Vision, Revolution, and Classical Situation: Schumpeter's Theory of Scientific Development[1]

Peter Kesting*

***Abstract*:** The aim of this paper is to show that the Austrian economist Joseph A. Schumpeter developed a sophisticated theoretical understanding of the process of scientific development in his writings. The existence of this theory of scientific development has been hardly recognised until now because Schumpeter never elaborated it systematically or presented it in a book or essay, so that it can only be derived from a number of statements scattered over his whole work. The main task of this paper is thus to reconstruct this 'unwritten chapter' of Schumpeter's work. It will be seen how Schumpeter developed his basic understanding of the process of scientific development already in his early work and how he elaborated this basic idea into a sophisticated theoretical framework in his later work. As a result, this paper finds astonishing parallels between Schumpeter's theory of scientific development and twentieth-century philosophy of science – and in particular that of Thomas S. Kuhn.

1 Introduction

The Austrian economist Joseph A. Schumpeter is primarily acknowledged for his analysis of capitalist development. However, for several years his methodological writings have also been receiving growing attention. By now, there exists a rich body of literature on this dimension of his work (Shionoya 1990a, 1990b, 1991, 1997 [1995]; Swedberg 1991a, 1991b; Streissler 1994; Kesting 1997, 2004; Ebner 2000; Graza Moura 2002). One very puzzling aspect of this literature is Schumpeter's contribution to the analysis of the process of scientific development. In many writings there appears a (sometimes very vague) perception that Schumpeter has *something* to say about this topic. But statements on this question are generally not very much elaborated and remain often unclear and sometimes even contradictory.

Probably the most important reason for this lack of understanding is that Schumpeter himself never elaborated his ideas about scientific development in a systematic way. He did not devote any single essay or even chapter to this topic. Instead he only made a number of statements, scattered over his whole work, mostly in the context of different issues. This raises the question of how deep Schumpeter's understanding of the process of scientific development really was.

The main thesis of this paper is that Schumpeter, in fact, did develop a comprehensive theoretical framework of scientific development that was very innovative and ahead of his time. Moreover, this framework, which I will label Schumpeter's *theory of scientific development*[2] accompanies his whole work as an important integrative element – in particular his work on methodology and on history of economics. Schumpeter had developed a basic understanding of the process of scientific development already in his first book *Das Wesen und der Hauptinhalt der theoretischen Nationalökonomie* (The Essence and Main Content of Theoretical Economics, 1908) and regularly shifted his focus back to the topic in

the following years. However, it was particularly in his *History of Economic Analysis* (1954) that he seriously turned back to his early ideas and further developed them to a comprehensive theory of scientific development.

One very strong indication that such a comprehensive theory does, in fact, exist, is that there is already a place where Schumpeter intended to write it down: the chapter on 'The Driving Forces of Scientific Work and the Mechanisms of Scientific Development', as a section of Part I of the *History*. Unfortunately, this chapter never saw the light of day because Schumpeter died before he was able to tackle it. Therefore, what remains is only an unrealised intention.

The main task of this paper is to reconstruct the structure and main content of Schumpeter's theory of scientific development, to work out what the content of a chapter called 'The Driving Forces of Scientific Work and the Mechanisms of Scientific Development' could have been. As a result, this reconstruction will uncover an approach of remarkable relevance to the current situation which still provides new and valuable insights into the process of scientific development in general and that of economics in particular. Moreover, this reconstruction will uncover astonishing parallels between Schumpeter and the philosophical theory of science, and in particular with Thomas S. Kuhn's 'paradigm' concept. However, due to space restrictions, this paper is not focused on the comparison of Schumpeter's theory of scientific development with other approaches but on the reconstruction of Schumpeter's own contribution.

Therefore this essay begins with an analysis of how Schumpeter laid the foundation for his basic understanding of the process of scientific development in his early work (section 2). However, the main focus of this essay is on Schumpeter's elaboration of his comprehensive theory of scientific development in his later work (section 3). This analysis is followed by a brief discussion of the question of how far Schumpeter, in fact, anticipated recent developments in the philosophy of science in general and that of Kuhn in particular (section 4).

2 The Theory of Scientific Development in Schumpeter's Early Work[3]

Schumpeter's early understanding of the process of scientific development is strongly embedded in his general economic methodology. This general economic methodology, again, is mostly driven by the need to provide a methodological foundation for Walrasian equilibrium analysis, which Schumpeter understood as the state of the art for economic theory at that point in time (Samuelson 1951; Swedberg 1991a; Kesting 1997). Particularly in *Das Wesen und der Hauptinhalt der theoretischen Nationalökonomie* (1908), he described Walrasian equilibrium models as axiomatic theories in a Machian sense, that is, as systems of elements.[4] But, in contrast to Mach, Schumpeter was convinced that it is generally not possible to justify axioms inductively on the basis of empirical investigations. Instead, at this point, he followed the conventionalist position (especially that of Henri Poincaré) according to which axioms can only be obtained on the basis of assumptions (cf. Poincaré 1905 [1902]). However, Schumpeter realised the consequence: in this case, theories are no more than provisional constructs that *ultimately* originate in the scientist's head. The quality of such theories cannot be proven until they are tested by confrontation with empirical evidence. Consequently, Schumpeter demands: 'If we come to a particular deductive result on

the basis of a system ... we have to prove it *like all other results* according to reality' (Schumpeter 1970 [1908], p. 60, emphasis added).[5]

This means that at least since 1908, and therefore more than a quarter of a century *before* Karl Popper's *Logic of Discovery* (1935), Schumpeter based his methodological understanding on a falsification principle. Consequently, as in Popper's work, the testing of a theory with empirical knowledge becomes not only the touchstone but also an essential engine of scientific development. This insight that theories do not provide evidence and 'can always be disavowed by the facts' is the methodological starting point of Schumpeter's theory of scientific development.

Schumpeter recognised right from the beginning that theories are complex structures that cannot be refuted by just one counterfactual observation, but only by a long process of academic 'upheaval'. He pointed out that a theory will not be immediately rejected if it fails in only one respect:

> Surely, this makes the whole system look very suspect to us because now one should be prepared to find other discrepancies in real life. However, if there are reasons for us to remain committed to the theory, we will look for a complementary hypothesis. Perhaps the most prominent example of such an approach in the history of science is the history of the *Ptolemaic* system. Ultimately, the number of provisional hypotheses needed to rescue a theory come to the point where they must be rejected and a new hypothesis, more general, simple and youthful, replaces the old one, not without facing the same destiny as soon as its task is fulfilled. However, nothing would be worse than to blame the old system or even to label it as totally void. *Besides the fact that it still might teach us something, we should never forget that there is nothing totally true and perfect....* (Schumpeter 1970 [1908], p. 60; emphasis added)

From this Schumpeter concludes that the development of science is by no means a continuous process. He points out that

> the development of human culture and of knowledge occurs erratically. There is a permanent alternating process of enormous progress and periods of stagnation, overwhelming hope and bitter disillusionment, and although something new may be based on something old, progress is not continuous. Our science can tell us a thing or two about that. (Schumpeter 1970 [1908], p. 8)

Writing this, Schumpeter clearly had one particular break in mind: the neoclassical revolution which began in the 1870s and ended in replacing the fundamental principles of classical economics. In his early work, Schumpeter was strongly convinced that the work of Jevons, Menger and Walras in fact represented a revolutionary departure from the past in economic theory and that neoclassicism (especially equilibrium analysis in a Walrasian sense) constituted a real overthrow of classical economics. This should not be very surprising, since Schumpeter grew up in the tradition of early Austrian economics and in particular that of Eugen von Böhm-Bawerk and Friedrich von Wieser, who belonged to the first generation of such 'revolutionaries'. In *Wesen und Hauptinhalt* he explicitly expresses this conviction:

> It was the advocates of the new theory who questioned classical economics. Did they destroy it and replace it with something new? This is a question that many have answered very differently. We are not going to resolve the matter here.... but neither are we going to hide our

> own opinion. *Yes, the system of the new theory is essentially new and even the results that correspond with those of the classical system have been achieved by different paths*. Certainly, we owe a whole arsenal of notions and ideas to the classics, certainly, the emergence of the new theory would not be possible without the old; *nevertheless, the latter has been superseded quite 'naturally',—just as much as the older literature of any other science has been*. (Schumpeter 1970 [1908], pp. 8ff; emphasis partly added)

Summarising, it has to be recorded that, even in his early work, Schumpeter had a clear and explicit perception of revolutionary breaks in economics in two respects: (i) methodology, since Schumpeter understood scientific revolutions as a constitutive element of scientific development, and (ii) historical content, as Schumpeter identified at least one of these revolutionary breaks in the history of economics.

As early as 1908, Schumpeter had a clear and explicit perception of the difficulties arising from the concern to 'falsify' a complex theory. More than that, Schumpeter drew the same conclusion from that observation as Kuhn did nearly half a century later: that falsification, and thus scientific development, does not appear as an orderly process of knowledge accumulation, but as 'a permanent alternating process of enormous progress and periods of stagnation'. Nevertheless, in his early work, Schumpeter presented merely a sketch of the process and not a sophisticated theory. This is not very surprising, as his main objective was not to explain scientific development but to understand and promote Walrasian equilibrium theory.[6]

3 The Theory of Scientific Development in Schumpeter's Later Work

After the publication of *Vergangenheit und Zukunft der Sozialwissenschaften* (History and Future of Social Sciences, 1915), Schumpeter did not deal seriously with the problem of scientific development for many years. It was only in his later work, and in particular in *History of Economic Analysis*, that he took up the topic again. However, as mentioned above, he was not able to complete this monumental book. This is particularly true for the crucial chapter that he wanted to entitle, 'The Driving Forces of Scientific Work and the Mechanism of Scientific Development'. Since Schumpeter was unable to tackle it at all, no sketches can be found in his manuscripts from which to gain further information. This fact significantly contributes to the lack of clarity concerning Schumpeter's ideas of scientific development that has been criticised, for instance, by Niehans (1992). Although the *History* is full of statements concerning the subject, they do not form an explicitly coherent approach. Nevertheless, the volume's structure makes it obvious that Schumpeter had a concept of the process of scientific development very similar to that in *Wesen und Hauptinhalt*. Moreover, in the *History* he not only reiterates the existence of scientific revolutions but also orients his entire understanding of the history of economic analysis toward their existence – leading us to the conclusion that he had extended his ideas decisively at this juncture. As a result, it was only in his later work that Schumpeter developed his vision into a comprehensive theoretical approach.

In what follows, this 'theory of scientific development' will be reconstructed in detail. To begin with, it will be shown how Schumpeter developed

an elaborated understanding of the 'individual' process of cognition in his later work.

3.1 The Individual Process of Cognition

Schumpeter considered the examination of the individual process of cognition to be a subject of ***Wissenschaftslehre*** or 'the science of sciences', 'an area of science' that 'starting from logic and to some extent also from epistemology, treats the general rules of procedure as applicable to the other individual sciences' (Schumpeter 1954, p. 33). In the History, Schumpeter draws a very accentuated picture of these 'general rules of procedure'. According to this, analytic effort:

> is of necessity preceded by a pre-analytic cognitive act that supplies the raw material for the analytic effort. In this book, this pre-analytic cognitive act will be called vision....Analytic effort starts when we have conceived our vision of the set of phenomena that caught our interest, no matter whether this set lies in virgin soil or in land that has been cultivated before. The first task is to verbalize the vision or to conceptualize it in such a way that its elements take their places, with names attached to them that facilitate recognition and manipulation, in a more or less orderly picture or schema. But in doing so we almost automatically perform two further tasks. On the one hand, we assemble further facts in addition to those already perceived, and learn to distrust others that figured in the original vision; on the other hand, the very work of constructing the schema or picture will add further relationships and concepts to, and consequently, eliminate others from the original stock. Factual work and 'theoretical' work, in an endless chain of give and take, naturally testing and setting new tasks for one another, will ultimately produce ***scientific models***, the provisional joint products of their interaction with the surviving elements of the original vision, to which increasingly more rigorous standards of consistency and adequacy will be applied. (Schumpeter 1954, pp. 41f)

According to this concept, the individual process of cognition consists of two distinctive parts: 'vision' and 'critical refinement', which result in the formulation of empirically and logically tested models.

In the ***History***, Schumpeter describes the (very famous, but often misunderstood) notion of 'vision' – in the sense of a pre-analytic act of discovery – as an intuitive perception of a phenomenon. Employing the term 'pre-analytic', Schumpeter points out that 'vision' is characterised by being neither systematically nor empirically proven, although it will certainly contain ***some*** empirical impact and logical consistency. In this sense, 'vision' is supposed to be an individual phenomenon. In fact, Schumpeter identifies such a vision for a number of outstanding economists, including Adam Smith, Friedrich List, Karl Marx, William Stanley Jevons, Eugen von Böhm-Bawerk and then, in particular, for John Maynard Keynes. However, it is obvious that the notion of 'vision' and Schumpeter's description of the individual process of cognition do not fit anything better than his own work.

According to Schumpeter, 'vision' has two different meanings for the individual process of cognition: first, it constitutes a starting point and is therefore an indispensable element of every scientific progression, and, second, as a result of its subjective and intuitive nature, it is a permanent source of irrational elements invading science.

Schumpeter dealt intensively with this double-edged role of vision in his presidential address to the American Economic Association in 1948. There he stated that the irrational element of vision has to be seen initially in its ideological bias. Schumpeter explicitly models his notion of ideology on that of Marx and Engels, thus understanding vision as being 'formed by the subjective class order'. Schumpeter's presidential address helps to sum up the meaning of vision for the economic process of cognition as follows:

> That pre-scientific cognitive act which is the source of our ideologies is also the prerequisite of our scientific work. No new departure in any science is possible without it. Through it we acquire new material for our scientific endeavors and something to formulate, to defend, to attack. Our stock of facts and tools grows and rejuvenates itself in the process. And so – though we proceed slowly because of our ideologies, we might not proceed at all without them. (Schumpeter 1951 [1949], p. 286)

Because of its pre-analytic nature, a vision can only be turned into 'scientific knowledge' if it is followed by a process that will prove its logical consistency and empirical content. However, within this process of 'critical refinement', the vision itself is no longer questioned. In the ideal case, the result of this process is an empirically and logically tested economic model.

Now, it would be fair to assume that the individual process of cognition represents the basis of the discontinuity of scientific development, in the sense that the introduction of a new vision anticipates revolution and critical refinement as precursors of a subsequent phase of continuous development. But it is not that easy. Such a view would not meet Schumpeter's standards for assessing the impact of individual cognition on the process of scientific development, or at least not immediately, since he writes explicitly:

> Intuitive perception of novel aspects is indeed never absent so long as a science is really alive. But vision of the kind that produces novel methods or propositions or else leads to the discovery of novel facts—which then enter the science in the form of new hypotheses or restrictions—only adds to and perhaps partly displaces existing scientific structures, the bulk of which is handed from generation to generation as a matter of course. (Schumpeter 1954, pp. 45ff)

At first glance, this view suggests that the process of individual cognition leads to a continuous development of science. In fact, in the *History*, Schumpeter firstly gives an alternative explanation for the existence of scientific revolutions – a sociological one.

3.2 Science as a Social Phenomenon

The consideration of its social dimension is probably the most important extension of Schumpeter's understanding of scientific development in his later as opposed to his earlier work. The sociological dimension of Schumpeter's understanding of science in his later work becomes especially obvious in the methodological Part I of the *History*. Here he wants the 'sociology of science' to be understood as its own, independent field of research and just as important as the theory of science.[7] He wants it to be seen as a field that treats:

> science as a social phenomenon. That is, it analyzes the social factors and processes that produce specifically scientific types of activity, condition its rate of development, determine its direction toward certain

> subjects rather than other equally possible ones, foster some methods of procedure in preference to others, set up the social mechanisms that account for success or failure of lines of research or individual performances, raise or depress the status and influence of scientists (in our sense) and their work, and so on. (Schumpeter 1954, p. 33)

That this sociological understanding of science assumes a central position in Schumpeter's understanding of science becomes especially apparent in the definition of 'science' he provides in the *History*:

> ....a science is any field of knowledge in which there are people, so-called research workers or scientists or scholars, who engage in the task of improving upon the existing stock of facts and methods and who, in the process of doing so, acquire a command of both that differentiates them from the 'layman' and eventually also from the mere practitioner. (Schumpeter 1954, p. 7)

In this definition, Schumpeter – in contrast to contemporary doctrine – fixes the demarcation of science using sociological criteria. According to this definition, anybody belongs to the scientific community as soon as he is a member of a group of people who differ from other people in their joint and exclusive command of special knowledge and techniques. In Schumpeter's work, nothing is mentioned about a methodological demarcation of science, for example, Popper's criterion of falsification or the linguistic criteria of Logical Empiricism[8]: science is what scientists are doing, and scientists are the members of the scientific community. This definition naturally implies a complete tolerance toward alternative methodologies (which was later particularly advocated by Paul Feyerabend 1975) and, in fact, Schumpeter writes:

> Science being tooled knowledge, that is, being defined by the criterion of using special techniques, it seems as though we should have to include, for instance, the magic practised in a primitive tribe if it uses techniques that are not generally accessible and are being developed and handed on within a circle of professional magicians. And *of course* we ought to include it in principle. (Schumpeter 1954, p. 7)

Within his definition, Schumpeter – like Kuhn later – elevates the scientific community itself to a constitutive feature of science. Consequently, science has to be understood and described as the very activity and functioning of such scientific communities. This is exactly Schumpeter's idea of scientific development when he writes:

> [It is] a more or less definite group of professionals who teach the rising generations not only their methods and results but also their opinions about the direction and the means of further advance. In a majority of cases competence in doing scientific work cannot be acquired, or can be acquired only by individuals of quite exceptional originality and force, from any source other than the teaching of recognized professionals....
>
> First of all it should be observed that this social mechanism is tremendously labor-saving. By means of it any beginner who follows the advice received and who does the work assigned to him acquires knowledge of facts, grasp of problems, mastery of methods with an economy of energy that should set the bulk of his force free for exploration of lands that lie beyond the boundary line at which the competence of the teacher ends. There should be no reasonable doubt about it, therefore, that primarily the social mechanism glanced at is not

> only favorable to the development of conceptual apparatus and to the accumulation of factual knowledge but even that it supplies the most potent motive power of what is usually referred to us as scientific progress. Obviously, however, there is also another side of the medal. Teaching in any established science stereotypes the mind of the tyro and many stunt such originality as he may have. This has another and less obvious consequence. Owing to the resistance that an existing scientific structure offers, major changes in outlook and methods, at first retarded, then come about by way of revolution rather than of transformation and elements of the old structure that might be permanently valuable or at least have not yet had time to yield their full harvest of result are likely to be lost in the process. (Schumpeter 1954, p. 46)

In contrast to his earlier work, here Schumpeter – again very similar to Kuhn – gives a purely sociological explanation for the existence of scientific revolutions. Scientific communities obtain their team spirit not only from their common usage of methods but in particular from their common opinions about the direction and means of further advance. Schumpeter labels this common basic conviction as a 'stereotype'.[9] The crucial point, however, is that he regards these stereotypes as only minimally flexible. For this reason, scientific communities cannot carry out major changes if these would lead to a revision of the communities' stereotypes. These changes can only succeed *against* the communities, that is through a scientific revolution.

Unfortunately, at this point, Schumpeter's manuscript breaks off. Hence we get to know very little about the structure of the 'group of professionals' and consequently about his concept of the 'sociology of science'. The very nature of the stereotype, along with its importance for the scientific community's team spirit, remains blurred as do the reasons for its inflexibility. However, despite all this lack of clarity, this sociological understanding of science constitutes the basis for the reformulation of the theory of scientific development in Schumpeter's later work.

3.3 The Process of Scientific Development

Just as his early work, Schumpeter's later work conceptualises the process of scientific development as a dialectic of revolutionary upheavals and continuity. However, he now presents this sequence in greater detail and characterises it as an 'ideal typical' pattern.

According to this characterisation, a revolution is followed by a time of upswing, which he labels a 'classical situation'. A classical situation ultimately finds its synthesis and, so to speak, its conclusion in a 'classical achievement', which is in turn followed by a time of stagnation in which the ground for a new scientific revolution is prepared. Schumpeter calls this entire process from revolution to stagnation a 'period'. For instance, in the *History* he describes one of these periods for the time

> between the 1790s and the end of the 1860s.... [T]here was, first, fresh activity that struggled hopefully with the deadwood; then things settled down and there emerged a typical classic situation in our sense, summed up in the typically classic achievement – again, in our sense of the term – of J.S. Mill, who underlined the fact by his attitude of speaking from the vantage ground of established truth and by the naïve confidence he placed in the durability of this established truth. Then

> followed a stagnation – a state that was universally felt to be one of maturity of the science, if not one of decay; a state in which 'those who knew' were substantially in agreement; a state in which, 'the great work having been done', most people thought that, barring minor points, only elaboration and application remained to be done. (Schumpeter 1954, p. 380)

Consequently, the whole process of scientific development has to be seen as a succession of such periods. Each of these periods implies a predominance of a stereotype that is removed in turn by each scientific revolution. Thus the process of scientific development can also be interpreted as a succession of different stereotypes.

At this point, the analogy between Schumpeter's theory of scientific development and his theory of economic development becomes more than obvious: in this analogy, the 'stereotype' is represented by a given production regime, the 'revolution' by a successful innovation, the 'classical situation' by the diffusion of that innovation, and the 'classical achievement' by the new equilibrium that the economy approaches after the successful innovation. A 'period' finds its equivalent in an innovation cycle or business cycle, respectively.[10] Nevertheless, for reasons I do not understand, Schumpeter never really made use of this analogy.

However, Schumpeter's conceptualisation of the process of scientific development leaves two particular questions open that have to be further investigated in detail: (i) how is this idea of scientific development related to that of the process of individual cognition? and (ii) what is the relationship between stereotypes and individual visions?

The missing link between the individual process of cognition and the process of scientific development points to the fragmentary character of Schumpeter's ideas as a whole. Whether, and if so how, Schumpeter wanted to make the connection in the unwritten chapter of 'The Forces of Scientific Work and the Mechanism of Scientific Development' remains speculative. Still, there are three connections to take into consideration.

First, one can follow Schumpeter's statement that 'Vision of the kind that produces novel methods or propositions or else leads to the discovery of novel facts .... only adds and perhaps partly displaces existing scientific structures, the bulk of which is handed from generation to generation as a matter of course' (Schumpeter 1954, pp. 45f). This would mean that the individual process of cognition leads to continuous development within a given structure. In this case, the 'individual process of cognition' would occur merely in a classical situation. Keeping in mind the notion of revolutions being characterised by a discretionary change of a structure (instead of its continuous expansion), we then have to search for the basis of revolutions somewhere besides the individual process of cognition.

That the individual process of cognition can occur in classical situations is very plausible, as long as the vision on which it is founded conforms to the predominating stereotype of the period. But it is very implausible that the process would be restricted to a classical situation. After all, 'vision' not only represents an irrational, but also a constitutive, part of science. There is no reason why a 'vision' cannot come into conflict with a stereotype and eventually triumph. Above all, this interpretation leaves the question wide open as to which event would be sufficient to trigger a scientific revolution if not an individual vision. Hence, restricting individual cognition to processes that conform to a given structure is obviously too narrow a view.

Second, one can assume that the processes of individual cognition and scientific development are identical, just described from different perspectives: the first from the perspective of the 'theory of science,' and the second as viewed by the 'sociology of science'. That the process of cognition does not need to be an individual occurrence but can result from the collective work of scientists becomes clear when Schumpeter writes: 'In practice, of course, no scientific worker ever goes through all the stages of the work beginning with an independent vision of his own' (Schumpeter 1954, p. 45).

Consequently, a nascent revolution would represent the process of the introduction and acceptance of a vision, while the classical situation would represent its critical refinement. The classical achievement would be equivalent to the synthesis of a vision in an empirically and theoretically tested scientific model that, in turn, would be raised to a new stereotype. Since major changes can only arise through a revision of an existing stereotype, this synthesis must inevitably be followed by a period of stagnation, as long as it is not revolutionised.

Such an interpretation is supported by a statement in the *History*, in which Schumpeter emphasises the discontinuity that results from the introduction of a vision:

> It is interesting to note that vision of this kind not only must precede historically the emergence of analytic effort in any field but also may re-enter the history of every established science each time somebody teaches us to *see* things in a light of which the source is not to be found in the facts, methods, and results of the pre-existing state of the science. (Schumpeter 1954, p. 41)

On the basis of this interpretation it is possible to achieve a much more detailed insight into the process of scientific development than would be possible without it. Particularly, it elucidates the concept of stereotype that so far has remained shadowy. Yet it could be understood simply as a vision that is shared by a specific group of scientists as a common underpinning for their work. The idea of discontinuity as the result of an antecedent vision also applies to the phases of scientific development, whose position within the total process can be clarified by a discourse on Schumpeter's concept of the individual process of cognition.

Speaking against such an interpretation, however, is the fact that Schumpeter ascribes the successful introduction of a vision to economists to whom he definitely does *not* ascribe the successful revolutionising of economics, particularly Smith, Marx and Keynes.

Third, it is possible to assume that the individual process of cognition is a comprehensive phenomenon that can result in both scientific revolutions as well as (non-revolutionary) scientific development within a classical situation. In this case, the successful introduction of a 'vision' can induce fundamental changes that can only be carried out against a given 'stereotype'; it can also induce minor changes that are compatible with the given 'stereotype' and can be interpreted as a 'critical refinement' of it.

Yet this third approach to a connection of individual 'vision' and shared 'stereotype' results in a problem: if we assume that every successful introduction of a vision brings about some qualitative change, the history of economics is supposed to be full of such qualitative changes. Therefore the question arises what exactly marks the difference between revolutions and interim revolutionary introductions of a vision. When exactly is a qualitative change deep or important enough to affect a 'stereotype' as a whole and when does it bring about only a 'critical refinement' of

it? To answer these questions, one obviously has to have a specific idea of the core content of the 'stereotype' for each period. For example, one has to be able to identify the core content of neoclassical economics to recognise which particular structural change would affect this 'stereotype' and which would not. Moreover, one has to be able to detect if the introduction of an incompatible 'vision' in fact succeeded in revolutionising a 'stereotype' or if the established stereotype survived and banned the new vision to a parallel world (or even assimilated it).

However, such differentiation is obviously very difficult to achieve. Hence it is not surprising that precisely this multi-layered problem – how to date particular periods and determine whether structural breaks or certain events mark a revolutionary break or not – caused serious difficulties for Schumpeter in struggling to interpret the history of economic analysis.

3.4 Revolutions in the History of Economic Analysis

At first glance, it appears as if Schumpeter could easily have applied his theory of scientific development to the history of economic analysis. Schumpeter was convinced that economic analysis in the second half of the eighteenth century culminated in a classical situation, summarised in Adam Smith's *Wealth of Nations* as the classical achievement (Schumpeter 1954, p. 52). In this era, economic analysis was pursued for the first time by professional groups, and thereby – in line with Schumpeter's definition – as a science. Smith's classical achievement was followed by a phase of stagnation that prepared the ground for the next revolution in which the era of classical economics was introduced. This era again culminated in the classical achievement of John Stuart Mill (Schumpeter 1954, p. 379). The classical achievement of Mill was followed by a typical phase of stagnation with all the symptoms of a scientific crisis. The crisis laid the foundation for the neoclassical revolution that was initiated by the work of Jevons, Menger and Walras in the 1870s and paved the way for a new classical situation. (It also prepared the ground for the rise of the Historical Schools to which Schumpeter in the *History* – like in his early work – explicitly does *not* attribute the revolutionising of economics.) Schumpeter identifies Marshall's *Principles of Economics* as a classic achievement of this period.

Following this line of reason, the history of economic analysis has to be understood as a succession of three periods, namely (i) the pre-classical period, which can be dated from the beginning of economic analysis to the 1790s, (ii) the period of classical economics which ended in the 1870s, and (iii) the period of neoclassical economics which, at the time Schumpeter wrote the *History* in the middle of the twentieth century, was in a phase of stagnation.

And, in fact, this succession of periods exactly corresponds with the structure of the *History*, around which the vast material treated in this book is organised.

Upon closer examination, however, Schumpeter's analysis of the history of economics loses a lot of clarity. This particularly applies to the time period before the classical achievement of Mill (that is, from the beginnings of economics up to the year 1848). Particularly, the description of the revolution by which the period of classical economics should have been introduced appears fairly unconvincing. It seems that Schumpeter has serious problems with identifying a revolutionary break between pre-classical and classical economics. In the *History*, Schumpeter writes that he can see 'no point in insisting on a particular year, but if we did so insist, we

might start a new period of analytic activity with Malthus's first *Essays on Population* (1798)' (Schumpeter 1954, p. 379).

But, shortly afterwards, Schumpeter contradicts this statement by rating the content of Malthus's work which is obviously not very revolutionary, since he writes:

> Except for his theory of saving and investment, which on the face of it seems to be Malthus's own, all the elements that enter into the analytic apparatus of the work, and even its terminological arrangements, point to the first book of the *Wealth of Nations*. (Schumpeter 1954, p. 482)

Who, if not Malthus, should have revolutionised economics at this time? I can only think of Ricardo (although the year of publication of his *Principles*, 1817, arrives considerably late in the period). Yet Schumpeter writes of Ricardo: '[Seen] objectively all the ideas of the *Principles* are individually met before, and we cannot attribute more than effective synthesis to Ricardo' (Schumpeter 1954, pp. 474f). He resumes: 'Ricardo's theoretical structure represents a particular way of re-forming the *Wealth of Nations*; Malthus's theoretical structure represents another way of doing this' (Schumpeter 1954, p. 472).

According to these statements, it can hardly be said that classical economics emerged by revolutionising the *Wealth of Nations*. Schumpeter suggests, instead, that economic analysis in the entire period from Smith to Mill developed more or less continuously, although in thrusts.

However, Schumpeter treats this period completely differently in an early version of the preface to his *History*:

> Still keeping to the English example, we can readily follow a smooth and straight road back to Mill's work that makes another classical situation (1848) and from Mill, *via* Ricardo to Smith the most typically classical of all economists. But there the English highway stops – there are no classical situations further back. (Schumpeter, *Beginnings*, p. 3)

And in his *Mexican Lecture* Schumpeter even goes so far as to state: 'Scientific economics found its systematic form in the 18th century (Beccaria, A. Smith, Turgot) and, after *various* "revolutions", in the *Principles of Political Economy* of J. St. Mill' (Schumpeter, *Mexican Lecture*, p. 4, emphasis added).

From all this follows what was already in the offing in the theoretical treatment of Schumpeter's idea of scientific development: Schumpeter had problems applying his scheme to the history of economic analysis, resulting from the fact that he had observed several outstanding achievements and even breaks in the history of economic analysis but which he found to have differing significance. At this point he had to decide which of the breaks led to a change of the predominant stereotype, and thus had to be understood as their *own* periods of scientific development. However, such differentiation between the revolutionising *of* a predominant stereotype and changes *within* it appears arbitrary to some extent.[11]

4 Schumpeter and the Philosophy of Science

Summarising, it does not seem exaggerated to me to conclude that Schumpeter's theory of scientific development and its implications was substantially ahead of his time. In some sense, Schumpeter followed the same course of reasoning, faced the same questions and came to similar answers as the twentieth-century philosophical theory of science did, but a reasonable time before that.

Schumpeter started out with an early application of positivism to Walrasian equilibrium theory. In particular, he employed the axiomatic approach of Ernst Mach, which is nowadays acknowledged as one of the main sources of the modern philosophical theory of science. Proceeding in that direction, Schumpeter was not only one of the earliest researchers who systematically applied philosophical positivism to economics, but also one of the earliest economists to strive for a consistent methodological foundation for Walrasian equilibrium theory.

Moreover, Schumpeter was not content with a mere application of Machian positivism to Walrasian equilibrium theory. Instead, he actively identified problems and provided solutions. However, the really astonishing fact is that, although Schumpeter was confronted with the same challenges, he did not step into the same traps that philosophical theory of science did afterwards.[12] Schumpeter never succumbed to the temptation to strive for any 'absolute' evidence of axioms like, for instance, Rudolf Carmap, Moritz Schlick and other members of the Vienna Circle did for a long time. In particular he refrained from any inductionism.[13] Instead, he accepted axiomatic systems right from the beginning as provisional constructs whose usefulness can only be proved by a confrontation of their theorems with empirical data (which Schumpeter, in his early naïve view, simply regarded as 'facts').

Nevertheless, Schumpeter did not react to this insight with some kind of 'mechanical' falsificationism in a Popperian fashion, but realised right from the beginning that theories are complex structures that can only be overcome via revolutionary 'upheavals'. Moreover, in his later work, he realised that science in general and the process of revolutionary replacement of theories in particular have strong sociological implications. This brings the theory of scientific development, at least in its elaborated version of Schumpeter's later work, in close proximity to the paradigm approach of Kuhn. Consequently, the final section of this paper will be focused on the comparison between Schumpeter and Kuhn. In particular, I will discuss to what extent it is appropriate to acknowledge Schumpeter as a predecessor of Kuhn's paradigm approach.

When comparing Schumpeter's statements about scientific development with those of Kuhn, one will first of all notice the maturity of the latter by comparison with the former. Schumpeter did not connect his ideas to an encapsulated structure. Moreover – as already shown – his statements often lack clarity and sometimes appear inconsistent. However, one of the most important intentions of this essay is to show that Schumpeter's ideas are far more than a mere intellectual pastime. On the contrary, they constitute a consistent approach that is well founded by an impressing command over detailed insights about economic history. Schumpeter simply never shifted his attention to the theory of scientific development as an independent subject of investigation and only employed it as a background framework for the investigation of different subjects.

But what was the reason for this? Why did he never connect his insights about scientific development to a coherent framework and expound it in a book, or at least in an essay? One answer to this question has already been given. He, in fact, intended to do so in the *History* when he picked up the topic of scientific development again after some thirty years, but was not able to finish his work before his death. However, although correct, this answer obviously does not tell the whole story. At the end, Schumpeter definitely had had enough time to tackle the topic before he started his work on the *History*. But more than that: he never really preoccupied himself with the contemporary philosophical theory of science at all.

There are almost no references to the Vienna Circle, to Popper, Wittgenstein and all the philosophical discussions about inductionism, falsificationism and so forth in his work. This is particularly amazing, as many of these developments and discussions took place in Schumpeter's old home town, Vienna. Consequently, ignorance can definitely be ruled out as a cause for Schumpeter's silence. Instead, it rather looks as if Schumpeter simply was not interested in this philosophical research.

And this is exactly what could provide an explanation. Schumpeter might have simply followed the basic conviction of Ernst Mach that epistemology and the theory of science do not constitute an interesting field of research on their own, but only contribute to applied research as an ancillary science – to physics in the case of Mach and to economics in the case of Schumpeter. (As explained above, Schumpeter's early methodological thinking was deeply influenced by Mach.) And in fact, Schumpeter's research interests were always focused on economic, sociological or political topics, like, for instance, the foundation and support of Walrasian equilibrium theory, the explanation of capitalist dynamics, or the understanding of the course of the history of economic thought. In this research, he made extensive use of methodological arguments, but always only to support his economic reasoning. However, Schumpeter never expressed his attitude concerning the philosophy of science explicitly, so that this line of reasoning finally remains speculative.

Yet, if one disregards this difference of elaboration, the approaches of Schumpeter and Kuhn, indeed, exhibit astonishing parallels.

Both of them stop defining science merely on the basis of the employment of a particular method, as did the Vienna Circle or Karl Popper. Instead, Schumpeter and Kuhn connect the existence of science to the action of professional communities: 'Science is what scientists do'. Consequently, both are convinced that a particular science can only be understood against the backdrop of knowledge about such scientific communities, that is, within sociological categories. At first, this view seems to imply absolute tolerance towards alternative methodologies because nobody dictates which particular methods a community has to agree upon to be considered scientific. However, neither Schumpeter nor Kuhn fall into scientific relativism – both are convinced that the process of agreement within scientific communities takes place, at least to some extent, on the basis of rational criteria.

Both Schumpeter and Kuhn are convinced that science has to be built around a non-rational core. For Schumpeter such a non-rational core is a 'vision'. Because of its programmatic character, Schumpeter's vision might seem to resemble the Lakatosian research programme rather than the Kuhnian paradigm. The crucial process of critical clarification, which leads to an increasing rationalisation of vision, in some ways resembles the Popperian procedure of testing and falsification. However, Schumpeter comes closer to Kuhn since he understands vision not only as an individual phenomenon but also (in its role as a stereotype) as a mutual programmatic cornerstone of scientific communities, and emphasises the importance of value judgements in perpetuating the activity of scientific communities. However, concerning this topic, neither Schumpeter nor Kuhn is very clear.

Another important parallel between Schumpeter and Kuhn, of course, is their basic concept of scientific development. Both of them distinguish phases of scientific development in which research is built on a given foundation (the stereotype or the paradigm, respectively) from phases in which an existing foundation is displaced by another one – scientific revolutions.

Another (even if only gradual) difference is that Kuhn emphasises the existence of a *Gestaltwandel*, the idea that against the backdrop of a new paradigm, the world will also be perceived in a new way, whereas Schumpeter – at least in his earlier work and in the tradition of Ernst Mach – gives the perception of the world a rather objective content.[14] However, in his later work Schumpeter broadens this view by writing that 'vision….may re-enter the history of any established science as soon as somebody teaches us to *see* things in a light of which the source is not to be found in the facts, methods, and results of the pre-existing state of the science' (Schumpeter 1954, p. 41).

Summing up, these parallels are far-reaching. It does not seem exaggerated to me to suggest that Schumpeter anticipated Kuhn's basic ideas, so that Schumpeter's work should be acknowledged as a precursor to Kuhn's – just as the work of the Polish physician Ludwik Fleck (1980 [1935]) preceded that of Kuhn.

Kuhn did know Fleck's statements, but he seemed unaware of Schumpeter's work,[15] which is astonishing as both of them had been at Harvard University at the same time. Moreover, Kuhn reports that he developed the most important ideas for his *Structure* just at the time when Schumpeter was writing the *History* (Kuhn 1970, pp. 7ff). This strikes me as further evidence that in the same intellectual climate similar ideas can develop independently.

5 Conclusion

The aim of this paper was to show that in Schumpeter's work one can discover a comprehensive theoretical concept of the phenomenon of scientific development. Moreover, it aimed to demonstrate that this concept was truly original at the time of its formulation and, as such, has to be acknowledged as an outstanding scientific achievement.

However, for the characterisation of Schumpeter's theory of scientific development one has to distinguish between his early and his late work. Although Schumpeter held a similar perception of the scientific development as a discontinuous process throughout his whole work, he changed his interpretation of this process decisively.

In his early work, Schumpeter interprets revolutionary breaks as part of the process of discovery. They are the result of the 'falsification' of established theories whose empirical content became doubtful and thus had to be replaced by the introduction of a new theory. Schumpeter describes this replacement as a purely intellectual change of basic assumptions and, thus, as independent of social structures. Schumpeter realised early on that science has a sociological dimension, but he systematically related the importance of such a dimension only to the *Methodenstreit*, the battle over methods, that for him played no constructive role in scientific development.

In his later work Schumpeter presents a much more sophisticated explanation, distinguishing between the individual process of cognition and the (overall) process of scientific development. With the individual process of cognition, Schumpeter describes the formulation and development of new ideas to understand economic reality. His description of the individual process of cognition provides a much deeper understanding of the emergence of new theories than that revealed in his early work. Moreover, he now recognises that not only unproductive controversies over methods, but also the whole process of scientific development, are determined by the existence of scientific schools and thus have to be explained

in sociological terms. This leads him to the belief that scientific development follows a certain pattern of recurrent periods that are initiated by scientific revolutions and culminate in classical achievements. A scientific revolution, he now recognises, is not simply a replacement of an old theory by a new one, but signals the prevalence of a new school with a new 'stereotype' that can be contrasted with the old one. At last (although he never resolves the relationship between the individual process of cognition and scientific development), Schumpeter provides a clear and comprehensive theoretical concept of scientific development.

What is the relevance of Schumpeter's theory of scientific development? There is not very much evidence that Schumpeter's theory of scientific development really influenced the scientific community in a remarkable way. There was scant interest in the methodological part of his work, and even less interest in his theory of scientific development. Even for those who acknowledged his overall concept, it was hard to understand the significance of this theory before Kuhn stirred the debate in his work. (I am not even sure if Schumpeter himself realised the significance of his own theory.) So I am a bit sceptical of Backhouse's (1996, p. 29) suggestion that Schumpeter's theory of scientific development paved the way for Kuhn.

But this does not mean that Schumpeter's theory of scientific development is only of little interest for today's research. On the contrary, from a methodological point of view it still has great relevance, for at least three reasons.

The first reason is that Schumpeter's theory of scientific development was substantially ahead of its time. It anticipated many developments of the twentieth-century philosophical theory of science. Consequently, the present philosophical theory of science should at least take note of Schumpeter's contributions (which, in fact, it totally ignores). In particular, it should grant Schumpeter (overdue) recognition as a predecessor of Kuhn's paradigm approach.

The second reason is that one important feature of Schumpeter's theory of scientific development is that it was developed by a social scientist and not, like most other philosophies of science, by a physicist. Consequently, it is designed against the background of the particular requirements of economic explanations, such as high complexity and the lack of laboratory experiments. Moreover, it is designed against the backdrop of historical change.

The third reason is that Schumpeter's theory of scientific development still provides very instructive insights into the history of economics and the development of new ideas. Even today, it is a stimulating piece of methodological literature that is worth studying.

However, the theory of scientific development has its highest relevance as background to understand Schumpeter's own work. The concept of scientific development as a succession of periods in which phases of relatively continuous work are followed by revolutionary upheavals, provides a basis not only for Schumpeter's understanding of history but also for the present and future of economics. In this context, the latter parts of the *History* (parts IV and V) can also be interpreted as an attempt to assess in which phase economics resided in Schumpeter's own time.

Schumpeter describes the result of his assessment very clearly in the introduction to the fifth part of the *History* when he writes that 'In a sense, our inquiry ends, at the foothills of the Marshall-Wicksellian mountain range, with a last glance at the classical situation around 1900' (Schumpeter 1954, p. 1139).

His statement that economic science over 'the last decade or so before the outbreak of the First World War' was characterised by 'signs of decay' and 'new

breaks in the offing, of revolutions which have not yet resulted in another classical situation' (Schumpeter 1954, p. 754), shows that Schumpeter was convinced of the imminence of a revolutionising of economics. It is well known that he believed that stagnation in economics stems, in large part, from its static character and that a revolution can only develop in one direction: it should lead to a 'system of general economic dynamics into which statics would enter as a special case' (Schumpeter 1954, p. 1160). This realisation sheds a new light on his work in two respects.

On the one hand, Schumpeter's later work on economic dynamics and especially his *Business Cycles* (1939), has to be understood as a conscious attempt to revolutionise economic analysis. His comprehensive research programme of social economics was intended to provide the methodological basis for a future dynamic analysis. Against this background, his bitter reaction to the weak response to *Business Cycles* becomes clear: he perceived it not only as a lack of interest, but also judged himself as a failure in his own attempt to revolutionise economics.

On the other hand, his later work, and especially the *History*, has to be understood as an attempt to assess to what extent other economists – after his own failure – succeeded in revolutionising economics and in introducing 'a system of general economic dynamics'. His assessment is mostly negative. He points out that the development of macro-dynamics that followed Keynes's *General Theory* (Harrod, Domar, Hicks, Samuelson, and so on) does not correspond to his own idea of economic dynamics. He is convinced:

> that the most serious shortcoming of modern business-cycle studies is that nobody seems to understand or even to care precisely how industries and individual firms rise and fall and how their rise and fall affects the aggregates and what we call loosely 'general business conditions'. (Schumpeter 1951 [1946], p. 329)

Thus, in the *History* he states:

> Such work cannot at present claim to be more than exploratory. But it explores the ground on which a new structure will stand some day.... However, no attack on the whole front of Walrasian theory has as yet developed and the analogy with a building plot is still painfully apposite. (Schumpeter 1954, p. 1161)

With this judgment, Schumpeter provided a decidedly negative answer to the question of whether the expected revolution of economic analysis had already been carried out by Keynesian doctrine. Against the tapestry of Schumpeter's ideas about economic dynamics, this answer appears entirely consistent.

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* Leipzig Graduate School of Management, Jahnallee 59, Leipzig, Germany, and Clare Hall, Herschel Road, Cambridge CB3 9AL, UK. Email: kesting@microec.hhl.de.

Notes

1 I am grateful to Rolf Johannes, Heinz Rieter, Paul A. Samuelson, Harald Scherf, Wolfgang Stolper, the late James Tobin, Arnis Vilks, two unknown referees, and the Harvard University Archives for their friendly support.

2 With respect to its incomplete character, it might appear more appropriate to use the expression of Schumpeter's 'vision' or 'theoretical insights'. However, as to be

shown, Schumpeter elaborated his idea of the process of scientific development far beyond a mere vision.

3 I label the period of his writings from the beginnings until the year 1918 as Schumpeter's 'early work'. See also Kesting (2004).

4 This concept of science as a description of 'systems of elements' is particularly elaborated in Mach 1922 [1885] and in Mach 1917 [1905]. Although Schumpeter never explicitly referred to Mach (in *Wesen und Hauptinhalt*, he only declared that his method corresponds to 'one branch of modern epistemology'), the connection between Schumpeter and Mach was immediately uncovered, in particular by Spann (1910, p. 789) and Wieser (1911, p. 399). For a contemporary analysis of the relation between Schumpeter and Mach see, for example, Shionoya (1990a, 1990b) and Kesting (1997).

5 All passages from this work are my own translation.

6 It has to be noted that Schumpeter explicitly did *not* apply his concept of scientific development to the *Methodenstreit*, the battle over methods that was initiated by an argument between Carl Menger and Gustav Schmoller in the 1880s and lasted into Schumpeter's days (an overview of the German Historical School and the *Methodenstreit* is given in Rieter (1994) and Hodgson (2001)).

In Schumpeter's eyes, the *Methodenstreit* differs fundamentally from the debate between classical and neoclassical economics. To him, the *Methodenstreit* was not about replacing an old theory with a new one. Rather it was a battle between two methods that occupied totally different levels of cognition in economic analysis (see in particular Schumpeter 1914). This assessment leads him even in his early work to disregard the *Methodenstreit* as a sign of scientific development, but instead to see it mainly as a mere sociological phenomenon that leads to a simple 'waste of scientific energy'. Thus, he postulates already in *Wesen und Hauptinhalt* 'that the argument [*Schulenstreit*] between the theoretical and historical school has to be considered as largely resolved. In any case, we don't want to participate in it' (Schumpeter 1970 [1908], p. VII, my translation). This judgment is an expression of a general rejection of any controversy about the predominance of a particular method. Schumpeter makes this attitude clear on two other occasions, when he warns against the danger of similar methodological controversies arising, first, in respect of early economics in the USA (Schumpeter 1910, 1926), and, second, at the founding festivities of the *Econometric Society* (Schumpeter 1933 [1951]).

7 Schumpeter does not distinguish the sociology of science from the sociology of knowledge. He uses the first term in English, whereas in the *History* he adds the German notion of *Wissensoziologie*, which is closer to the sociology of knowledge. However, in employing the latter terminology he refers to Max Scheeler and Karl Mannheim.

8 See, for example, Caldwell 1982. See also the essays collected in Schleichert (1975).

9 It has to be noted that Schumpeter uses the word 'stereotype' only in the manuscript for the *History* and not even there in a strict sense. However, in employing 'stereotype', he labels a phenomenon that occupies a central position in his understanding of scientific development.

10 In a letter dated 5 May 1997, Paul A. Samuelson wrote to me: 'You know his [Schumpeter's] contrast between equilibrium and development. This resembles Kuhn's (exaggerated) contrast between "normal science" and "paradigm-shifting revolutionary science"'.

11 In the *History*, Schumpeter even goes so far as to express some doubts concerning the depth of the neoclassical revolution, which is really surprising since the existence and relevance of the neoclassical revolution not only belongs to the basic convictions of

his early work, but obviously still has some relevance for his later understanding of economics (Schumpeter 1954, pp. 886, 891f).

12 I am convinced that it was finally Schumpeter's experiences with the *Methodenstreit* and the neoclassical revolution – or, so to speak, his economic background – that disillusioned him right from the beginning of his work and that it was primarily this disillusionment which prevented him from making the same mistakes as the Vienna Circle and Karl Popper.

13 It has to be remarked that, in his early work, Schumpeter was driven by the same intention as early positivists and in particular the members of the Vienna Circle: to ban all metaphysics from scientific investigations. See, for example, Schumpeter (1908, pp. 23ff); see also Kesting (1997, chapter 2.2).

14 I am grateful to Heinz Rieter for this comment.

15 At this point, I can refer only to a conversation Arnis Vilks had with Thomas S. Kuhn at the *International Conference on Languages of Science* on 25-27 October 1995, in Bologna.

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