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Chapter 2

TOWARDS A THEORY OF AWARENESS AND OF CONSCIOUSNESS

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ABSTRACT

Consciousness is repeatedly observed, thus deserving efforts to explain it scientifically. Early studies of it came to prove it illusory. These are irrelevant, as persistent illusions invite explanation: why do they persist? And how do we distinguish between conscious and subconscious decisions, and between decisions and self-deception? Efforts to overcome Hume's criticism of the theory of knowledge by limiting it to conscious thinking raise interest in consciousness.

Nevertheless, the view is popular that consciousness is scientifically irrelevant. This, too, invites explanation.

Traditional psychology of learning rightly begins with the study of proper research. It demands that research should rest on no presuppositions. This is impossible. When in trouble, researchers try to articulate some of their tacit presuppositions so as to improve them. This is vital for the progress of science. The most significant supposition is that perception is passive. It is amply refuted yet it stays popular. And prevents the understanding that consciousness is a dimension of perception that is irreducible to other qualities of perception and so it requires special attention. For that criticism of received theories of perception and of

consciousness are vital, especially the view that scientific controversy is objectionable. For, properly run controversy always boosts science.

Removing the obstacles on the way to progress in research on consciousness is not enough but is all that is attempted here. This paper ends with a refutation of the idea that Turing's test vindicates his dismissal of consciousness as inviting research.

PREFACE

Consciousness is a common experience. Awareness is either consciousness or the disposition to be conscious. Thus, studies of awareness usually come under studies of consciousness. My example for these studies is the whole field of speed reading that is almost entirely devoted to aspects of the consciousness of reading and of its outcome. The demand to ignore consciousness (as subjective and thus allegedly as not objective and thus inferior) rests on the erroneous disregard for the fact that the subjective world is a part of the world that we wish to understand. The central scientific convention regarding experience, the sole convention that the scientific community endorses unanimously ever since its establishment during the scientific revolution, is this: every repeated and repeatable observation is scientific and invites scientific explanation. Repeated observations concerning consciousness thus invite scientific explanations. These invite the construction of some framework that should guide research into consciousness. Towards this let me draw attention to errors popular in the research world that impede it.

Explanations of observations, obviously, may deny existence to their objects. For example, science denies existence to the objects of mirages and of hallucinations. Explanations of mirages present them as manifestations of an optical illusion plus the understandable self-delusion of their very thirsty observers. Hence, to explain mirage is to refer to consciousness. This makes current explanations of human conduct different from, say, the current explanation of the attraction of moth to fire that is presumably automatic. This difference annoys some thinkers and delights others. We may try to study of this fact too.

CASE STUDY: SPEED READING

Speed reading developed as soon as the tachistoscope showed that reading is very fast: images of words thrown on a screen for a duration order-of-magnitude far shorter than that of the perception threshold (one-tenth of a second) are read with ease. This raised problems. The theoretical problem was neurological: what makes different threshold phenomena so different in magnitude?[1] The practical problem is, how can this help improve the speed of reading? Various impediments to reading thus became apparent. The biggest impediment turned out to be readers' use of muscles other than those that move eyeballs, especially of the neck and of the lips: they get tired fast. Surprisingly many people waste energy this way *unawares*. They must become *aware* of this before they can *try* to improve. It is far from easy to help them do that. The easiest way to make people *aware* of this is to photograph them and show them their *unconscious* waste of effort. The second impediment to improvement is the *reluctance* to skim that educators instill in *effort* to enhance the *care* of *careful* reading in order to make the reader fully *conscious* of what they *read*. To overcome this, readers have to become *aware* of their skimming habit, and to learn that doing so *aware* is to raise *control* over action. Generally, we possess gearboxes that habitually regulate action ─ including the speed of reading: becoming *aware* of gearboxes and learning to *control* them is immensely beneficial. (Good learning is not of high speed but of variable speed.) The increased *awareness* of our *control* mechanisms improves performances. Next to speed is learning to retain the information accrued in reading. While reading it is advisable to sum up information accrued in short statements. The enhancement of speed, comprehension and memory go together and separately. The information mentioned thus far suffices for the present discussion of *awareness*.

This is not to deny that for some habits awareness is an impediment. This was discussed in great detail and with much added philosophical fluff in Eugen Herrigel's *Zen in the Art of Archery* (Yamada Shōji, 2001). This shows that we can control the degree of our awareness.

EXPLAINING *VERSUS* EXPLAINING AWAY

[1] The puzzle is general for all thresholds. The paradigm case is holding a stick horizontally with both hands, sensing a blow on its middle: the slightest deviation from the middle leads to the proper sensation of the hand near it as first.

The first item on the agenda of any scientific discussion of consciousness is the question, does it exist at all, and does it merit scientific attention? We better ignore this question. Even if consciousness is but an illusion (an epiphenomenon of the brain, to use the philosophical jargon, Searle, 1997; see also Crick, 1994), it invites explanation, of course. The explanation of shadows is the paradigm (of epiphenomena); they do not exist. But geometrical optics explains them to full satisfaction. The explanation of mirages and hallucinations similarly assume their objects non-existent. So we may want to explain consciousness too. Nevertheless, this dual question persists: does consciousness exist and does it merit scientific attention? The best known discussion of it is that of Alan Turing: science can ignore consciousness, he said, since it is irrelevant to the problem whether machines can think (Turing, 1950, page 447). He tried to prove that science can ignore consciousness without loss; received opinion has it (erroneously) that his proof (the Turing test) is conclusive. And so his recommendation that science should ignore consciousness (for a while) was influential: a volume on the topic (Sayre, 1969) and an essay on an adjacent topic (Shaffer, 1965) display the paucity of literature on it then. Sayre suggests conscious computers are possible, although of a kind we still do not possess (Sayre, 1969, p. 170). Shaffer says, a machine that emulates human conduct perfectly is constructible, yet whether perfect simulation is good enough is in doubt (Sayre, 1969, p. 90). It would then be hard to dismiss Turing's assertion, Sayre adds, that science will dismiss this doubt as pointless. Artificial intelligence doyen Marvin Minsky went further: he took consciousness to be no more problematic than the construction of self-monitoring computers, which is obviously unproblematic (Minsky, 1988, Ch. 28 §8, see also Damásio, 1010).

This sounds odd, but it is legitimate. Thus, the explanation of sound sensation as the resonance[2] of bio-acoustic-sensors to waves in the medium keeps the subjective feeling of sound outside the discussion. Similarly, explaining a color sensation as neural photoelectric response does not explain the subjective feeling that accompanies the sensation of color. (We have no idea how to do that.) Hence, the objection that Minsky's explanation is not intuitive is invalid. Still, the question remains: is his explanation true?

Popper and Eccles said, no: consciousness is not passive monitoring but a locus of brain activity. "Our present hypothesis regards the neuronal machinery as a multiplex of … structures: the experienced unity comes, not from a neurophysiological synthesis, but from the proposed integrating character of the

[2] The idea that consciousness is some sort of resonance ─ reverberation ─ within the brain is reverberating in the literature ever since Donald O. Hebb has suggested it in the1940's (Hebb, 1949, p. 73).

self-conscious mind. … [The] self-conscious mind is developed in order to give this unity of the self in all its conscious experience and actions." (Popper and Eccles, 1984, p. 362) There is no need here to decide between Minsky's view and that of Popper and Eccles: even on his view his conclusion that science can ignore consciousness is invalid. The reason is simply that observations of consciousness, and these are many, are repeatedly reported and so they merit explanation. Let me elaborate on this point.

The loss of naïve realism is painful. Some philosophers make great efforts to retrieve it (G. E. Moore, 1924, and Wittgenstein, 1953, §306), whereas others find the loss unavoidable (Russell, 1965, opening). Sir Arthur Stanley Eddington noted (Eddington, 1928, Preface) that quantum mechanics deems illusory the desk in his study: the naïve image of it as a smooth brown surface is false: it comprises an almost empty space in which minute particles swirl at immense speed. Eddington was in two minds about that desk. At times he took its presence for granted and declared physical theory a *façon de parler*; at times he deemed theory a description of the facts more accurate than naïve realism. This duality is resolvable: scientific theories comprise a series of description of reality, where the better theory explains its predecessor as less accurate and less comprehensive. And then all past theories are series of approximations to the truth, not the final truth. Naïve realism and commonsense are then older theories, be they scientific or proto-scientific.

Nevertheless, not all corrections of past views are of the same ilk. Consider this: Aristotle said, both gravity and its opposite, levity or buoyancy, are basic. He thereby dissented from Plato, who denied that levity is basic, considering bodies that buoy up heavy, though less heavy than their environment: low gravity looks like levity. Aristotle rejected this idea and considered it muddled. Archimedes proved him the muddled party. Here is a newer example. Naïve realism and some old theories of physics take as basic both heat and cold. Modern thermodynamics takes them to be mere degrees of concentration of motion-energy of molecules. This is not to deny that heat is there, but to deny the older, important idea that heat is a thing (phlogiston and its heir caloric) as well as the idea that cold is a thing: cold is low degree of heat. Similarly, a debate lasted for about a century as to whether negative electricity exists or whether it is but the low level of positive electricity (as compared to the surroundings). Both positive and negative magnetism were deemed real, until Ampère and Faraday proved magnetic poles illusory. Ampère declared electricity a thing; Faraday and Maxwell did not. J. J. Thomson discovered the electron and thus disproved them all.

Is Eddington's desk the same thing as the magnet that is there but not as a thing? Or is it better ignored as a thing, together with Aristotle's fire? We want science to take account of that desk, and we would not reject it as totally as we reject the phlogiston. Wherever the difference between Eddington's desk and phlogiston lies, it is very important. It is the difference between explaining and explaining away. We thus explain self-delusion, and as we do that we also explain away the ghosts that the deluded observe. Now in many cases it does not matter overmuch whether we choose explaining or explaining-away. Thus, it is well known that Einstein first declared the pervasive ether as non-existent and then spoke freely about the ether, meaning roughly-empty space. i.e., space with forces that it embeds but with no matter. Some people found this confusing, but it is not. Yet not all cases are like that. When we explain away ghosts stories and such we do not leave much room for them just because they are not explanations so that doing away with them is no loss. In the very early days of the scientific revolution the tendency to deny the existence of colors as illusory was popular. They were soon declared proper subject for inquiry as their appearance is repeatable. Later, they were explained as wavelengths. The heat of pepper and the cold of opium were deemed thermal mattes but this was soon explained away once and for all.

AWARENESS OBSERVED

As awareness is the disposition to be conscious, it is naturally as repeatedly observable as consciousness. Unable to offer an explanation for it, I hope that my presentation will help future efforts to explain it: I present it (or rather degrees of it) as a dimension of perception that is irreducible to other qualities of perception, so that it requires special attention. Classical perception theory prevented that, and awareness of this fact enhances the comprehension of perception ─ in experience and in theories alike. This claim is not new. Already in the seventeenth century Leibniz expressed it explicitly: he distinguished between perception and awareness by observing the awareness of the very absence of some perception (Leibniz, 1996, pp. 112-118). The striking fact he drew attention to is that the miller wakes up when the sound of the windmill's engine stops as the wind slacks. This shows that the sleeping miller is not totally unaware of the surrounding, that in sleep awareness is reduced but not shut down. This raises intriguing questions that today engage quite a few researchers, regarding lower animals and regarding patients in comma. Do other animals

possess some degree of awareness?[3] Leibniz claimed that they do, but his answer was a part of a metaphysical view that the scientific community never took seriously: all objects, he said, possess awareness to some degree, even sticks and stones. The problem regarding the state of deep comma is more interesting: is it a total shutdown of consciousness? Some evidence suggests that it is not always so. Discussion on this point illustrates that consciousness (includes expectations) regarding sense experience differs from sense experience. This refutes the perception theory that was generally received until the early twentieth century, as this theory applies equally to humans and other animals, as David Hume noted approvingly (Hume, 1986, pp. 176-9).

The observations that Leibniz made were lost on his contemporaries because he had no perception theory to replace the received one that took its building blocks to be sensations of units of perception (allegedly given to the senses) and their associations. John Locke, George Berkeley and David Hume stressed the fact that they took for granted the sensationalist theory of perception (as applicable equally to all higher animals, Hume added). This plays down the place of awareness in perception. Since that theory was the only one extant, it is understandable that they took it for granted. It was a *tour de force* that Hume stated it explicitly in stark clarity. He did so in order to show that it has no room for causality. This renders all causal assertions theoretical, which is very surprising. Worse, it presents all assertions about causality as not founded on facts, indeed as incapable of finding empirical foundations. The same holds, if less clearly, for Berkeley, who argued ─ rightly ─ that what is a thing and what not is beyond the domain of the empirical as construed at the time. At times it is empirical, for sure, but by a newer view of the empirical. What this shows is incontestable; hence, all arguments that support the claim that causality or "thinginess" is perceived are arguments against the sensationalist theory of perception. Strangely, whereas psychologists are increasingly aware of the defects of the sensationalist theory of perception, philosophers of science adamantly insists on clinging to it. Their reason is that they wish to validate the empirical foundations of science. For, in the wish to prevent circularity in their arguments, they try to present pure perceptions, namely, perceptions free of any theory. These should be observations for science to endorse unhesitatingly, regardless of any controversy within science.

Notoriously, observation-reports are theory-laden. This leads philosophers of science to efforts to strip them of their theoretical bias, namely, of their

[3] Today the hypothesis that other animals are aware is empirically testable by reference to the spindles in their sleep (Dang-Vu *et al*., 2010).

theoretical components. This effort rests on the hypothesis that some perceptions precede theorizing. Notoriously, empirical information refutes this claim: most empirical researchers of perception (unlike most philosophers of science) consider it false. This does not deter most philosophers of science: to evade it they view the precedence of the empirical over the theoretical not a fact but a logical characteristic: the empirical component of ordinary, theory-laden observations is independent of its theoretical component. What these philosophers of science claim need not be questioned, at least not here. What we should here notice is that their theory leaves no need for any discussion of consciousness and no room for it. The casualty of this exclusion is the empirical study of consciousness. Once this enters the equation, the hypothesis that ordinary observation-reports can be divided to the purely empirical and the purely theoretical has been given up completely.

This became the cornerstone of the most influential non-sensationalist theory of knowledge, that of Immanuel Kant. This theory, however, rests on Hume's observation that no theory can rest on empirical foundations. Most philosophers of science therefore ignore it so that his perception theory influenced scientists (Gregory, 1989), whereas philosophers miss it in their eagerness to provide solid empirical foundations for scientific knowledge. To that end they try to pinpoint the weakness in Hume’s argument against the view that this exercise can be successfully concluded. For, they say, we know that Hume was in error, since we know that scientific knowledge does exist and that it is empirical. This knowledge is awareness of sorts and so reference to it leads them straight to inconsistency: they ignore the fact that sensationalism is the error of Hume that they seek.

Hume tried to cope with the situation differently. Finding our theoretical apparatus defective, he tried to find out how we apply our knowledge in practice. He argued that we do so *unawares*: he observed that learning to do something very well amounts to doing it unawares. (The best examples today are things like driving a car or riding a bicycle, but walking is as good an example and to the sensitive just as impressive, perhaps even more so, seeing that we learn to walk before we develop our awareness sufficiently to remember the fact, and seeing that people who spend long periods in bed have to re-learn to walk. More impressively, the ability to stop a car at a gas station to refill its nearly empty tank is improved when it happens regularly utterly unawares.)

The problem that Hume has raised persists: how is theoretical learning from experience possible? Throughout the history of learning theory since Hume raised that problem, his proposed solution to it occurred repeatedly to students of that problem, although possibly with increased clarity. The most recent ones

were those of Gilbert Ryle (Ryle 1949, p. 45) and Michael Polanyi (Polanyi, 1967, p. 4). Polanyi observed that people who apply their knowledge are not always able to describe it in words. It is clearly there, as it is operative, but efforts to articulate it may fail all the same. Polanyi called this "tacit knowledge" and noted that the knowledge that artists possess and transmit to their apprentices is such, and he declared researchers artists, experts in the art of scientific research. Ryle was a member of the school of philosophy that advocates clinging to ordinary language and as such he preferred to present his view as a verbal distinction (although this forced him to use stilted language that is far from the ordinary[4]): he distinguished knowing that [a given assertion is true] from knowing how [to perform a given act properly], tacitly suggesting that the second kind of knowledge is not hit by Hume's critique, as already Hume had explicitly suggested. Hume had added to this, we remember, that the operative kind of knowledge need not involve awareness, that, indeed, the performance of an act improves with the learning to do it unawares and thus unconsciously.

No need to discuss any specific case of awareness or consciousness here: the general case suffices. Consciousness is the general disposition to be aware. It is hard to decide how to begin discussing consciousness: the very choice of a gambit already rests on many presuppositions, and these may be highly problematic and deserve preparatory studies. This trouble is not quite specific to the study of consciousness: it is general within philosophy (Wettersten and Agassi, 1978). It seldom receives the airing it deserves. In science things are obvious: most scientific fields of study take their traditions for granted, and these include traditional presuppositions and the problems that these give rise to and that contributors to the field are invited to take as read. Scientists thus often skip the discussion or even the posing of the problems whose solutions, old or new, they try to present and to discuss; from the very start they consider obvious to their readers the problems that are at issue and their backgrounds. Many papers in philosophy follow this pattern and they are almost never right as their background suppositions and the problems that they discuss are not obvious even to experts.

Thus, most texts that discuss consciousness are troublesome: their presuppositions include the sketch of a view on consciousness that their studies develop. This way they take too much for granted, and, as often happens, the

[4] Ordinary language permits talk about knowing that an act is proper just as knowing how to confirm a truth.

tacit assumptions behind the discussion render their studies good for home consumption only: their authors preach to their parishes.

Can we begin with statements of these assumptions? Clearly it is not always easy to know the assumptions implicit in a given discussion, as the example of the axiom that every geometrician makes and that Moritz Pasch stated only in the nineteenth century (Davis and Hersh, 1981, page 160). Can we at least in principle always make all of our presuppositions explicit? This question was raised only in the twentieth century and only as Ludwig Wittgenstein gave it his affirmative answer (Wittgenstein, 1922, Preface). He stated it emphatically and as a matter of principle, yet at once he qualified it: we can articulate only what is given to articulation, he added (*loc. cit.*). This way he adumbrated an important idea that later authors elaborated on: we have tacit ideas: we may be aware of them but we cannot articulate them. First R. G. Collingwood said so (Collingwood, 1940, pp 33-9: "people are not ordinarily aware of their absolute presuppositions") and then Polanyi did (Polanyi, 1966, "We know more than we can tell"). This is a limitation on reason, of course, as Michael Oakeshott has noted (Oakeshott, 1962, p. 61, "The … situations of normal life are met, not by consciously applying to ourselves a rule … but by acting in accordance with certain habits"). Thomas S. Kuhn went further: when the feeling is overwhelming that the literature is stuck, there is a feeling that a change in the tacit suppositions is in demand. He described this feeling and called the time of its prevalence a revolutionary period and he declared it brief. After it the new tacit suppositions have to be broadcast, yet without being articulated: "universally recognized scientific achievements that for a time provide model problems and solutions" are the vehicles of this broadcast. He aptly called these vehicles paradigms (Kuhn, 1970, page viii).

All this is questionable: the basic presuppositions of a scientific field need not be tacit: at times they are given to explicit statement and to critical discussions, propaedeutic (pertaining to teaching) or heuristic (pertaining to discovery). Often they belong not to science proper but to the adjacent field of the philosophy of science – in general or of a specific science. Most scientific fields of study take their traditions for granted, and these include traditional problems that contributors are invited to take for granted or to criticize (on the presupposition that it is recognized as sufficiently important to discuss critically). Scientific researchers thus often skip the discussion or even the posing of the problems on their agendas, namely, those whose solutions, old or new, they try to present and discuss critically; from the very start they consider obvious to their readers what problems are at issue. The problem of consciousness is an example for this: first it was largely neglected, since

psychology had little room for it. On the contrary, learning theory presented humans and animals as possessing the same learning mechanisms, we remember, and so both language and awareness were given little or no role in learning. Only with the study of hysteria ─ of Charcot and more so of Freud ─ that brought to the limelight subconscious thinking and decisions (Freud, 1896)[5], did conscious thinking and decisions win better attention. Later this led to the study of consciousness as such. It then became the central issue in the studies of some current groups of students of the human mind. This seems to have brought about a consensus of sorts: it is hoped that efforts to find out what determines the constructions of the self and that this will alter social and psychological studies. This is why consciousness has become fashionable in different disciplines. Yet this seeming consensus is misleading. It looks as if the diverse studies of consciousness are complementary; in a sense they are. Yet they come from opposing viewpoints: they bespeak competing philosophies. Their integration toward the emergence of a new encompassing approach to consciousness is perhaps impossible and perhaps it requires a new integrative philosophy, but there is no guarantee for it and there is no argument for the idea that this integrative philosophy is at all possible.

TO UNIFY DIVERSE STUDIES

The situation is odd. The theory of knowledge is supposed to promote objectivity and thus detract from subjectivity and thus allegedly detract also from the study of subjectivity, including consciousness. Yet consciousness is central to this field: scientific research is the activity which is performed as consciously as possible: it is an integral part of the Ego in Freud's theory[6]; authors repeatedly stress the need of researchers to be alert. In particular, in the second half of the twentieth century much attention was given to the question, what presuppositions do researchers rely upon in their researches? Can they be made explicit? Can they be defended? Need they be defended? Etc.

[5] To be precise, Freud does not discuss decision here. In a footnote he says, "I purposely leave out of this discussion the question of what the category is to which the association between the two memories belongs (whether it is an association by simultaneity, or by causal connection, or by similarity of content), and of what psychological character is to be attributed to the various 'memories' (conscious or unconscious)." This shows he was clear about the difficulty to square psychoanalysis with associationism; yet he kept his faith in associations all his life.

[6] In a very famous book (Hadamard, 1945, 52), Jacques Hadamard discusses attempts to control the unconscious ; he also refers to both Poincaré and Einstein as aware of the role of unconscious thinking in their work.

There is one famous presupposition that is taken to be common to all researchers: nature is law-abiding. This presupposition interests those who wish to justify science. Others stress presuppositions in research that are not universally endorsed. This diversity seems too obvious. Yet when historian of science Kuhn observed it, he won world fame overnight. His catch-word, "paradigm", became a keyword that is here to stay despite his having admitted that it is too vague for comfort and despite his decision to cease using it. Since his contribution is so very influential, it deserves a careful exposition. "Paradigm" was initially his word for an example of a case of scientific success that researchers allegedly emulate, we remember. These exist only in some fields of research, and he declared scientific all of them and only them. Why do some fields have paradigms and other not? Why does astronomy always command theoretical consensus ─ is allegedly run by paradigms ─ and psychology never does? No answer. No matter. The main thing according to Kuhn is, paradigms change, but not the unanimity with which they rule. What then is unanimity good for? Kuhn hardly noted this question. When he did, he said, unanimity is about the question what paradigm is the most suitable? Unanimity is therefore the most efficient way to go about a paradigm once it is there. This seems reasonable. It is a serious error nonetheless: given two complementary paradigms, it may be useful to have some researchers follow this and others follow the other. Kuhn even agreed to this: there may be two paradigms, he admitted, and even more. Thus, whereas traditional lore takes every theory that commands unanimity within science to keep this quality and never lose it, Kuhn admitted that the consensus may be temporary, yet when a paradigm shift occurs, it occurs with unanimity: disagreement about a paradigm is short-lived. This cannot be the case for dual or multi paradigms. This he ignored. Thus, his theory is severely incomplete, not to say plainly inconsistent.

How does a discipline attain a paradigm? By applying discipline: Kuhn deemed unanimity as due to regimentation. We find such regimentation in other places, of course, such as in totalitarian states. Yet leaders there are not interested in research and so their motives are alien to science. What happens if the leaders of science become tyrants? Then the scientific public will dismiss them or else they cause research to atrophy. How do researchers know when their field is vibrant and when it atrophies? Kuhn made the goat the guardian of the garden: he declared fighting deterioration is the task of the scientific leadership: when field of scientific research begins to deteriorate, they declare a scientific revolution within that field. What if they become tyrants and block all change? Then the field atrophies. All this is somewhat mysterious, but this cannot be helped: just as the activities of research scientists are above the heads

of the common public, so the activities of the scientific leaders are above the heads of their peers. (The inequality of intellects, incidentally, is central to most contemporary theories of consciousness.)

Kuhn's view has the merit that, viewed from a certain angle, it seems minimal deviation from traditional lore or presupposition that science commands consensus. The lore, then, modified by Kuhn or not, is amply exemplified: scientific researchers often take for granted and thus do not mention their background knowledge and specific background situations, including their specific background problem-situations, problems, and problem-presentations. This makes it necessary to add some background information for novices or for readers from another discipline or a later era. And these are available and often make for quite intriguing reading. Indeed, Kuhn took this as his starting point, which explains his move from the discipline of the history of science to that of its philosophy.

All this becomes especially intriguing when the study of the background of a piece of research refers to controversy. (They invariably do.) For, in full contrast to traditional lore, what in science enjoys full agreement is utterly unclear. Science does command a tremendously broad consensus. Amazingly, although science applies no authority – religious, political, or any other – it commands much broader a consensus than any other fields of human culture. Philosophers take it for granted that within science what is agreed upon is rational and what is rational is unique and so not given to dispute. Yet just as scientific consensus is an observed fact, so is the prevalence of scientific dispute: contrary to the traditional view that the consensus pertains to scientific theories (the claim, that is, that all and only those theories are scientific that reason commands assent to), some but not all scientific theories command universal assent. Scientific information it is that is uncontroversial, whereas theories under scientific research are controversial. Advocates of the traditional view say, scientific progress is the rendering some controversial theories uncontroversial knowledge. This puts a barrier between scientific knowledge and scientific research!

A very significant reason for the adoption of this view comes from the adoption of the idea that action requires accord and rational action requires rational accord. Traditional theory justifies much action as science-based and explains the consensus that went into it as rational, as science-based. All this is an example of a prejudice: a strongly advocated theory despite familiar contrary information. The consensus that leads to rational group decisions is usually due to compromise, as John Watkins has observed (Watkins, 1957-8).

SCIENTIFIC JUSTIFICATION VERSUS SCIENTIFIC DISPUTES

The received view of science as consensus seems very comfortable, yet it is very frustrating, as it declares all dissent due to some serious defect, to some deviation from scientific norms. The word for this deviation is prejudice. The theory of prejudice was discovered repeatedly in diverse variants and it is therefore attributed to diverse thinkers, the latest of whom are Gordon Allport (Allport, 1954) and Leon Festinger (Festinger, 1954). Earlier, Karl Marx spoke of class prejudice; Sigmund Freud ascribed it to childhood emotional scars (traumas).

What all these versions of the theory share is the idea of the source of the trouble: observations are theory-laden. This was the discovery of Galileo and of Bacon, already four centuries ago. Galileo said, but for our theories, we could just as well say that as you stroll down the street of Florence on a moonlit night the moon jumps from rooftop to rooftop like a cat (Galileo, 1633, 1953, p. 256). Bacon went further (Bacon, 1620, Bk., I, Aph. LXXXVII and Aph. CXV). He said, anyone who entertains any hypothesis is bound to fall in love with it and then be blinded by it, so that the precondition for the ability to be a researcher is the readiness to give up all of one's opinions and entertain no idea unless proven. Galileo said, we cannot help having prejudices and this is why we should subject our views to repeated examinations. Bacon disagreed: he said we all examine our prejudices repeatedly, but to no avail. Most writers on prejudices agree with Bacon here. And so the question is, how do we get rid of prejudices? Or do we?

Despite centuries of frustration, most philosophers still continue to discuss the question, what is the rational consensus about a theory? They still resolutely ignore the much more fruitful question, what is rational disagreement? They may suggest, or at least imply, that dissent rests on ignorance. But then, how come they fail to take cognizance of the prevalence of scientific dissent? They do not discuss rational action as they take it for granted that rational action rests on rationally endorsed theory, despite the observed fact that rational action rests on compromise.

There is much truth to the traditional lore: there is a lot of consensus within science, and not only about observation reports and the desire to explain them, but also as to problems and their backgrounds ─ the problem-situations so-called. This is best shown when science is compared with the best of pseudo-science. It is common to compare today's physics with today's psychology, and the comparisons always come to answer the question, is psychology a science?

The answer, affirmative or negative as it may be, naturally rests on a criterion, and the criterion is usually controversial, as is the answer to the question, is the criterion properly applied? Oddly, when we consider a specific controversy, these problems hardly arise, since we wish to approach the controversy with an open mind and this often suffices. We may comfortably take this as the default option. Texts whose authors express contempt for some opinion are thus rightly suspected. Of course, some opinions are contemptible, yet the default option is not to refer to them and definitely not honor them with serious arguments. Even respectable but weak opinions are often rightly ignored, at least as the default option. All this seems commonsensical. Yet it leads to the disregard of most traditional writings on consciousness since in this field contempt is alas very common.

The disregard of texts infected with expressions of contempt, let me repeat, is the default option. Some texts of this kind are too valuable to ignore. It is then advisable to cleanse their valuable parts, to apply to them a background that makes sense of the controversy to which they contribute and to a contrast of them with cleansed texts that present contrary opinion. This goes a long way to divide the sheep from the goats with no reference to the criterion of demarcation of scientific theories from unscientific ones. The criterion is important for many ends, but for the study of problem-situations it is inessential: what was thus far said here should practically suffice.

Consider texts of metaphysics proper. Theses seldom present their backgrounds; usually they refer to controversial matters while ignoring their being controversial. Consider the famous controversy on the nature of Man: many options exist; Man is a rational animal, a laughing animal, or a working animal; Man is fear and trembling, or fear and boredom, or fear and nausea. One might expect some debate about all this. There is practically none.

Consider individual psychology then. The behaviorist and the psychoanalytic literatures contemptuously ignore each other. Within the psychoanalytic tradition the different strands also ignore each other: very little genuine dialogue (namely, respectful criticism) takes place between them. Many debates concern Freud's division of the mind to conscious (super-ego-plus-ego), front-conscious, sub-conscious, and unconscious (id). Clearly, these pertain to the study of consciousness. It is doubtful whether it is worthwhile to undertake the Herculean task of clearing this vast literature. It is better to use it as a mere prompt for listing some salient repeatable empirical information on consciousness. The opening paragraph of this essay lists some such facts on consciousness; this is totally absent from that literature. But there are better examples. Perhaps the best is Hume's observation that a task repeatedly

performed gets performed unawares. Perhaps here is the place to invite the role of consciousness and of awareness in common thinking. Perhaps scientific thinking is better, as there the thinking process is simpler and more conspicuous.

What is the route from a list of repeatable items of factual information to a theory that explains them? This is the classical problem of induction: how do observations convey theoretical information? Students of this problem are seldom interested in this route ─ possibly unawares. They hardly ever mention it: their concern is to justify induction, not to understand it. This justification they discuss ─ endlessly. This way they take much for granted, and, as often happens, the tacit assumptions behind their discussion prevent success (Popper, 1963, pp. 57-8).

TRADITIONAL STUDIES OF CONSCIOUSNESS

Why not take consciousness as a part of nature and its study a part of the natural sciences proper? This question is central to philosophy, and advocates of the study of the human sciences as a universal science insist on the view that we should study consciousness as a part of nature and as essential to human nature and thus as indifferent to the different cultures and traditions and to their impact on consciousness. Repeatedly philosophers call this view "naturalism" and they call themselves "naturalists". If there is a dispute here, then it has to do with the detailed specification of naturalism. The chief influence here is that of Descartes; he divided the world into the internal and the external, and he said, whatever humans share with other animals belongs to mechanics proper but the rest belongs to psychology. There are only two sciences, Descartes said, physics and psychology. The alternative idea is that there is only one science. It belongs to Descartes' disciples, chiefly Julien Offray de La Mettrie, author of *Man a Machine*. Briefly, cognitive science is not a science but a philosophy. It comprises effort to update La Mettrie in the name of current science. It includes the part of philosophy that is traditional learning theory: the theory that we learn from experience ─ by induction. This theory was alternately taken to be philosophical and empirical. It was therefore refuted doubly. As a part of philosophy it was meant to show that science is rational as it rests on inductive proof. Therefore, since the days of David Hume, mock-proofs repeatedly appeared that show induction to be in no need for proof. Hume himself admitted induction on empirical grounds of sorts. (This is Hume's naturalism.) Therefore, the theory earned also empirical refutations in psychology, especially in the

hands of Oswald Külpe and his disciples, of the Wurzburg and the Gestalt schools (Wettersten, 1985, p. 4889; Singh, 1991, Chapters 4 and 11).

Does cognitive science go beyond traditional cognitive philosophy? Is it empirical? If not, then there still is the hope that research will render it empirical. How? What makes a research project scientific or empirical? What makes a research program scientific or empirical? Grants committees repeatedly face these questions. Are they conscious of it? Do they possess some criterion or do they employ sheer gut feelings? How does this apply to current studies of consciousness?

Views on cognitive science vary. Its interdisciplinary character, however, is not contested: cognitive science borrows ideas from philosophy, physics, and neurophysiology; from psychology, linguistics and anthropology; and from computer theory and technology. The status of neurophysiology as an empirical science is not challenged. Whether neurophysiology contributes anything substantial to cognitive science, however, is not clear. Whatever it may contribute to the understanding of cognition, such as the fascinating matter of reverse correlation in neurophysiology (the technique for studying the way sensory neurons combine signals from different locations and generate responses), relates to animal cognition in general, not specifically to human cognition; at least not as yet. Nevertheless, the wedding of neurophysiology with computer technology, such as the pioneering study of David Marr on vision (Marr, 1982), throw light on physiological processes (not on cognition) by its use of computer simulations. Paul and Patricia Churchland go a bit further as they use neurophysiological information to improve upon the commonsense view of consciousness (Churchland, 1991). But, of course, traditionally the onus of cognitive science is to present the human mind as a machine, with the simplest task of formalizing language acquisition and learning processes so as to have computers simulate humans properly. Unfortunately, the project is often blocked by the endorsement of associationism or of some of its consequences, or, worse, of vague versions of anti-associationist linguistic theories, such as those of Noam Chomsky.

Two camps of students of consciousness are exceptionally hostile to each other: those who have swallowed whole the traditional view of association and induction, and those who reject it offhand. Consider for example the new studies that employ experiments in which high powered, sophisticated machines record electromagnetic activities in brains ─ human or not. They incontestably belong to natural science. Do they divulge any information on consciousness? Why do people deem observations of brain activities more revealing of consciousness than the observation of the notorious effect of alcohol on consciousness? This

is no rhetoric question. On the contrary: it is intriguing. It is also pivotal: those who do not agree on this at a glance will probably dismiss this essay upon reading this claim, if they did not do so already.

Computer brain imaging does show activity associated with decisions. So decisions are in. As it happens, the brain-activity in question indicates, we are told, that awareness or consciousness of a decision appears a little while *after* the decision was made. Hence, we are triumphantly told, consciousness is illusory. Of course, opponents will say that the brain activity in question signifies that consciousness takes time to receive expression. It is clear that as long as there is no knowledge of how to read brain activity, the matter is controversial. The interesting fact is, the dispute shows how much the current views are prejudices. And so, here the computer serves a traditional dispute but is so of little help. A strange event took place here: a recent empirical observation seems to support the philosophical disregard of awareness: a recent brain neurology study suggests that awareness of a decision comes a brief period past its occurrence (Gregori-Grgič R *et al*., 2011). The evidence cannot possibly be decisive as too little is known about the meanings of electric activities in the brain and indeed Gregori-Grgič and his co-authors are wary. Yet preference for associationism makes many brain researchers very glad to see awareness go and the tendency to read this into the experiment is marked.

CONCLUSION: THE METAPHYSICAL DISPUTE IS UNAVOIDABLE

The discussion of consciousness is often conducted with little preliminary presentation of the problem and its background. It is even difficult to find in the literature the question explicitly worded. (To speak of *the* problem of consciousness will not do; there are too many problems regarding it and there is no consensus as to which of them is dominant.) To start the discussion of consciousness more in line with the rules of the game, it must develop much more slowly. Emotions stand in the way: the discussion usually gets heated because of religion. The religious insist that the soul survives the body and the materialists deny this. The dispute is as metaphysical as they come. Efforts to try to decide it by empirical means are foolish. It is futile to try and to settle empirically the dispute as to whether the soul leaves the body or dies with it. And yet this is the motive beyond much of the research about the mind, and more specifically about consciousness.

One major development in this direction is due to Alan Turing; it is less than a century old. He did not wish to settle the dispute; he wished to render it irrelevant. At least that is what he declared in the opening of his celebrated essay on the famous test that bears his name. He failed: many students of consciousness treat it as emotionally as before. Now the Turing test is taken more often than not as anti-religious rather than as keeping religion out of the research concerns.

Turing meant his test to eliminate the discussion of the mind (or soul) from science. Three or four traditional philosophical views of it survive. One is, matter is real but the mind is not: it does not really exist. The other is, the mind is real and matter does not really exist. The third is that both are real, although the mind is ephemeral. A possible fourth is, the two run in parallel, in coordination: one being a reflection of the other. Turing rightly ignored (as not serious) the idea that matter is unreal. He also ignored parallelism as echoing materialism. His centered on dualism: is it possible that both matter and spirit are real? Yes. And Turing argued that the assumption that the spirit is real is of no empirical import.

Turing envisaged computers that are as powerful as possible. He assumed that no one will ascribe a soul to a computer, no matter how powerful. He imagined a computer that can emulate human conduct. If this is possible, then the materialist way of explaining the conduct of a computer is the way of explaining human conduct.

This is the whole of the idea of Turing. He presented also a pictorial variant of it: imagine an investigator studying a human and a computer in only one manner; the investigator asks questions and both subjects answer them. At the end of the process the investigator has to decide which of the two is human and which not. If the investigator is unable to decide, then the ascription of a soul to one party but not to the other is of no empirical import.

This is a powerful argument. Since we cannot as yet explain even the conduct of a computer, it is clear that the discussion is in matters of principle, not of fact: no one suggest that the ascription of some spiritual powers to a computer is necessary. Since there is no reason to assume that a computer cannot emulate any human intellectual conduct, there is likewise no reason to assume that humans are more than computers of sorts.

Two arguments against Turing are known. One is due to John Searle, and is known as the Chinese Room Argument. It is, briefly, that computers have no feelings and humans do. Computers may be able to emulate human feelings but not to feel. This argument is open to critical discussion, and the literature is full of it. Yet its force is moral: we should reduce suffering, human and animal,

perhaps also of machine if machines really suffer, not however if they only emulate suffering. The second argument is stronger. It is this: if the machine fails to pass Turing's test, we may improve it; if it passes the test, then we may improve the test. This is incontestable. Hence, the Turing test is not conclusive. Yet it was valued only because of its claim for conclusiveness. And so we are not rid of consciousness, not yet and possibly not ever. The long and the short of it is this: there are repeatable empirical observations that want explanations. As long as we can explain them only by the assumption of goal-directed behavior, this must stay (Mises, 1957, page 1), and as long as some of these observations refer to consciousness, the same holds for consciousness.

The long and the short of it is this. The magical philosophy that most people still hold ascribes meanings ─ intentions ─ to every thing and behind every event. The mechanistic view denies that intentions exist, conscious or not. And then it had to deny the existence of consciousness too. Is it too strange to suggest that only living things have aims and only the more developed ones are aware of them? More cannot be said in general. We know that we have conflicting unknown ends. Moreover, our ends are hewn out of our images of the world, and these are usually obviously false and in propitious moments only possibly true but not very likely so. And then it is just about impossible for us to know what we want and how we should go about to meet our ends.

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