Papers on Social Representations
Volume 25, Issue 1, pages 13.1-13.24 (2016)
Peer Reviewed Online Journal
ISSN 1021-5573
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[http://www.psych.lse.ac.uk/psr/]

How Popper's 'Three Worlds Theory' Resembles Moscovici's 'Social Representations Theory' But Why Moscovici's Social Psychology of Science Still Differs From Popper's Critical Approach

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ABSTRACT

This paper is to my best of knowledge the first to discuss similarities and differences between Karl Popper's 'three worlds theory' and Serge Moscovici's 'theory of social representations'. Karl Popper maintained that to be subject to criticism, and hence to falsification attempts and subsequent improvement, scientific theories must first be formulated, disseminated, perceived, and understood by others. As a result, such a theory becomes a partially autonomous object of world 3, the "world of products of the human mind" in contrast to world 1, the "world of things", and world 2, the "world of mental states" (Popper, 1978, p. 144). Popper's three worlds theory resembles Moscovici's social representations theory insofar as social representations / world 3 objects cannot be reduced to individual states of minds, are embedded in interactions between people and objects, and are always rooted in previous representations / knowledge. Hence, Popper – who was very skeptical of the usefulness of a 'psychology

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of science'– *did* in fact employ elements of a 'social' social psychology of science in his later works. Moscovici himself in turn may have failed to notice that to Popper science does not take place within a separate 'reified universe' in his 'Social Psychology of Science' (1993). Although to Popper science aims at increasing objectivity and reification, it is still a part of the social world and the 'consensual universe'.

Keywords: Social representations; critical rationalism; three worlds theory; philosophy of science; psychology of science.

Karl Popper did not believe in the usefulness of a 'psychology of science' and is notorious for (in-) famous statements such as "… the answer to Kuhn's question 'Logic of Discovery or Psychology of Research?' is that while the Logic of Discovery has little to learn from the Psychology of Research, the latter has much to learn from the former" (1970, p. 58). Popper launched his scientific career in 1928 with a doctoral dissertation supervised by Karl Bühler on the psychology of thinking (for an extensive discussion of Popper's psychological 'roots' within the 'Würzburg school' of psychology founded by Otto Selz, see Ter Hark, 2003). However, Popper abandoned psychology in the early 1930s and turned to mathematics and formal logic to find a middle way between the naivety of positivism (i.e. the belief that 'true knowledge' can be obtained through the strict and impartial application of the 'scientific method') and the cynicism of (philosophical) skepticism (i.e. the assumption that there is no true knowledge and that a growth of knowledge is impossible). His arguably greatest scientific achievement was to prove the possibility of a growth of scientific knowledge without making use of the 'principle of induction' and the somewhat problematic concept of empirical confirmation.

Nevertheless, from the late 1960s onwards, everyday knowledge became a pivotal field of interest for Popper (see also Numar, 2010), for example in his 'three worlds theory' (TWT), which he first elaborated upon in depth in 'epistemology without a knowing subject'

(1971/1967)[1]. Here, world 1 is defined as the "… world of physical objects or of physical states" (1971/1967, p. 106) and world 2 as the "… world of states of consciousness, or mental states" (ibid.). In contrast, world 3 is "… the world of *objective contents of thought*, especially of scientific and poetic thoughts and of works of art" (ibid; emphasis as in the original). In his "Three Worlds" Tanner Lecture on Human Values in 1978, Popper described world 3 as "… the world of the products of the human mind, such as languages, tales and stories and religious myths; scientific conjectures or theories, and mathematical constructions; songs and symphonies; paintings and sculptures. But also aeroplanes and airports and other feats of engineering" (p. 144).

I will argue that, unknowingly, Popper did in fact employ elements of a 'social' psychology of science, focusing on group processes and social dynamics in his writings from the 1970s onwards. His 'world 3' closely resembles Moscovici's concept of 'social representations'. In neither approach, can knowledge in a social/objective sense be reduced to individual states of minds, as it is always embedded in interactions between people and objects and new knowledge is always integrated into existing knowledge structures.

Moscovici himself entered into the realm of epistemology when he developed his 'social psychology of science' (1993) from his 'social representations theory' (SRT) and his research on minority effects (e.g. Moscovici, Lage, & Naffrechoux, 1969). There are differences between the theories of Popper and Moscovici when it comes to questions of whether everyday knowledge and scientific knowledge differ from each other and whether a growth of scientific knowledge is possible and if so, how?

This paper begins with a summary of Popper's 'critical approach' and a more detailed account of his TWT. This is followed by a brief introduction to Moscovici's SRT and a more elaborate discussion of his 'social psychology of science' (1993). At the end, the similarities and differences of the two approaches are discussed.

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[1] It should be noted that Popper's concept of 'objective knowledge,' that is knowledge which is "… independent of anybody's whim" (1959/2002, p. 22) in form of conjectures that can be inter-subjectively tested was prominent already in his opus magnum 'The Logic of Scientific Discovery.'

CRITICAL RATIONALISM – AN OVERVIEW

Popper's solutions to the problem of demarcation and the problem of induction

Two important philosophical problems lie at the center of Popper's philosophy of science, which he claimed to have solved in (or before) his 1934 'opus magnum' 'Die Logik der Forschung [The Logic of Scientific Discovery]': the 'problem of induction' and the problem of the demarcation between science and non-science. Popper's solution to the problem of demarcation is fairly simple and has arguably become part of 'everyday' scientific knowledge: "One can sum up all this by saying that the criterion of the scientific status of a theory is its falsifiability or refutability or testability." (1962, p. 37). Still, one could ask why Popper differentiates between falsification and verification at all. A scientific theory makes certain predictions about observable events and they happen to be true or not (or something in between) and the theory is either falsified or verified (to some degree). To understand the difference between the two, it is necessary first to understand the problem of induction and Popper's solution to it.

Since antiquity, scientific theories have been considered the product of inductive reasoning; that is, the drawing of general conclusions on the basis of specific observations. We observe regularities in nature; then we formulate a general law to explain them; then we test our assumptions and ideally, we find 'empirical proof' to bolster our reasoning (this is more or less also the way the 'scientific method' is usually explained in social psychology textbooks; see Holtz & Monnerjahn, under review). The main problem with induction is that it cannot be proven. Inductive reasoning relies on the assumption that "… the future will (largely) be like the past" (Popper, 1971, p. 168). We have observed regularity in the past, and we expect it to be there in the future as well, but we simply cannot prove it without referring to our observation that 'usually' the future is more or less like the past, and that is in itself inductive reasoning again. Popper addressed this problem by reformulating it in terms of formal logic. Theories by and large can be reformulated as logical statements of the form 'if A, then B' with A being a set of premises and B being an empirically observable event. According to the rules of formal logic, we cannot conclude from knowing that B is true that A is true as well. This means that we cannot (necessarily) draw the conclusion from confirming observations that our theory is true. However, if B is *not* true, we can indeed draw the valid conclusion that A is not true as well.

Whereas we cannot 'prove' a theory by means of any number of empirical confirmations (or any statistically significant result), we can – at least by means of formal logic – disprove it through a single disconfirming observation. Hence, we do not need the principle of induction to prove the possibility of a continuous growth of scientific knowledge: If we always replace a disproven theory with a theory that not only explains everything that the older theory could explain (the so called 'consistency condition') but also makes additional predictions, we will inevitably have over time a growth of knowledge. In practice, an empirical refutation is just as 'unprovable' as an empirical verification. Still, Popper believed that the logically provable mechanism of growth of knowledge through falsification and continuous improvement should serve as a model for scientific practice, instead of the logically impossible model of knowledge acquisition through induction and generalization, the model of positivistic epistemologies (see e.g. Creath, 2014/2011). Popper called his approach 'critical rationalism' (1962, p. 26) or the 'critical approach' (ibid, p. 51), whereas others (e.g., Lakatos, 1970) described the underlying epistemological principle as 'falsificationism' in contrast to positivism's 'verificationism'.

Although Popper believed that 'true knowledge' is finally unattainable, he was also an enemy of 'relativism,' that is … "the doctrine that truth is relative to our intellectual background or framework: that it may change from one framework to another" (1987/1976, p. 35). Consequently, Popper was a sharp critic of Kuhn's (1962) concept of 'incommensurability' of scientific paradigms: whereas according to Popper's 'consistency condition' a new theory should explain all the phenomena that an older theory was able to explain, Kuhn (1962), pointed out that this has rarely been the case in the history of science. Instead, for him there is more of a conceptual change in 'scientific revolutions': old concepts, terms, and other elements of theories are simply not needed anymore, or they slightly change their meaning to fit in with a new paradigm, which makes a direct comparison between old and new paradigms difficult at the very least, and makes the consistency condition obsolete. Hence, 'Kuhnian revolutions' do not necessarily lead to a growth of knowledge; sometimes old problems are simply relinquished and new problems addressed. Popper (e.g. 1971/1967) admitted that sometimes such setbacks and discontinuities might occur, but he maintained that overall, on a longer time scale, science progresses as he outlined it in the 'logic of scientific discovery' (1959/2002; first German edition 1934).

In 'The Myth of the Framework' (1987/1976), Popper traces the roots of 'relativism' back to a mistaken belief in 'absolute truth'. Of course, a 'perfect' understanding, for example between speakers of different languages or members of different cultures (or two people in general), is unattainable, but that does not mean that understanding is impossible and that a discussion between participants from a different background is pointless. The opposite is true: such a situation offers the chance to increase mutual understanding and to question, reformulate, and improve on one's own understanding of a given problem. Hence, the goal of such a discussion is not to 'win' (or to 'prove' something), but to exchange and criticize ideas and mutually enhance understanding, which in turn opens up the possibility of getting closer to the truth, which may not be attainable in absolute terms, but which still is needed as a *regulative idea* to motivate people to *try* to understand each other. Popper remained a metaphysical realist throughout his life: he strongly *believed* in the truth, but he also understood the impossibility of *proving* that there is truth in an absolute sense. His main argument in favor of realism was that although inductive reasoning cannot be proven, it is hard to argue that there *has* indeed been a tremendous growth of scientific knowledge since the Renaissance, at least in the natural sciences and particularly in the field of physics. As Hilary Putnam once put it (1975, p. 73): "realism is the only philosophy that does not make the success of science a miracle".

Three Worlds and Objective Knowledge

Why do we need three worlds? Here, we can just follow Popper's argumentation against a reductionist 'materialism' or 'physicalism' (the belief that there is only one world: the world of things or physical objects) in his 'Three Worlds' lecture (1978). To a materialist, there are only 'concrete' physical objects. Mental states can also be reduced to physical states, such as certain neurological conditions, memory engrams, sensory excitation, and the like. To use an example from social representations research, a materialist would reduce historical monuments (e.g. Sen & Wagner, 2005; Raudsepp & Wagner, 2008) to their embodiments within people's minds and their current 'physical materializations' such as the monuments themselves (if they still exist) as well as pictures, videos, and descriptions. However, social groups will share different collective memories of a historic event and related objects of commemoration will play a key role within these knowledge structures. Popper argues that even the entirety of all the monument's current

physical embodiments (including states of mind) could still not fully capture the abstract world 3 object. For example, a contested object of collective history can have an influence on events to come such as the future relations between two social groups and hence future world 1 objects such as boundaries and legislations. To Popper, a materialistic reduction would necessarily miss out on these dynamics and hence an important part of what makes a world 3 object as an abstract entity what it is. In his lecture, Popper uses works of art such as Shakespeare's dramas or Beethoven's Fifth Symphony as well as examples from history such as the "US Constitution" as examples for world 3 objects that transcend their physical embodiments (pp. 144-145).

Next, Popper addresses the main issue: scientific theories cannot be reduced to their world 1 'materializations' (for example formulations in books) and to states of mind as well. To a certain degree, a theory is an abstract and autonomous entity. But how can we know if these world 3 objects are real? What makes us regard physical objects as real? It is the fact that we can interact with them and that they have a causal effect upon other things: things we can touch have an impact on our sensory perception. Scientific theories undeniably have a causal influence on us and our world, as is clear from all the technical advancements around us. However, world 3 objects are not real in an *absolute* sense. They can only exert causal influence on the physical world if they are "… *grasped* and *understood* by a *mind*" (p. 164; emphasis as in the original). It should be noted as well, although this is not said per se in 'Three Worlds,' that, for example, 'Santa Claus' is of course as much a 'real' world 3 object (causing many children to expect presents on December 25th) as 'phlogiston,' the element that for a long time was believed by chemists to be released in combustion. 'Real' does not mean 'true' or 'objectively true' in the same way that a theory can never be proven.

To Popper, there is a difference between "… knowledge in the subjective sense and knowledge in the objective sense" (p. 156). Whereas the former consists of thought processes, the latter represents thought *contents*, or in the case of science "… linguistically formulated theories" (ibid.) which can become *objects* of communication. The world 3 content of a theory must be grasped by individual scientists or inventors to have an effect on world 2 and consequently on world 1 as well. A dualist may point out now that this objective content is nothing but an abstraction from a state of mind (world 2). But Popper counters that the world 3 object is necessarily more than any corresponding world 2 object. For example, Einstein was not aware of all the consequences of his theory of special relativity in 1905, but by formulating and publishing

it he allowed for criticism and falsification attempts, so some of these consequences were later discovered while some other mysteries perhaps still remain to be solved. The world 3 object is not only comprised of the underlying world 2 thought process, but also of "… the system of all the theorems that can be derived in it" (p. 162) and hence a necessarily infinite universe of conjectures. It should again be noted that the theory of phlogiston also allowed for infinite deductions and surprises and may even have substantially advanced science in its time (at least according to Kuhn, 1962, p. 56 ff.). In Popper's words: "Nothing depends here on the use of the word 'real': my thesis is that our world 3 theories and our world 3 plans causally influence the physical objects of world 1" (p. 164).

To conclude the summary of Popper's epistemology, his 'evolutionary approach' to 'objective knowledge' has to be discussed at least briefly. Towards the end of both texts (1978 and 1976/1960), Popper expresses his belief in the possibility of a growth of knowledge through the application of a critical approach and contrasts it with the "… malaise of existentialism" (1976/1960, p. 104) and the destructive power of nihilism and skepticism. He argues in favor of an optimistic outlook towards 'scientific evolution'. In 'Evolution and the Tree of Knowledge' (1971/1961), Popper discusses his understanding of the theory of biological and scientific evolution in depth. At the very core of Darwin's theory, Popper sees the discovery that a series of apparently accidental and unrelated mutations over a long period of time can simulate the goal-driven behavior of a conscious creator (cf. Dawkins, 1986). What we can observe is the *emergence* of higher life forms out of a seemingly chaotic process; nevertheless, all existing life-forms are the result of a history of more or less successful *attempts at problem solving* (organisms trying to survive and to procreate). Whereas there may be chaos and chance on the level of individual mutations, overall only those mutations that were at least once before successful solutions to problems of survival and procreation survive.

Although I will not discuss Popper's ideas about the further development of the theory of evolution in detail, it is important to pay attention to a certain pattern in his argumentation: on the micro-level of individual organisms, evolution can be reduced to random mutations, which he equates with random attempts at problem solving in science or everyday life; but on the macro-level over the course of millions of years, there undeniably *has been* an evolution from "… an amoeba to Einstein" (see 1971/1970, p. 60). The very same is true for science: On the micro-level, there are attempts at solving problems, and there is no 'scientific method' which ensures

'true knowledge' – there are only methods to criticize theories and to distinguish between 'better' and 'worse' solutions. But on a larger time scale, the 'miracle of science' and the 'explosion of knowledge' *did* occur in the 19th and20th centuries. The increasing speed of the evolution of knowledge can be explained by the fact that by becoming autonomous world 3 objects, scientific theories allow for "exosomatic evolution" (1971/1965, p. 242). This is not to say that the scientist(s) who proposed a tentative solution to a problem have to 'die' as in the evolution of life forms; instead a theory can simply be replaced with a better one. Although Popper leaves open the possibility of a continuous evolution, for example in the arts (e.g. 1978, p. 151 f.) and in politics, this evolutionary process is among the world 3 objects somewhat unique to science. This is not because of a specific 'scientific method,' but because of a *critical tradition* in science. But what is the cause of this tradition, which makes the difference between science and other world 3 realms such as fiction and religion? To Popper, the cause stems from an underlying *belief* in truth: *scientific realism.* "That is to say, it [the scientific tradition] was inspired by finding true solutions to its problems: solutions which corresponded to the facts" (1971/1966, p. 290). Without this belief in realism, it would be difficult to motivate scientists to renounce their theories if discrepancies between their ideas and the observable facts came to light.

SOCIAL REPRESENTATIONS AND THE SOCIAL PSYCHOLOGY OF SCIENCE

Social Representations, Everyday Knowledge, and Scientific Knowledge

According to Moscovici, "… most knowledge and ideas circulating in the mass media are actually of scientific origin" (1988, p. 215). He developed the SRT from the question as to "… how a (scientific) theory inflects a society's behavior, way of thinking, and language; and is thus transformed, through its very circulation, into a social representation" (Moscovici 1963, p. 251). Moscovici's original study (2008/1976; first edition 1962) analyzed how psychoanalysis was taken up by the French public and how it consequently was transformed through communication into something else. A theory is not transformed by criticism and falsification attempts into a 'better' version of the original theory. Instead it is integrated into people's lives wherever it helps them to make sense of things they do not understand and wherever it is useful for communicating

mental states to others. "Social representation is defined as the elaborating of a social object by the community for the purpose of behaving and communicating" (ibid.). This transformed version of a scientific theory is not just an "impoverished" (1963, p. 252) version of the original theory, but it is "… fulfilling of the requirements of the elaboration of social reality" (ibid.). It is *knowledge for a purpose* (Gaskell & Bauer, 1999; Wagner, 2007). Like Popper's world 3, the world of social representations cannot be reduced to individual states of minds. It is the very nature of such a social representation to be in constant flux and re-negotiation, a collective phenomenon: "… *a social representation is the ensemble of thoughts and feelings being expressed in verbal and overt behaviour of actors which constitutes an object for a social group*" (Wagner, Duveen, Farr, Jovchelovitch, Lorenzi-Cioldi, Marková, and Rose, 1999, p. 96; emphasis as in the original). In turn, social representations play a role in the formation of social institutions and consequently (using Popper's terms) physical world 1 objects as well.

An object of the mind can only become a social representation if it is communicated and 'grasped and perceived' by others. Consequently, psychoanalysis is represented differently in different public domains. Moscovici (2008/1976) distinguished between the meanings of psychoanalysis in the communist, the Catholic, and the urban-liberal milieus. In each of these communities, psychoanalysis is represented differently and speaking of it caters to different needs. Nevertheless, regardless of the respective community's interests and needs, two processes of transformation are always at play: anchoring and objectification. Anchoring means that something 'new' is anchored in something 'old.' If we face, for example, a scientific theory hitherto unknown to us, we will integrate it into what we already know. Anchoring consists of two separate processes: classifying and naming: "By classifying what is unclassifiable, naming what is unnameable, we are able to imagine it, to represent it" (Moscovici, 2001; p. 42). Objectification signifies the process of making the abstract concrete. This concretization can take the form of icons, metaphors, tropes, or other symbols that help to make the diffuse 'graspable.' A symbolic objectification is successful if it corresponds with a "… group's experiential world and the negotiated consensus of the group members" (Wagner et al., 1999, p. 100). Hence, it does not need to be 'true' or correspond with the facts, it 'just' has to be "… good to think" (ibid.). These metaphorical objectifications are anchored in existing representations.

Both scientific knowledge and everyday knowledge have the same origins: "When all is said and done, the representations we fabricate – of a scientific theory, a nation, an artefact, etc., - are always the result of a constant effort to make usual and actual something which is unfamiliar […]" (Moscovici, 1984, p.40). But whereas everyday knowledge exists within a 'consensual universe' where knowledge has the function of enabling communication and is constantly being transformed and re-negotiated to effectively fulfill its function, 'modern' (see ibid, p. 41) science exists within a 'reified universe'. Here, constructs are not a consequence of negotiated consensus; they are instead based upon the 'objective' nature of things in the sense of "… solid, basic, unvarying entities, which are indifferent to individuality and lack identity" (ibid, p. 34). As a means of achieving 'objectivity', a reified universe relies on "rules and regulations" (ibid, p. 35) to limit the role of individuality and ambiguity. "It [science] must, furthermore, lay down certain laws – uninvolvement, repetition of experiments, distance from the object, independence from authority and tradition – which are never fully applied" (ibid). Here, Moscovici leaves open the possibility that the 'reified universe' of science may after all be not as solid and unvarying as some scientists may believe. Nevertheless, as a consequence of trying to move beyond consensus and common sense, science increasingly aims "to make the familiar unfamiliar" (ibid., p. 41) insofar as it is "constantly demolishing" (ibid) established consensual concepts and rephrasing them in its own language.

Whereas the theory of social representations emerged from studying the popularization of scientific knowledge, it has since been applied to many other disciplines, such as social representations of world history (e.g. Liu, 1999; Liu & Hilton, 2005), the study of societal and political phenomena (e.g. Jovchelovitch, 2007; Howarth, 2006) and many other fields of scientific inquiry (see Sammut, Andreouli, Gaskell, & Valsiner, 2015, for an overview of the SRT's 'state of the art').

Moscovici's 'Social Psychology of Science' (1993)

A paper that Serge Moscovici wrote relatively late and in which he directly addressed the issue of a 'social psychology of science' (1993) is of particular importance for the discussion presented here. In the paper, Moscovici elaborated on the aforementioned open question in his earlier works

(e.g., 1984) of whether everyday knowledge and scientific knowledge constitute different universes of meaning or whether – 'when all is said and done' – science is just one representational system (arguably a rather reified one with strict procedural rules for the negotiation of meaning) among other systems. Moscovici started by expressing his "… dissatisfaction […] with the image that the dominant social psychology gives of common thought, namely that it is extremely prone to errors, biases, stereotypes and every kind of cognitive miserliness" (p. 343). He went on to argue that common sense and science follow the very same principles. Moscovici put forward two main arguments: First, that science is a social phenomenon because scientific knowledge is based on negotiated consensus; hence, there is a "… profound unity" (p. 344) among knowledge, influence, inquiry and persuasion. Second, he argued that knowledge producing groups were diversified in the sense that there is always "… a majority epistemology and a minority epistemology – which achieve different ends" (ibid.).

His first argument is directed against the idea that scientific consensus is based on 'facts' and that scientific dissent can only be the result of a lack of reliable data. He argues against the idea that there is a fundamental difference in this regard between the natural and the social sciences: "… on the one hand [in the natural sciences] the silence of facts, on the other [in the social sciences] the contest of words" (p. 346). In the natural sciences there are also disputes, he argues and by citing examples from Kuhn (1963 & 1977) he argues that it is rarely the case that 'undisputed facts' make scientists give up an old paradigm (or theory) for a new one consensually. In contrast, the *possibility of new insights necessarily causes a division* in the scientific community: on one side are those who believe in the new theory; on the other are those who prefer the old one.

Moscovici argues that "… theories, like beliefs of every kind, are representational" (p. 350), meaning that they are socially shared and constantly re-negotiated collective phenomena that transcend individual minds. Scientific theories cannot be reduced to their 'objective content', for example in the form of axioms and logical conjectures; scientific communication always takes place in a social environment and always contains – unwillingly or not – meta-statements about the "… qualities and intentions of the author" (ibid.). In a scientific debate, scientists identify with a problem by taking sides and by making this decision part of who they are. In the case of new discoveries, minorities – those who endorse the new theory – try to get the upper hand by making themselves heard and by trying to convince others of their cause. Here,

Moscovici makes use of his theory on the differences between minority and majority influence (e.g., Moscovici, Lage, & Naffrechoux, 1969; Moscovici & Faucheux, 1972).): if a minority is consistent and retains credibility, it will be able to create a change in the opinions of at least some majority members by arousing curiosity and making people reconsider their beliefs, though there may not be an immediate change in their overt behavior. On the other hand, a majority exerting influence on a minority (majority influence) is very likely to cause a change in overt behavior by means of authority and obedience, while the 'true' opinions of the dissenters will remain the same.

In the next paragraph of his paper, Moscovici contrasts Popper's falsificationist approach (without quoting a specific text) with Kuhn's analysis of scientific revolutions (1963; 1977) and relates both approaches to minority and majority influences. He summarizes Popper's approach as follows:

> "Popper has argued that scientific knowledge can never be verified, only falsified and that falsification is the aim of scientific research. Bold conjectures are to be followed by attempts at refutation and disconfirmation. But is this really how scientists work? Actually, scientists may pay lip service to falsification while continuing to resolve their problems according to tricks handed down by tradition (p. 359)."

On the other hand, there is Kuhn's description of 'normal' scientists who in a positivist way "… seek for the predictions of a shared paradigm" (ibid.). Whereas Popper's approach resembles a minority's attempt to propagate its ideas and change the minds of majority members, the Kuhnian 'normal scientist' resembles a majority member seeking consensus or at least obedience. Moscovici points to findings from social psychological experiments in the tradition of Wason (1960) showing that usually participants prefer 'verificationist' attempts when trying to find a production rule behind a series of numbers such as (2, 4, 6, …). Only when confronted with the information that a minority disagreed with the 'obvious' solution do participants turn to falsification (e.g., Legrenzi, Butera, Mugny, & Perez, 1991). To Moscovici, both strategies – verification and falsification – are important for science: the first applies to 'normal' consensus-oriented scientists who slowly expand existing knowledge within a unanimously shared paradigm

(or under a uncontested hegemonic social representation); Popper's falsificationism applies to the wild and comparatively rare instances of 'extraordinary' revolutions "… pursued by dissensus-oriented scientists" (p. 363). It seems that to Moscovici both consensus and dissensus or phases of consolidation and of revolution are needed for scientific progress: without revolutions, there could be no progress and without phases of verification and consolidation, there would be a chaos of incommensurable paradigms.

SIMILARITIES AND DIFFERENCES BETWEEN POPPER AND MOSCOVICI

Similarities

The social nature of knowledge. To both Popper and Moscovici, knowledge has as its main purpose to enable communication in a wider sense, including social activities and the formation of social institutions. Moscovici's 'mantra' of social representations, making 'the unfamiliar familiar', could be easily reformulated as one of the problems that underlie man's quest for knowledge in a Popperian sense: How can we share our world 2 mental states with others, reach mutual understanding, and collectively translate world 2 ideas into actions that shape world 1's physical reality? In both approaches, the negotiation of consensus is of crucial importance, and world 3 objects / social representations are shaped through the interactions between people and the objects. In turn, world 3 objects / social representations in turn influence the ideas and activities of those who interact with them.

To take up the example of a historic monument, both Popper and Moscovici would agree that qua being an abstract object of communication and resulting shared knowledge structures, such an object cannot be reduced to its representations within people's minds and its physical materializations: Even non-existing objects such as (presumably) Nessie or the Holy Grail can have massive effects on people's lives and shape the way a social group or a society thinks of itself in relation to other social entities. To both Popper and Moscovici, there is no such thing as a 'mere fact': Just as world 1 needs to be translated first into individuals' mental states (world 2) and then into a consensual world 3 object to become effective and to evolve, social representations trigger processes within individuals' minds that can result in changes to our physical world and perhaps –in the long run – to something like 'progress'. Moscovici himself

uses at one point abstract scientific concepts to exemplify the developmental potential of social representations:

> Generally speaking, explanatory concepts are likely to be abstract and ill defined, as was true of the gravitational force in mechanics, the atom in physics, the gene in biology and social classes in Marxism. Their existence was assumed to be proven and then many things were explained by their intervention, although they themselves remained as obscure as ever. […] But once something is conceived and endowed with an explanatory power, one must try to advance further and grasp the reality of the force or the phenomenon in question. Progress can be made no other way (1988, p. 223).

In the paragraph in question, Moscovici was responding to Gustav Jahoda's (1988) criticism that the concept of Social Representation is not defined precisely enough to meet scientific standards sufficiently. Both Popper and Moscovici would agree as well that not only scientific motives drive the production of science and that scientific knowledge is embedded into wider knowledge structures such as beliefs and ideologies.

Anchoring and consistency. To both Popper and Moscovici new knowledge is always integrated into existing knowledge structures. In a similar vein, new ideas are always rooted in older ideas. Furthermore, the way we integrate something into existing structures, for example by classifying and naming it, has an influence on our interactions with the respective object. As a consequence, our patterns of interactions with the respective object can in turn influence and shape the object of knowledge itself.

To Popper, the belief that there is 'something real' behind our representations (although we never can be sure) is necessary to motivate scientists to accept criticism and to try to improve on previous knowledge. Within the Social Representations community, the question of constructivism and the relationship between representations and 'brute facts' has been an intense field of discussion (see for example Wagner, 1998). Nevertheless, at least Wagner seems to share

Popper's concerns for a too radical or 'strong' version of constructivism in the sense that it is assumed that there is no actual reality behind our representations of the world:

> The constructive epistemology of social representation theory does not imply that there is nothing beyond the socially constructed worlds. Social representations theory gives an account of the social world of groups, not the world beyond, which, perhaps, is represented by other groups and/or by science. The world beyond any representational system sets limits which must figure in the theory if it is not to take the airy idealist position of postmodern 'Beliebigkeit' [arbitrariness]. (1998, p. 313)

On a side note, in social representations theory, there have been debates whether it makes sense to speak of 'representations of something' in the case of abstract entities such as concepts such as 'freedom' or 'democracy' (e.g. Bauer & Gaskell, 199, p. 169): what are these entities other than representations in the sense of socially shared knowledge? In Popper's TWT, this problem can be solved very easily: 'Freedom' is at the same time a world 1 object (for example in the form of a dictionary article or rallies for freedom and democracy), a world 2 object (in form of individual people's representations of freedom), and a world 3 object (in form of a socially shared object of communication) – and all three worlds together by means of their interactions are constitutive of the abstract concept of 'freedom'.

Two universes or two sides of a coin? A rather 'naïve' reading of Moscovici's (e.g. 1984) dichotomy of the reified and the consensual universe would imply that he believed that the 'fabric' of scientific knowledge is thoroughly different from that of faulty everyday knowledge. However, Foster (2003) argued very convincingly that this rhetorical figure was mainly meant to justify the study of 'faulty' common sense understanding towards positivist colleagues from 'mainstream' social psychology, who may indeed believe that only scientific knowledge can be 'true' in a metaphysical sense. In accordance with Foster, I believe that Moscovici regarded scientific and everyday knowledge as separate 'representational projects' (Bauer & Gaskell, 1999), and not as two distinct universes. To Popper, such an insistence on a difference between

scientific knowledge and everyday knowledge would attenuate the critical mindset or the 'dialogicality' (Jovchelovitch, 2007) that is needed for a growth of scientific knowledge.

Popper would hence agree with the statement in Moscovici's 'social psychology of science' that there is no fundamental difference between everyday knowledge and scientific knowledge. There are no different universes of knowledge, and scientific knowledge is – just like any knowledge – based upon negotiated consensus. Not only the 'quest for the truth', but a whole lot of genuinely human motivations play a role: The quests for fame, money, and social belonging, for example.

Still, science as a cultural phenomenon and as a representational system can be differentiated from other traditions and domains such as religion, arts, and politics. Here, the differences between the two approaches become apparent.

Differences

What is special about scientific knowledge? In his 1984 work 'The Phenomenon of Social Representations' as well as in his 1993 'Social Psychology of Science', Moscovici uses elements of a rather 'positivistic' or 'verificationist' epistemology and statements resembling Popper's 'falsificationist' approach interchangeably to characterize science and to differentiate it from other cultural traditions. For example, the statement "…The scientist is required to falsify, to try to invalidate his own theories and to confront evidence with counter-evidence" (1984, p. 41)" is absolutely in line with Popper's epistemology, whereas the aforementioned alleged claims for "uninvolvement, repetition of experiments, distance from the object, independence from authority and tradition" (1984, p. 35) in science are more reminiscent of a positivistic 'scientism' that Popper rejected (e.g. in 1969/1976, p. 90-91). To Popper, *every* observation is theory driven – in the 'natural sciences' as well as in the 'social sciences' and the humanities'; and every scientist is 'prejudiced' in the sense that they have certain expectations. What enables growth of knowledge in spite of these biases among individual researchers is the "… friendly-hostile division of labour among scientists, of their co-operation and also of their competition" (1976/1960, p.95). One could maybe say that the 'checks and balances' created by competing scientists make it possible for individuals to overcome prejudice and to attain *increasing objectivity* and to get *closer* to the *truth*.

For Moscovici, a positivistic approach focusing on the rules and procedures of scientific inquiry and Popper's focus on criticism and falsification are both aiming at the creation of a 'reified' universe: In the long run, the 'individuality' and the 'ambiguity' of knowledge will be reduced. Instead of treating knowledge as something that allows an individual to make sense of the world and to communicate with his environment ('consensual universe'), the knowledge seeking subject is erased and only the mere 'objects' of scientific inquiry remain – stripped naked of their ambiguous and individualistic ramifications ('reified universe'). Popper would indeed agree that this is the aim of science, although it never can be reached. However, he would still maintain that it is not the positivistic idea of the removal of 'idols' such as subjectivity and prejudice that drives the increasing objectivity of scientific knowledge, but the removal of the 'idol of certainty' (Popper, 1959/2002, p. 281): Only through criticism and by means of never being content with the explanation at hand can science progress towards the unattainable goal of objectivity.

The difference can maybe be summarized by saying that for Moscovici science aims at creating a parallel universe to the consensual common sense universe, in which subjectivity and ambiguity are removed – a task that is impossible and doomed to fail. Hence, Moscovici seems to remain a skeptic when it comes to the question of whether a growth in scientific knowledge is possible. To Popper, there are no parallel universes; but within the consensual universe, science occupies a privileged place among the representational systems by means of allowing for and endorsing and accepting criticism more than other institutions such as arts and politics – and because of this 'critical tradition', scientific progress is possible *within* a consensual universe.

Normal science and its dangers. Popper was an outspoken critic (see for example Popper, 1970) of what Moscovici called 'ordinary science' in his article 'Social psychology of science' (1993). In the tradition of Kuhn (1962), 'ordinary' or 'normal' science refers to scientists who work within the limits of a certain paradigm. Only when the discrepancies between the inferences 'allowed for' by the respective paradigm and observations or scientific 'facts' in general grow to be too abundant, is there a chance for an extraordinary scientist to 'spill out' a new scientific idea, the implications of which are consequently 'mopped up' (ibid) by a group of normal scientists working within the limits of the new paradigm. Popper equates normal science with bad science

which contributes nothing to a growth of knowledge and is the result of a lack of training in critical thinking (1970, p. 51). Actually, Moscovici himself would completely agree that without minorities forcing revolutions upon a majority's worldview, progress would be impossible. But his claim still holds that phases of consolidation are needed to prevent chaos from uncontrolled destructive attempts at revolutions. Popper would agree that in spite of all criticality scientists still sometimes have to stand up and defend their theories and not be willing to give them up prematurely. But if scientists in principle agree to 'play by the rules,' there should still be an improvement *over time* which is only driven by criticism and falsification and the aforementioned 'checks and balances' through other scientists. Again the importance of understanding science as a social enterprise becomes obvious.

Popper, while equating the term 'paradigm' with a (widespread and generally accepted) theory, would also agree that observations are always theory-driven and hence take place within a certain paradigm. The difference with Kuhn is that for Popper the very fundamentals of the competing paradigms/theories can and should always be criticized and discussed as well, while Kuhn (1962) argues that mutual understanding depends on a common set of assumptions and can only take place within a paradigm. When applied to scientific representations, contemporary social representations research (e.g. Howarth, 2006) seems to be more in line with Popper's position then with Kuhn's: Social representations can be contested, discussed, and renegotiated, if people are motivated to try to understand each other's 'worldview' or 'ideology'. Moscovici himself emphasized the necessity of a "strife of ideas" – in science as well as in society – in a conversation with Ivana Markova (Markova & Moscovici, 1998, p. 3 403).

Popper's 'psychological argument' in favor of a critical approach. In the end, the question of the incommensurability of frameworks/paradigms in scientific discovery can be resolved neither by empirical means nor by logical arguments. Instead, it may come down to a question of beliefs and of consequences of different beliefs. If we believe that the truth depends on a framework, we may too easily give up on trying to find better solutions for the problems we face, whereas a belief in mutual understanding and the possibility of an underlying truth can motivate us to strive for improvements. This is only true as long as the quest for truth is not impeded by the 'idol of certainty', for example in the form of a propagation of a 'scientific method' that assures 'true

knowledge' in a positivistic sense (as seems to be the case in contemporary 'mainstream' social psychology; see Holtz & Monnerjahn, under review): truth is only helpful as an unattainable regulative ideal; as soon as someone claims to have found the truth, the quest for improvement is over and science grinds to a standstill. From this follows that the more a scientific discipline follows a critical approach, for example by devising methods and methodologies that force scientists to question their assumptions and to give up ideas they hold dear in favor of others under certain circumstances, the more or faster a discipline will progress. Moscovici seems to be unaware of this very convincing 'psychological' argument in favor of a critical approach; at least, he does not discuss it in his attempt at a psychology of science (1993).

CONCLUSION

Whereas there are substantial similarities between Popper's TWT and Moscovici's SRT when it comes to everyday knowledge, there are differences with regard to the question of how scientific knowledge and everyday knowledge differ from each other, also regarding the questions of whether a growth of scientific knowledge is possible and how. To Moscovici, science is 'after all is said and done' a part of the consensual universe, although it gives the impression of a 'reified universe'. In using positivistic and falsificationist ideas and 'jargon' interchangeably, he seems to be unaware of the fact that whereas Popper's epistemology aims at what Moscovici calls a 'reified universe' (e.g. 1984) which is finally unattainable, every step along the way towards increasing objectivity takes place completely within a consensual universe.

Scientific knowledge is – just like *any* knowledge –a social phenomenon that necessarily transcends individuals' states of minds and their embodiments (materializations, reifications, artifacts, world 1 objects). A growth of scientific knowledge is only possible if we are discontent with what we have and try to improve on that. Measuring new solutions against the best ones at hand prevents as well possible chaos caused by 'wild' and random attempts at improvement. Being aware that scientists are never 'value-free' and that they will more often than not go to great lengths to defend their ideas, science first of all needs a critical approach in the sense of a critical mindset – irrespective of any scientific discipline or its methods of scientific inquiry.

'After all is said and done' – Popper, who often criticized the idea that a 'psychology of science' could make any meaningful contribution to a 'philosophy of science', in fact created

with his 'three worlds' a theory that very closely resembles contemporary 'socio'-psychological (in the sense of a 'social' social psychology beyond the 'mainstream' social psychology's methodological individualism; e.g., Taylor & Brown, 1979) approaches, such as Moscovici's theory of social representations. Reducing 'knowledge' to something that happens exclusively within individuals' minds was pointless to Popper – just as it was pointless to Moscovici. But both would probably have agreed that without taking into account the social dynamics of knowledge construction, a philosophy of science would be incomplete. However, Popper would probably maintain that the question of the possibility of a growth of knowledge can only be answered by resorting to formal logic. Still, science does not happen in a vacuum, but takes place right within our social world as an inseparable part of the social world.

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