of the mechanistic myth. This myth, especially as manifested through our software-related pursuits, is the greatest danger we are facing today. Thus, no criticism can be too strong. However, since we are all affected by it, a criticism of the myth may cast a negative light on many individuals and organizations who are practising it unwittingly. To them, the author wishes to apologize in advance.

Contents

Preface

The book's subtitle, *The Mechanistic Myth and Its Consequences*, captures its essence. This phrase is deliberately ambiguous: if read in conjunction with the title, it can be interpreted in two ways. In one interpretation, the mechanistic myth is the universal mechanistic belief of the last three centuries, and the consequences are today's software fallacies. In the second interpretation, the mechanistic myth is specifically today's mechanistic *software* myth, and the consequences are the fallacies *it* engenders. Thus, the first interpretation says that the past delusions have caused the current software delusions; and the second one says that the current software delusions are causing further delusions. Taken together, the two interpretations say that the mechanistic myth, with its current manifestation in the software myth, is fostering a process of continuous intellectual degradation – despite the great advances it made possible. This process started three centuries ago, is increasingly corrupting us, and may well destroy us in the future. The book discusses all stages of this degradation.

The book's epigraph, about Newspeak, will become clear when we discuss the similarity of language and software (see, for example, pp. 411–413).

Throughout the book, the software-related arguments are also supported with ideas from other disciplines – from philosophy, in particular. These discussions are important, because they show that our software-related problems are similar, ultimately, to problems that have been studied for a long time in other domains. And the fact that the software theorists are ignoring this accumulated knowledge demonstrates their incompetence. Often, the connection between the traditional issues and the software issues is immediately apparent; but sometimes its full extent can be appreciated only in the following sections or chapters. If tempted to skip these discussions, remember that our software delusions can be recognized only when investigating the software practices from this broader perspective.

Chapter 7, on software engineering, is not just for programmers. Many parts (the first three sections, and some of the subsections in each theory) discuss the software fallacies in general, and should be read by everyone. But even the more detailed discussions require no previous programming knowledge. The whole chapter, in fact, is not so much about programming as about the delusions that pervade our programming practices. So this chapter can be seen as a special introduction to software and programming; namely, comparing their true nature with the pseudoscientific notions promoted by the software elite. This study can help both programmers and laymen to understand why the incompetence that characterizes this profession is an inevitable consequence of the mechanistic software ideology.

There is some repetitiveness in the book, deliberately introduced in order to make the individual chapters, and even the individual sections, reasonably independent. Thus, while the book is intended to be read from the beginning, you can select almost any portion and still follow the discussion. An additional benefit of the repetitions is that they help to explain the more complex issues, by presenting the same ideas from different perspectives or in different contexts.

The book is divided into chapters, the chapters into sections, and some sections into subsections. These parts have titles, so I will refer to them here as *titled* parts. Since not all sections have subsections, the lowest-level titled part in a given place may be either a section or a subsection. This part is, usually, further divided into *numbered* parts. The table of contents shows the titled parts. The running heads show the current titled parts: on the right page the lowest-level part, on the left page the higher-level one (or the same as the right page if there is no higher level). Since there are more than two hundred numbered parts, it was impractical to include them in the table of contents. Also, contriving a short title for each one would have been more misleading than informative. Instead, the first sentence or two in a numbered part serve also as a hint of its subject, and hence as title.

Figures are numbered within chapters, but footnotes are numbered within the lowest-level titled parts. The reference in a footnote is shown in full only the first time it is mentioned within such a part. If mentioned more than once, in the subsequent footnotes it is usually abbreviated. For these abbreviations, then, the full reference can be found by searching the previous footnotes no further back than the beginning of the current titled part.

The statement "italics added" in a footnote indicates that the emphasis is only in the quotation. Nothing is stated in the footnote when the italics are present in the original text.

In an Internet reference, only the site's main page is shown, even when the quoted text is from a secondary page. When undated, the quotations reflect the content of these pages in 2010 or later.

When referring to certain individuals (software theorists, for instance), the term "expert" is often used mockingly. This term, though, is also used in its normal sense, to denote the possession of true expertise. The context makes it clear which sense is meant.

The term "elite" is used to describe a body of companies, organizations, and individuals (for example, the software elite); and the plural, "elites," is used when referring to several entities, or groups of entities, within such a body. Thus, although both forms refer to the same entities, the singular is employed when it is important to stress the existence of the whole body, and the plural when it is the existence of the individual entities that must be stressed. The plural is also employed, occasionally, in its normal sense – a group of several different bodies. Again, the meaning is clear from the context.

The issues discussed in this book concern all humanity. Thus, terms like "we" and "our society" (used when discussing such topics as programming incompetence, corruption of the elites, and drift toward totalitarianism) do not refer to a particular nation, but to the whole world.

Some discussions in this book may be interpreted as professional advice on programming and software use. While the ideas advanced in these discussions derive from many years of practice and from extensive research, and represent in the author's view the best way to program and use computers, readers must remember that they assume all responsibility if deciding to follow these ideas. In particular, to apply these ideas they may need the kind of knowledge that, in our mechanistic culture, few programmers and software users possess. Therefore, the author and the publisher disclaim any liability for risks or losses, personal, financial, or other, incurred directly or indirectly in connection with, or as a consequence of, applying the ideas discussed in this book.

The pronouns "he," "his," "him," and "himself," when referring to a gender-neutral word, are used in this book in their universal, gender-neutral sense. (Example: "If an individual restricts himself to mechanistic knowledge, his performance cannot advance past the level of a novice.") This usage, then, aims solely to simplify the language. Since their antecedent is gender-neutral ("everyone," "person," "programmer," "scientist," "manager," etc.), the neutral sense of the pronouns is established grammatically, and there is no need for awkward phrases like "he or she." Such phrases are used in this book only when the neutrality or the universality needs to be emphasized.

It is impossible, in a book discussing many new and perhaps difficult concepts, to anticipate all the problems that readers may face when studying these concepts. So the issues that require further discussion will be addressed online, at *www.softwareandmind.com*. In addition, I plan to publish there material that could not be included in the book, as well as new ideas that may emerge in the future. Finally, in order to complement the arguments about traditional programming found in the book, I plan to publish, in source form, some of the software applications I developed over the years. The website, then, must be seen as an extension to the book: any idea, claim, or explanation that must be clarified or enhanced will be discussed there.

The Problem of Pseudoscience

1

A pseudoscience is a system of belief that masquerades as scientific theory. The list of pseudosciences, ancient and modern, is practically endless: astrology is founded on the belief that the heavenly bodies influence human affairs on earth; phrenology claims that we can determine various personality traits from the shape of a person's skull; graphology claims that we can determine traits from a person's handwriting; dowsing maintains that it is possible to discover underground water just by walking over an area; alchemy holds that it is possible to transmute base metals into gold. Other pseudosciences are based on the belief in psychic phenomena, visits from aliens, faith healing, prophecy, magical objects, and so on.

Astrology has been with us for five thousand years, but most pseudosciences lose their popularity over time and are replaced by new ones. The continuing appeal of pseudoscience rests on its promise of simple solutions to difficult problems, as opposed to the relatively modest claims made by science. Widespread education has not eradicated what seems to be a basic human need – our craving for supernatural powers – and it has been noted that pseudosciences, superstitions, and the belief in the paranormal are actually on the rise throughout the modern world.[1]

A distinguishing characteristic of pseudoscience is the acceptance of a hypothesis as unquestionable truth, and the refusal to review it later in the light of falsifying evidence. Whereas serious researchers insist on careful and objective tests of validity for their theories, pseudoscientific theories depend on the enthusiasm of the practitioners and the credulity of their followers. When subjected to controlled experiments, the success rate of these theories is usually revealed to be no better than chance. Pseudoscientific theories do not work, but believers interpret the chance successes as evidence of their truth, and belittle the significance of the failures. It is important to note that the practitioners' sincerity is often above suspicion; it is precisely their *belief* that prevents them from recognizing the falsity of their theories. But because there are no serious validity tests, pseudosciences also attract many charlatans – practitioners who knowingly deceive the public.

Despite their variety, the traditional pseudosciences have been addressing the same concerns since ancient times: our fears and desires, our longing for omnipotence and immortality. But today the mechanistic delusions are

[1] See, for example, Paul Kurtz, *The Transcendental Temptation: A Critique of Religion and the Paranormal* (Buffalo, NY: Prometheus Books, 1991).

fostering a new kind of pseudosciences: various academic pursuits that are part of modern disciplines and spheres of knowledge. And they are also fostering a new kind of pseudoscientists: researchers, professors, and theorists working in universities and other institutions. While these academic pursuits resemble scientific research, they belong to the pseudoscientific tradition insofar as they too are founded on a hypothesis that is taken as unquestionable truth. The hypothesis is that all phenomena can be explained with the mechanistic principles of reductionism and atomism. Although this belief is different from the beliefs upon which the traditional pseudosciences are founded, the ensuing pursuits acquire a similar character: they become systems of belief that masquerade as scientific theories. Thus, I call these pursuits *the new pseudosciences*. The new pseudosciences belong to the class of theories we examined in chapter 1 under scientism.

Like the traditional ones, the new pseudosciences do not work. Also like the traditional ones, blatant falsifications leave their supporters unperturbed. Instead of recognizing falsifications as a refutation of their theory, pseudoscientists think their task is to *defend* it; so they resort to various stratagems to make the theory appear successful despite the falsifications. Their work, thus, while resembling scientific research, is in reality a series of attempts to save from refutation an invalid theory.

❖

We saw in chapter 1 how mechanistic delusions lead to futile pursuits (see pp. 106–108). If the phenomenon in question can only be represented with a complex structure – if, in other words, it cannot be usefully approximated by separating it into simpler, independent phenomena – the only way to explain it is by studying it as a whole. This is a difficult, often impossible, task. The researchers believe that a simple structure – in the form of a mechanistic theory, or model – can represent the phenomenon accurately enough to act as explanation. So they extract one of the simpler phenomena from the complex whole, hoping that a mechanistic model based on it alone will provide a good approximation of the whole phenomenon. They are committing the fallacy of reification, but they see this act as a legitimate method, sanctioned by science.

Science sanctions this method only for *mechanistic* phenomena. The researchers cannot know in advance whether their subject is indeed mechanistic, so the possibility of explaining the complex phenomenon by isolating the simpler phenomena that make it up is only an assumption. To validate this assumption, they must arrive at a successful explanation of the original phenomenon; specifically, they must discover a mechanistic approximation that is close enough to be useful. But even when they find explanations for the isolated phenomena, the researchers fail to explain the original, complex phenomenon. We know, of course, why: the complex phenomenon includes the *interactions* between structures, and these interactions were lost when they separated the structures. They mistakenly assumed that the interactions are weak enough to be ignored, so the model based on reified structures does not represent the actual phenomenon accurately enough.

In their work, these researchers may be following the strictest methods. In their study of the isolated structures, their theories and procedures may be faultless. Thus, their activities may be indistinguishable from those of real scientists. The more complex the problem, the more opportunities there are to separate it into simpler problems, then to separate these into even simpler ones, and so on.

It is obvious, then, why the mechanists perceive these activities as important work. At any point in time, what they are doing resembles true research – the kind of work that in the exact sciences brings about great discoveries. Consequently, solving one of the isolated problems is seen as progress, as a contribution to the solution of the original problem. Besides, the theory does work in certain cases. It is in the nature of poor approximations to work in some cases and not in others, but the mechanists interpret the odd successes as evidence that their ideas are valid.

At this stage, they have forgotten that the entire project is based on the *assumption* that the original phenomenon can be explained mechanistically. The assumption is wrong, so all these activities – no matter how rational and scientific they may appear when judged *individually*, and no matter how successfully they may solve *isolated* problems – constitute a delusion. Not surprisingly, no theory that explains the original phenomenon is ever found. Modern mechanistic pseudosciences last several years, or several decades, and then they are quietly abandoned.

What is especially striking in pseudosciences, thus, is to see people engaged in activities that are entirely logical *individually*, even while the body of activities as a whole constitutes a delusion. All it takes is one wrong assumption; and if this assumption is never questioned, the research is nonsensical no matter how rational are the individual activities.

By its very nature, therefore, the mechanistic assumption engenders pseudosciences: If we assume that a non-mechanistic phenomenon can be explained by breaking it down into mechanistic ones, we will end up studying the latter. So, like real scientists, we will be engaged at all times in the exact work associated with mechanistic phenomena. We will be pursuing a delusion, but this will not be evident from the *individual* activities. The only way to recognize the delusion is by questioning the mechanistic assumption itself.

❖

If it is so easy to fall prey to mechanistic delusions, how can we differentiate between those scientists engaged in important research and those who pursue hopeless, pseudoscientific ideas? Clearly, if we agree that science means simply the pursuit of mechanistic theories, regardless of whether they work or not, it is no longer possible to distinguish true scientists from crackpots and charlatans.

Note that it is not the *failure* of these theories that must concern us. Ambitious or revolutionary ideas often prove to be mistaken, so the risk that a theory may eventually fail should not prevent us from pursuing it. What we must question, rather, is whether the pursuit of a theory should be considered science simply because the theory is mechanistic. Science ought to mean the pursuit of *sound* theories: mechanistic ones for mechanistic phenomena, and non-mechanistic ones for complex phenomena.

Is there a way to avoid this enormous waste of resources – and, worse, its consequences? For, if we take the *software* theories as an indication of where this degradation can lead, the consequences are the destruction of knowledge, a return to the irrationality of the Dark Ages, and a totalitarian society. Once we recognize that software phenomena are non-mechanistic, any research program based on mechanistic software notions looks absurd, no different from the research program of the alchemists or the astrologers. It is only through the mechanistic hypothesis – namely, the assumption that any phenomenon can have a mechanistic model – that the software theories can be said to belong in the domain of science, rather than pseudoscience.

Thus, we have reached perhaps a critical point in history, where there is an urgent need to revise our conception of science. If the software delusions are an indication, the survival of our civilization may well depend on our decision whether or not to retain mechanism as an article of scientific faith.

2

Since mechanistic delusions undermine logical thinking in the same way that other delusions did in the past, the problem we are facing is a problem that has preoccupied philosophers for centuries: In our quest for new knowledge, how can we avoid irrational thinking, fallacious arguments, and unsound judgment? If what we discover is really new, how can we know whether it is true? For, the only way to be absolutely sure that something is true is by proving it on the basis of *previous* knowledge – knowledge whose truth is established. But then, a successful proof will also indicate that it depends entirely on facts we knew before, leading to the conclusion that it is not really new. It seems, therefore, that we can gain new knowledge only if we do not also expect to be certain of its truth. This inference is very disturbing, as it suggests that the advance of knowledge depends entirely on something rather doubtful: the human capacity for faultless reasoning.

Many methods have been suggested for improving our thinking habits – methods ranging from rules of common sense to procedures of formal logic. In the seventeenth century, for instance, Francis Bacon, who stressed the importance of experimentation and logical thinking in scientific research, described four categories of reasoning errors (which he called "idols of the mind"). And in the nineteenth century, John Stuart Mill popularized a set of methods that can be used in any experimental inquiry to check the validity of hypotheses and to avoid drawing mistaken conclusions.

To the traditional principles we must add a new one if we want to guard against *mechanistic* delusions: Before attempting to explain a phenomenon by separating it into several independent phenomena, we must first prove that the interactions between these phenomena can be ignored. In other words, we must determine that the original phenomenon can indeed be modeled with simple structures. Since only mechanistic phenomena lend themselves to this treatment, if we commence our project by isolating structures we merely beg the question: we start by assuming the very fact that needs to be determined – the mechanistic nature of the phenomenon. Thus, if the project is to be considered science and not speculation, we must start by proving that the links *between* structures are weak relative to the links *within* structures; specifically, we must prove that they are weak enough to be ignored. And if such a proof is impossible, the phenomenon must be deemed non-mechanistic. Any search for a mechanistic theory is then known in advance to be futile, so it cannot be considered a serious scientific activity.

In particular, most phenomena involving human minds and societies consist of interacting structures, and weak links between these structures are the exception. Scientists isolate these structures precisely because they want to avoid the complexity generated by their interactions. They fail to see, though, that once they eliminate the interactions they are no longer studying the original phenomena. So we must not be surprised when, years later, they are still searching for a useful model. But we must remember that, by applying a simple logical principle, they could have avoided this futile work.

❖

No system has been found that can guarantee sound reasoning while also permitting creativity, innovation, and discovery. The problem of reconciling these conflicting ideals remains a difficult one. Descartes believed that the "geometrical method" is the answer: if we treat all knowledge as simple hierarchical structures, we will discover, without ever falling into error, everything the human mind can comprehend. Only in a world limited to mechanistic phenomena, however, could such a naive method work. And it is precisely his legacy – the belief that non-mechanistic phenomena, too, can be explained with the geometrical method – that engenders the new pseudosciences.

This problem has evolved into what is known today as the problem of *demarcation*: how to differentiate between scientific and pseudoscientific theories. The best-known and most successful principles of demarcation are those developed by Karl Popper. These principles can be used to assess, not only formal theories in the traditional disciplines, but any concepts, statements, and claims. In the domain of software, particularly, we can use them to assess notions like structured programming, the relational database model, and software engineering in general. These notions are in effect empirical theories, insofar as they make certain claims concerning the benefits of various methods or aids. The principles of demarcation will help us to determine whether these theories express important software concepts, or whether they are pseudosciences.

Popper's Principles of Demarcation

1

Sir Karl Popper, generally recognized as the greatest philosopher of science of the twentieth century, created a philosophy of knowledge and progress that can be applied consistently in all human affairs. It is useful for scientific theories as well as social and political ideas, for difficult decisions as well as common, everyday puzzles.[1]

Popper held that it is impossible, in the empirical sciences, to *prove* a theory; so we can never be sure that our knowledge is correct or complete. The only way to advance, therefore, is through a process of trial and error, by learning from our mistakes: we must treat all ideas and theories as *tentative*

[1] See, in particular, these books by Karl Popper: *Conjectures and Refutations: The Growth of Scientific Knowledge*, 5th ed. (London: Routledge, 1989); *The Logic of Scientific Discovery* (London: Routledge, 1992); *Realism and the Aim of Science* (London: Routledge, 1985).

solutions, as mere conjectures, and we must never cease to doubt them. It is our responsibility, in fact, to attempt to refute our own theories – by subjecting them to severe tests. And we must always try to find better ones. In this way, our theories will keep improving, and we will get nearer and nearer to the truth. But, because the world is so complex, this process can never end. Indeed, even if one day we do arrive at the truth, we will have no way of knowing that we did.

Theories turn into worthless pursuits when their supporters choose to ignore the falsifying evidence. Unlike true scientists – who seek the truth and know that their theories, even when apparently successful, may be mistaken – pseudoscientists believe their task is simply to defend their theories against criticism.

Popper considered demarcation to be "the central problem of the theory of knowledge."[2] It must be noted that he sought to distinguish the empirical sciences not only from pseudosciences, but also from metaphysics and purely logical theories. He recognized the value of these other types of knowledge; but, he said, they are different from the empirical sciences. For example, some theories considered scientific today originated in antiquity as pseudosciences, so even as pseudosciences they must have been useful; and purely logical systems like mathematics, while not part of the real world but our invention, can provide invaluable models (by *approximating* the real world). Any theory, thus, can be useful. But the theories of empirical science occupy a special position, because they alone permit us to develop knowledge that matches reality. So, if we want to improve our knowledge of the world, we must have a way of determining whether a given theory belongs to one category or the other.

It may seem odd to place the rigorous theories of pure mathematics in the same category as pseudosciences. These theories *are* alike, though, when viewed from the perspective of empirical science; that is, when judged by their ability to represent the world. The mechanistic *software* theories provide a nice illustration of this affinity. The structured programming theory, for instance, and the relational database theory, are founded upon mathematical principles. But these principles reflect only minor and isolated aspects of the phenomenon of software development, not whole programming projects. In their pure form, therefore, these theories are useless for creating serious applications, because they do not approximate closely enough the actual software phenomena. They became practical (as we will see in chapter 7) only after renouncing their exact, mathematical principles and replacing them with some vague, informal ones. And this degradation is one of the distinguishing characteristics of

[2] Karl R. Popper, *The Logic of Scientific Discovery* (London: Routledge, 1992), p. 34.

pseudoscience: the experts continue to promote their theory on the basis of its exactness, even while annulling, one by one, its exact principles.

Mechanistic software theories, thus, can exist only as purely logical systems and as pseudosciences; and in either form they cannot be part of empirical science. Empiricism stipulates that theories be accepted or rejected through actual tests, through observation and experiment. As logical systems, the mechanistic software theories were tested in the real world, and failed; and in their modified form, as pseudosciences, these theories offer no exact principles to begin with, so they cannot be tested.

2

Popper's interest in a criterion of demarcation started in his youth, when he "became suspicious of various psychological and political theories which claimed the status of empirical sciences, especially Freud's 'psychoanalysis,' Adler's 'individual psychology,' and Marx's 'materialist interpretation of history.'"[3] Popper was struck by the *ease* with which one could find confirming evidence for these theories, despite their dubiousness. A Marxist could find evidence of the class struggle in every event and every news item, and also in the *absence* of certain events or news items. A Freudian or Adlerian psychoanalyst could find confirming evidence of Freud's or Adler's psychological theories, respectively, in every act performed by every person; and had a person acted differently, that behaviour too could have been explained by the same theory. Any event seemed to fit quite naturally within these theories. In fact, one could not even *imagine* an event that would have contradicted them.

While these dubious theories were so easily *verified*, Popper was impressed by how easy it was to *falsify* a true scientific theory. Einstein, for example, boldly predicted several events from his theory of relativity, and declared that if they did not occur as stated he would simply consider the theory refuted.

Popper realized that it was precisely the *ease* with which a theory can be confirmed that *reduces* its scientific value, and this led him to his criterion of demarcation: "But were these theories testable?… What conceivable event would falsify them in the eyes of their adherents? Was not every conceivable event a 'verification'? It was precisely this fact – that they always fitted, that they were always 'verified' – which impressed their adherents. It began to dawn on me that this apparent strength was in fact a weakness, and that all these 'verifications' were too cheap to count as arguments.… The *method of looking for verifications* seemed to me unsound – indeed, it seemed to me to be the

[3] Karl R. Popper, *Realism and the Aim of Science* (London: Routledge, 1985), p. 162.

typical method of a pseudoscience. I realized the need for distinguishing this method as clearly as possible from that other method – the method of testing a theory as severely as we can – that is, the method of criticism, the *method of looking for falsifying instances*."[4]

Several years later, Popper recognized that the problem of demarcation is closely related to the classical problem of induction, and that the two had to be considered together.[5] The problem of induction is this: When we develop a theory in the empirical sciences, we draw general conclusions from a limited number of observations and experiments; we reason from singular facts to general statements; we believe that we can explain an infinite number of situations that have yet to occur, from the study of a finite number of situations that we observed in the past. This concept – induction – is indispensable in science, for we could have no theories without it. Logically, however, induction is invalid, because there is no justification for deriving general laws from the observation of unique events. The only way to practise science, therefore, is by trusting the principle of induction even as we know that it is invalid.

But there can be no doubt that induction *does* work: our knowledge *has* been increasing, and this shows that we *can* draw valid conclusions from past events, and we *can* have useful theories. We accept induction, therefore, simply because it works; and it works because there are regularities in the world: some future events *will* be similar to past ones, so it is possible to discover theories and to make predictions, especially if we are content with approximations.

Unfortunately, this expectation of regularities also tempts us to see patterns where there are none, leading us to fallacious thinking and irrational behaviour. Pseudosciences and superstitions are theories that predict future events from current knowledge, just like the theories of empirical science. For example, if we noticed once the position of the planets while a happy event took place, we will plan our activities based on their future position; and if we noticed once a black cat while a tragic event took place, we will avoid black cats in the future. With pseudosciences and superstitions, thus, we also use induction; we also draw general conclusions from the observation of a few events; so we also reason from particular facts to general statements. The only difference from science seems to be that our observations are less careful, so our conclusions are less accurate and our predictions less successful.

The belief in induction is closely related to the belief in causality. We must accept both principles in order to develop theories, and both stem from the way our mind works: we expect to find regularities in our environment. When

[4] Ibid., pp. 162–163.

[5] For Popper's views on induction, see ibid., ch. I, and his *Objective Knowledge: An Evolutionary Approach*, rev. ed. (Oxford: Oxford University Press, 1979), ch. 1.

an event occurs simultaneously with another, or shortly thereafter, we tend to conclude that they must be related, that one caused the other or perhaps a third one caused both. This belief is reinforced by the belief in induction, when we observe a repetition of that pattern.

As with induction, though, no matter how often we notice the pattern, we have no logical grounds to conclude that there is a causal relation between the two events. We feel that such a relation is likely, of course, just as we feel that an event which occurred frequently in the past is likely to recur in the future. But these are strictly subjective notions, which spring from our habits of mind, from our natural tendency to expect regularities. According to the theory of probability, if we observed only a finite number of events and there are an infinite number of future events, the probability of predicting anything about those future events from the past ones is the first number divided by the sum of the two, which is practically zero.

Causality and induction, then, are hardly the solid and objective foundation we would like to have for our empirical sciences. It is true that science, unlike pseudoscience and superstitions, demands more observations before concluding that one event causes another; and it is true that scientific theories are more than just our expectation to see in the future a repetition of past events. Nevertheless, it is disturbing that our scientific knowledge has the same foundation as our superstitions: our habits of mind, our inclination to expect regularities, perhaps a propensity resulting from the evolution of the brain.

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If you think these problems ought to concern only philosophers, remember the sad story of the chicken that believed in causality and induction. The chicken noticed, day after day, that the farmer sheltered it, fed it, and watched its health. After observing this pattern for many days, the chicken felt justified to conclude that the farmer's acts were motivated by love, and that it would enjoy the same comfort in the future. Soon after, though, the farmer killed the chicken and ate it – which had been his intent, of course, from the start.

What philosophers are trying to determine is whether, from the information available to it, the chicken could have known the truth.[6] Or, rather, they are trying to determine whether *we*, from our current knowledge, can arrive at the truth. For, at any given time, we are in a position not very different from that of the chicken: we must make decisions about *future* events by using the doubtful theories we developed from the observation of relatively few *past* events. And when, recognizing the limitations of our personal knowledge, we

[6] It is Bertrand Russell who first noted the chicken's quandary.

listen to scientists and experts, to corporations and universities, to governments and media, all we do is trust the doubtful theories that *others* developed from those few past events.

For example, when we accept the programming methods concocted by software theorists because they seem to work with some simple textbook examples, or when we judge the value of a software system from a few "success stories" or "case studies," we are using in effect a few past events to make decisions about the future. But how can we be sure that we are not making the same mistake as the chicken?

So, if the problem of demarcation is how to distinguish our scientific from our pseudoscientific theories, the problem of induction is that *all* theories are logically unjustifiable, so there is no real difference between the scientific and the pseudoscientific ones in any case.

The problem of induction and its disturbing implications were first studied by David Hume, who resigned himself to complete skepticism. His conclusions had a profound influence on the development of Western thought, as they cast doubt on the possibility of rationality and objective knowledge: "The growth of unreason throughout the nineteenth century and what has passed of the twentieth is a natural sequel to Hume's destruction of empiricism.… It is therefore important to discover whether there is any answer to Hume within the framework of a philosophy that is wholly or mainly empirical. If not, there is no intellectual difference between sanity and insanity.… This is a desperate point of view, and it must be hoped that there is some way of escaping from it."[7]

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Popper found a solution to Hume's problem of induction, and to the skepticism engendered by it, through his solution to the problem of demarcation: "If, as I have suggested, the problem of induction is only an instance or facet of the problem of demarcation, then the solution to the problem of demarcation must provide us with a solution to the problem of induction."[8] He agrees that induction and past confirmations are insufficient to prove a theory; but he does not agree with the conclusion drawn by the earlier philosophers – namely, that this limitation will forever prevent us from distinguishing between our rational theories and our delusions.

What Popper proposes is to combine the methods of induction, which are indispensable for discovering new theories but cannot prove them, with the

[7] Bertrand Russell, *A History of Western Philosophy* (New York: Simon and Schuster, 1972), p. 673.

[8] Karl R. Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge*, 5th ed. (London: Routledge, 1989), p. 54.

methods of *deduction*, which cannot create new knowledge but *can* prove statements. Deduction allows us to prove the validity of a statement by showing that it can be derived logically from other statements, which are known to be valid. Mathematical and logic systems, for example, are based on deduction: a conclusion is derived by combining premises; a new theorem is demonstrated by combining previous, simpler theorems. With strict deduction, there can be no knowledge in a new statement that is not already contained in the original ones (this is what guarantees the validity of the new statement). But, even though they do not create new knowledge, the methods of deductive logic are still important, because the new statements may express the same knowledge more clearly, more economically, and more usefully.[9]

Popper was impressed by the *asymmetry* between trying to *prove* a theory and trying to *refute* it. A theory is a universal statement that makes a claim about a large, perhaps infinite, number of events. Consequently, any number of confirmations are insufficient to prove its validity. At the same time, just one event that *contradicts* the theory is sufficient to *refute* it. Imagine, for instance, that we wanted to verify the universal statement "all swans are white" (one of Popper's favourite examples). No matter how many white swans we observe, these confirmations would not verify the statement, for we could never be sure that we saw all the swans in the world; but observing just one black swan would suffice to *refute* the statement.

This is how Popper explains his idea: "My proposal is based upon an *asymmetry* between verifiability and falsifiability; an asymmetry which results from the logical form of universal statements. For these are never derivable from singular statements, but can be contradicted by singular statements. Consequently it is possible by means of purely deductive inferences (with the help of the *modus tollens* of classical logic) to argue from the truth of singular statements to the falsity of universal statements."[10]

Modus tollens states that, if we know that whenever p is true q is also true, then if q is found to be false we must conclude that p is false. So what Popper says is this: if p stands for any one of the assertions that make up a theory, and q stands for any one of the conclusions derived from this theory, then just one instance of q being false will refute the theory.[11] In other words, while no number of "q is true" claims that are true suffices to *prove* the theory, just one "q is false" claim that is true suffices to *refute* it.

[9] The induction discussed here must not be confused with the method known as *mathematical induction*, which employs in fact deduction.

[10] Popper, *Scientific Discovery*, p. 41.

[11] Ibid., p. 76.

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The first thing we learn from Popper's discovery is how absurd is the popular belief that we must *verify* our theories, that we must search for *confirming* evidence. For, no matter how many confirmations we find, these efforts can prove nothing. Rather than attempting to show that a theory is valid, we must attempt to show that it is *invalid*; and the theory will be accepted as long as we *fail* in these attempts. It will be accepted, not because we proved its truth (which is impossible), but because we failed to prove its falsity.

Thus, if we sincerely attempt to refute our theories, if we agree to accept only those that pass the most severe tests we can design, our knowledge at any point in time is guaranteed to be as close to the truth as we can get. This, says Popper, is all we can hope to achieve: "Assume that we have deliberately made it our task to live in this unknown world of ours; to adjust ourselves to it as well as we can; to take advantage of the opportunities we can find in it; and to explain it, *if* possible (we need not assume that it is), and as far as possible, with the help of laws and explanatory theories. *If we have made this our task, then there is no more rational procedure than the method of trial and error – of conjecture and refutation:* of boldly proposing theories; of trying our best to show that these are erroneous; and of accepting them tentatively if our critical efforts are unsuccessful."[12]

With this method we combine, in effect, the benefits of induction and deduction. In our search for new theories, we can now use induction as often as we want. We need no longer worry about our habits of mind – about our inclination to expect regularities. Ideas revealed to us in our dreams are as good as those discovered through formal research methods. We can use our imagination and creativity freely, and we can propose theories that are as bold and original as we like. We can do all this because we need no longer fear that our thought patterns may be wrong, or that our conclusions may be mistaken. The discovery of a theory is now only the first stage. The theory is accepted provisionally, and it is in the next stage that the most important work is done: attempting to refute the theory by subjecting it to severe tests.

If we allowed the *uncertainty of induction* in order to *discover* the theory, we rely on the *certainty of deduction* in order to *refute* it. We benefit from deductive logic in two ways. First, as noted earlier, in the knowledge that the failure to pass even one test will prove that the theory is invalid. Second, we must use deductive methods – formal logic, mathematics, established theories, controlled experiments – in the tests themselves. It is pointless to devote any effort and to insist on deductive methods for tests that *verify* the theory; for, no

[12] Popper, *Conjectures and Refutations*, p. 51.

matter how scrupulous these tests are, each confirmation of the theory does not increase significantly the likelihood of its validity (since there will always remain an infinite number of unverified instances). Instead, we must devote this deductive effort to tests that try to *falsify* the theory. Logically, we can learn little or nothing from any number of instances that confirm it, but we can learn a great deal from just one instance that falsifies it.

Popper's solution, thus, has rescued the principle of empiricism – the requirement that theories be accepted or rejected on the basis of observations and experiments – from the destructive consequences of induction. All we must do is replace the principle of *accepting* a theory on the basis of *confirming* evidence, with the principle of *rejecting* the theory on the basis of *refuting* evidence. Empiricism "can be fully preserved, since the fate of a theory, its acceptance or rejection, is decided by observation and experiment – by the result of tests. So long as a theory stands up to the severest tests we can design, it is accepted; if it does not, it is rejected. But it is never inferred, in any sense, from the empirical evidence. There is neither a psychological nor a logical induction. *Only the falsity of the theory can be inferred from empirical evidence, and this inference is a purely deductive one.*"[13]

What Popper's solution amounts to, in essence, is a trade. We agree to give up the dream of knowing with certainty whether a theory is true or false; in return, we save the ideals of empiricism, the possibility to distinguish rationality from irrationality, and the hope for intellectual progress.

3

If the correct way to judge theories is by subjecting them to tests that try to falsify them, it follows that we cannot even consider theories that do not lend themselves to tests and falsification. This quality, then, is the criterion of demarcation that Popper was seeking: "Not the *verifiability* but the *falsifiability* of a system is to be taken as a criterion of demarcation.… I shall require that its logical form shall be such that it can be singled out, by means of empirical tests, in a negative sense; *it must be possible for an empirical scientific system to be refuted by experience.*"[14]

Most people think that to test a theory means to show that it works, so they choose for their tests situations that confirm the theory. But such tests are worthless: "It is easy to obtain confirmations, or verifications, for nearly every theory – if we look for confirmations."[15] The criterion of demarcation

[13] Ibid., p. 54. [14] Popper, *Scientific Discovery*, pp. 40–41.
[15] Popper, *Conjectures and Refutations*, p. 36.

prescribes the opposite; namely, for a theory to be included in the domain of empirical science, there must exist tests that, if successful, would *falsify* it. Thus, scientific theories are falsifiable; theories that are unfalsifiable are pseudoscientific.

It is important to understand the difference between the two qualities, *falsifiable* and *falsified*. The criterion of demarcation is not concerned with the theory's validity; it only determines whether the theory should be considered part of empirical science. If our tests – our attempts to find falsifications – are successful, the theory is rejected; if unsuccessful, it is accepted. But it must be falsifiable to begin with, in order for us to be able to *apply* the tests; and this quality is what makes it scientific.

A scientific theory is always falsifiable, but it may or may not be eventually falsified by tests (and even if falsified, and then abandoned, it does not lose its scientific status). Pseudoscientific theories, on the other hand, are unfalsifiable, so they can never be falsified by tests. They are, therefore, untestable. The fact that they are never falsified makes them appear successful, but in reality they are worthless; for, they do not earn their success by *passing* tests, as do the scientific theories, but by *avoiding* tests. We will examine shortly how theories can be made unfalsifiable, but we can already see the simplest way to accomplish this: by keeping their predictions vague and ambiguous, so that any event appears to confirm them. (This is typically how pseudosciences like astrology manage to appear successful.)

The principle of falsifiability can also be expressed as follows. A scientific theory makes a statement about a universe of events, dividing them into two categories: those events it permits and those it forbids. The more specific the statement (i.e., the less it permits and the more it forbids), the more valuable the theory: "Every 'good' scientific theory is a prohibition: it forbids certain things to happen. The more a theory forbids, the better it is."[16] A falsification of the theory takes place when one of the forbidden events is observed to occur. So, a good scientific theory is also a theory that is relatively easy to falsify: because it forbids many more events than it permits, it actually helps us to specify tests that, if successful, would refute it.[17]

A good theory, therefore, makes a bold statement and takes great risks:

[16] Ibid.

[17] Thus, for an object moving at a certain speed in a given time period, a theory stating *The distance is the product of speed and time* is better than one stating *The greater the speed, the greater the distance*. The number of events permitted by the theory (i.e., the *correct* combinations of values) is much smaller in the first case than in the second; and the number of events forbidden by it (i.e., the *incorrect* combinations of values) is much larger. This difference is what makes the first theory easier to test, and hence, if invalid, to falsify. So this difference is what makes it more valuable.

"Testability is falsifiability; but there are degrees of testability: some theories are more testable, more exposed to refutation, than others; they take, as it were, greater risks."[18] (We hope, of course, that these tests will fail and the theory will be accepted. But the failure or success of tests, and the consequent acceptance or rejection of the theory, is a separate issue. The criterion of demarcation merely prescribes that such tests be possible.) Whereas a good scientific theory forbids a great deal, a pseudoscientific theory forbids little or nothing: any conceivable event belongs to the category of permitted events. Thus, it takes no risks. Nothing can falsify it. It is worthless precisely because it appears to work all the time: "A theory which is not refutable by any conceivable event is non-scientific. Irrefutability is not a virtue of a theory (as people often think) but a vice."[19]

Recall the problem of the growth of knowledge (the fact that we can never be certain of the validity of our current knowledge) and the conclusion that the only way to progress is by trial and error. Since we cannot *prove* our theories, we must accept them with caution; we must doubt them, try to show that they are wrong, and continue to search for better ones. Seen from this perspective, a theory that cannot be falsified is a dead end: because we cannot show that it is wrong even if it is, we can never reject it; we must accept it on faith, so it is not a scientific idea but a dogma.

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Published in 1934, Popper's principles of demarcation were misunderstood and misinterpreted from the beginning. Nevertheless, these principles are well known today, and are often used to expose pseudosciences. Most philosophers and scientists respect them. At the same time, we notice that few of us actually use these principles to decide whether to accept or reject a theory; that is, few of us seriously attempt to falsify our theories by subjecting them to severe tests. The mistaken belief that we must prove a theory by searching for confirmations continues to guide our decisions; and, incredibly, it affects even academic research.

It is easy to see the reason for this delusion. We tend to fall in love with our theories. We cannot bear to see them criticized. And it is even more difficult to accept the idea that it is *our* responsibility, if we are serious workers, to attack our theories. It takes a great deal of intellectual integrity, which most of us lack, to consciously design tests through which we may refute our own ideas. So, although we appreciate the falsification principle, we find it hard to adhere to it. In the end, we succumb to the temptation of *confirming* evidence.

[18] Popper, *Conjectures and Refutations*, p. 36. [19] Ibid.

Another reason why we cannot trust verifications is that our observations are subjective and open to interpretation: "Observations are always collected, ordered, deciphered, weighed, in the light of our theories. Partly for this reason, our observations tend to support our theories. This support is of little or no value unless we consciously adopt a critical attitude and look out for refutations of our theories rather than for 'verifications.'"[20] In other words, we must design our tests in such a way that their success would constitute a *falsification* of the theory, not a confirmation. The observations collected in a particular test are significant only if that test sought to *falsify* the theory; they are meaningless when the test sought to *confirm* it. Thus, "every genuine *test* of a theory is an attempt to falsify it, or to refute it."[21]

Moreover, we must specify the nature of the tests, and which results should be interpreted as confirmation and which ones as falsification, at the time we propose the theory – and then *stay* with these criteria. This reduces the temptation to avoid tests found later to falsify the theory, or to modify the theory to fit the results of tests: "*Criteria of refutation* have to be laid down beforehand; it must be agreed which observable situations, if actually observed, mean that the theory is refuted."[22]

Popper stresses an important aspect of the testing procedure: the requirement for "*the severest tests we have been able to design*" and for "*our sincere efforts to overthrow*" the theory.[23] Only if we resort to such severe tests and sincere efforts does their failure count as an indication of the theory's validity. Popper calls these results *corroborating evidence*: each failed test provides additional support for the theory (although, of course, not a proof). The qualities "severe" and "sincere" in these requirements are not subjective assessments of the researcher's attitude; they are exact, technical concepts.[24] Specifically, they mean that only *comprehensive* attempts to falsify the theory count as tests; that is, only tests which, given all current knowledge, are the most likely to falsify the theory.

[20] Popper, *Aim of Science*, p. 164.

[21] Popper, *Conjectures and Refutations*, p. 36. Popper appears to be using the terms "falsify" and "refute" interchangeably. Although the difference is often subtle, in this book I use "falsify" for the individual tests, and "refute" for the theory as a whole. Since one falsification suffices to refute it, a theory that is "falsifiable" is also "refutable," and if "falsified" it is also "refuted"; but the two terms still refer to different aspects of this argument.

[22] Ibid., p. 38 n. 3.

[23] Both quotations are from Popper, *Scientific Discovery*, p. 418.

[24] Karl R. Popper, "Replies to My Critics," in *The Philosophy of Karl Popper*, vol. 2, ed. Paul A. Schilpp (La Salle, IL: Open Court, 1974), p. 1079.

4

Before continuing this study, let us pause for a moment to reflect on the significance of what we have learned. For, we can already recognize how far Popper's principles are from the *actual* way we accept new ideas and theories. We have been aware of these principles for many years, and it is an indication of the irrationality and corruption of our present-day society that we continue to base our decisions on confirmations rather than on falsifications.

It should be obvious that we must apply these principles, not only to scientific theories, but also to everyday personal and business decisions; for example, to the adoption of a new product. Products are in effect theories, not unlike the theories of empirical science, insofar as they make certain claims – claims that can be verified or falsified through experiments. So, if we want to make the best decisions possible from the knowledge available to us, we must follow the same methods when considering a new product as we do when considering a scientific theory. Since many of these products greatly affect our life, there is no reason to treat them less seriously than we do our scientific theories.

The methods employed in promotional work like advertising and public relations offer a striking example of fallacious decision-making principles. Promotions are based entirely on *confirming* evidence – typically in the form of testimonials, or case studies, or success stories. These promotional devices describe a few applications of a product, asking us to interpret them as evidence of its usefulness. Most people believe that the issue here is one of veracity: if the claims are honest, the product must indeed be as useful as it appears. But the honesty and accuracy of the claims are irrelevant, since, from Popper's principles, the very idea of assessing the usefulness of a product by means of confirming evidence is unsound. (Still, it is worth noting that, technically, the use of isolated testimonials or success stories *is* dishonest, even if the claims themselves are true. It is dishonest because it does not include the *whole* truth – i.e., all pertinent cases; and this omission is, logically and legally, equivalent to lying. The similarity of this argument to Popper's principles is hardly coincidental: since these principles are based on logic, a claim that ignores them does not reflect reality, so it is necessarily untrue.)

We see this type of promotion everywhere: in books and periodicals, on radio and television, for ordinary consumer products as well as major corporate and government issues. From pain remedies to management theories, from fitness gadgets to software systems, this type of promotion is so prevalent because it is effective; and it is effective because it exploits our natural tendency to draw general conclusions from the observation of a small number of events – the same tendency that leads us, as we saw, to develop superstitions as readily as we develop sound theories.

But from Popper's principles we know that confirming instances prove nothing, that it is the *falsifying* instances that we must examine. What this means in practice is that the successes may be due to some unusual conditions. So we could learn a lot more by studying the *failures*. We might find, for example, that the failures exceed by far the successes, or that our situation resembles more closely those situations where the product failed than those where it succeeded.

Instead of being deceived by these promotional tricks, then, we could use them to our advantage. For, now we know that the promoters select a few confirming instances precisely because this is the only evidence they have, because the product is *not* as useful as they claim. We know that if they were honest, they would seek and discuss the *falsifying* instances – of which there are always thousands.

The link between promotions and theories is easy to recognize when we examine the *way* the promoters present their products and the *way* we assess them. The promoters propose, in effect, a theory – the theory that a given product has certain qualities and provides certain benefits; and we, on our part, develop in our mind a similar theory about its qualities and benefits. Like all theories, this theory makes certain claims and predictions regarding future situations and events; for example, the prediction that certain operations would be performed faster, or better. Logically, therefore, both the promoters and we must accept or reject this theory, not by searching for confirmations, but by subjecting it to severe tests: by sincerely attempting to falsify it. We would then accept it – we would adopt, that is, the product – only as long as we cannot falsify it, only if it survives the harshest possible criticism.

Not only do we not observe this principle, but we ignore the many falsifications (situations where the product does not work as expected) that present themselves even without deliberate testing. By ignoring these falsifications, or by belittling their significance, we render in effect the theory unfalsifiable: we accept its claims and predictions in an act of faith. Our decision-making process when adopting a product on the basis of confirming instances is, thus, an irrational act, just like accepting superstitions.

Even more disturbing is that we find this fallacy – relying on confirmations – in the most respected sources. What is the most common method of deception in advertising is also found in professional, business, and even academic publications. Articles that purport to inform or educate us, for example, are little more than stories about specific situations. Decades after Popper has shown us why we must base our decisions on falsifications, our entire culture continues to be founded on the absurd search for confirmations. It seems that we have given up the quest for knowledge and reason, and have resigned ourselves instead to our natural tendency to irrationality.

The most blatant demonstration of this irrationality can probably be found in the world of software and programming, which, because of widespread ignorance, resembles the world of primitive man. (We studied the similarity of software-related beliefs to primitive beliefs in "Anthropology and Software" in the introductory chapter.) Respected trade and business publications routinely extol the merits of software concepts on the strength of isolated success stories. Thus, while Popper's principles state that *one* falsification suffices (logically, at least) to refute a concept, thousands of falsifications lie all around us (instances where a software concept was *not* useful) without even being mentioned in those publications. What ought to be the most important evidence in assessing a given concept – the failures – is deliberately ignored.

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If the method of selecting ideas and theories through criticism – by attempting to falsify them rather than confirm them – appears to us too severe, it may help to remember that we only feel this way about our *current* theories. We find this method perfectly logical when judging *old* theories, which have already been discredited. It is when recalling those theories that we appreciate the wisdom of Popper's principles, because with old theories we have no difficulty recognizing how absurd is the method of searching for confirmations.

Consider, for example, geocentrism – the theory that the earth is the centre of the solar system and the universe. When we believed that the planets, the sun, and the stars revolve round the earth, we had no difficulty *confirming* this theory. After all, everything in the sky appears to move, and the ground under us appears stationary. For centuries the idea that the earth is rotating and flying through space was ridiculed. So how did we eventually reject the wrong theory and accept the heliocentric one? We did that by noting the *falsifications* of geocentrism, not its confirmations; that is, not by dismissing, but by *studying*, the discrepancies between the phenomena predicted by the theory and those actually observed. Looking back, we can easily see now that the *only* way we could progress past our geocentric delusion was by ignoring the confirmations and accepting the falsifications. Had we continued to test the theory by searching for confirmations, we would be discovering confirming instances to this day, and we would still believe that the planets and the sun are moving round the earth. And the same is true of *all* knowledge: we can only make progress by taking the falsifications of our theories seriously – indeed, by *searching* for falsifications.

It is also interesting to note that serious programmers, even if they have never heard of Karl Popper, scrupulously apply the falsification principle when testing their software. A new piece of software is similar to a theory in empirical science, in that it makes certain claims about some events – claims that can be tested through experiments and observation. Specifically, we predict that, given certain data, certain effects will occur when using the software. Thus, similarly to a theory, we assess a new piece of software by subjecting it to tests: we *accept* it as long as our tests *fail* – fail, that is, to contradict the predictions; and we *reject* it if the tests *succeed* – succeed, that is, in refuting its claims. (The rejection is only temporary, of course: we modify the software to correct the errors – creating, as it were, a new theory – and then we repeat the tests.)

It is easy to see that this testing procedure amounts to an implementation of Popper's falsification principle: we don't test the software by searching for confirmations, but by trying to falsify it. Even when an application has many errors, there are countless situations where it runs correctly; in other words, situations that *confirm* the claims made by the software. But, while it is gratifying to see our new software run correctly, we understand that it is silly to restrict testing to these situations. We all agree that the only effective way to verify software is by specifically searching for those situations where deficiencies may be found; in other words, those situations most likely to *falsify* the claims made by the software. Imagine testing software by searching for confirmations; that is, restricting ourselves to situations where it runs correctly, and avoiding situations where it may fail. We would never find errors, so the application would appear perfect, when in reality it would be unverified, and hence worthless.

The reasons for accepting or rejecting theories, or concepts, or products are very similar logically to the reasons for accepting or rejecting new software. Thus, to recognize the absurdity of accepting concepts and products on the strength of confirmations – testimonials, case studies, success stories – all we need to do is imagine what it would be like to accept a new piece of software by testing only those situations where we already know that it is correct.

5

To summarize, two principles make up Popper's criterion of demarcation between scientific and pseudoscientific theories: first, the theory must be *falsifiable* (an unfalsifiable theory cannot even be considered, because we have no way to test it); second, we accept a theory because it passes tests that attempt to *falsify* it, not because we find confirming evidence. If we remember these principles, it is not difficult to recognize pseudoscientific theories and irrational ideas, because what their defenders do is cover up the fact that they are being falsified; and the only way to accomplish this is by disregarding the two principles.

We all wish our theories to be proved right; that is, to remain unfalsified when exposed to the reality of tests and criticism. But unlike good theories, which remain unfalsified because they are useful and make correct predictions, the unscientific ones remain unfalsified thanks to the dishonest stratagems employed by their defenders: they are made unfalsifiable from the start, or become unfalsifiable later.

The simplest way to avoid falsifications is to make the theory unfalsifiable from the start. This is typically done by formulating the claims and predictions in such a manner that they cover most eventualities, so the theory cannot be effectively tested. Thus, the claims are so vague that almost any subsequent event appears to confirm them. The fact that the theory cannot be tested – and therefore is never falsified – makes it look successful, but we already saw the fallacy of accepting a theory when all we have is confirmations. A theory is successful when it *passes* tests, not when it *avoids* tests.

Popper uses Freud's and Adler's psychoanalytic theories as examples of theories that were unfalsifiable from the start.[25] It is important to emphasize again that the issue here is not whether these theories are valid, but whether, in the absence of any means to test them, they are scientific; in other words, whether we can rely on their interpretations. There probably is a great deal in them that is important. Few question, for instance, the concept of an unconscious mind, or that childhood experiences affect us later in life. And, on the whole, no one denies that these theories have contributed greatly to our understanding of human behaviour. However, "those 'clinical observations' which analysts naively believe confirm their theory cannot do this any more than the daily confirmations which astrologers find in their practice. And as for Freud's epic of the Ego, the Super-ego, and the Id, no substantially stronger claim to scientific status can be made for it than for Homer's collected stories from Olympus. These theories describe some facts, but in the manner of myths. They contain most interesting psychological suggestions, but not in a testable form."[26]

The most common stratagem, however, is not to make a theory unfalsifiable from the start, but to make it unfalsifiable later. Most pseudoscientific theories start by being falsifiable, and thus indistinguishable from the scientific ones. They are, therefore, testable. But when in danger of being falsified by certain events, their defenders find a way to save them. One can save an invalid theory

[25] Popper, *Conjectures and Refutations*, p. 37.

[26] Ibid., pp. 37–38.

by avoiding tests, or by testing it without sincerely attempting to refute it, or by studying only situations that confirm it, or by ignoring the falsifications (claiming that the tests were wrong, or belittling their significance).

Although crude, these stratagems are quite effective. I will not dwell on them, though, for it is the more sophisticated stratagems that we want to examine: those employed, not by propagandists, advertisers, or irrational people, but by the academics and the experts who create the new pseudosciences. The trick they use is to suppress the falsifications as they occur, *one at a time*. And they suppress them by *modifying* the theory; specifically, they *expand* the theory so as to make the falsifying situations look like a natural part of it.

Thus, while the theory remains testable and falsifiable *in principle*, it is rendered unfalsifiable *in fact*, by incorporating into it every falsifying situation. What the pseudoscientists are doing is *turning falsifications of the theory into new features of the theory*. This stratagem may be difficult to detect, because the theory appears, at any given moment, very similar to the serious, scientific theories. It only differs from them when threatened by a falsifying situation. At that point, rather than being abandoned, it expands so as to swallow that situation – thus eliminating the threat. This task accomplished, it appears again to be a serious theory – until threatened by another falsifying situation, when the same trick is repeated.

Popper called the tricks used to avoid falsifications "immunizing tactics or stratagems,"[27] since their purpose is to immunize the theory against falsifications. Popper anticipated some of these tactics, but recognized that new ones can be easily invented.[28] He singled out the stratagems that modify a theory in order to make it correspond to the reality that would have otherwise refuted it – the trick I have just described. We will examine these stratagems in detail later, when discussing specific pseudosciences.

To combat these stratagems, Popper added a third principle to his criterion of demarcation: a theory, once formulated, cannot be modified. If we want to modify our theory (to save it from being falsified by evidence), we must consider the original theory refuted and treat the modified one as a *new* theory: "We decide that if our system is threatened we will never save it by any kind of *conventionalist stratagem*.... We should agree that, whenever we find that a system has been rescued by a conventionalist stratagem, we shall test it afresh, and reject it, as circumstances may require."[29]

[27] Popper, "Replies to My Critics," p. 983. (Popper attributes this phrase to Hans Albert.)

[28] Popper, *Scientific Discovery*, pp. 81–82.

[29] Ibid., p. 82. "Conventionalist stratagem" is the term Popper used earlier, before adopting "immunizing stratagem." It derives from the conventionalist philosophical doctrine, which holds that a theory may be used even if falsified by observations (ibid.).

Recall the interpretation of theories as statements that permit certain events and forbid others. Recall also that a good theory makes very specific claims, and hence permits relatively few, and forbids most, events. Falsifying events are those events that are forbidden by the theory but do occur. Since a pseudoscientific theory forbids little or nothing, almost any event is compatible with its predictions; and consequently, it has little empirical value. Viewed from this perspective, stratagems that modify a theory in order to suppress the falsifying events reduce the number of events the theory forbids. They succeed in rescuing the theory from refutation, but at the price of reducing its value. A theory may start by making bold claims, but if it is repeatedly expanded so as to transfer previously forbidden events (which are now found to falsify the claims) into the category of permitted events, its empirical value is no longer what it was originally. It becomes increasingly unfalsifiable (that is, permits more and more events), and eventually worthless – no different from those theories which are unfalsifiable (that is, permit most events) from the start.

Popper uses Marxism as an example of theories that start by being falsifiable but are later modified by their defenders in order to escape refutation. Some of Marx's original ideas were serious studies of social history, and as such they made predictions that were testable. It is, in fact, because they were testable that they were falsified by subsequent historical events. The events, therefore, *refuted* the theory. "Yet instead of accepting the refutations the followers of Marx re-interpreted both the theory and the evidence in order to make them agree. In this way they rescued the theory from refutation; but they did so at the price of adopting a device which made it irrefutable … and by this stratagem they destroyed its much advertised claim to scientific status."[30]

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It is always unscientific to trust a theory unconditionally; and it is this dogmatic belief that prompts its defenders to try to rescue the theory, even at the risk of turning it into a pseudoscience. We can understand now even better the requirement to doubt and criticize our own theory, to subject it to tests that sincerely attempt to refute it. Clearly, the immunizing stratagems – which aim to suppress falsifications – violate this requirement, and hence exclude the theory from the domain of science. Scientists know that they must *doubt* and *attack* their theory; pseudoscientists think their task is to *defend* their theory.

Because they do not question the validity of their theory, the pseudoscientists are bound to interpret a falsification as an insignificant exception. They

[30] Popper, *Conjectures and Refutations*, p. 37.

feel justified then to modify the theory to make it cope with that situation. They do not deny that the theory is deficient; what they deny is that it has been refuted. They don't see the modification of the theory as a dishonest move, but as an improvement. They believe that only a few such exceptions exist, and that their effort to make the theory match reality constitutes serious research work.

This delusion is enhanced by the fact that the falsifications are discovered *one at a time*; so each falsification looks like a small problem, and also like the only one left. But in the case of pseudoscientific theories there is no end to falsifications. The reason these theories keep being falsified is that their claims are fantastic, and thus unattainable. Pseudosciences typically attempt to explain a complex phenomenon through some relatively simple concepts. Since the simple concepts do not work, the falsifying events are not exceptions but an infinity of *normal* occurrences. By the time the theory is modified to cope with them all, there is nothing left of the simplicity and exactness it started with.

The only way the pseudoscientists can deal with these "exceptions" is by incorporating them into the theory. And they accomplish this by contriving various extensions, which they describe as enhancements, or new features. The extensions, thus, are only needed in order to bring the falsifying events into the realm of events that the theory can be said to explain. So their true effect is not to *improve* the theory, but to *degrade* it – by reducing its rigour and precision. In the end, the patchwork collection of features ceases to be a theory. Its defenders, though, still fascinated by the beauty of their original fantasies, continue to believe in it and to expand it.

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All mechanistic *software* theories, we will see in this book, start by being testable and falsifiable but are later modified in order to suppress the falsifications. Consider, for example, the theory behind the relational database model (we will study it in detail in chapter 7). This theory started by claiming that, if we separate the database from the rest of the application, and if we agree to "normalize" our files, we will be able to represent the database structures with a formal system. This will ensure that the result of database operations reflects accurately the stored data. Moreover, we will access the data from a higher level of abstraction: every database requirement will be expressed simply as a mathematical combination of relational operations. Ultimately, the relational model will turn database programming into an exact, error-free activity.

Now, an experienced programmer would immediately recognize the absurdity of this concept, without even using it in a real application: since the database structures interact with the other structures that make up the application, and since most of these interactions occur at the low level of records and fields, it is impossible to separate the database operations from the other types of operations, or to raise their level of abstraction. So the relational model was essentially an academic concept, unconcerned with reality. For example, in order to restrict programming to high-level operations on normalized files, it had to assume that processors and disk drives have infinite speed.

Still, despite its absurdity, the relational model was a falsifiable theory. And if one does not expect practicality from software concepts, it was even an interesting theory. It became testable when software companies decided to implement it in actual database systems – systems intended to serve real business requirements. At that point, needless to say, it was refuted. But instead of studying the reasons and admitting that the relational model has no practical value, its advocates started a long series of "enhancements": they suppressed the falsifications by expanding the model so as to include the falsifying situations. This made the theory unfalsifiable, and the relational database model became a pseudoscience.

Strict data normalization, for instance, was found to be impractical, so the concept of "denormalization" was introduced; now a file could be either normalized or not – a reversal of a fundamental relational principle, and a return to the pragmatic criteria of the traditional design methods. Then, separating the low-level database entities from the other entities of the application was found to be impractical, so various alternatives were introduced in the guise of new relational features. The purpose of these "features" was to move more and more parts of the application into the database system, and thereby reduce the need to link the database structures to the application's other structures.

In the end, as the invention of a new relational feature to suppress each new falsification proved too cumbersome, SQL, a simple database language originally intended just for queries, was expanded into a *programming* language. Although quite primitive in its new role, SQL allows us to deal with most falsifications through the traditional expedient of programming. In other words, SQL restored in a complicated and roundabout manner the low-level links between database entities, and between database entities and the other types of entities – links which the traditional programming languages had been providing all along. So SQL, while described as a relational database language, is not used to *implement* the relational principles, but to *override* them. For example, the relational model restricts us to manipulating whole files or logical portions of files, and SQL permits us to bypass this restriction: now we can specify operations for individual fields and records, and we can control these operations by means of conditional and iterative constructs, just as we do when using the traditional file operations.

Thus, there is nothing left of the idea of high levels in today's relational database systems. As it turned out, the restriction to relational operations is much too awkward and inefficient. Nor is there anything left of the promise to separate the database structures from the application's other structures. In fact, most modifications made to the relational model were prompted by the need to restore the low-level interactions between these two types of structures. So the relational model could be rescued only by expanding it to include the very features it had originally excluded, and from the exclusion of which it had derived its simplicity and formality.

If this fact escapes the notice of software practitioners, it is because the features were given new names. Thus, the relational theory also exemplifies another practice frequently found among pseudoscientists: the use of new and pompous terminology for the features introduced to suppress falsifications. This trick serves to mask the fact that these are not new features at all, but reversals of claims: reinstating well-known and indispensable concepts, which the theory had attempted to eliminate in its quest for simplicity.

Recall Popper's principle that a theory which was modified to escape refutation must be treated as a different theory: its claims must be assessed afresh, and it must be subjected to the same severe tests as a new theory. Imagine now that the relational concepts were introduced for the first time today, as they are found in the latest relational systems. In other words, we would be exposed from the start to the complexity and inefficiency of these systems, and we would have to assess their *current* benefits and drawbacks. It is safe to say that no one would see the point in adopting these systems. It would be obvious to everyone that the so-called relational model is merely a more complicated variant of the *traditional* database model. The relational theory established its reputation through its *original* claims – precisely those claims that had to be abandoned later in order to save it.

The relational theory, thus, exemplifies yet another deceptive practice employed by pseudoscientists: advertising the original benefits even after the theory was modified, its principles were forsaken, and those benefits were lost. The chief claim of these theories is that they enable us to solve complex problems with relatively simple methods. It is this combination of power and simplicity, this promise to give us something for nothing, that makes them so enticing. (The relational theory, for example, did not promise any capabilities that the traditional, low-level file operations did not already provide. What it claimed was that the restriction to high-level operations made the database concept so simple that inexperienced programmers too would enjoy those capabilities.) And when this promise proves later to be a fantasy, when the theory is expanded to incorporate the falsifying situations and thereby loses its simplicity, its advocates continue to defend it by invoking the benefits of the *original* theory. But all we have to do is remember the principle that a modified theory must be treated as a different one, to recognize the dishonesty of this stratagem.

6

Popper's criterion of demarcation is one of the most important contributions to the theory of knowledge and to the philosophy of science. It provides an excellent solution to the problem of distinguishing between science and pseudoscience – between true research and the pursuit of delusions. It is a sad reflection on our civilization, therefore, that in the period of time since these principles were discovered, our delusions have been multiplying and flourishing at an even higher rate than before. We can observe this not only in the traditional pseudosciences (which attract the ignorant and the gullible), or in the useless products and concepts (which are, as we saw, similar logically to pseudosciences), but also in the new pseudosciences, including the software pseudosciences, which are practised in universities and corporations. We must take a moment to investigate this phenomenon.

When there exists a simple method for recognizing worthless pursuits, one would expect scientists to adopt it and to rely on it in their work. And when this method can detect delusional thinking, not only in scientific research but in all our affairs, one would expect scientists, philosophers, and teachers to explain it to the rest of us. The intellectuals have failed in this task, however, and have left the education of society to advertisers, propagandists, and charlatans. But, what is worse, they have ignored the method even in their own work. As we will see later in this chapter, academic research means today, more often than not, simply the pursuit of a mechanistic idea. And when the idea turns out to be a fantasy, research becomes the pursuit of a pseudoscience: looking for confirmations and for ways to cover up the falsifications. What masks this degradation is the formal tone in which these worthless activities are reported.

Popper's demarcation principles are well known, of course. But instead of being used to assess theories, concepts, and products, they have become a topic of debate in the philosophy of science.[31] The debates involve issues such as these: When facing a falsification, how can we decide what is closer to the

[31] For examples of these debates, see Paul A. Schilpp, ed., *The Philosophy of Karl Popper*, 2 vols. (La Salle, IL: Open Court, 1974).

truth, the claim that the theory was falsified or the claims made by the theory? Or, when two theories remain irrefutable, or when both are refuted but in different degrees, how can we determine which theory is the better one? Popper recognized these problems, and he addressed them on many occasions. But it is important to understand that these philosophical subtleties are irrelevant when we employ his principles only as a criterion of demarcation, which was his original intent. We don't have to concern ourselves with any fine points when all we want is a way to recognize as early as possible a worthless idea. Thus, no philosophical subtleties can vindicate the frauds we are studying in this book. (Could the settlement of such issues correct the fallacies of behaviourism, or universal grammar, or structured programming, or the relational database model? The principles of demarcation expose the uselessness of these theories no matter how we choose to interpret the finer points.)

To understand why the academics prefer to ignore the true significance of Popper's principles, let us imagine what would happen if they applied these principles to their theories. Clearly, if they did that, most research work in disciplines like psychology, sociology, linguistics, economics, and programming would have to be classified as pseudoscientific. Thus, their failure to observe the demarcation principles doesn't stem from a concern with the finer philosophical points, but from professional dishonesty. They cannot *afford* to recognize the importance of these principles; for, if they did, they would have to admit that most of their work is not scientific research but a pursuit of mechanistic fantasies.

The mechanists are in an awkward position: they cannot *reject* Popper's principles, because their validity is obvious; but they cannot accept them either. So they resolve this conflict by debating their philosophical meaning instead of studying their practical applications. In addition, they deliberately *misinterpret* the principles. Some scientists and philosophers, for example, misinterpret them as a method of discovering new theories, and conclude that they are inadequate for that purpose; others misinterpret them as a study of the social and psychological aspects of scientific discovery, and end up treating them as an alternative to Kuhn's famous theory of scientific revolutions.[32] Popper's ideas were misinterpreted, in fact, even when judged by their fundamental philosophical aspects; for example, they were mistakenly viewed as merely an attempt to base the criteria of meaningfulness on falsifiability instead of verifiability.[33]

[32] Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago: University of Chicago Press, 1970).

[33] See, for example, Popper, "Replies to My Critics," pp. 967–974.

So Popper had to spend much of his career explaining again and again his demarcation principles, correcting the misinterpretations, and repeating over and over what he had said clearly in his original writings. He called the mistaken views "the Popper legend": "There were books in which I had protested against the various parts of the legend, and older books and papers to which I referred in these protests, and which needed only to be read to disprove the legend. Nevertheless, the legend grew, and it continues to grow."[34] And regarding his solution to the problem of induction, he remarks: "Few philosophers have taken the trouble to study – or even to criticize – my views on this problem, or have taken notice of the fact that I have done some work on it. Many books have been published quite recently on the subject which do not refer to any of my work, although most of them show signs of having been influenced by some very indirect echoes of my ideas; and those works which take notice of my ideas usually ascribe views to me which I have never held, or criticize me on the basis of straightforward misunderstandings or misreadings, or with invalid arguments."[35]

The most common error, and the most important, is to interpret his criterion of demarcation as a requirement to *actually* falsify a theory through experiments, when in fact it is the requirement for a theory to be falsifiable *in principle*: we must be able to specify some conditions that, *if* they occurred, would falsify the theory. (Of course, if we can design *actual* tests, so much the better.) Popper agrees that it may be impossible to actually falsify a theory, but he stresses that this is not required by his demarcation criterion: "An entire literature rests on the failure to observe this distinction.... And the difficulties, in many cases the impossibility, of a conclusive practical falsification are put forward as a difficulty or even impossibility of the proposed criterion of demarcation.... This would all be of little importance but for the fact that it has led some people to abandon rationalism in the theory of science, and to tumble into irrationalism. For if science does not advance rationally and critically, how can we hope that rational decisions will be made anywhere else? A flippant attack on a misunderstood logical-technical term [falsifiability] has thus led some people to far-reaching and disastrous philosophical and even political conclusions."[36]

[34] Popper, "Replies to My Critics," p. 963.

[35] Karl R. Popper, *Objective Knowledge: An Evolutionary Approach*, rev. ed. (Oxford: Oxford University Press, 1979), p. 1.

[36] Popper, *Aim of Science*, pp. xxii–xxiii.