

**THE LEGITIMACY CRISIS OF SCIENCE IN LATE-  
MODERN PHILOSOPHY: TOWARDS A  
REFORMATIONAL RESPONSE**

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**Title:** The legitimacy crisis of science in late-modern philosophy: towards a reformational response.

**Keywords:** Science, postmodern philosophy, scientific legitimacy, objectivity, relativism., reformational philosophy.

## SUMMARY

This study investigates the challenges to the legitimacy and authority of scientific research in late-modern philosophy of science. The author suggests that the different challenges to the legitimacy of science have led to relativism and amount to a crisis. Keeping in mind the positivist background, he illustrates the legitimacy crisis of science in the period from Popper to the present. In particular his analysis focuses on the "historical school" (Kuhn, Feyerabend etc.) in philosophy of science.

The main question of this study is: *what are the causes and the nature of the legitimacy crisis emerging in the contemporary philosophical assessment of science?* To answer this question, a few specific challenges to the legitimacy of science emerging in particular areas are analysed: for example the difficulties of anchoring scientific certitude to its proper object of study, the loss of objectivity, growing skepticism about the possibility of communication and scientific progress.

After substantiating the gradual emergence of relativist and skeptical approaches in the above-mentioned areas, this study provides a "diagnosis" aiming at identifying the causes of the crisis. The humanist ground motive of nature and freedom and the choice of anchoring scientific certainty either in the subject or in the object of knowledge are considered the main sources of the crisis. They lead to arbitrary absolutisations of particular aspects of the scientific enterprise and (in the case of subjectivist approaches) to skeptical approaches to the possibility of scientific objectivity, communication and progress.

This study also indicates a few possible resources, available in the reformational tradition, to counteract the legitimacy crisis of science. The main resource indicated in this study is the recognition of the structural order for reality, which is accessible to scientific analysis, "constrains" scientific research but also constitutes a common ground for researchers. Other important resources are the recognition of the link between scientific and pre-scientific knowledge and the acknowledgment that universality and individuality are traits of everything that exists.

**Titel:** Die legitimeitskrisis van die wetenskap in laat-moderne filosofie – 'n reformatoriese antwoord.

**Sleutelkonsepte:** Wetenskap, postmoderne filosofie, wetenskaplike legitimeit, objektiwiteit, relativisme, reformatoriese filosofie.

## OPSOMMING

Hierdie studie ondersoek die uitdagings wat deur die laat-moderne wetenskapsfilosofie aan die legitimeit van die wetenskap gestel word. Die outeur redeneer dat hierdie uitdaginge tot relativisme en 'n krisis in die wetenskap aanleiding gee. Teen die agtergrond van die positivistiese wetenskapsideaal illustreer hy die wetenskapskrisis in die tydperk vanaf Popper tot vandag. Die analise fokus veral op die sogenaamde "Historiese Skool" in die wetenskapsfilosofie en op figure soos Kuhn en Feyerabend.

Die sentrale probleem van hierdie studie is die vraag na die oorsake en aard van die legitimeitskrisis wat blyk uit die huidige beoordeling van die wetenskap. Die studie ondersoek 'n aantal temas in hierdie verband, onder andere die probleme met die begroning van wetenskaplike sekerheid, die verlies van objektiwiteit, die groeiende skeptisisme met betrekking tot die moontlikheid van kommunikasie en die vraag na wetenskaplike vooruitgang. Nadat die studie aangetoon het hoe die relativisme en skeptisisme geleidelik in die wetenskap tot ontwikkeling gekom het, bied dit 'n diagnose wat daarop gemik is om die oorsake van die krisis van die wetenskap vas te stel. Die humanistiese grondmotief van natuur en vryheid en die keuse om wetenskaplike sekerheid in of die objek of die subjek van die wetenskap te veranker, word gesien as die hoofoorsake van die krisis. Die faktore gee aanleiding tot verabsoluttering van verskillende aspekte van die wetenskap en in die geval van subjektivistiese benaderinge gee dit aanleiding tot twyfel in verband met wetenskaplike objektiwiteit, kommunikasie en vooruitgang.

Hy dui vervolgens bronne in die reformatoriese filosofiese tradisie aan wat hierdie krisis van die wetenskap kan teenwerk. Die hoofbron is die noodsaak van die erkenning van die struktuurorde wat geld vir die werklikheid en wat ook as gemeenskaplike basis vir die beoefenaars van die wetenskap kan dien. Ander belangrike bronne is die band tussen wetenskaplike en voorwetenskaplike kennis en die erkenning van die universaliteit en individualiteit as kenmerke van alle syndes.

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# 1

## INTRODUCTION

### 1.1 Orientation and statement of the problem

#### 1.1.a Basic orientation

When considered from a historical point of view, the assessment of the legitimacy and authority of modern science seems to be subjected to dialectical tension. Periods of confidence and optimism are followed by periods of skepticism. Such attitudes are not simply due to optimistic or pessimistic feelings, but plunge their roots into the deeper ground of ontological, epistemological and pre-scientific convictions.

According to the “received view” of scientific theories (Suppe 1974, 6-118) natural science was supposed to be highly objective, rational and characterised by constant progress (cf. Botha 1988, 40-43). Although the credibility of natural science was not equally enjoyed by the so-called humanities and social sciences, the natural sciences generally enjoyed great authority. From a reformational (see 1.6.f) point of view many agreed that natural science, at this stage, enjoyed *excessive* authority.

Things started to change, however, with Popper’s doubts on verification (Popper 1963, 228ff.; 1976, 90). The ability of science to “prove” its claims was drastically relativised. In Popper’s view the scientific enquiry does not start from data and perceptions, but rather from selected (i.e. active) observations focusing on specific problems. We do not simply look for some abstract truth, according to Popper (1963, 229), we look for useful and interesting truth, in order to solve certain specific problems. In this way the human subject and his problems were placed at the center of the scientific enterprise while the objects of scientific study, compared to a positivist view, were moved to a more peripheral position.

It became clear to Polanyi (1946, 42-62) that non-scientific premises play a considerable role in the scientific enquiry. He promoted the view that science is not simply based on impersonal rationality and objectivity, but requires personal commitment to certain ideals (1946, 44ff.). These ideals, for the Catholic Polanyi, had certain links with his religious beliefs and this insight argued for an analogy between the community of faith and the community of science. The latter also had its “disciples”, its conversions and schisms and for many it seemed to be more and more characterised by belief, rather than certainty. All this was just the beginning of what some authors regard to be a more radical turn towards irrationalism.<sup>1</sup>

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<sup>1</sup> I use this definition in accordance with Vollenhoven’s (1961) classification of currents in philosophy of science. “Irrationalism” does not refer to philosophical systems that are considered contradictory, in-

Kuhn's proposal of the role of paradigms in scientific theorising (Kuhn, 1970, 43-136) underlined in a new and radical way the role of frameworks in science. Presuppositions, once carefully kept out of the scientific sphere, now seemed to determine science and to shape it from the beginning. As a consequence, the communication of scientific ideas could no longer be regarded as unproblematic. Scientists holding to different paradigms were said to experience great difficulties in their scientific dialogue (Kuhn 1970, 111ff.). Even when they used the same terms, the meaning was said to be modified by the particular paradigm assumed as valid. The result was the birth of the notion of scientific incommensurability. At the same time the optimistic expectations of cumulative progress of scientific theorising were replaced by increasingly skeptical views, until progress itself was doubted. Feyerabend even considered the possibility (actually a certainty in his view) that modern science may constitute an involution with respect to more ancient types of science and technical achievements (Feyerabend 1975, 306ff.).

Relativism and skepticism today are still threatening the legitimacy and credibility of science. One of the inroads to this was the doubts cast on the traditional notion of knowledge as representation. According to Lyotard (1984, 60ff.) postmodern science should not try to represent the real world anymore. It should rather be anti-representational (i.e. not to represent reality). It should consider itself as a language game and should find its legitimacy in its own strategies. Postmodern science, in his view, concerns itself with "undecidables", the limits of precise control, conflicts characterised by incomplete information, pragmatic paradoxes. It theorises its own progress as catastrophic, non-rectifiable and paradoxical. According to Lyotard (1984, 60ff.) postmodern science values the unknown, rather than knowledge and tries to prevent agreement or consensus in the scientific community. It aims at constant revolution and dissensus.

The late-modern humanist philosophy of science caused a healthy reduction of the over-estimated authority of science, on the one hand. It also "spread" this authority more equally among the natural, social and human sciences. But it introduced, on the other hand, much more skepticism concerning the objectivity and the credibility of science. From a reformed point of view Ratzsch (1986, 44-61) argues that from the 1960s scientific authority and legitimacy (previously celebrated by positivism) have been rather endangered in a considerable number of contemporary philosophies of science. In the middle 1980s Ratzsch (1986, 63-74) indicated that

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consequent or non-rational. It rather refers to philosophical currents that do not consider reason as the main or superior element in human knowledge, but try to re-valuate other agencies (like passions, ethical commitments and so forth) as equally or more important than reason (see also Venter, n.d., 16). Furthermore, according to Strauss (1988, 15-16) irrationalism is characterised by an over-estimation of individuality as the only source of knowledge, and leaves little room for knowledge of the universal (which is conceptual knowledge). On the contrary, rationalism is characterised by an over-estimation of

after the revolutionary phase of the 1960s and '70s philosophy of science seemed to show a less radical and less relativist character. But I would maintain that, considering the more recent contributions of Collins, Rorty, Lyotard and others, it is not inappropriate to say that we are still facing a legitimacy crisis which emerged in the 1960s and concerns the philosophical reflection about science.

A final remark about the focus of this study. Late-modern philosophy includes several quite different traditions (e.g. the catholic one), but this study will focus especially on the humanist tradition, giving particular (though not exclusive) attention to the “historical school”<sup>2</sup> of philosophy of science (e.g. Kuhn, Feyerabend etc.)

### 1.1.b *When can we speak of a legitimacy crisis?*

There is a view that scientific *legitimacy* is constituted by the fact that science shows a certain degree of objectivity, communication and progress. However, it would be a mistake to assume that science is more legitimate the more it is considered objective, rational or progressive. In fact, these characteristics can be exaggerated and science can be regarded as more objective (rational, progressive etc.) than it could ever be. The question is: how can one determine when scientific rationality or objectivity is over-estimated? How can one determine, on the other hand, when the credibility, legitimacy and authority of science are “below the standard”? When are we allowed to speak of a crisis? Is this not just an arbitrary evaluation, based on philosophical prejudices? On which grounds do we affirm that (e.g.) the reformational school provided a positive reduction of the authority of science, while Kuhn provided an illegitimate reduction of the same authority?

To speak of “objectivity” or of a “loss of authority”, means to assume implicitly a standard which is also linked to a certain philosophical position. Probably not everyone would agree on this standard, with the philosophical perspective that it implies. Nevertheless, without any standard it would be impossible even to speak of objectivity, rationality or progress at all. It is therefore necessary to assume a standard, but it remains the task of the philosopher to demonstrate that this standard is credible. It remains his/her task to demonstrate that what s/he considers to be a crisis or loss of scientific legitimacy is something real. It is my conviction that

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universality as the only source of knowledge, and leaves no room for genuine knowledge of individuality (i.e. pre-scientific knowledge). My use of “irrationalism” refers especially to these two definitions.

<sup>2</sup> Among the authors analysed in this study I consider Polanyi, Kuhn and Feyerabend as representatives of the historical school. In Suppe (1974, 135ff. and 166ff.) the contributions of both Kuhn and Feyerabend are classified as “worldview analyses” (*Weltanschauungen* analyses). In this category one could include both the historical and the sociological schools (Suppe 1974, 125-127). In fact the approaches of the two schools are close to each other and one may even observe that the historical emphasis “leads to the sociological turn” (Botha 1994, 21). However I prefer to distinguish the historical and the sociological school on the basis of their respective emphasis on two different aspects of the scientific enterprise. Also Botha (1994, 21) recognises and uses this distinction.

a legitimacy crisis is affecting the late-modern reflection on science because we face specific difficulties. These difficulties concern for example the accessibility of the object of study, the objectivity of the scientist, the possibility of communication, and the notion of progress. Nowadays, these issues are experienced as problematic at least by some sections of the philosophical community who reflect upon science. I hope to demonstrate in this study that such an assessment is not arbitrary.

#### *1.1.c Different contributions to the crisis*

Another possible objection to my approach is that it is not fair to place philosophers like (e.g.) Popper, or Polanyi, in continuity with a movement which later led to relativism and irrationalism. Is not each thinker responsible only for his own ideas? How can Polanyi, for example, be responsible for a crisis which emerged more clearly only in the work of more recent philosophers of science? Thinkers like Popper might be said to have preserved scientific legitimacy to a considerable extent. So why should such philosophers be regarded as forerunners of those who willingly endangered the legitimacy of science in a subsequent period?

I recognise that the different authors examined in this study did not contribute to the process of scientific de-legitimation with the same intensity. I will try to assess the different contributions individually, but without losing sight of the fact that they established a common philosophical climate in a certain period. As a consequence, the question concerning a possible common denominator of these individual contributions cannot be totally ignored. For many years, these philosophies were interacting, influencing each other and creating a tradition in which all shared several basic convictions. In this respect, Dooyeweerd's philosophy will be useful to reveal the common ground of different authors and the consequences of this common ground for their philosophies and for those who followed. Of course the loss of scientific legitimacy is not brought about just overnight. But the development of the most relativist systems of thought would not have taken place without the support of those who started to move towards a (moderate) relativist position.

#### *1.1.d Different reactions to the crisis*

What I have not claimed in this study is that within late-modern humanist philosophy of science there are no attempts at attenuating the legitimacy crisis. My analysis tries to take into account some among the most influential scholars in this field of study. It attempts to demonstrate that the challenges to legitimacy, objectivity and so on have been not only real but also increasingly serious. This does not mean, however, that there are no counter-tendencies.

The present study does not deal extensively with those counter-instances because its aim is to understand a crisis and the difficulties that it entails. I believe the challenges to the legitimacy of science have nowadays affected the most influential schools within philosophy of science. However I do not claim that the crisis affects all in the same way or to the same extent. I don't claim that all schools follow the same path towards increasing subjectivism<sup>3</sup> or irrationalism. In late-modern philosophy, for example, there are still those who support realist tendencies (e.g. Boyd 1983; Mc Mullin 1991) and rationalist tendencies (Newton-Smith 1981). Even positivism is not totally absent.<sup>4</sup> I also don't claim that reformational philosophy is the only exception in a philosophical context in which relativism and irrationalism triumph.

A couple of specifications are however needed. First, in some cases these counter-tendencies, are to be found outside the humanist tradition. Many realist thinkers, for example, are found within the Christian tradition (e.g. Mc Mullin). These thinkers cannot be regarded as attenuating a legitimacy crisis within a tradition of thought to which they do not belong. Secondly, quite often those who do not adhere to an irrationalist type of philosophy of science, have simply remained within the older traditions of positivism and rationalism. The present study does not deal directly with the past positivism, but offers enough clues to the fact that that position does not represent a solution to the present challenges targeting scientific legitimacy. We need to look for contemporary responses to the crisis and not at outdated ones.

Having said this, I will, however, admit that even within the humanist ranks there are schools and individuals that contribute towards an attenuation of the crisis. To mention an example among many, Nersessian and Andersen (1997) aim at "softening" the idea of incommensurability. Xiang Chen and others (1998) try to build on the more moderate views expressed by Kuhn in his later philosophical development.

It will be admitted, however, that counter-instances like these constitute a minority and are clearly less influential and authoritative than the mainstream schools in philosophy of science. This is another reason why I feel justified in speaking about severe challenges to the legitimacy and authority of science, amounting to a crisis. I recognise that not all philosophical currents are equally affected by this crisis and of course even within the humanist tradition there are those who are not affected by radical subjectivism or relativism. I am also convinced that not all the contributions provided by reformational philosophers are always preferable or even

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<sup>3</sup> I use the term *subjectivism* to indicate the (epistemological or ontological) position that considers the *subject of knowledge* as the most appropriate anchor of order and sound knowledge. It must be noted that the subject of knowledge is not necessarily individual, therefore subjectivism is not the same as individualism. I use "relativism" as a synonym for subjectivism (see the Introduction to ch. 2).

<sup>4</sup> Ratzsch observes that positivism is still influential in some disciplines (e.g. some social sciences and theology). In addition, there are also reportedly still a few positivists among professional philosophers (Ratzsch 1986, 38).

satisfactory and I have tried to offer concrete examples in this respect (see 4.6.e-f, and 5.5.c). Yet the presence of substantial challenges to the credibility of science should be acknowledged.

#### *1.1.e Statement of the problem*

The main problem of this research is: *What are the nature and the causes of the legitimacy crisis which emerges in the contemporary philosophical assessment of science?* This central question will be sub-divided into a few other questions, related to more specific issues with respect to the legitimacy crisis in philosophy of science.

1. Why does it seem so problematic, in the late-modern philosophical reflection, to tie the scientific enquiry to an anchor of certitude?
2. What lies at the root of the relativistic attitude towards scientific objectivity?
3. Why is there a growing skepticism concerning the possibility of dialogue between scientific “schools” and a growing emphasis on the notion of incommensurability?
4. What causes scientific progress to be doubted and even denied?

Sometimes (see the positivist period) science enjoys excessive authority and trust. The reduction of the legitimacy of science, therefore, is not always and only a negative phenomenon. But in the contemporary philosophy of science such a reduction seems to be based on premises that, in their mature expression, lead to skepticism and relativism. Answers to the four questions above point to the diagnosis of the symptoms of the crisis. To the extent that this diagnosis is accurate, it also points towards possible remedies and potential alternatives. This brings us to the aims and objectives of the research.

### **1.2 The objectives of the research**

The objective of this study is to explore “a response” (see the title) to the lack of authority, objectivity, communication and progress in the late-modern philosophy of science. In order to do this, it will be necessary:

1. to substantiate and illustrate the loss of credibility and authority of science in the philosophical reflection from Popper up to the present. This needs to be done against the positivist background and recent philosophies in the humanist tradition. In particular the analysis will focus on the “historical school” of philosophy of science.
2. This study will attempt to provide a “diagnosis” of the causes of the crisis in the contemporary reflection upon science.
3. Finally, I will indicate the possible resources, available in the reformational tradition, to counteract the crisis in the postmodern conception of science. This will mean

recommending and critically evaluating plausible alternative views, without falling into outdated positivistic attitudes.

In the following section I present the main theoretical arguments of this study by following the threefold division sketched above. The division is based on a healing metaphor including three moments: 1) description of the symptoms, 2) diagnosis and 3) therapy.

### 1.3 Leading theoretical arguments

#### 1.3.a Illustrating the crisis and its consequences

The crisis affecting contemporary humanist philosophy of science can be observed from different angles. In chapter 2 I will consider the theme of the certitude of scientific research and the debate concerning realism and nominalism. I shall argue that late-modern humanist philosophy has increasingly adopted a subjectivist position, thus implicitly denying or questioning the existence of a universal order for reality.<sup>5</sup> Three problems are especially illustrated in this context as consequences of the subjectivist and nominalist strategy of stripping concrete reality of its universal trait. First, the object of scientific research becomes more elusive, inaccessible and dependent on the views of the scientific community. Second, the *locus* (Lat: place, foundation) of order (which is formally said to be inaccessible or non-existing) is nevertheless sought/placed in the subject of knowledge. At the same time, an Archimedean point<sup>6</sup> is often sought in various aspects<sup>7</sup> of reality, which constitutes a telling paradox and indicates that the search for the elusive universal is still continuing.

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<sup>5</sup> I would define this structural order as 1) stable and 2) universal order, 3) accessible to the knower and 4) conditioning scientific thought. I sometimes refer to it as “cosmic” or “creational” order. From a reformational point of view one may say that it is necessary to distinguish the *order of* reality (the orderliness of things) from the *order for* reality, the law governing a certain area of our experience (Hart 1984, 38, 85, Van der Walt 1994, 133). Another important distinction is between the structural law-order and what is “subject-ed” to this order. This is the distinction between “law and subject”, and it is quite popular in reformational circles. Unfortunately in this distinction the term “subject” may result in some confusion. In fact, it can be confused with the human *subject* of knowledge (who is also subject-ed to the law-order). In this research, however, this particular use of the term “subject” (as subject to the law) will be mentioned only in section 5.6, in connection with Stafleu’s adoption of the term.

<sup>6</sup> I have tried to distinguish two different choices: 1) placing the *locus* of order in the subject or object of knowledge and 2) searching for an archimedean point in different aspects or modalities of experience (for the meaning of *modal aspects* see fn. 7). The phrase “archimedean point” is familiar in reformational philosophy. According to Dooyeweerd (1984, I, 8-12) every philosophy is obliged to search a vantage point from which it can obtain a totality-view of reality. For the reformational thinker this archimedean point must satisfy the requirement of transcending the modal diversity and be found in the totality and radical unity of the latter (Dooyeweerd 1984, I, 99). Unfortunately “immanence” philosophy often seeks this vantage point of view within the modal structure of reality itself (hence the qualification “immanence”), and in some specific modal aspect. On this topic see also fn. 18.

<sup>7</sup> In Dooyeweerd’s philosophy a modal aspect, or modality, is one of the fundamental ways of being of created reality. Modal aspects are also fundamental ways of looking at reality. According to Dooyeweerd (e.g. 1979, 214) they are fifteen, namely the numerical aspect (referring to discrete quantity, number) the

A third problem is implicit in the nominalist approach. Nominalism recognises only the existence of the individual traits of created entities, but scientific knowledge is abstract knowledge referring to patterns and regularities which in turn are based on (or conditioned by) the universals. An incongruence is therefore created between the nominalist understanding of reality and the character of scientific theorising. Nominalism does not seem to be able to offer an answer to this problem.

The problem discussed in chapter 3 is related to the role of the knowing subject, and more precisely the role of his/her pre-scientific or pre-theoretical presuppositions.<sup>8</sup> Late-modern humanist philosophy has placed a growing emphasis on the role of frameworks, premises and paradigms in scientific theorising. As this emphasis is not balanced by a recognition of universal norms which are not the creation of the knowing subject (or community), I argue that scientific objectivity appears to be threatened. From a historical point of view, in the late-modern era scientific theories increasingly appeared to be regarded as products of beliefs and worldviews (which are sometimes shaped by ideological or non-scientific types of interests).

The notion of objectivity targeted by the majority of contemporary philosophers is of course one closely tied to the positivistic legacy and the "received view of scientific theories" (Suppe 1974, 6-118). While the latter has been and is also a target of critique by reformational philosophy, I am going to argue that it is important to retain a proper notion of scientific objectivity and not to follow the relativistic temptations promoted by the late-modern humanist philosophy of science.

A third problem (chapter 4) is given by the distrust concerning scientific communication. I shall argue that the increasing emphasis on frameworks also leads to the conviction that it is extremely difficult to communicate with scientists holding to different views, paradigms or theories. The problem of incommensurability has therefore become a very crucial debate among contemporary philosophers of science. The idea that it might not be possible to compare certain standpoints or to sustain a dialogue between certain schools has been increasingly proposed and accepted. Initially this possibility was contemplated with considerable concern, more recently it seems to be welcomed as paving the way to a new and liberating pluralism. Some argue that postmodern science should not look for communication and consensus, but possibly for dissensus (Lyotard 1984, 65). I shall argue, however, that these are radical and highly ideological positions that undermine the legitimacy of science itself.

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spatial aspect (extension) the kinematic aspect (motion), the physical aspect (energy) the biotic (organic life), the psychic or sensitive aspect (feeling, emotional life), the logical aspect (logical distinction), the historical (formative power) the lingual (symbolic meaning), the social, economic, aesthetic, juridical, moral and pistic (i.e. faith-life) aspects.

<sup>8</sup> I use *theoretical* as synonym of scientific.

Finally, the crisis of postmodern philosophy of science can be observed from the perspective of the possibility of scientific progress (see chapter 5). I will argue that the cumulative view of scientific progress, typical of the positivist period, has been replaced by more “modest” views, and this has contributed to a more realistic assessment of science. However, after the abandonment of the positivist views the notion of progress seems to be disappearing from the horizon of the most recent philosophies of science. In some instances progress is only related to scientific societal usefulness and internal puzzle-solving, without reference to truth and objectivity (Kuhn 2000, 85-86). In other cases it is even argued that contemporary science constitutes a regress with respect to previous forms of scientific or technical achievement (Feyerabend 1975, 306-7).

### 1.3.b *The causes of the crisis*

Concerning the late-modern shift (chapter 2) in philosophy and philosophy of science, towards positions that oppose the realist approach, I will argue that this shift finds its roots in the cartesian dichotomy between the object and the subject of knowledge. Caught in this polarisation, late-modern philosophy does not acknowledge the presence of a law-order which conditions the existence of both the subject and the object of knowledge. The lack of recognition of the law for reality lies at the root of the incongruence between science (aiming at the universal) and concrete reality (presenting itself as individual). Modern philosophy of science has either ignored the presence of this order for reality or declared it inaccessible and as a result has placed the *locus ordinis* either in the knowing subject or in the object of enquiry. As the emphasis has increasingly been placed on the subject, the object of scientific investigation has increasingly “become” elusive or inaccessible. Furthermore, the cartesian dichotomy has led the subject to search an archimedean point for his enquiries in the different aspects of created reality. This search is however characterised by lack of insight in the modal structure of created reality and does not meet the requirement that the archimedean point should be found outside the horizon of created reality (Dooyeweerd 1984, I, 99).

Concerning the growing emphasis on presuppositions, frameworks or paradigms (chapter 3), and the difficulties of scientific communication (chapter 4), I will argue that Dooyeweerd’s idea of religious ground motives is a very useful tool to interpret the crisis of humanist philosophy of science as a consequence of the swing (from positivism) towards historicism and subjectivism. In Dooyeweerd’s philosophy the “religious ground motives” are the basic pre-theoretical starting points of philosophy and culture in general. They control theoretical thought by means of a triad of transcendental ideas concerning a) the origin, b) the unity of meaning and c) the relation of coherence and diversity between the different aspects of created reality (Dooyeweerd 1984, I, 93-102).

Dooyeweerd has characterised these ground motives as driving powers directing philosophy and science. He has identified (1984, I, 61ff.) the main ground motives of Western culture. They are: 1) the motive of form and matter which was characteristic of Greek philosophy, 2) the motive of creation, fall and redemption (the biblical ground motive), 3) the motive of nature and grace (directing the catholic medieval culture and philosophy) and 4) the religious motive of nature and freedom (humanist philosophy).<sup>9</sup>

I intend to show that the transition from positivism to the new philosophy of science is illustrative of the dialectical tension between the two opposite “poles” of the religious ground motive characterising humanist philosophy. This purported tension has been explored by Dooyeweerd (e.g. 1984, I, 190-495) in relation to philosophy and culture in general, but not specifically in relation to recent developments in philosophy of science. I will briefly illustrate this dialectical tension with reference to the role of presuppositions, frameworks and paradigms (chapter 3) and the difficulties of communication, in particular the problem of incommensurability (chapter 4) in contemporary philosophy of science.

Dooyeweerd argues that the humanist ground motive “contains” two poles in dialectical tension between them: the pole of *nature* and the pole of *freedom*. Historically, the pole of nature is dominated by the ideal of natural science, implying the scientific control of nature. But the ideal of the control of nature, conflicts with the second pole of the humanistic ground motive, namely the pole of *freedom*. In fact, control over nature also means control over the human being, who is part of nature. Once rigid control is exercised over all nature, there is no more room for human freedom (Dooyeweerd 1979, 152-153).

According to Dooyeweerd it is impossible to find a *stable* synthesis or reconciliation between the two opposite poles of the same ground motive. The reason is that a religious synthesis (unlike a theoretical synthesis) cannot be achieved simply by theoretical means. Furthermore, Dooyeweerd sees the world as an integral and coherent reality, which does not tolerate the emphasis on one element to the detriment of another. Declaring one dimension of reality as primary, inevitably leads to a tension in which the other dimensions call for recognition. Unfortunately, when the thinker finds himself in the grip of the dialectical tension caused by the humanist ground motive, he will usually attribute the primacy to one of the two opposite poles, while depreciating the other. Yet the depreciated pole, usually, in due time finds a way to regain the priority and to overcome the rival pole. In the humanist ground motive, we have on the one side the control of nature through science. On the other we have the pursuit of the freedom of the individual human personality. The first ideal stresses science and its rules, the second one freedom. The first stresses nature, the second man and his culture.

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<sup>9</sup> Attempts at identifying and defining non-Western religious motives have been undertaken by Choi (1999) and Inagaki (1992), concerning respectively the Korean and the Japanese (traditional) cultures.

Positivism was inspired by the theme of the control of nature via natural science. Great importance was attributed to nature itself: facts were supposed to speak for themselves while the intervention of the human personality was experienced as an intrusion. The role of the scientist was reduced to a minimum and had to be characterised by absolute objectivity, to the point of neutrality. However, when the role of the free human personality and its creativity had to be granted much more room, the presence and the role of various types of presuppositions was recognised as a fundamental component of all scientific research.

Unfortunately, due to the influence of the freedom motive, this positive recognition could not be accompanied by a parallel acknowledgment of the universal order conditioning and constraining the knowledge of the subject. The recognition of the role of presuppositions, therefore, could not be understood within its proper limits and did not have only beneficial effects. In fact, it also contributed to an excessive emphasis on the subject and his creative freedom, which was in direct conflict with the recognition of an objective reality, not dependent on the subject holding to certain presuppositions.

It is also possible, I will argue, to relate the problem of the lack of communication and incommensurability (chapter 4) to the humanist ground motive of nature and freedom. The ideal of the control of nature requires a world which is the same for all and a language that is understandable by all. The motive of freedom, on the other hand, allows the existence of totally different worlds, frameworks of thought and languages. This pluralism of points of view and languages is the result of the creative activity of the human personality. Under the hegemony of the freedom motive, incommensurability becomes a threat because the common ground of a creational order is rejected in favour of the recognition of a plurality of language games, frameworks and paradigms. The problem is that when the language is not shared, there is no real possibility of dialogue or communication. This is the final historicistic outcome of the historico-sociological and linguistic turns in philosophy of science.

In chapter 5 I will argue that there is a connection between a relativist view of scientific progress and the adherence to the humanistic ideal of freedom. Historically, in fact, the acceptance of the ideal of freedom is associated with a rather relativist and even reactionary attitude towards cultural progress in general. Choosing an example from European history, romanticism, idealism and similar movements of the 19<sup>th</sup> century (in which the freedom ideal was dominant) have been characterised by a rather conservative attitude in politics, as illustrated by the period of European history called "Restauration". As the adherence to the pole of nature stimulated the faith in cumulative scientific progress, the opposite pole of freedom increasingly determined a much more skeptical attitude.

This relativist attitude according to Dooyeweerd implies an absolutisation of the historical mode of experience. Historicism regards norms and structures from the point of view of the

ever-changing historical developments and therefore no universal norms are recognised. When we move to the more specific discussion concerning scientific progress, the relativist approach means that progress is disconnected from universal norms that might constrain the freedom of the scientific community. Scientific progress is then regarded as proceeding in a “catastrophic, non-rectifiable and paradoxical way” (Lyotard 1984, 60). As progress is no longer linked to universal norms but to the legitimation of the scientific community (the subject of knowledge) irrationalist and relativist views of progress will inevitably emerge.

Another (more secondary) reason for the present skepticism concerning progress is the confusion between structure and direction. The term “structure” refers to the theme of creation, to the constant creational constitution of any thing (that which makes it the thing or entity what it is). The term designates a reality that the philosophical tradition of the West has sometimes indicated by terms like “substance”, “essence” or “nature”. “Direction”, by contrast, refers to the theme of sin and redemption. It designates the way in which such entity, thing or process is used, directed or implemented. According to Wolters (1986, 49) “anything in creation can be directed either (...) in obedience or disobedience to the [divine] law”. In this case we can distinguish between a positive and a negative “direction”.<sup>10</sup> Perhaps in a simple way one could say that structural corresponds to “ontological” and directional corresponds to “religious” (in the sense clarified by fn. 48 in this thesis). The term “religious”, however, does not indicate a separate compartment or area of life. Therefore I will use the term “direction”, in a way that does not exclude applications and references to social life (5.5.d) to ethical and political problems, to scientific progress (5.4.b) and so on.

In Feyerabend’s view (to make a concrete example) one can discern a confusion between these two notions, or more precisely the elimination of the structural dimension of progress and the elevation of its directional dimension to the only norm. Feyerabend argues, for example, that the aim of science is to promote the happiness of the individual and the satisfaction of his manifold needs (Feyerabend 1970, 210). In this way Feyerabend only says that scientific progress can be “good or bad” (the directional dimension). He does not clarify what scientific progress is from a *structural* point of view. He does not state how we can recognise it and what its characteristics are. By following this approach, some forms of progress are likely to be disregarded, namely when their “contribution” to the happiness of the individual does not appear clearly from the beginning.

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<sup>10</sup> Of course not every reference to the terms “structure” and “direction” in this study will necessarily imply the specific meaning defined above. For example when I mention the *directions* of research described by Stafleu (5.6.a) or Kuhn’s idea of progress without a specific *direction* (5.3.b) or the *structural* order, I am not referring to the distinction mentioned above. But I trust the context will in each case clarify the meaning of the terms.

### 1.3.c Suggested remedies

Concerning the problem of the anchor of certitude for scientific knowledge (chapter 2) I will argue that the recognition of the law-order for reality provides a solution to the problem of incongruence which is especially acute in nominalist philosophies. I am referring to the incongruence between reality (which is individual) and scientific knowledge (which aims at the universal). Instead of trying to anchor scientific knowledge to the object or to the subject of knowledge, it is necessary to acknowledge the universal law-order which constitutes the *locus ordinis* for reality and the appropriate anchor of certitude.

Furthermore, reformational philosophy can help shed light on the above-mentioned incongruence by re-visiting the realism-nominalism dilemma. Universality and individuality are not entities but traits of everything that exists. It is possible to know both these traits, but knowledge of individuality is typical of the pre-scientific attitude of thought, which focuses on concrete reality. Knowledge of universality, on the other hand, is the result of the scientific attitude. The link between scientific and pre-scientific knowledge in reformational philosophy is therefore a resource to be explored in order to affirm the possibility of knowing both individuality and universality. Finally, the argument is defended that the structural order for reality, which is the proper object of investigation of the different scientific disciplines, is accessible to scientific investigation. The role of abstraction, theory and the metaphorical use of language is explored to support the above argument.

Concerning the growing emphasis on presuppositions, frameworks and paradigms (chapter 3) I will argue that reformational philosophy offers interesting insights towards a better comprehension of the nature and role of presuppositions, their link to the religious sphere in the knowing subject and their influence on the scientific endeavor. But this does not yet constitute a solid contribution towards a renewed version of objectivity. It is the recognition of a created order that provides the needed external anchorage for scientific knowledge. The recognition that presuppositions are only one side of the coin while there is also an order for reality, partially independent of the knower, counteracts relativism and offers a more balanced picture of the scientific activity. Following the dooyeweerdian tradition I will argue that it is necessary to recognise both the religious starting point of all theorising and the existence of states of affairs that are conditioned by structures for reality. These structures place constraints on our theorising.

The problem of incommensurability and/or lack of communication (chapter 4) can also be examined in a new way by following a reformational approach. While reformational thinking does recognise the reality of different approaches and views, once again the structures which condition states of affairs constitute a constant reference point for scientific dialogue. Dooyeweerd's approach will be taken as an example of such a reformational attitude taking into

account both the created structures and the different points of view. Though divided into “schools”, the scientific community has received a common task, a call to emulation and cooperation. This call implies dialogue, and the christian does not claim any privileged position within the scientific community.

While it is necessary to recognise the radical antithesis between the biblical religious ground motive and the non-christian ground motives at the root of theoretical thinking, I will argue that a radical antithesis is only possible at this deep religious level. When we consider the theoretical level, the antithesis can only be relative and therefore complete incommensurability cannot exist. I will defend the thesis that statements and concepts are only partially (not completely) dependent on theories and theories are only partially dependent on broader “paradigms”, frameworks or presuppositions. The possibility of comparing and criticising competing theories, arising from different presuppositions, will be defended.

Finally, concerning scientific progress (chapter 5) I will argue that a recognition of the different “coordinates” of scientific progress is a beneficial and necessary instrument for a more elaborate and realistic assessment of scientific progress. I will also argue that it is necessary to link progress to the process of disclosure of the universal law-order, namely to the opening up of the analogical moments<sup>11</sup> of the modal structures of created reality. This will emerge when examining the idea of scientific progress proposed by Stafleu (e.g. 1980, 5-28). A final section is dedicated to the dilemma known as meaning variance-invariance (a topic which is also relevant for the discussion on communication and incommensurability). In this section I will argue that it is necessary to move beyond the idea that after “revolutionary” changes either nothing is the same or nothing has changed at all. Stafleu’s idea of “opening up” of a modal aspect will shed light on the necessity of acknowledging both change and continuity in the development of science.

#### **1.4 Methodology**

A combination of methods will be utilised in this study. Following Herman Dooyeweerd (1984, I, 36-37), I will attempt a philosophical critique of the deepest presuppositions of a certain system of thought. This form of criticism is necessary in this study, in order to explore the deep foundations of a certain philosophy (or philosophical tradition) and to identify its real essence,

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<sup>11</sup> The modal aspects (see fn. 7) are the fundamental modes in which realities exist and can be observed (e.g. numerical, spatial, biotic, social, juridical and so on). These modal aspects are not to be imagined as closed compartments, there are also analogies linking the different aspects to each other. For example when we speak about the “extension of a concept” we have in mind a strictly logical affair. Yet we are speaking of a kind of logical “space” which refers to (is reminiscent of) the spatial modality. When the reference is to an earlier modality in the succession of aspects, it is called “retrocipation”. When the reference is to a later modality it is called “anticipation”. Analogy is therefore a collective name including both anticipations and retrocipations (see also 5.6.b).

its aims and problems. With respect to our main research problem, this type of criticism will help us to identify the deep roots of the legitimacy crisis of contemporary science.

Consequential criticism will also be used in the present study. This type of criticism is intended to verify the coherence between the most basic (pre-scientific) assumptions of a philosophy and its concrete ideas (on the scientific level). It will be also useful to test the coherence between the various ideas proposed on the scientific level by a certain system of thought. This will allow us to detect contradictions, lack of consistency and other difficulties in a certain philosophical system.

### 1.5 The proposed contribution

Although the acknowledgment of relativist tendencies in the late-modern conception of science is well known in philosophical literature, a systematic effort to assess the legitimacy crisis from a reformational point of view has been attempted only in part.<sup>12</sup> This study will build on those projects that have attempted to analyse this situation and also to provide alternative solutions. The suggestions provided by reformational authors will be collected, organised, and elaborated in order to utilise them for the particular purposes of this research. This will constitute a specific contribution, which will involve both a “diagnosis” and a proposal of alternative possibilities and solutions. The indication of a few analyses and proposals from the reformational circle that I consider less satisfactory will constitute another specific contribution.

### 1.6 Clarifying a few terms and ideas

#### 1.6.a Science and scientists

In this research the terms “science” and “sciences” indicate not only the natural sciences but all the sciences which were part of the methodological tradition connected with the Latin term *scientia* and the German term *wissenschaft*. Following the dooyeweerdian tradition, I attribute scientific status to all disciplines which focus on the law side of an aspect of reality by way of modal abstraction (see 3.6.a). Some sciences focus on more than one modal aspect (Skillen 1988, 48). In this view (which I endorse) theology and philosophy, as well as all the so-called human and social sciences are understood as having scientific status. As a consequence, the term scientist is also used in a broad sense to include not only those who deal with the natural sciences but all scholars dealing with scientific disciplines as defined above.

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<sup>12</sup> On this theme the most productive contributor has been Stafleu (e.g. 1987 and 1979). See also Van Riessen (e.g. 1992) and Botha (e.g. 1988 and 1994). But apart from many single articles touching on

### 1.6.b Late-modern

In the title, and often in the text, I prefer the term “late-modern” to postmodern. The reason is that the term *post-modern(ity)* might be understood as indicating a period “after” modernity with characteristics of its own, and therefore would be distinguishable from those of the modern period. But, as many recent studies have demonstrated, the “postmodern” period seems to be rather a continuation of themes and tendencies which were already part of modernity. This is true in philosophy (irrationalism is also present in the modern period), but also in politics, in the arts, architecture, or in economics.<sup>13</sup>

The difficulty of locating the beginning of “post-modernity”<sup>14</sup> (or the end of modernity) seems to confirm the idea that the period in question is not situated *post* (modernity) and should rather be regarded as late-modernity or hyper modernity. Both modernity and post-modernity present internal contradictory tendencies which make it difficult to distinguish the two periods neatly. Postmodernism for example, seems to be both capitalist and non-capitalist; in the arts it both rejects and promotes representation (Bertens 1994, 67). It is true that concerning science postmodernity is more explicitly anti-representational. But in general, *post*-modernism seems to both establish and undermine the conventions and presuppositions that it appears to challenge. As Bertens points out, for example, “postmodernism both confirms and subverts representation, the subject, liberal humanism and even capitalism” (Bertens 1994, 104).

For all the reasons mentioned above, it is preferable to think of the period in question as a development within modernity. However, I will not completely avoid the terms postmodern and

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various aspects of late-modern philosophy of science, the only efforts of a more comprehensive and systematic character have been provided by Stafleu.

<sup>13</sup> The analysis of several artistic disciplines, for example, brings Chabot (1991) to the conclusion that there is not yet a clear common denominator between them to imagine the beginning of a *post*-modern period. Chabot argues that in some of these disciplines modernity is continued and not concluded. Also for Jenks (1991), much of what is called postmodern in contemporary architecture should rather be called “late-modern”. Callinicos (1990, 132-144) rejects Jameson’s (1984) thesis that economies are postmodern (i.e. enter the third phase of capitalism) when the state shows insufficient power to intervene in the economy. Callinicos points to crucial state interventions in monetary matters, characterising recent economic history. Concerning globalisation (another presumed indicator of the postmodern era) Bertens (1994, 245) observes that in today’s world there are also many signals of fragmentation and ethnic division.

<sup>14</sup> An example of this difficulty is offered for example by De Chirico (1997; translation mine). He traces the first appearance of the term *postmodern* in 1917, but he would not imply that postmodernity started there. He clarifies: “it is from the 1950s and ‘60s that it [the term] appears in practically all thinkable contexts of western culture” (1997, 18). But postmodernity did not yet begin: “it is from *the beginning of the 1970s* that the reflection concerning the crisis of modernity can be called postmodern” (De Chirico 1997, 24; italics mine). Did such reflection in the ‘70s begin postmodernity? De Chirico does not answer this clearly. But according to Bertens (1994, 53ff.), we cannot properly speak of a debate about postmodernity before we are *well into* the 1970s. And “up to the early 1980s the debate on postmodernism remained almost exclusively confined to architecture and the arts” (Bertens 1994, 111). Given the difficulties, it is not rare to read the strategic conclusion that modernity (or postmodernity by implication) “is a qualitative rather than chronological category” (De Chirico 1997, 6). The question remains: when did modernity end, so that *post*-modernity could begin?

postmodernity, which are more familiar to the reader and largely used. I will rather use them with the meaning that I have clarified above.

### 1.6.c Presuppositions, premises, frameworks

One can distinguish between the scientific and pre-scientific presuppositions of science. Scientific presuppositions are for example philosophical presuppositions and certain law statements (Stafleu 1987, 85-86). Pre-scientific presuppositions are for example worldviews, beliefs, religious ground motives. Of course pre-scientific convictions can be elaborated into or be the starting point for scientific theories. In my view however, this does not eliminate the distinction between scientific and pre-scientific (cf. 2.6.b). I agree with Wykstra (n.d., 14) that it remains possible to determine the original context in which a certain concept or belief has developed. As a consequence, a belief that is shaped (for example) in the context of pre-scientific reflection can be defined as pre-scientific. An axiom which is forged in the context of theoretical reflection is scientific in nature.

Of course our pre-scientific views can then influence scientific developments and, vice-versa, scientific theories can influence our pre-scientific view of life. I believe this influence, within certain limits, can be reciprocal,<sup>15</sup> yet in my opinion the distinction between scientific and pre-scientific is not cancelled by their interaction or by the reciprocal influence upon each other.

In this study I will focus especially on pre-scientific presuppositions. This does not mean that there will be no interest at all for the scientific type of presuppositions (cf. 3.2.d). There is however a good reason to make pre-scientific presuppositions a special focus of interest in this study. Historically speaking, in fact, the idea that *scientific* presuppositions do have a (legitimate) influence on scientific theories has seldom been disputed (though the possibility exists from a systematic point of view, I am not even aware of any example). The only (relative) exception concerns perhaps the impact of *philosophical* presuppositions on science, which has been sometimes questioned even after the fall of the positivist regime. On the contrary, the fact that pre-scientific presuppositions do influence science (and the fact that, within certain limits, this is normal and legitimate), has been more difficult to acknowledge and to accept. This is why the role of pre-scientific presuppositions constitutes a special (though not exclusive) focus of this research.

In this study, I will often use adjectives like *scientific* and *pre-scientific* to qualify more precisely the type of presuppositions that I am indicating. When terms like presuppositions, premises and so on, are used in this study without further qualification, they indicate all kinds

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<sup>15</sup> Although I accept this reciprocal influence, I find Wolterstorff's (1989, 84-86) "interactive view of science" a bit excessive. The way in which he supposes scientific theories influence (and sometimes even abolish!) our religious commitments is in my opinion too radical. I have tried to provide a constructive critique of Wolterstorff's view of the interaction between science and religion in Coletto 2002, 108-112.

of *non-scientific or pre-theoretical* assumptions, beliefs and presuppositions. The reason is that pre-scientific presuppositions are the special focus of this study, and it would be annoying to read the qualification “non-scientific” every time premises or presuppositions are mentioned. I have therefore opted for a simplification in the use of terms. I trust it will make the text more readable without diminishing its precision.

When dealing with frameworks that are constituted by both scientific and pre-scientific elements (e.g. Kuhn’s paradigm) I will specify that we are dealing with a “hybrid” type of framework. Specific types of (pre-scientific) presuppositions will be indicated by more specific terms like worldview, world picture, ground motive. The same holds for specific scientific presuppositions, for example philosophical (called “metaphysical” by some authors - e.g. Popper 1963, 197) presuppositions or premises of a special-scientific character.

#### 1.6.d “Deep” and “shallow” presuppositions

When using the adjectives *deep* or *shallow* (with possible reference to both scientific and pre-scientific presuppositions) I have in mind the position of certain presuppositions in an ideal sequence presenting different levels. Without aiming at completeness or even at precision,<sup>16</sup> the different levels of presuppositions can be illustrated by the following scheme:

- |  |
|--|
| .....scientific:   |
| 7. theorems  |
| 6. axioms  |
| 5. philosophical/metaphysical presuppositions              |
| ..... pre-scientific:                                      |
| 4. convictions concerning the particular field of research |
| 3. world pictures  |
| 2. worldviews  |
| 1. Religious ground motives                                |

The numbering of the terms shows which presuppositions come “before” others and therefore are “deeper” (i.e. belong to a deeper level) than others in the scheme. The “depth” of a certain presupposition is given by the fact that it functions as a preliminary basis for the presupposition(s) that come(s) later in the scheme. For example, we cannot have theorems

<sup>16</sup> The scheme proposed on this page is just a simplified illustration for didactical purposes and does not represent all the complex interactions between presuppositions. In some cases, for example, “shallow” (scientific) presuppositions may (illegitimately) function as religious beliefs! In this respect one may remember, for instance, the conviction of the Vienna Circle that in order to attain scientific (i.e. objective) knowledge it was necessary to exclude all metaphysical elements from science.

without accepting certain particular axioms. We cannot have ontological elaborations without worldviews or religious ground motives. Another characteristic (which increases proportionally to the depth of presuppositions) is the scope and breadth of their focus. As a consequence, the (scientific) presuppositions at the top of the scheme are more specific (but also more limited) in their scope.

In addition, the depth and nature of a presupposition are also determined by the context in which they are elaborated (Wykstra n.d., 14). For example, a belief that is accepted or shaped in the context of religious commitment belongs to the level of religious presuppositions or ground motives. Of course there are interactions between beliefs of different levels, and the picture is far more complex than the one drawn by my simple and didactical scheme above (Coletto 2002, 114-120). All I'm trying to suggest, however, is that there is a plurality of presuppositions with specific characteristics,<sup>17</sup> belonging to different levels of awareness and reflection.

In each author, "deep" presuppositions are present and active in his/her work. The link between *shallow* presuppositions and the *deep* ones (such as e.g. religious motives) of an author can be traced through philosophical analysis. However, some authors may not be aware of the presence of deep presuppositions and of their influence on scientific work. When this is the case and the thinker discusses only the more superficial presuppositions in his system (or in others) we can say that s/he is discussing only the *shallow* type of presuppositions. We should not assume that s/he does not hold deeper presuppositions or that they have no influence in his/her work. We shall rather assume that there is no sufficient awareness of the role and nature of presuppositions.

#### 1.6.e Legitimacy, authority, credibility...

I will use the term "legitimacy" as a summary term. In this study several terms are used to indicate different aspects of scientific legitimacy: terms like credibility, authority, validity, dignity, prestige and so on. I have chosen "legitimacy" to summarise them and I will use the different terms to point to specific aspects of this legitimacy.

This research will focus especially on the legitimation of science as an epistemic project. In other words, it will deal mainly with the "internal" legitimation of science, with the factors allowing science to be legitimate science from an epistemological point of view. These factors are for example the rationality and objectivity of science. The "external" legitimacy of science, (e.g. the credibility and power that science enjoys in contemporary society) are discussed only occasionally. In the following section I'm going to sketch a short historical introduction to reformational philosophy. This philosophical movement is mentioned often in this study but not

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<sup>17</sup> About the characteristics of worldviews see for example Olthuis (1989, 29-38). About the specific characteristics of religious ground motives see Dooyeweerd (1984, I, 61-113).

all readers may be equally familiar with it. This is the reason why it is also necessary to sketch briefly the central tenets of this philosophical school.

#### 1.6.f *Reformational or neo-calvinist philosophy: a short introduction*

In the 19<sup>th</sup> century, a section of the reformed community in the Netherlands found in Abraham Kuyper the catalyst and promoter of a particular type of Christian worldview, which was soon labelled as “neo-calvinist” and in the 20<sup>th</sup> century as “reformational”. I would like to clarify, therefore, that I am not going to follow the habit (recently becoming popular) of using the adjective *reformational* just as a synonym of reformed. *Reformational* (or *neo-calvinist*) indicates a certain community and a certain tradition within the reformed history,<sup>18</sup> and I would like here to introduce briefly such tradition.

The reformational worldview is based on the biblical theme of creation, transgression and redemption, and in Niebhurian terms it would be described as supporting the attitude “grace transforms nature”. It insists on God’s sovereignty in the whole creation and therefore on the integral commitment of the Christian in all areas of life (Wolters 1986).

Abraham Kuyper, who is regarded as one of the founding fathers of the reformational movement, was not primarily a philosopher but also a theologian. He exerted a considerable influence on Dutch public life as a leader of a political party, prime minister, editor, church leader and founder of the Free University of Amsterdam. The latter was instituted in order to promote Christian-reformed principles and leaders in all spheres of life.

It was from this institution and from this background that the Dutch philosopher of the science of law Herman Dooyeweerd and his brother in law D.H.Th. Vollenhoven in the 1930’s elaborated a reformed type of philosophy called the “philosophy of the cosmonomic idea”. The latter was not a theological reflection on philosophical issues, but a fully-fledged philosophy. Vollenhoven worked especially on a method for the study of the history of philosophy from a christian perspective.

One of Dooyeweerd’s major contributions was the notion of religious ground motives: driving forces behind all philosophical traditions, shaping and determining the philosophical reflection (see 1.3.b.) and his development of a new critique of theoretical thought, a method of

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<sup>18</sup> According to Bartholomew (2000, 3, fn. 1) Calvin Seerveld was the first to use the term *reformational* in 1959. The term, says Bartholomew, tried to “catch several related meanings” and he lists a few of them. My use of the term does not insist on all of these meanings, but especially on the fact that scholars in this movement “build on Dooyeweerd’s and Vollenhoven’s work”, as Bartholomew says (2003, 3, fn. 1). Normally, therefore, the term *reformational* is used more or less as a synonym of neo-calvinist. Nevertheless it is possible to follow Wolterstorff’s (1989) use of *reformational* to indicate the neo-calvinist branch that is more influenced by Vollenhoven. This use of the adjective can be disputed: many scholars outside this specific branch of the movement regard themselves as *reformational* thinkers as well. On the other hand the strategy provides a way to designate the specific Vollenhoven-inclined branch, as I have done myself (cf. Coletto 2002, 2, fn. 1 and p. 32, fn. 23).

transcendental philosophical analysis which could reveal the deepest religious assumptions of an author, school of thought or discipline. This placed the christian philosophical tradition (often regarded as “biased” by many) on the same ground as all other traditions. All traditions were now regarded as being grounded by a particular basic “motive” and therefore depending on some presuppositions which were normally not subjected to questioning. Dooyeweerd also contributed to the elaboration of a basic ontology and epistemology, and was flanked by a first generation of neo-calvinist philosophers in the Netherlands. Among them were K.J. Popma, J.P.A. Mekkes, S.U. Zuidema, H. Van Riessen and others.

At the time, the language used by this philosophical school was mainly Dutch, which probably did not help the spreading of the neo-calvinist philosophy to other parts of the world (except to South Africa and the Dutch immigrant communities in North America). Nevertheless, in the second half of the 20<sup>th</sup> century reformational philosophy reached the United States, Canada, England and many other countries, in all continents. A few scholars to be mentioned in this respect are H.E. Runner, H. Rookmaker, B.Goudzwaard, H. Hart, J. Olthuis, C. Seerveld, S. Fowler and many others. Reformational philosophy also reached South Africa and in particular the Universities of Potchefstroom and Bloemfontein, where several reformational thinkers (e.g. H.G. Stoker, N.T. van der Merwe, J.A.L. Taljaard, B.J. van der Walt, D.F.M. Strauss, J.H. Smit) contributed to the expansion and consolidation of this philosophical school. Among them is also the promoter of this research, namely Prof. M.E. Botha (and Prof. J.J. Venter, who acted as promoter during the first eighteen months of development of this study).

### 1.7 Outline of the chapters

In the **Introduction** I attempt a presentation of the basic research problem of this study, by indicating its historical background and by providing a general orientation in this issue. The basic research problem is then subdivided into relevant questions allowing an appropriate elaboration and investigation of the problem itself. The objectives to be achieved by the present study are indicated in the following section. The main theoretical arguments to be developed are illustrated by following a healing metaphor implying the three steps of: 1) observation of the symptoms, 2) diagnosis and 3) proposed therapy. After this, I indicate the methods to be utilised in order to reach the objectives indicated in the previous section. Finally, the introduction contains a clarification of basic terms and ideas and a sketch of the content of the chapters.

In **chapter 2** I explore the legitimacy crisis of late-modern philosophy of science by focusing especially on the “anchor of certitude” of scientific knowledge. In the first part of the chapter

(sections 2.1 to 2.4) I illustrate the gradual adoption of a subjectivist position, implying the gradual denial of the universal order for reality. A first important consequence of the relativist approach is illustrated, namely the fact that (by placing the *locus* of order in the knowing subject) the object of science has increasingly been considered as “hidden” or in-accessible to scientific investigation. A second problem is discerned, namely that the different “turns” in philosophy of science indicate that the search for the elusive universal is still continuing and the archimedean point to understand reality is “placed” or sought in several different aspects of reality. Finally, I point to the problem of the incongruence between science and reality, which is a problem affecting both nominalist and realist philosophies.

In the following “diagnostic” part ( 2.5) I suggest that the roots of the problem are to be found in the cartesian division between the object and the subject of knowledge. Following that dichotomy, modern philosophy of science has either ignored the presence of the order for reality or declared it inaccessible. As a result the source of order has been placed either in the knowing subject or in the object of scientific enquiry. These are the roots of the incongruence between scientific knowledge (aiming at the universal) and concrete reality (which is individual). These are also the roots of the increasing “eclipse” of the object of research (i.e. of the fact that it is regarded as hidden or inaccessible) and of the never-ending search for the elusive universal of late-modern scientific enquiry.

The third part of the chapter (2.6) deals with the possible remedies for ontological aspects of the problem. Concerning the incongruence between reality and scientific knowledge, the proposal of a recognition of the structural order for reality (as distinct from the subject and the object), provides a solution to the problem of incongruence. Concerning the epistemological side, the discussion continues by affirming the possibility of knowing both universality and individuality as traits of everything that exists. Knowledge of individuality is typical of the pre-scientific attitude of thought, while knowledge of universality is possible in the scientific attitude. The final section (2.6.c) of the chapter defends the thesis of the accessibility of the universal order for reality, which is the proper object of study of all scientific enterprises.

In **chapter 3**, I focus especially on the role of the knowing subject, and more precisely on the role of his/her pre-scientific presuppositions. The first part of the chapter (3.1 to 3.4) aims at substantiating the objectivity crisis. Late-modern humanist philosophy has placed a growing emphasis on the role of frameworks, premises, paradigms. Such emphasis has opened liberating directions in the conflict with positivism. However, this emphasis on presuppositions is not balanced by a recognition of the structures for states of affairs which are not the creation of the knowing subject (or community).

The following section (3.5) offers a diagnosis concerning the causes of the unbalanced emphasis on presuppositions, frameworks or paradigms. This section introduces Dooyeweerd's idea of religious ground motives as a very useful tool to interpret the crisis of humanist philosophy of science and the swing (from positivism) towards historicism and subjectivism. In fact the humanist ground motive is characterised on the one hand by the ideal of the control of nature through science. On the other hand we have the pursuit of the freedom of the individual human personality.

Positivism was inspired by the theme of the control of nature via natural science. Great importance was attributed to nature itself: facts were supposed to speak for themselves while the intervention of subjective and personal factors was experienced as an intrusion. However, when the role of the human personality and its creativity had to be granted much more room, the presence and the role of various types of presuppositions was recognised as a fundamental component of all scientific research. Unfortunately, this positive approach was not accompanied by the recognition of the existence of structures for reality not depending on the human subject but conditioning and constraining the knowledge of the subject.

The last part of chapter 3 suggests an alternative to counteract the growing emphasis on presuppositions. The recognition of a created order provides the needed anchorage for scientific knowledge. The recognition that presuppositions are only one side of the coin while there is also a created order, partially independent of the knower, counteracts relativism and subjectivism and offers a more balanced picture of the scientific activity. It is necessary to recognise both the religious starting point of all theorising and the existence of structures for creation constraining our theoretical endeavors.

In **chapter 4** I will deal especially with incommensurability and the distrust concerning the possibility of true scientific communication. In the first 4 sections (4.1 to 4.4) I illustrate the increasing emphasis on the role of frameworks and the consequent difficulties of communication in contemporary philosophy of science. The hypothesis that it might not be possible to compare certain standpoints or to sustain a dialogue between certain schools has increasingly been proposed and accepted.

In the following (diagnostic) section (i.e. 4.5) the problem of the lack of communication and of incommensurability is once again linked to the humanist ground motive of nature and freedom. The ideal of the control of nature requires a world which is the same for all and a language that is understandable to all. The ideal of freedom, on the other hand, allows for the existence of totally different worlds, frameworks of thought and languages. This pluralism of points of view and languages is required by the emphasis on the free creativity of the human personality. Under the hegemony of the freedom motive, incommensurability becomes a

reality because the common ground of a creational order is rejected in favour of the recognition of a plurality of language games, frameworks and premises. The problem is that when the language is not shared, there is no real possibility of dialogue or communication. This is the final historicistic outcome of the historical, sociological and linguistic turns in philosophy of science.

Concerning the possible remedies for the problem of incommensurability or lack of communication, the issue is examined by following a reformational approach (4.6). First of all, Dooyeweerd's philosophy of communication is sketched to exemplify the characteristics and implications of a basic reformational approach to scientific dialogue. In the following step the reformational idea of antithesis is explored. While it is necessary to recognise the radical antithesis between the biblical religious ground motive and the non-christian ground motives at the root of theoretical thinking, it must be admitted that such a radical antithesis is only possible at the deep religious level. When we consider the theoretical level, the antithesis can only be relative and therefore complete incommensurability cannot exist.

Proceeding along this line of thought I introduce the idea that concepts are only partially (not completely) dependent on theories and theories are only partially dependent on broader "paradigms", frameworks or presuppositions. There is therefore a possibility of comparing and criticising competing theories, arising from different presuppositions. The chapter is concluded by a critical discussion of the philosophy of Jacob Klapwijk, a reformational philosopher whose suggestions I find less fruitful than those of other reformational authors mentioned in this chapter.

In **chapter 5**, the crisis of postmodern philosophy of science is observed from the perspective of the possibility of scientific progress. The first part of the chapter (5.1 to 5.4) contains a historical overview. In the reflection of contemporary philosophy of science, the linear-cumulative view of scientific progress, typical of the positivist period, has been gradually replaced by more "modest" views, and this process has contributed to a more appropriate assessment of science. Unfortunately, for a kind of opposite extremism, the notion of progress seems to be gradually disappearing from the most recent philosophies of science. In some instances progress is only related to scientific or societal usefulness and internal puzzle-solving, without reference to truth and objectivity. In some cases it is even argued that contemporary science constitutes a regress with respect to previous forms of scientific or technical achievements.

The second part of the chapter (5.5) contains a diagnostic attempt. According to this analysis the adherence to the humanistic ideal of freedom causes a historicist or relativist way of thinking which is accompanied, historically, by a rather pessimistic attitude towards progress.

This pessimism concerns cultural progress in general, but does not exclude scientific progress as well. After an exposition of this insight derived from Dooyeweerd's philosophy, I examine a "less convincing diagnosis" by B. Goudzwaard.

Another (more secondary) reason for the present skepticism concerning progress is the confusion between structure and direction (see end of section 1.3.b). Feyerabend, for example, takes into account only the directional dimension while neglecting the structural one. I indicate Griffioen's basic distinction between structure and direction as a basic but important tool to detect (at least in some cases) unilateral approaches in the postmodern views of progress. The distinction between structure and direction points towards the necessity of distinguishing the basic "coordinates" of scientific progress. This discussion is continued in the following final part of the chapter.

The third and final part of the chapter (5.6) is dedicated to the necessary alternatives. The reformational view links progress to the opening up of the analogical moments of the modal structures of created reality. This emerges when examining Stafleu's view of scientific progress and of the "opening up" of a field of science. The chapter is concluded by a discussion of (conceptual) meaning variability and invariability. Though the topic is also linked to the problem of communication (i.e. to chapter 4) it could only be discussed after analysing Stafleu's notion of "opening up" of a scientific field. However, the discussion also sheds light on the fact that both revolutionary changes and constant themes in science are made more understandable from a reformational point of view.

In the **Conclusion**, I will present a synthesis of the relevant themes arising from the main arguments developed in the chapters and indicate some future avenue for research.

## AN ANCHOR OF CERTITUDE FOR SCIENTIFIC RESEARCH

“The locus of order is society”  
(Harry Collins 1992, 148)

### Introduction

Why is it so difficult to find an anchor of certitude<sup>19</sup> for contemporary science? To answer this question we will have to go back to crucial philosophical debates, like the conflict that Bernstein (1985, 8) calls “the *agon* between objectivism and relativism”. The objectivist believes that there is a firm grounding for knowledge, something we can appeal to in determining the nature of rationality, reality and truth. The relativist claims that what is taken to be foundational, true or right is at best only culturally stable, certainly not eternal, indubitable, ultimate or necessary. At this point, the objectivist often accuses the relativist of self-inconsistency. In fact, while doubting all truth claims he or she takes relativism to be absolutely true! One particular version of this ancient dilemma is the debate between realism and nominalism.

Historically speaking, nominalism (and conceptualism as a particular version of it) gradually gained predominance in philosophy of science. The shift from realism to nominalism started within positivism itself, as a reaction against the initial mechanistic-materialistic worldview (Botha 1988, 39). The gradual victory of nominalism however, brought about a few problems. As the nominalist places the universal within the knowing subject (or community), the *object* of scientific enquiry, the external world, is increasingly experienced as depending on the theories, the language or the worldview of the scientific community. The object of science seems to become increasingly elusive and inaccessible to scientific investigation.

<sup>19</sup> I use this phrase as synonym of *locus ordinis* (Lat. the foundation of order). The *locus* of order is also the “anchor of certainty” and sound knowledge. In postmodern philosophy this anchor is often sought in the human subject of knowledge. Furthermore, an anchorage is sought within particular modal aspects of reality. I distinguish this “second” type of anchoring as the search for an archimedean point (see fn 6). The distinction is more didactical than systematic. In fact, the search for the *locus ordinis* and for the archimedean point can be regarded as one single attempt at grounding scientific knowledge. In addition, what is sought for in both cases (from a reformational point of view) is the “elusive universal” (cf. the title of Botha 1994), the structural order, the cosmic law-order. What I have defined as the search for the archimedean point (causing the appearance of the “turns” in philosophy of science) is defined by Botha as “diverse articulations of the locus of order” (Botha 1994, 29).

Furthermore, given the nominalist tradition of the most recent philosophy of science it is out of the question to expect an acknowledgment of universal norms, structural order and the like. In fact, the notion of a universal order is resisted because it is regarded as leading to essentialism and other problems that are linked to a realist approach to reality. And yet the paradox is that “the search for the elusive universal” (see the title of Botha 1994) seems to continue. In fact, the archimedean point of knowledge is sought within different aspects of reality (the linguistic, the social, the logical and so on) that are then considered fundamental. In philosophy of science certain modal aspects have characterised the infinite “turns” of the late-modern era: the historical turn, the logical turn, the sociological turn and so on. The fact that an archimedean point is sought within different aspects shows not only that the search itself is still going on, but also that it does not seem to lead to the solid ground on which scientific certainty should rest.

While I will illustrate the growing difficulties caused by the two above-mentioned problems, a third problem is implicit in the nominalist<sup>20</sup> approach. For nominalism the only existing entities are individual, and the universal is regarded as a product of our linguistic or conceptual elaborations. Concrete reality, as a consequence, is stripped of its universal traits and only individuals are said to exist and to constitute the object of scientific investigation. But at this point we notice an incongruence: scientific knowledge is abstract and aims at the universal while concrete reality is individual. An incongruence is thus created between science and its purported object of study, namely concrete reality.

At this point it is necessary to introduce the different levels of the philosophical discussion concerning the certainty of knowledge, the status of universals, language, the *locus* of order and so on.<sup>21</sup> The first level of this discussion is constituted by a “classical” and well known dispute: *realism versus nominalism*. This debate takes place in the ontological arena. The nominalist maintains that the only “things” that exist are individuals, while the realist acknowledges the existence of universals as well. For the realist there are different irreducible ontological features of the world, corresponding to various kinds of human representations.

The second level of conflict is epistemological; here the dispute can be labelled as *realism versus idealism*. The realist here argues that the objects of the world exist independently of our knowledge or experience of them. For the idealist the existence of such objects is to some extent dependent on (or constituted by) the activity of the knowing subject. For the idealist our direct awareness concerns ideas and representations constructed or elaborated by humans. As this position might lead to a radical individualism and loss of objectivity, idealists have often

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<sup>20</sup> Of course the problem of the incongruence between scientific thought and concrete reality is not exclusive of nominalism, but concerns realism as well, though in another form (see 2.5.b).

argued that all rational beings experience the world in the same manner, thus avoiding the most radical individualist positions.

A third level of the debate can be defined as *realism versus anti-realism*. Here the conflict takes a linguistic turn. For the realist statements are true or false in virtue of a reality existing independently of the knower. The anti-realist replies that the analysis of statements in terms of simple truth conditions should be substituted by an analysis in terms of conditions of verification or assertability. This debate is therefore about *what can be said*. Foundational to all three these debates is the issue concerning the existence of universals or “natural kinds”, and the way scientific language attaches to these characteristics of the world (Delaney 1985, 1-10).

A final note can be dedicated to *scientific realism and anti-realism*. According to Mc Mullin (1984, 25), the basic claim of scientific realism is that the entities and structures postulated by scientific theories actually exist. Anti-realism (in its instrumentalist version or in others) claims that the theoretical entities of science ought to be denied ontological status.<sup>22</sup> Limited anti-realism denies ontological status only to certain classes of theoretical entities (Mc Mullin 1984, 25). In some cases the anti-realist can accept theories for virtues that are not linked to representation (for example for their problem-solving effectiveness). In other cases he can accept theories for very limited representational virtues (for example for their ability to “save” the observable phenomena).

Is there a common denominator between these conflicts or dilemmas? The debate is certainly related e.g. to the central theme of the universals and the way scientific language “attaches” to the world. At this point we have to ask an important question: is there a link between the various levels of realism on the one hand and nominalism, idealism and anti-realism on the other? According to Bernstein (1985, 8 ff.), the answer is affirmative. The fundamental division concerns the existence or non-existence of a solid ground, “some permanent, ahistorical matrix or framework to which we can ultimately appeal in determining the nature of rationality, knowledge, truth, reality goodness or rightness”. For the objectivist such solid ground exists and it is the task of the philosopher or scholar to discover the nature of rationality or truth. The relativist not only denies the claims of the objectivist but argues that when we examine the most fundamental concepts they must be understood as relative to a certain conceptual scheme, paradigm, form of life, society or culture (Bernstein 1985, 8).

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<sup>21</sup> The following distinction of levels is derived from Delaney (1985, 1-10).

<sup>22</sup> This attitude is for example visible in the instrumentalist view of scientific theories. Mach for example, was convinced that atoms and similar particles do not exist as ontological entities. Nevertheless he regarded the atomic theories as useful tools to direct our investigations. In this view theories are not true or false. In fact, instruments are not true or false but can be more or less useful (Mach 1975, 354, 358) Similar solutions were elaborated in philosophy by Schlick, Reichenbach and Carnap (Popper 1983, 117-128).

It seems to me possible, therefore, to use the terms *relativism* and *objectivism* as generic and encompassing terms. Objectivism refers to and includes all the forms of realism mentioned above. Relativism refers to and includes all the forms of non-realism (i.e. nominalism, idealism etc.) mentioned above. I will, however, feel free at least to alternate the term *relativism* with *subjectivism*. In fact, what the relativist claims is not that there is no truth, knowledge, certainty and so on, but rather that they are relative to (or dependent on) a certain subject (which is not necessarily individual but can be a community, a certain culture or society). According to Strauss, the fundamental disagreement between the realist and the non-realist is due to the fact that the *locus ordinis* is placed in the object by the realist and in the human subject of knowledge by the non-realist (cf. Strauss 1988, 19).

The above discussion about the anchor of certainty brings to mind the issue of foundationalism. The foundationalist approach was characterised by the belief that genuine science is based on propositions that are known non-inferentially and are recognised as being true by anyone (Wolterstorff 1976, 24-26, 106). Foundationalism has been severely criticised by contemporary philosophers and we can say it has been abandoned. The postmodern approach rejects the existence of foundations constituting a common ground for all subjects of knowledge or for entire civilisations (Lyotard 1984, xxiv). However my systematic distinction between relativism and objectivism is based on the assumption that no philosophy can be completely foundationless. Although contemporary philosophy is non-foundationalist, it still has to take a standpoint concerning the nature and foundation of the order that we experience in the world. This is why even non-foundationalist philosophies establish their (often undeclared) anchor of certainty (and an archimedean point that grants further solidity to that anchorage).

In the first part of this chapter (2.1 to 2.4) I am first of all going to substantiate the growing relevance of the problems created by the subjectivist approach of late-modern philosophy. I have already mentioned these three problems above: 1) the “eclipse” of the object of study, 2) the search for the elusive universal and 3) the incongruence between science and reality.

In the following “diagnostic” phase (i.e. section 2.5), I will search for the origins of the difficulties affecting the nominalist approach and I will suggest that they are linked to the cartesian division between the subject and the object of knowledge. In this dichotomy, nominalism privileges the knowing subject as the final foundation of knowledge, while realism privileges the object. The nominalist strategy is responsible for the gradual “disappearance” of the object of scientific study, which is experienced as hidden and elusive.

Furthermore, the grounding of certainty in the subject seems to be experienced as inadequate and the search for the “elusive universal” continues by searching an archimedean point within different aspects of being. Finally, the reason why an incongruence between science and

concrete reality is created, is that both nominalism and realism miss the importance of the universal order of reality as a proper object of scientific investigation.

In the final part of the chapter (in 2.6) I will first of all offer a viable alternative to the realism-nominalism dilemma. I will propose the recognition of the structural order for reality as the appropriate anchor of scientific certainty. This ontological proposal will be accompanied, on the epistemological level, by the suggestion that it is possible to know both the individual and the universal. In the final section of this chapter I will also argue that, from a reformational point of view, the creational order is accessible to scientific investigation and therefore is not as elusive as many late-modern philosophers of science believe.

Our exploration of the growth of nominalism in the late modern philosophy of science begins from a philosopher who is not a nominalist, Karl Popper. He actually was a special type of realist. He forged his own type of realism, which he considered a “third way” between nominalism and a more naive type of realism.

## 2.1 Popper’s realism

### 2.1.a Popper in dialogue with instrumentalism and essentialism

Popper provided an extensive defense of realism (e.g. Popper 1983, 80ff.). His view was posited on the assumption of realism.<sup>23</sup> Realism suited perfectly, for example, his view of the demarcation criterion between science and non-science. In fact, the criterion of *falsification* presupposes an objective reality which can “contradict” our theories. Realism also suited Popper’s rejection of the instrumentalist view of theories. To him theories are not only useful tools, they can be genuine descriptions of the world (Popper 1983, 101). Finally, the task of providing conjectures and refutations requires the realist position. In fact, says Popper, “critical argument (...) would be pointless without an objective reality, a world which we make it our task to discover” (Popper 1983, 81).

In his *Conjectures and Refutations* (1963, 97-119) Popper analyses “three views concerning human knowledge”, he compares instrumentalism and essentialism with a “third way” of his own elaboration. As we have seen, instrumentalism is an anti-realist strategy rejecting the realist *epistemological* claim that scientific theories are true or false by virtue of their objective representation of true states of affairs. Instrumentalism views scientific theories as mere tools that don’t need to be true or false, or to reflect real states of affairs. Although Popper cannot

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<sup>23</sup> Although realism is not discussed as a thesis in his *Logic of scientific discovery*, Popper says that even in that book it “forms a kind of background” (Popper 1983, 81). Popper deals especially with scientific realism. Although he discusses nominalism, essentialism, idealism and other related topics, his main

accept this view, he appreciates the instrumentalist suggestion that theories cannot provide ultimate explanations. According to Popper (1963, 103) the strength and attractiveness of instrumentalism lies precisely in this attack on aristotelian essentialism.

The second position analyzed by Popper is essentialism. This position implies a search for the “essences”, the true realities hidden behind the phenomenal world that we can observe. In this case, science aims at discovering something “essential” that lies beyond visible phenomena. Essentialism can be regarded as a realist position which is slightly different from the type of realism that Popper wants to defend. The essentialist would maintain that an independent reality does exist and it is possible to attain final explanations of it. To discover the essences the scientist has to lead his/her research beyond the superficial layer of phenomena. What Popper appreciates in essentialism is the fact that it views theories not only as instruments but also as legitimate descriptions of a real world (Popper 1963, 103).

In his “third way” Popper includes some of the elements of both essentialism and instrumentalism. Popper would like to save the essentialist idea that theories are valid descriptions of the world. At the same time he would like to save the instrumentalist idea that theories cannot provide final, ultimate explanations (Popper 1963, 103). Ironically, Popper starts this discussion of his realism by praising the Galilean tradition. But while he laments the abandonment of that tradition by the instrumentalists, he has to admit that his own “third way” deviates from Galileo at crucial points. In fact, in his own words, he defends “the non-Galilean view that though this [a truer description of the world] remains the aim of the scientist, he can never know for certain whether his findings are true” (Popper 1963, 114-115). In other words theories do not offer final explanations.<sup>24</sup>

### 2.1.b Popper against instrumentalism and essentialism

Having observed a few instances of Popper’s agreement with instrumentalism and essentialism, we should also have a closer look at his *criticism* of both positions. Popper criticises the view that theories are simple instruments, with little or no correspondence to reality. He expresses true disappointment at the idea that nowadays instrumentalism (the view of theologians like Osiander, Cardinal Bellarmino and Bishop Berkeley) has won the battle (Popper 1963, 99).

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focus remains on the implications of those ontological and epistemological positions for the role and nature of theories, their epistemological claims, for scientific knowledge and so on.

<sup>24</sup> I believe that Popper, by focussing mainly on specific issues (in which he might agree or disagree with Galileo), loses sight of the fact that the Galilean tradition is characterised by a solid nominalist spirit. For example Galileo’s law of inertia was formulated through a thought-experiment without taking into account any sense-experience. In a sense, it was imposed on moving entities by the subject of knowledge. According to Strauss (1988, 19) this can even be regarded as “the crucial epistemological turn in modern epistemology, in ascribing the primacy no longer to the object but to the subject”. Kant was strongly influenced by Galileo when he elaborated the view that the laws of nature are a-priori contained in human understanding and prescribed to nature (Strauss 2005, 211).

Those who were not prepared to contradict a passage of the Old Testament (Popper 1963, 98), or who feared a decline of religious faith invented the (philosophical) view that theories were nothing but “convenient instruments” (1963, 99), without reference to any truth. How sad, says Popper (1963, 100), that nowadays so many have joined their ranks, often without realising that they have thus “accepted a philosophical theory”<sup>25</sup> and have “broken with the Galilean tradition”.

Fortunately resistance to the instrumentalist view is maintained by many physicists who “devoted themselves to the search for truth as he [Galileo] understood it” (Popper 1963, 99). Popper cannot accept instrumentalism because in his view certain theories are simply better than others. And this “better” means more tuned to (reflecting better) the known *facts*. Here his realism emerges clearly. “Critical argument” he writes, “would be pointless without an objective reality” (Popper 1983, 81).

On the other hand Popper also criticises the idea that there would be a kind of essence beyond visible phenomena, and the aim of science would then be to describe these essences. In Popper’s view there is no essence beyond the cosmos, and in any case it would be neither accessible to science, nor the aim or object of science. Science can only produce plausible conjectures that are open to scientific criticism. Science constantly aims at producing better conjectures, which bring us increasingly closer to the truth. Although the final truth cannot be attained, there is always a possibility that new and better conjectures may be produced.

### 2.1.c Laws and universals

In line with the realist position, Popper affirms that universals do exist and it would be impossible to operate within science without recognising their existence and their role (Popper 1963, 119). Although Popper rejects essentialism, he accepts the existence of universals. The existence of universals leads him to the recognition of dispositions and propensities, but Popper (1963, 119) is not prepared to conjecture the existence of “ultimate and inexplicable” dispositions: the essences. However, it is not clear how the nature and role of universals is related to his view of the aims and methods of science. In fact, Popper describes and criticises at length the nominalist and essentialist positions (which he rejects). But when it comes to matters concerning science in a more specific way, often Popper avoids engaging in the broader ontological debate. He prefers to choose “another path” which avoids the discussion about the universals (Popper 1961, 28).<sup>26</sup>

<sup>25</sup> By “philosophical theory” Popper means something like “dubious presuppositions” (see 3.1.b). However he admits that realism is a metaphysical position as well (Popper 1983, 82).

<sup>26</sup> Popper (1961, 28) writes for example: “However the methodological issue I’m now going to discuss may in fact be considered independently of the metaphysical issue. We will approach it along another path, one that avoids the question of the existence of universals and singular objects, and their differences”.

According to Popper (1963, 118) “all universals are dispositional”. Universal terms indicate that the thing named by the term shows a certain behaviour under certain conditions. Such behaviour is a “law-like behavior” (Popper 1963, 278) and therefore Popper creates a link between universals and laws. The laws of nature exist and are universal in scope. However, for Popper (1961, 5) a synonym for “the laws of nature” is “physical laws”. As Popper denies that there are historical or social laws but only “trends and patterns” (1961, 115) the only laws of nature that he recognises are physical laws. In this sense, Stafleu’s (1987, 204) observation that Popper absolutises the physical aspect of reality is justified.

Furthermore, the laws of nature are for Popper the object of study of the natural sciences while the other sciences are supposed to focus on individual types of events, while *presupposing* the natural (i.e. physical) laws (Popper 1961, 144; 1963, 341). Here Popper offers the example that a historian, while describing the cause of Giordano Bruno’s death on the stake, focuses on individual events while presupposing the universal law that “all living things die when exposed to intense heat” (Popper 1961, 145). According to this view, of course, the historian does not need to pay much attention to such universal law.

Popper restricts genuine universals to the world explored by the natural sciences. It is difficult to see why he does this: if we say that certain objects are breakable under certain conditions why would it be incorrect to say that people have a disposition to behave in a certain way under certain conditions? Why would natural dispositions be more real than social dispositions? Popper’s reduced version of the universals raises another question. In his system of thought we certainly find the universals. Do we also find a universal *order*? Perhaps the answer can still be “yes”, but in my view we are confronted with a rather restricted version of the law-order. This is unfortunate, given the positive traits of Popper’s ontology. In fact, his recognition of laws, conditions and universals, is more accurate and promising than what we are offered in other philosophies in the nominalist tradition.

#### *2.1.d Towards an evaluation of Popper’s contribution*

Popper’s philosophy is situated at the point where a new conception of science starts to emerge. His philosophy in many ways represents a transition between two epochs. It is not tuned to the mentality of the Vienna Circle anymore. Popper has resisted and challenged logical positivism and logical empiricism. Yet in many respects Popper’s philosophy is not yet part of the more irrationalist trends of the post- or late-modern philosophy of science. Popper found himself placed between two worlds, probably feeling an outsider in both. Having abandoned the positivist position, often he could not agree with the subsequent developments of the ‘60s and ‘70s (e.g. Kuhn and Feyerabend). In many respects, Popper criticised the positivist project, yet

he did not simply abandon it. According to Rossi (1975, 13) Popper's main purpose was to "save" a basic idea of empiricism: it is experience which determines the fate of theories.

For Popper the universals are not simply names, and theories tell us something of an objective reality. According to Popper it is futile to anchor scientific certainty and objectivity in the individual scientist, in the community of scientists or in the psychic or social characteristics of the human nature (Popper 1961, 155-159). Objectivity cannot depend on society, on the subject of knowledge. In his view this "naive solution" does not offer any guarantee (Popper 1961, 155). Objectivity must rather be anchored in the object of scientific enquiry. In Popper's philosophy, though the universals are presupposed, the anchor of scientific certainty is sought mainly in the facts, in an objective reality independent of the knower. In Popper's philosophy, truth remains "correspondence to the facts" (Popper 1963, 224).

It must however be observed that Popper's objectivism is not a crude one that dismisses the role of the knowing subject altogether. On the contrary, the subject becomes more important than in the positivist era. Popper's solution is original and "blends" together elements of essentialism and instrumentalism. It remains realist in its main traits, but the object of scientific research is not considered to be accessible in the same manner as naive realists would have it. It is not possible to verify that our theories are true, that they precisely reflect the facts. The object of science becomes slightly more elusive while the subject acquires a new and relevant role. Nevertheless, in Popper's philosophy the objects of knowledge still retain a relevance that they have often lost in more recent philosophies of science. In fact, in Popper's philosophy they remain the anchor of certainty and sound scientific knowledge.

In the next section I will introduce the philosophy of Michael Polanyi, a thinker who contributed to the change of atmosphere in late-modern philosophy of science. In his philosophy the role of the subject of knowledge becomes the *locus ordinis* of the scientific enterprise.

## **2.2 Polanyi: certainty and personal commitment**

### *2.2.a Introductory remarks*

The difference between Popper's and Polanyi's philosophies can be captured in the titles of their most important books. While Popper wanted to achieve *Objective Knowledge*, Polanyi insisted on the concept of *Personal Knowledge*. While Popper anchors scientific certitude and knowledge especially in the objects and the facts explored by science, Polanyi tries to anchor them in the person, the knower and subject of knowledge. The purpose of this whole section on Polanyi is in fact to illustrate and substantiate this basic anchoring of certitude in the subject. I will take into account two themes: the formation of universal concepts and (in 2.2.b) the theme of scientific discovery.

In her introduction to Polanyi's philosophy, Drusilla Scott (1985) dedicates a whole chapter to demonstrate that Polanyi was confident in the existence of an external reality, independent of our subjective acceptance or rejection of such an idea. In her opinion Polanyi worked toward restoring the faith in the existence of an objective reality. That faith had been shattered by philosophers like Pearson or Mach, who "maintained that we can only have knowledge of our own sense impressions" (Scott 1985, 64). Polanyi, on the contrary, even when speaking of personal commitment, was committed to an external reality (Scott 1985, 66). According to Scott (1985, 65) therefore, Polanyi was a *realist*. I will argue that in my opinion Polanyi's philosophy has more to do with nominalism than Scott admits.

It is interesting to observe that Scott (1985, 65) feels that it is also necessary to specify that Polanyi's idea of reality was quite "original". Being very complex, reality for Polanyi could present "an infinite range of unexpected manifestations" (Polanyi 1974, 133). Reality for Polanyi was somehow hidden, and Scott (1985, 65) also quotes Polanyi saying that he was committed to search for the truth through his own "intimations" of this reality (Polanyi 1974, 133), knowing that there was no strict rule by which his conclusions could be justified.

At this point one might start suspecting that the position of the famous Hungarian thinker is not so easy to define, when it comes to the object of scientific enquiry. One has the feeling that if he is a realist, he is not in the same boat with Popper. If we take for example the existence of universals, we notice that Polanyi does not endorse a "classical" realist position. On the contrary he criticises realism (Polanyi 1974, 165). True, he criticises nominalism as well, by way of a couple of critical questions. For how can one justify, asks Polanyi, labelling an entirely different collection of individuals by the same name? And how can one expect to subsume under the same name all future instances of men who differ so widely from each other? (Polanyi 1974, 166). If Polanyi has his own original solution, such solution does not seem to be a strictly "realist" one. Let's see for example, how Polanyi deals with the formation of universal concepts.

### 2.2.b *The gap between individual clues and universal concepts*

According to Polanyi the process of forming universal concepts is virtually the same as the process of visual perception. The latter process entails the integration of numerous individual perceptions of objects into the same class. In the same way, the formation of universal concepts entails the integration of numerous individual instances into a common meaning. This occurs while all the particulars are relied upon as clues to the universal concept in which they cohere (Polanyi 1974, 166-167). The process of arriving at universal concepts is regarded as a process of "empirical induction" (1974, 166), while the universals in themselves are defined as "the joint meaning of things forming a class" (1974, 170). The process of arriving at universals,

therefore, is simply another instance of tacit knowing, namely the integration of objects into a whole, the same process that is used in learning skills, in recognising physiognomies or in the use of tools. All these processes are presented as “variants of the same organismic process” (Polanyi 1974, 167).

A most fundamental issue for Polanyi is to demonstrate that the transition from particular clues to universal concepts is not achieved by explicit inference. In tacit knowing, in general, the relationship between subsidiary particulars and their focal meaning is essentially unspecifiable. The specific act of integration cannot be explicitly retraced. There remains a gap that only tacit knowing can bridge, not logical inference (Polanyi 1974, 171). As a consequence, the relationship between individual clues and universals remains unspecifiable: there are no specific logical rules for deriving general laws from specific instances. In fact, according to Polanyi, the difficulties that have tormented the dialogue concerning the universals are caused by “seeking an explicit procedure for forming collections of objects which can be justifiably designated by the same universal term” (1974, 166).

At this point, this element of unspecifiability seems to “disturb” the Hungarian philosopher for a moment. Polanyi (1974, 167) therefore asks: “is there any evidence that tacit knowing can establish a uniform meaning for clues which, regarded in themselves have nothing that is the same in them?” The answer he gives may initially sound alarming: “tacit knowing can in fact integrate conflicting clues in various ways” (Polanyi 1974, 167). At a closer scrutiny, however, one can see that Polanyi is not arguing that contradictory clues are just attributed an arbitrary uniformity. He rather relies on the fact that the joint meaning of subsidiary clues transcends the meaning of each particular clue taken by itself. To explain this, he refers to the example where an object sends different pictures of itself to the retina of each eye. In this case the discrepancy is solved as perception reveals “a joint meaning of conflicting clues in terms of a new quality” (1974, 168). A similar operation is at work in the formation of universal concepts.

The central process of tacit knowing implies an assent to the manner in which a certain number of particulars cohere in a focal center. Such assent involves the active participation of the knower. The knower must be *committed* to believe that a set of individual instances jointly point to a universal concept. It is personal commitment that plays a central role in tacit knowing (in general) as well as in its particular instances (like the formation of universal concepts). To complete the picture provided by Polanyi we must also mention the fact that the formation of universal concepts is held with universal intent. They will be regarded as universally valid, as a genuine token of reality, not as fictitious entities (Polanyi 1974, 170-171). But it must be observed that Polanyi anchors the whole process of forming universal concepts in the role of the knower. It is only the knower who can bridge the gap between individual clues and

universal concepts via his own active contribution. A very similar strategy is used by Polanyi to describe the nature of scientific discovery.

### *2.2.c The gap between discovery and the hidden reality*

Scientific discovery, for Polanyi, is not first of all a matter of critical rationality but a matter of personal commitment. In the scientific activity this commitment discloses itself according to its original tacit structure. According to Polanyi, problems are basic to science, but personal commitment and interest are basic in the selection of problems. In fact “nothing is a problem or discovery in itself; it can be a problem only if it puzzles and worries somebody” (Polanyi 1958, 122). The selection of a problem entails in itself a certain risk, for example one could select a problem that is too demanding (1958, 124). But most importantly the selection of a problem discloses a “logical gap” (Polanyi 1958, 123). This is the gap that must be crossed by solving the problem. The presence of the gap indicates that there is no formalised logical procedure to solve the problem or make an important scientific discovery. The latter can never be traced by a logically connected sequence of determinable steps.

True discovery implies an unspecifiable element. The presence of certain elements of unspecifiability, originality and personal risk in discovery points toward the notion of tacit knowing. Originality is not only necessary in the process of discovery but also the true and characteristic mark of discovery. However, the important question is: what is the decisive factor to cross the logical gap? At one specific instance Polanyi (1958, 123) speaks of “illumination”. More in general he maintains that intellectual passions are the key factor, and are directly linked to personal commitment. As in the case of the formation of universal concepts, the commitment of the person is the key factor to cross the gap, in this case a gap between a hidden reality and a committed knower. It is now time to provide a few evaluations on Polanyi’s attempt at finding a reliable anchor for scientific certitude.

### *2.2.d Evaluating Polanyi’s position*

My main thesis in this section is that Polanyi anchors scientific certainty in the knowing subject, thus moving towards a more explicit relativism than Popper. I will start my assessment from the theme of the formation of universal concepts. In Polanyi’s philosophy universal concepts are formed by the committed subject and then held with universal intent. The emphasis of his discussion lies on forming concepts, on distinguishing objects that can be placed in the same classes. Polanyi does not dedicate particular attention to the question whether the formation of universals reflects anything structural in reality or is simply a linguistic or logical exercise. He does not refer to anything like a universal order.

It might be argued that he refers to universal standards of truth and rightness. But these standards are also grounded in personal commitment. Even in this case, in fact, we are not dealing with universal standards that we submit to, through our commitment. We rather deal with a commitment that grounds universal standards in itself and then adheres to them with universal intent. Commitment is therefore the crucial factor in Polanyi's epistemology, and this emerges also when he considers the issue of scientific discovery.

Scientific discovery looks like an activity of bridging the gap between the knower and a hidden reality, a reality that Polanyi often declares to exist independently of the knower. The bridge between the two, between knower and knowable, is provided by the commitment of the knower, as in all processes of tacit knowing. This commitment, however, lies entirely on the side of the knower. This raises the question: how can this commitment be the condition for the relationship between knower and knowable when it lies completely on the side of the knower? On the one hand this commitment appears as determining the possibility of the relationship between knower and reality. On the other it is a subject within that same relationship. In other words, in Polanyi's epistemology the role of the subject appears to be much more decisive than the role of the object.<sup>27</sup> One should acknowledge however, that Polanyi's intention is not to eliminate or delete completely the object, the external reality. His philosophy tries to account for it and does not propose any wild or crude subjectivism.

With Polanyi, however, the role of the subject of knowledge becomes the anchor of scientific knowledge. Polanyi's approach moves away from realism. Polanyi has taught us that the object of science cannot provide totally objective, impersonal knowledge. He rejects Popper's idea that there can be knowledge without a knower (Popper 1983, 92 and 95).<sup>28</sup> Knowledge is a matter of commitment and is personal. A view of impersonal knowledge leads to a universe

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<sup>27</sup> Anastasiou (1979, 107 ff.) not only recognises this pre-eminence of the subject in Polanyi's epistemology but even doubts that reality as a whole exists independently from the subject in Polanyi's work. Anastasiou admits that in many instances Polanyi states his belief in this independent reality. But he wonders whether in Polanyi's case we deal with an *ontic* independence or just with a *believed* one. In fact, argues Anastasiou (1979, 108), the actual independent reality seems to be reduced to a *belief* in its independent existence. Anastasiou is not saying that Polanyi admits or even desires such conclusion. Nevertheless the consequences of positing personal commitment as the crucial element bridging the gap between subject and object do point, according to Anastasiou, towards this undesired result. The argument, though not implausible, seems to me a bit radical. I am a bit hesitant to conclude that in Polanyi's philosophy the role of an external reality is simply absorbed by the committed knower. I nevertheless agree that the role of the subject seems to be the anchoring one in Polanyi's epistemology.

<sup>28</sup> It is quite amazing for example, to read Popper's statement that in the case of a logarithmic table "there is nobody who 'knows' the table (...) yet the table represents 'knowledge', objective knowledge (...). And this knowledge is not 'known' to anybody (not even to the compiler); it is only *available* (Popper 1983, 95). A few pages earlier he states: "the subjectivist theory of knowledge fails for various reasons. (...) it naively assumes that we cannot speak of knowledge without a knower, a knowing subject" (Popper 1983, 92). In these pronouncements the influence of positivism on Popper appears to be still relevant. Polanyi's approach, in this respect, was exactly the opposite: knowledge is always personal knowledge.

without man. However, the opposite danger of subjectivism started to raise its head in Polanyi's philosophy: the danger of creating a man without universe. Very soon the idea that it is the subject that creates his own world was to be proposed with confidence.

Another possible risk is the creation of a man with too many universes! In the following section we will ask ourselves whether this risk is not present in the philosophy of Thomas Kuhn, when he proposes, for example, the thesis of a "multiplicity of phenomenal worlds". More in general, in the following section we will analyse and evaluate Kuhn's contribution towards finding an anchor of certainty for scientific enquiry.

## 2.3 Kuhn: constituting the object

### 2.3.a A preliminary problem

My main thesis in this section is that in Kuhn's philosophy the *world* or *nature* (as objects of the scientific enquiry) appears as more elusive and inaccessible than in previous philosophical works. Kuhn's pronouncements about the object of scientific enquiry are quite complex and have undergone several changes during the years. But before I describe the developments of his position, it is necessary to face a preliminary problem. In fact, Kuhn's use of terms like *nature* and *world* may be experienced as quite confusing. Indeed, already in *The Structure of Scientific Revolutions*<sup>29</sup> we find pronouncements that might be regarded as contradictory. Let us consider a classical example:

"Though the world does not change with a change of paradigm the scientist afterwards works in a different world" (Kuhn 1970, 121).

Passages like these<sup>30</sup> can easily be treated with a certain impatience: does the world change or not, after a revolution? If we decide that such statements are simply contradictory there would be little interest in pursuing an analysis of Kuhn's view of the object of scientific enquiry. Yet Hoyningen-Huene (1993, 32ff.) has provided a serious attempt at demonstrating that such pronouncements can be "reconstructed" as to reveal a sufficient degree of consistency.

What is, therefore, a possible explanation of the apparent contradiction in the quotation above? Hoyningen-Huene (1993, 32-36) explains that it is necessary to differentiate between a "world in itself" (which is not changed by scientific revolutions) and a "phenomenal world" (the world in which the scientist lives; the one that changes after a revolution). The world in

<sup>29</sup> From now on, this title will be abbreviated as *The Structure*.

<sup>30</sup> Other instances of this type can be found in *The Structure* (p. 6, 61, 106, 111, 117, 118, 120, 122, 134, 147-48, 150). Newton-Smith (1981, 117-121) is convinced that such statements simply lead to relativism and "agnosticism". Concerning the above quotation Ratzsch shows a more moderate attitude and observes that "a consistent reading of that sentence requires that *world* be used in two different ways" (Ratzsch 1986, 51, fn. 27).

itself is not accessible to scientific investigation. Only the phenomenal world is accessible. One important characteristic of the phenomenal world is that it is constituted, at least in part, by the subject himself.

I prefer to grant the possibility that some apparent contradictions in Kuhn's philosophy can be "reconstructed" and explained. In this way, I choose to follow Kuhn along his path for as long as it is possible, rather than insisting on those instances that can appear as possible contradictions. Yet one must admit that to follow Hoyningen-Huene on this road a certain "generosity" is needed. In fact, in *The Structure* there is no uniform use of "world" or "nature" justifying a neat distinction between the two concepts. In addition, each term cannot be said to consistently refer to the phenomenal world or to the world "in itself". In *The Structure* the use is rather confused. Occurrences of synonymy of "world" and "nature" can be inferred for example from *The Structure*, p. 77. Here "the comparison of that theory with the world" is paraphrased in the next sentence by "the comparison of both paradigms with nature" (Kuhn 1970, 77).

### 2.3.b Kuhn's plurality of worlds

Nevertheless, many apparent difficulties in Kuhn's works seem to be solved once one accepts the distinction between a phenomenal world and a world in itself. Let us briefly clarify this idea and indicate with more precision the passages of *The Structure* in which Kuhn seems to presuppose this distinction. The world in itself is not accessible to scientific investigation (Kuhn 1970, 111, 114, 118). It does not change after a change of paradigm but is merely "seen in a different way" (Kuhn 1970, 53 and 118). In practice, we cannot say much about the world in itself (Kuhn 1970, 171).

On the contrary the phenomenal world is accessible to scientific investigation and it is at least in part constituted by the subject himself (Kuhn 1970, 112, 125) who projects a certain paradigm on the phenomenal world (1970, 110). This is "the scientist's world" (Kuhn 1970, 7, 111), the world in which scientists "live" (Kuhn 1970, 117, 134). The phenomenal world is therefore accessible to "scientific work" (Kuhn 1970, 6, 121, 147) and it is the world that changes with a change of paradigm (Kuhn 1970, 7, 106). These ideas have a definite kantian flavour, but for Kant only one phenomenal world existed and was constituted by the subjects. For Kant, the categories constituting the phenomenal world were the same for each individual. With Kuhn on the other hand, as the paradigms differ, a multiplicity of phenomenal worlds are created.

The subsequent developments of Kuhn's philosophy did not dramatically change the basic structure described above. The first modifications appeared in the *Postscript*<sup>31</sup> to *The Structure of Scientific Revolutions*. A new emphasis on the concept of *stimulus* will play a large role in the further development of Kuhn's ontology. Now he does not want to say that members of different scientific communities live in different worlds and scientific revolutions change the world in which a scientist works. Kuhn now rather wants to say that:

“members of different communities are presented with different data by the same stimuli. Note however that change does not make phrases like ‘a different world’ inappropriate” (Kuhn 1974, 473, fn.18).

The members of different schools “do in some sense live in different worlds” (Kuhn 1970, 193). They do so in the sense that “the given world, whether everyday or scientific, is not a world of stimuli” (Kuhn 1974, 473). Our given world is not populated by stimuli but rather “by the objects of our sensations” (Kuhn 1970, 193). These are the objects that appear to us through our sensations.

From these passages it appears that the picture created by his new ontology, based on the idea of stimuli, is not very different from the picture presented in *The Structure*. The hypothetical world-in-itself becomes now the posited world of stimuli. The phenomenal world is now substituted by the world of objects revealed to our sensations. The thesis of the plurality of phenomenal worlds becomes the non-uniqueness of the relation between stimuli and sensations. In fact, according to Kuhn: “very different stimuli can produce the same sensations (...) the same stimulus can produce very different sensations” (Kuhn 1970, 193).

### 2.3.c Difficulties and further modifications

Unfortunately, the concept of stimulus was rather unclear to Kuhn's critics because it was used in two different ways and without sufficient specifications. In the first sense the stimuli do not change but nothing else can be known or said about them. They are “neutral” with respect to different phenomenal worlds. In this way, Kuhn was trying to avoid the idea that each scientist is confronted with a unique world of stimuli, inaccessible by another scientist (1970, 192-93. See also 1970b, 276).

In the second meaning of *stimuli*, the opposition between stimuli and sensations plays a large role. While sensations are “given” to us, we know of stimuli only by way of theoretical construction. They are simply what scientific analysis identifies as the causes of our sensations (e.g. sound waves, photons and the like). In this second sense, stimuli are therefore accessible

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<sup>31</sup> Notice that in this section 2.3.a, all the quotations from *The Structure* (Kuhn 1970) are in fact quotations from the *Postscript*.

by scientific investigation. They are not purely neutral with respect to phenomenal worlds. On the contrary: they are a construction contingent on a particular phenomenal world.

Two important modifications of this system were introduced in 1979. First, Kuhn (1979, 418) “eliminates” the notion of a world-in-itself. He describes his new position as “Kantian but without the ‘things in themselves’” (Kuhn 1979, 418-19). Though he claims his position to be “realistic” it is not clear what he means, and he does not clarify the issue further (Kuhn 1979, 415). We are however going to face the question concerning Kuhn’s realism in the next section. The second change concerns the phenomenal world. In *The Structure* and before 1979 the main metaphor employed to indicate access to the phenomenal world was a visual one (e.g. “they see the world in the same way”). Now the emphasis shifts to language and Kuhn speaks of “clusters of interrelated terms” (Kuhn 2000, 47).

The above description can give an introduction to Kuhn’s view of the object of scientific research. In the next sections I will formulate more articulate evaluations on this topic. For the moment, however, I think one is allowed to say that with Kuhn’s philosophy, the object of scientific analysis has become more complex, but also more elusive and inaccessible. In this regard, another important question is to know whether Kuhn is a realist or a nominalist. From a broader perspective, the question would be: is Kuhn an objectivist or a subjectivist?

#### 2.3.d *Is Kuhn an objectivist?*

In some instances Kuhn would describe his position as a realist one (e.g. Kuhn 1979, 415). In my opinion, however, Kuhn moved deeper than Polanyi into a non-realist (i.e. subjectivist) position. Initially Kuhn granted the independent existence of a world-in-itself, but in the last developments of his philosophy he found it unnecessary to posit the existence of such a world, inaccessible to and totally independent from the human subject. The only world that exists in his writings after 1979 is the phenomenal world, which is partly constituted by the knowing subject. As a consequence, the possibility of a world existing independently from the subject (as epistemological realism would have it) was eliminated. This is a first clue pointing towards the adoption of an idealist position.

A second clue, pointing in the same direction, is constituted by the fact that according to Kuhn the scientist encounters especially individual realities in the phenomenal world. It is by means of ostensions and exemplars that he learns to classify individuals as members of certain classes (Kuhn 1974, 472ff.). But there seem to be no fixed borders in the reality out there that would impose themselves on the knower. There are no “joints” in the world, no constant structures conditioning our classifications (Kuhn 1979, 418). The only existing realities are the objects of our phenomenal world, while our classifications are names and concepts that exist only in our language and in our mind. This is a clue pointing towards nominalism.

Thirdly, moving to the level of what can be said, even when the world-in-itself was posited it was considered as inaccessible to scientific description. Our scientific statements therefore, do not reflect any independent state of affairs. Our theories cannot reflect the world in itself because it is inaccessible. They might to a certain extent reflect states of affairs of our phenomenal world, but the latter is partially constituted by the knower. In addition, a certain theory can be contingent to a certain phenomenal world, but the latter is only one of the possible worlds. As a consequence, it seems difficult for Kuhn to join realism in its claim that statements are true or false in virtue of a reality existing independently of the knower. In this context Kuhn's position is rather an anti-realist one.

When comparing the positions of Kuhn with Polanyi's or Popper's philosophy, therefore, there are concrete elements to conclude that Kuhn's philosophy has moved with more determination towards a non-realist position. It is true that Kuhn himself initially regarded himself as being a realist,<sup>32</sup> but in his later writings Kuhn himself (2000, 76) describes his position as posing a threat to realism. The reason is that a world constituted by similarity relations is at least in part constituted by the subject moment and therefore cannot be as "objective" as realism would have it. I therefore agree with Boyd (1983, 60ff.) and Newton Smith (1981, 120) who regard Kuhn respectively as an anti-realist and a non-realist.

There are however areas of Kuhn's philosophy in which a certain sort of realism is maintained. This, I believe, constitutes a contradiction. Although Kuhn's position is usually a non-realist one his approach to the history of science can be considered a realist one. For example, Kuhn seems to have no doubt that the historiography of science tries to understand science as it really happened. It tries to "display the historical integrity of (...) science in its own time" (Kuhn 1970, 3) or to "analyze an older science in its own terms" (1970, 167). For Kuhn a particular assertion about the history of science "is simply a statement from historic fact, based upon examples" (Kuhn 1970, 77 and 96). Kuhn is willing to regard some of the results of the old historiography of science as simply erroneous (1970, 142). This is quite surprising, in view of the fact that Kuhn does not consider abandoned or obsolete positions as "mistakes" and has often opposed (e.g. 1970, 121) this type of language. Some authors (e.g. Radnitzky 1982, 71) have definitely considered Kuhn's realistic reading of the historiography of science as an instance of internal contradiction in his philosophy.

The previous contradiction might constitute an interesting clue to the fact that it is precisely in history that Kuhn tries to place the archimedean point of the scientific enterprise. To phrase it in Bernstein's words, Kuhn appeals to "moments of the history of science, or more accurately

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<sup>32</sup> See for example Kuhn's argument that both Popper and himself "insist that scientists may properly aim to invent theories that *explain* observed phenomena and that do so in terms of *real* objects, whatever the latter phrase may mean" (Kuhn 1970a, 2). Even more explicit is Kuhn's declaration that Boyd and himself would be "both unregenerate realists" (Kuhn 1979, 415).

to interpretations of selected moments of the history of science as evidence for (...) what is characteristic of or most vital for understanding science” (Bernstein 1985, 73-74). But in this case the appeal to history is not sufficient. It is illusory to think we can appeal to history to justify general assertions about the nature of science. In fact, if observation is theory-laden, so must be the study of the history of science. This appeal to history, however, is a clue towards Kuhn’s relativism or subjectivism.

### 2.3.e *Is Kuhn a subjectivist?*

The anchor of certainty is placed by Kuhn in the human subject of knowledge. It is the scientific community that gradually becomes “the initiator and sanctor of the legitimacy of scientific knowledge” (Botha 1994, 21). During the 1970s, the Popperian school of philosophy of science considered Kuhn’s emphasis on the scientific community as being definitely excessive. As a consequence, Kuhn was accused of justifying “mob psychology” (Lakatos 1970, 140 fn. 3, 178). In other words, he was accused of presuming that what the community says is the truth for a certain time and context. Kuhn (1970b, 260-263) defended himself from those allegations by saying they were based on misunderstandings of his views. But although Kuhn was not promoting arbitrariness he had certainly moved to a much more relativist position by anchoring his epistemology in the subject of knowledge.

To acknowledge this move it is sufficient, for example, to look at the paradigm concept and to realise how dependent it is on the human subject. Each paradigm is the achievement of a scientific community and without community there is no paradigm. A paradigm ends the controversies among schools of the pre-paradigmatic phase of science and establishes the consensus of a scientific community. In fact, initially the paradigm concept was elaborated by Kuhn to account for the consensus of a scientific community. The phases that in *The Structure* are called “pre-paradigm” and “paradigm” periods (Kuhn 1970, 47, 63), were initially defined as “preconsensus” and “firm consensus” phases (Kuhn 1977, 231-232- first published in 1959).

The paradigm concept is so tied to the community concept that Kuhn can say: “a paradigm is what the members of a scientific community share, and, conversely, a scientific community consists of men who share a paradigm” (Kuhn 1970, 176). Although Kuhn will later specify and modify this basic idea, its fundamental meaning will remain unchanged.

It is true that the communal or sociological dimension is more present than in Polanyi, who emphasises the personal dimension, discoveries by individuals and so on. It is true that Kuhn avoids radical relativism and solipsism exactly by avoiding an individualist view of science. It is also true, as Kuhn often remarked, that his position is not characterised by a radical relativism. However, for the purpose of recognising Kuhn’s subjectivism it is not important whether the emphasis is placed on an individual or collective subject. It is also not crucial to

know whether his relativism is nuanced or not. In fact, in Kuhn's philosophy we are offered a subjectivist position. In the following section I will attempt a provisional evaluation of Kuhn's view of the object of scientific enquiry and of the *locus* of order for science and reality.

### *2.3.f Kuhn on the object of scientific enquiry: a few evaluative thoughts*

With Kuhn, the object of scientific research becomes more elusive. First of all, he creates a double order of reality. There are two "worlds" in his philosophy: the world-in-itself (inaccessible to human knowledge) and the world that we can access, the phenomenal world. There is therefore a "part" of our reality, an aspect or a dimension that cannot be reached, known or described by the knowing subject. In a sense, we may say that in Kuhn's philosophy the object of science has "shrunk". In fact, science does not extend its researches to the whole of reality. There is a "part" of reality that simply cannot be known. Ironically, the part that can be known is the one that is partially constituted by the subject himself. As a consequence, we don't know an object independent from our awareness of it, but only an object that is (at least in part) constituted by the subject of knowledge.

In addition, in Kuhn's philosophy the object of science is "privatised" (and again made more elusive) by the multiplication of the phenomenal worlds. In this respect we may observe that each phenomenal world becomes a "private property" of the community that has contributed to the constitution of that particular world. Only that community can have a proper access to that world, while the others, to a certain extent, "live in a different world". Secondly, the existence of multiple phenomenal worlds implies the existence of multiple objects of scientific research and multiple results of such research. It might be observed that this standpoint implies on the one hand a certain neutrality of the researcher towards the different worlds (because no world can be said to be preferable to the others). But on the other hand this same approach implies that the researcher can approach the standpoint of others only from within his own phenomenal world, so that no neutrality is possible.

But apart from pointing to a few problematic facets, my argument remains that (when compared to his predecessors in philosophy of science) Kuhn advances towards a more relativist position. While the knowing subject acquired the fundamental role, the object of science was in part declared inaccessible, in part constituted by the subject and finally "multiplied" (or "divided"?) into many different phenomenal worlds.

This was a consequence of the attempt at anchoring scientific certitude in the knowing subject, i.e. the scientific community. From this starting point, the aspects of historical development attracted Kuhn's attention in a special way and seemed to be more fundamental than others to understand the scientific enterprise. History seemed to constitute the necessary archimedean point to understand science, but in the meantime the object of scientific enquiry

was suffering an “eclipse” in which it became more and more invisible and elusive. However, more radical positions were still to come. In the following section we will consider the points of view of a few more recent philosophers of science.

## 2.4 Further developments: Feyerabend, Rorty, Collins...

### 2.4.a Feyerabend and realism

The purpose of this whole section 2.4 is to show that after Kuhn the relativist approach became more influential and the object of scientific enquiry became more vague and elusive. One can also observe another particular phenomenon. While Feyerabend was still inclined to search for an archimedean point especially in the historical aspect, philosophy of science also took a *linguistic* turn and a *sociological* turn. In the following sections I am going to analyze briefly two representatives of these schools of thought, in order to substantiate the claim that they supported and strengthened a relativist point of view. We will begin by analysing Feyerabend’s approach.

Given his declared anarchistic strategy, one would expect Feyerabend to promote an open nominalist approach to the object of science. As epistemological anarchism is centered on the human subject and rejects external constraints and rules, one would expect nominalism to be Feyerabend’s choice. Yet in Volume 1 of his *Philosophical Papers* (Feyerabend 1985) he surprises his readers by saying very little about nominalism and by discussing realism in much more depth and with an openly supportive attitude. Initially, realism was the position that Feyerabend (1978, 113 ff.) defended. In some editions of *Against Method* one can read Feyerabend’s words dedicating the book to his late lamented friend Imre Lakatos, who is then praised for being “a Realist”. In Feyerabend’s (1985, 201-202) *Philosophical Papers* we learn that realism is always preferable to whatever form of instrumentalism. We learn that there are many types of realism, both scientific and “philosophical”, and that he agrees especially with the scientific versions (i.e. scientific realism).

Although he was slightly less sympathetic toward its philosophical versions, initially realism was the position that Feyerabend endorsed. But then the anarchist approach emerged more clearly in his philosophy and Feyerabend tried to combine anarchism and realism. In the long run, his anarchism prevailed. Feyerabend’s interest centered especially on the possibility of a proliferation of different theories. The existence of an eventual “independent” reality gradually became much less interesting.

It is my opinion that initially Feyerabend imagined that the realist position might have placed his anarchistic approach on a more solid ground. For example, in *Against Method* we find the surprising statement that chaos and counter-inductive science are suitable and even necessary not only because they suit the personal taste of some thinker, but because “the laws that are

valid in this world” contradict the traditional methodologies (Feyerabend 1975, 40). A chaotic methodology suits the order of our cosmos, because the latter is chaotic! If Feyerabend could have proved that this is the case, epistemological anarchism would have found a much more solid foundation than the subjective preferences of those who are sympathetic to the anarchistic approach.

Yet this attempt did not last long. In the first volume of his *Philosophical Papers*, Feyerabend (1985, xiii) claimed that chapters 16 and 17 “contain first steps towards undermining this intellectual arrogance [of realism]”. Then, introducing Volume 2, Feyerabend complained that realism “only reflects the wish of certain groups to have their ideas accepted as the foundations of an entire civilization and even of life itself” (Feyerabend 1985, xiii). He was referring to philosophical realism, which he didn’t fully appreciate from the beginning. His anarchistic approach required that “we decide to regard those things as real which play an important role in the kind of life we prefer” (Feyerabend 1985, xiii). In other words,

“accepting a form of life L we reject a universal criticism and the realistic interpretation of theories not in agreement with L. Proceeding in this way we notice that instrumentalism is not a philosophy of defeat; it is often the result of far-reaching ethical and political decisions” (Feyerabend 1985, xiii).

Having abandoned the realist position, Feyerabend moved towards pluralism and anarchism.

#### 2.4.b Feyerabend, pluralism and anarchism

In the previous quotation we encounter a classical conviction<sup>33</sup> of Feyerabend: science (and life in general) is what we decide it should be. The philosophers of science who have tried to tame the irrationality of science with the help of standards and methodologies that transcend research, have failed (1985, xiii). But their failure does not put an end to the anarchist’s attempt to adapt science to our favourite form of life. It only “frees the attempt from irrelevant restrictions” (Feyerabend 1985, xiii), namely the restrictions imposed by the philosophers of science mentioned above.

Feyerabend walks on the edge of the cliff: it is necessary not to impose to science standards that would direct the scientific process from outside. The standards must arise from inside science. And yet we must try to “adapt science to our favorite forms of life”. At the bottom of these rather contradictory statements, I believe, lies the conviction that epistemological

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<sup>33</sup> Feyerabend recognises the contradictory character of some of his contributions collected in the two volumes of his *Philosophical Papers*. He says: “the reader will notice that some articles defend ideas which are attacked in others”. This is partially due to the fact that the chapters have been written in different phases of his philosophical development. But that’s not the main reason: “this reflects my belief (which seems to have been held by Protagoras) that good arguments can be found for the opposite sides of any issue” (Feyerabend 1981, xiv).

anarchism is the only standard that can be imposed on science without damaging it, even though it arises from outside science. The reason is that it corresponds to Feyerabend's "favourite form of life".<sup>34</sup>

Concerning the object of scientific investigation, we can say that, for Feyerabend, views and theories must have priority above evidence, facts or external states of affairs. Only in this way a plurality of views, a proliferation of theories can be maintained and even enforced. External realities should not be allowed to dictate our theories or our accounts of those same realities. The task of scientists is not to select the best theory, but to resist the falsification of weaker theories by defending them for as long as possible (Feyerabend 1975, 30). It is at this point that a "science without experience" becomes at least "conceivable" and is said to present several advantages (Feyerabend 1985, 132-135). Only in this way plurality and proliferation will become reality.

Here one meets what I consider one of the main dilemmas of Feyerabend's philosophy: pluralism and proliferation must be defended at all costs, but anarchism is the best solution. Actually, the ideal would be that all other solutions be defeated while anarchism triumphs (more on this problem in 4.4.c). Here the temptation rises to "found" the anarchist approach according to realistic standards. Anarchism corresponds (better) to the laws of our world! But as this approach is contradictory to anarchism itself, Feyerabend must in the end resort to something else. Namely to "regard those things as real which play an important role in the kind of life we prefer" (1985, xiii).

What is certain is that, in Feyerabend's approach, worldviews are more interesting and more important than reality. The subject of knowledge is more important than the object. Epistemological changes are ontological changes.

"we know that there are changes which are not results of a causal interaction between object and observer but of a change of the very conditions that permit us to speak of objects, situations, events. (...) a change of universal principles brings about a change of the entire world. (...) we no longer assume the existence of an objective world that remains unaffected by our epistemic activities (...) our epistemic activities may have a decisive influence even upon the most solid piece of cosmological furniture – they may make gods disappear and replace them by heaps of atoms in empty space" (Feyerabend 1978, 70).

The *locus* of order is placed in the subject while the archimedean point is found in the ever-changing stream of history, where nothing is fixed or absolute. This position however, did not convince all. According to Rorty, what is really crucial to understanding science is the linguistic aspect of the scientific community

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<sup>34</sup> On the authoritarian temptations (or even consequences?) of Feyerabend's libertarian philosophy see

#### 2.4.c Rorty and the linguistic turn

Concomitant with the historical turn, late-modern philosophy of science has experienced a linguistic turn. While the historical school regarded the history of science as the main key to understanding scientific knowledge, the linguistic school regards language as being that key. Language becomes a metaphor for reality and knowledge, in line with Wittgenstein's (1961, 5.6) dictum "the limits of my language mean the limits of my world". The structure of reality becomes a projection of the grammar of language. The investigation into how words are used is at the same time an enquiry into ontology.

Richard Rorty is one of the most outstanding representatives of this movement. He is not specifically a philosopher of science: his works deal with a broad range of philosophical topics, from ontology to politics. But in his postmodern approach he dedicates considerable attention to science. His approach is characterised by a sincere relativism and by an attempt to get rid of some of the classical problems of Western philosophy. Rorty tries to renew the vocabulary of this tradition. To renew the vocabulary means to renew the basic categories of our thinking and theorising.

Rorty (e.g. 1991b, 4-5) rejects the idea that the "truth is out there" and can be "found" or discovered. A real world might be out there, but the truth about it must be "made", not discovered. In fact, whenever we deal with truth we deal with words, with language. Now language cannot simply be "out there"; it is something that only human beings can produce. Without language we cannot elaborate any truth. It is through language that truth is constructed. Language becomes therefore the true origin of truth. In line with the best pragmatist tradition, the truth is not what mirrors a certain state of affairs "out there". It is rather what solves our problems, what is efficient and useful. We construct for ourselves truths that help us to cope in a given situation.

This is clearly a pragmatist approach. It is not important to know whether our *nomina* reflect any universal order, as long as they are useful to us. Our truths are simply linguistic constructions, and our scientific activities are founded on language games. But as long as these games are helpful, it is important to keep them going. It doesn't matter whether our theories reflect true states of affairs: science should aim at solidarity, not at objectivity (Rorty 1993, 35-45). In any case, objectivity must be re-defined as meaning especially agreement within the scientific community (Rorty 1985, 13).

Rorty's approach is also a relativist and subjectivist one. Although he rejects such definitions as belonging to the vocabulary of the Enlightenment (Rorty 1993, 44-49), it is clear that his epistemology is anchored in the subject. To phrase it like Mary Hesse (1985, 31) did, in one of

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fn. 68.

her dialogues with the American philosopher, Rorty's ontology contains "texts without types and lumps without laws". Admittedly, in a world without types and laws, there are still objects. But according to Rorty:

"Pragmatism views knowledge not as a relation between mind and an object, but, roughly, as the ability to get agreement by using persuasion (...) I think of objectivity as a matter of ability to achieve agreement on whether a particular set of desiderata have or have not been satisfied" (Rorty 1985, 11 and 13).

The key-factor, in epistemology, is therefore the agreement between the subjects of knowledge. Given this general background in Rorty's philosophy, let's now observe, more specifically, the main traits of his philosophy of science.

#### *2.4.d Rorty's philosophy of science*

One of the most strategic points to observe Rorty's conception of science is his brief comment on Habermas' and Lyotard's philosophies of science (Rorty, 1993). In particular, his dialogue with Lyotard is of special significance for our present purpose. In his philosophy of science Lyotard conducts a robust anti-representational campaign. He parallels anti-representational science with the postmodern condition itself (Lyotard 1984, 60). The postmodern society is crossed by a huge variety of language games, which are engaged in constant struggle against each other and/or against themselves. Although the political and cultural implications of Lyotard's position are not always palatable to Rorty (1993, 211-222) he shares with the French philosopher the basic idea that science does not aim at mirroring reality. The language of science (remembering Wittgenstein's lesson) is to be regarded as a tool, not as a mirror (Rorty 1990, 9). Scientific knowledge (in this case remembering Dewey's<sup>35</sup> lesson) is not shaped by permanent external constraints. We need not enquire about the "conditions of knowledge" (Rorty 1990, 9).

According to Rorty (1991, 85), Lyotard "argues invalidly" that the aim of science should be permanent revolution. We should not forget Kuhn's lesson: science is an alternation between revolutionary and normal science (Rorty 1991, 85-86). To say that science must aim at revolution is equivalent to say that politics should aim at revolution. Lyotard on the other hand is right, according to Rorty (1991, 86), when arguing that the language of science is metaphorical and its logic is circular interpretation. Rorty agrees with this implicit rejection of empiricist and positivist philosophy of science and appreciates Lyotard's rejection of the need

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<sup>35</sup> Dewey is of course a key-figure to understand Rorty. For a reformational critique of Dewey's epistemology and philosophy of science an old contribution by Hart (1966) is to be recommended for the depth of its systematic analysis. For a more recent contribution about the impact of pragmatism on contemporary academic life but also on society in general see Venter (n.d.1).

for the foundations and legitimations of science that are based on metanarratives (Lyotard 1984, xxiv).<sup>36</sup>

We must go back to Bacon, says Rorty (1991, 92), the prophet of self-assertion and self-grounding. The golden thread of modern philosophy then, will appear to be our willingness to hope in the future and in the success of our descendants. In Rorty's view we should turn our backs on Descartes and on his contention that our most human characteristic is the ability to manipulate "clear and distinct ideas". What is distinctively human is rather the ability to accomplish feats of social engineering. The recovery of a Baconian perspective on science will also make clear that there is no basic difference between the aims of politicians and the aims or procedures of scientists (Rorty 1991, 92).

In Rorty's subjectivist approach there is not much discussion of the object of scientific research. The whole discussion turns around the subject and his needs, his aims, his language games. It is in the subject that the *locus ordinis* should be found and from there one should recognise symbolic signification as the fundamental aspect of reality. But somehow Rorty must feel that this is not sufficient, because he has "supported" this basic theme of his philosophy with three other fundamental themes.

First, we can think for example of the relevance of the social aspect, because language is always the production of a given community. After all, truth is what our colleagues will *allow us* to say. Secondly, we can think of the importance of pragmatism in Rorty's system. After all, the purpose of all language games and all social interactions is to be able to cope, in practical terms, with certain specific conditions. Finally, we can also think of the theme of the non-correspondence between theory and reality. A good question, at this point, would be to ask whether these different anchoring points support each other or rather oppose each other. Clouser (2005, 18-19) for example, is of the opinion that if any one of them is seriously taken to be fundamental, the others must be excluded from playing an equally fundamental role.

#### 2.4.e Collins and the sociological turn

If the *locus ordinis* has for Rorty especially a linguistic character, for Collins it is the scientific community that constitutes the order of the world that we explore. Collins is part of a larger

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<sup>36</sup> Lyotard analysed the problem of the legitimate foundations of science. The term *legitimation* is borrowed from Habermas, but for Lyotard it becomes the key concept of *The Postmodern Condition* (Lyotard 1984). These meta- (or "grand") narratives are the transcendent and universal truths that supported and legitimised Western civilization. What Habermas proposes, according to Lyotard, is still indebted to the problem of metanarratives. According to Lyotard (1984, xxiv) postmodernity itself is in essence nothing else than "incredulity towards metanarratives". Lyotard affirms that these meta-narratives have now been substituted by a certain number of language games (a concept which he borrows from the later Wittgenstein). Such language games range from various forms of utterance (denotative, performative, prescriptive, with their own set of rules) to *petit recits* or small narratives. For

movement that initiated the “sociological turn” (Botha 1994, 22). His *Empirical Programme of Relativism* is closely related to the *Edinburgh School of the Sociology of Knowledge*. Among the most representative figures of this “turn” are Brown (1984), Barnes and Bloor (1995).

Collins challenges the commonsense assumption that there is an order of reality that does not change, that shapes scientific theories, and that can (to a certain extent) be represented by those theories. According to Collins the essence of this order is rather sociological. Because there are communities that explore such order and share a certain language, we experience uniformities and regularities. But the foundation of this order is sociological: “the locus of order is society” (Collins 1992, 148). It is not the order that constitutes the possibility of a common language and of a scientific community. It is just the opposite. The existence of a scientific community is the origin of the perceived order (Collins 1992, 5).

Collins sets out to demonstrate that the origin of replication in science has a sociological foundation. He explores the case of the TEA-laser (for experiments in gravitational radiation) and 2 studies on paranormal phenomena. Through detailed observations he shows that the object of scientific enquiry has very little relevance in shaping our conclusions. What really matters is our perception. We perceive regularities and order, we expect the future to be like the past. But, asks Collins (1992, 6), is there any guarantee that our “inductive inferences - generalisations from past experience - can ever be certain or even probable?” Collins invites the reader to remember Hume’s lesson about causality. We see the movement of a billiard ball across the table and we are inclined to say that the ball is propelled by another ball. Its movement is “caused”, so our experience tells us. But, asks Collins, supposing that the regularity of the a - b sequence were just an extended coincidence, “how would we see the difference?” (Collins 1992, 7).

“In other words, what is it that we see in the impact of the billiard ball that makes us view it as a casual relationship (...) rather than an extended coincidence, which we could not expect to continue? The answer is: nothing” (Collins 1992, 7).

If we want to be able to solve this type of dilemmas, according to Collins we must look carefully into our social life and our language. The two are so intermingled that our habits of speech help determine the way we see the world and thus help form the basis for social interaction (Collins 1992, 11). It is exactly there that our attention should focus. We perceive regularity and order because any perception of irregularity in an institutionalised context is translated by ourselves and by others as a fault in the perceiver or in some other part in the

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those who live inside them, they appear as inevitable and superior. Yet they have only limited social and historical validity, which means only limited legitimation.

chain of perceptions (Collins 1992, 147). Scientific rules are only rules by virtue of social conventions (1992, 145).

But is there no room then, it might be asked, for scientific revolutions created by brilliant individuals who dare to go “the wrong way”, against the social network and its settled conventions? Collins (1992, 148) admits that “it is only individuals who can provide the material for conceptual change”. But an individual’s act of creativity is nothing unless it becomes institutionalised. Even in this case, the wider “network” and the wider society provide the conditions for the success of some new institutions but not others. “Man proposes but society disposes”, says Collins (1992, 149). Curiously, in his statement “society” substitutes the “God” of the original proverb. Indeed one might have the feeling that Collin’s social networks explain everything, cause everything yet are in their turn not dependent on anything or needing any explanation. Clouser (1991, 21-24) would classify this as a “religious belief”. In fact, it is nothing else than an absolutization of the social aspect of our experience. Society then becomes the foundation of order and of scientific truth.

In this analysis of several authors I have especially illustrated the move towards nominalism, the “eclipse” of the object of science, and the search for the elusive universal in several aspects of experience. Before moving to the next section, which is dedicated to an evaluation of the symptoms, I must mention another problem which accompanies both realism and nominalism: the problem of the incongruence between science and reality. This problem does not emerge as clearly as the ones I have mentioned above from the analysis of the authors. It remains rather implicit and it needs to be “brought to light”. I am referring to the incongruence created by the fact that scientific knowledge aims at the universal while concrete reality presents itself in its individuality.

The rejection of realism has brought about nominalist approaches that reduce the universals to concepts or words, and the only entities that can be proposed as the object of study of science are individuals. Science, however, does not aim at knowledge of individuality, not even of all individuals (which would be an impossibility). Science aims at universal knowledge but the nominalist approach seems to exclude this possibility from the beginning and an incongruence is thus created. The realist approach may be imagined to rest on more solid ground, but its solutions cannot be recommended either. In fact realism, already from Plato, is inclined to objectify the universal, to see it as an entity (not as a trait of everything that exists). We might say that nominalism and conceptualism place the universal in the subject, while realism reduces the universal to an object. In both cases the problem of the incongruence between science and reality remains a dilemma (I will return to this topic and expand it in 2.5.b).

Having sketched a panoramic view concerning the problems surrounding the theme of the anchor of scientific investigation in late-modern philosophy of science, we must now move toward an evaluation. This will be the goal of the next section. In it we will try to penetrate into the roots of the problems and difficulties that have been identified and illustrated in the present section.

## 2.5 Diagnostic attempts and explorations

### 2.5.a *The cartesian divide*

The developments that I have described in the previous sections seem to be basically determined by a kind of polarity: the *locus ordinis* is to be searched either in the object (my only example in this case has been Popper) or in the subject of knowledge (e.g. the historical school). This polarity goes back to the prescriptions of the cartesian either/or. Descartes divided the whole reality in extended matter and/or mind. Either the subject or the object are supposed to be the *locus ordinis* of reality and of human knowledge.<sup>37</sup> Nominalist philosophy has inherited from the Enlightenment the conviction that rational knowledge is possible, but tries to anchor it to the subject. Realism on the other hand prefers to anchor sound knowledge to the object.

What is missing in this polarity (and in the cartesian divide) is the recognition of the Law, the universal order for reality. The lack of this recognition leads to unilateral and unsatisfactory views of science. In fact, placing the anchor of certainty in the object leads to an underestimation of the role of the subject and his influence in the process of knowing (this situation was typical of the positivist era). On the other hand, placing the anchor of certainty on the subject leads to the “eclipse” of the object of scientific analysis, which I have tried to substantiate by exploring several trends and authors of late-modern philosophy of science.

Descartes himself found the *locus ordinis* in the subject after a process of systematic doubting. When everything else is doubted, the subject of knowledge emerges as the anchor of certainty of existence and knowledge. But Descartes introduces a further notion. It is not simply the subject which anchors certitude, it is more precisely the *thinking* subject (*cogito ergo sum*). In other words Descartes, after placing the *locus ordinis* in the subject, finds a further anchorage of knowledge (which I have called the archimedean point) in the logical aspect of reality. The interesting point is that in the previous section we have seen several late-modern philosophers repeating the same operation. The only difference was that they preferred to find the archimedean point in the historical, social or linguistic aspects of our experience. In section

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<sup>37</sup> On the topic of the “subject-object alienation” starting with Descartes, R. Wuriga has written a Master’s thesis from a reformational point of view, at the Potchefstroom University for Christian Higher Education (see Wuriga 1999).

2.5.d I will criticise this attempt at anchoring certainty by absolutising one of the modal aspects of reality. But it is first necessary to deal more in detail with the realist and the nominalist positions and with the incongruence that they create between scientific knowledge and the supposed object of scientific investigation.

#### 2.5.b *Realism, nominalism and the incongruence between science and reality*

From an ontological point of view, realism can be defined as the position that recognises the existence of universals (and individual entities as well) among the objects existing in this world. From an epistemological point of view, for the realist the objects of the world exist independently from a human experience of them. Another fundamental characteristic of realism is that these objects of knowledge are, to a considerable extent, considered as *accessible* to knowledge. Views about the degree of accessibility or knowability may vary, but the moment the realist should declare that these entities exist but are not knowable he would have joined, in practice, the nominalist position. The realist also has a particular position when it comes to the philosophy of language. For the realist statements can be true or false, and their truth and falsity can be assessed by virtue of a reality existing independently of us.

Nominalism, on the other hand, does not recognise any ontic status of universals. They can be described as human ideas or words (cf. the Lat: *nomen*: the name/word for something). When universals are seen especially as concepts in the mind, we have the *conceptualist* position. From a nominalist point of view, ontologically speaking, the only entities that exist are individual. In any case, entities do not exist apart from or independently of human experience or interpretation of those realities. Epistemologically speaking, the knowledge we acquire is given by the elaboration of human representations and ideas. Knowledge is to a large extent a human elaboration. Is then objectivity simply lost? The nominalist often answers that all rational beings experience the world in the same way, therefore individualism and solipsism are not to be feared. Concerning the issue of language, the anti-realist argues that the analysis of statements in terms of truth conditions should be substituted by an analysis in terms of verification (or assertibility) conditions.

Fundamental to the whole debate are issues like the existence of universals, the nature of the universal order for the cosmos, the existence of natural kinds and the way scientific language reflects these features of the world. In any case, while describing the main characteristics of nominalism and realism we should of course keep in mind the fact that there are many gradations in (and variations of) both positions.<sup>38</sup>

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<sup>38</sup> There are of course also different evaluations and interpretations of both realism and nominalism. From a reformational point of view, for example, Strauss (1993) maintains that nominalism has both an irrationalist and a rationalist version and is the main force shaping modern thinking. Strauss does not

It is fundamental to observe that both realism and nominalism meet a problem that I have defined as *the incongruence between reality and scientific knowledge*. The reality that we experience is mainly individual, but scientific knowledge is not knowledge of individualities. It is rather knowledge aiming at the universal. These issues are obviously not limited to the authors. In the previous pages I have analysed the contemporary philosophers of science. In order to understand their specific problems better, it might be interesting to have a look at what we can consider the historical roots of the problem of incongruence in the realism-nominalism debate.

### 2.5.c Historical roots of an incongruence

In Plato's realism, ideas are attributed real existence, independent from the world of concrete realities. Science is thinking of the ideas, and each idea is the universal model for many concrete realities corresponding to it. In what is referred to as the crisis of Plato's theory of ideas, Plato aims at affirming the logical unity of the ideas while at the same time he tries to approach concrete reality itself via the contemplation of the ideas. This was an attempt at solving the problem of incongruence between science and concrete reality. In this phase Plato gradually introduces new members in the world of ideas, in order to account for those phenomena (in concrete reality) that had not yet been accounted for.<sup>39</sup> This attempt to solve the incongruence by adaptation, led Plato to a dead end.

Aristotle rightly reproached Plato for trying to create a duplication of reality in his world of ideas. Science, for Aristotle, implies abstraction. The universal exists for Aristotle too, but not in a separate world of ideas. It is rather inherent in the single objects. Aristotle thus identifies reality and essence to a large extent. But what Aristotle's and Plato's solutions have in common is the assumption that the knowable, the universal, must resemble reality. Therefore they assign an entity-like character to the universals. This seems to remain the major difficulty of realism well into the 21<sup>st</sup> century.

Nominalism avoids assigning an entity-like character to universals and at the same time re-evaluates the concrete, individual reality. The best contribution of nominalism and conceptualism probably lies in the importance they attribute to reality, the re-valuation of the

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provide many examples of realist philosophies and in my opinion gives the impression (cf. Botha's questions in 1994, 16 and 21) that realism had no truly relevant impact on modern philosophy.

<sup>39</sup> Initially Plato (1925, *Lysis* 219, c) mentions a limited range of ideas like the "good in itself", the "equal in itself" and so on. In *Parmenides* (Plato 1969, 130, c) Socrates is hesitant when Parmenides asks him whether there exist even an idea of a hair, of mud or of dirt. At this stage Plato does not provide an answer but acknowledges the problem. But in the *Republic* Plato (1966, 596, b ff.) distinguishes between a bed painted by an artist, a bed constructed by a carpenter and the bed provided by the creator: the "essential bed". In *Timaeus* (Plato 1973, 29, a-b) we learn that the *Demiourgos* has created the world in which we live as a copy of another world which is "identical and uniform", thus containing the ideas of everything that we know and experience.

role of the individual and unique, which is indeed neglected by the realist. Yet the problem of incongruence remains. In fact, what nominalists and conceptualists have to offer as alternative to realism is a *subjective view of the universal* (which is only a name for the nominalist and only an idea in the mind for the conceptualist). This is an unsatisfactory solution.

In fact, while reality is individual and unique scientific knowledge of reality aims at giving an account of the universal dimensions of reality. Therefore, nominalism has no true solution to bridge the gap between the uniqueness of concrete reality and the universality which is the aim of scientific knowledge. This is the problem of incongruence between science and reality. At this point, a typical nominalist “remedy” consists in introducing a similarity between individual and universal. Most point to similarities in reality itself, and they use terms like “kinds”, “classes”, “sets” and so on.<sup>40</sup> However, as these similarities remain within the individual character of reality, the problem of the incongruence between science and reality remains.

Van Riessen (1992, 54-55) argues that the missing element in the humanist reflection on this topic is the universal order for reality, which Doyeweerd indicated with the term “law”. I believe this is a correct diagnosis. Because this dimension was not acknowledged by humanist philosophy, the latter was obliged to “locate” the law either in the subject or in the object. The recognition of the structural order for reality, not identified with the subject and the object, represents the correct solution to the problem of incongruence. But this positive suggestion will be discussed in the section (2.6.a) dedicated to the alternative solutions provided by a reformational point of view. In the following section I will continue the discussion on the topic of the universal order from a diagnostic point of view, keeping in mind especially the problem of the search for the elusive universal and the absolutisation of several aspects of our experience.

#### 2.5.d *The elusive universal*

I have indicated that the universal order for creation is the missing dimension in the humanist discussion about the universals. To support this thesis I would like to mention an interesting analysis by Botha (1994). The analysis moves from a reformational belief, namely that although it is possible to distort and misuse creation, it is not possible to silence its “calling” altogether. Botha admits that the role of the created order has been neglected in contemporary philosophy. The debates about a possible independent, universal order for reality constituting a common ground between scientists has mostly been abandoned. Yet on the other side, observes Botha (1994, 20ff.), we are confronted with infinite “turns” in contemporary philosophy of science. In her view these “turns” are more or less associated with the different philosophical

<sup>40</sup> For a detailed historical survey and deep systematic analysis of the discussion concerning natural kinds see Hacking (1991). Hacking presents the discussion for a nominalist point of view. A reply from a

schools. Russell, Whitehead and logical positivism constituted the *logicistic* turn. Wittgenstein inaugurated the *linguistic* turn, which was followed by the *historical* turn. There we find the historical school of Hanson, Kuhn, Feyerabend etc. The historical turn leads to the recognition of the scientific community as the crucial factor in the production and legitimation of scientific truth. It is not surprising therefore, that the following turn was a *sociological* one (Collins etc.).

Botha's point is that although most authors shy away from postulating some accessible independent and conditioning universal order (mostly due to the problems surrounding the traditional view of linguistic or ontological universals and essentialism) the paradox is that in each historical turn some or other aspect of reality or entity is declared ultimate and the final *locus* of order (Botha 1994, 29). It is Botha's conviction that late-modern philosophy has not really eliminated the search for the elusive universal (e.g. Botha 1995, 162). It has tried to ignore it; but the issue seems to re-emerge in unexpected ways.

Botha's indication of an absolutisation of some aspect of reality goes back to Dooyeweerd's view concerning the absolutisation of modal aspects. According to Dooyeweerd (1984, I, 13-61), non-Christian (i.e. "immanence") philosophy, missing an archimedean point outside the horizon of human experience is more or less obliged to absolutise the relative aspects of the created order. This is why immanence philosophy creates continuous "isms" such as materialism, mechanicism, biologism, economicism and so on (Dooyeweerd 1984, I, 46). In this process, the aspects that are not absolutised often become sub-aspects of the one aspect that is chosen as "absolute". Generally speaking, these absolutisations look very sound to their proponents, because of the fact that modal aspects are connected via analogical moments (see fn. 11) to all other modalities. It is therefore "plausible" to connect all possible modal aspects to the absolutised modality and to see the latter as the "root" of all meaning. The problem is that such absolutisations are applicable to whatever aspect one would like to choose. As a consequence, historicism is as plausible as logicism, as biologism or whatever other "ism".

Botha's analysis applies the basic dooyeweerdian insight to the "turns" appearing in the history of contemporary philosophy of science. If the different "turns" are attempts at indicating the changing understanding of a stable order of the world, they remain however prisoners of the "Cartesian either-or" (Botha 1994, 17), namely the attempt at anchoring objective knowledge to the subject or to the object of knowledge. In her opinion neither solution is acceptable. I believe that her suggestions about the role of the universal structural order provide new and liberating perspectives on this issue. I would even want to modify Botha's statement (1994, 27) that a proper alternative consists in "arguing the case for a position between realism and nominalism". It seems to me that the alternative she is proposing is more than a simple "middle-way"

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<sup>40</sup> For a detailed historical survey and deep systematic analysis of the discussion concerning natural kinds see Hacking (1991). Hacking presents the discussion for a nominalist point of view. A reply from a

between realism and nominalism. It rather represents an original alternative which acknowledges the positive sides of nominalism and realism but requires the recognition of another ontological order and moves beyond the realism-nominalism dilemma. In the next section I would like to add a few hypotheses and questions on the basis of the diagnostic perspectives discussed above.

#### 2.5.e *Minor diagnostic observations*

In the light of the previous discussion on the subject-object polarisation it would be interesting to ask whether it is possible to link this polarization to the humanistic ground motive discussed in the introduction (see 1.3.b). After all, the subject-object polarisation appears to correspond rather closely to the nature-freedom polarisation in the humanistic ground motive. In fact, the basic conflict inherent in the humanist ground motive is also a conflict between subject and object. It is a conflict between the autonomy of the human *subject* and the natural world-*object* which threatens human freedom with its ineluctable mechanisms. Therefore, I believe the hypothesis that the cartesian polarization between subject and object stems from the humanist ground motive of freedom and nature is at least plausible.

If this is true, we would be allowed to say that the philosophies linked to the nature-pole of the humanistic ground motive, are more inclined than others to privilege the role of the object. These philosophies would therefore be, generally speaking, more inclined to realism. On the contrary we may say that the philosophies linked to the freedom-pole of the humanistic ground motive, are more inclined than others to privilege the role of the human subject, and therefore are inclined to a nominalist position. Two examples can be offered in this respect. Popper is a moderate realist and in fact (at a ground-motive level) he is busy trying to elaborate a synthesis between nature and freedom (see 3.5.a and Stafleu 1987, 255-256). Rorty on the contrary is a nominalist, and in fact relies on the freedom-pole of the humanistic ground motive. It might be that counter-examples could be found in abundance; all I propose is that the exploration of the link between the two polarisations should be pursued further.

Another interesting hypothesis is that once the choice is made for a certain pole of the basic ground motive, that choice limits the range of the aspects in which the archimedean point is sought. Taking modern humanist philosophy as an example, my suggestion is that once (e.g.) the *locus ordinis* is placed in the subject, the probability increases that certain specific modal aspects (and not others) will be chosen as the archimedean point. Of course once a certain aspect is chosen for that purpose it will be considered as fundamental for the whole reality and for the other aspects. To use a reformational term, it will be "absolutised".

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realist point of view is offered by Boyd (1991).

More explicitly, we can expect that such absolutization will concern those aspects which “qualify” the cultural activities of the human subject, and not the others. For example, we have seen that late-modern relativist philosophy has been inclined to absolutise the logical aspect, then the historical, the linguistic and the social aspect. These are all aspects typically related to human culture and creative freedom. On the contrary positivism, in its inclination to the nature-pole (with its ideal of a scientific control of nature), has often chosen to absolutise especially the mathematical, physical and biotic aspects. These are the aspects that “qualify” the objects of nature.

All this points towards the conclusion that humanist philosophy has not been that “autonomous” in the development of its views. As Vollenhoven (1961) has pointed out, the initial choices in philosophy already restrict the range of the subsequent possible moves that remain permissible from a systematic point of view. Although this is true in principle for all philosophical schools, Dooyeweerd (1984, I, 190-200) has often pointed out that when one’s philosophical elaboration is based on the religious ground motive of humanistic philosophy, it falls prey to certain conflicts and certain dilemmas. In the following section, the further exploration of these philosophical questions will enable us to start providing viable alternatives, or at least suggestions, from a reformational point of view.

## **2.6 Reformational opinions: knowledge and the structural order**

### *2.6.a Recognising the creational order*

Van Riessen (1992, 55) rightly suggests that the solution to the problem of incongruence between science and reality can only be found in the recognition of an order which cannot be fully identified with the subject or the object of knowledge. This is the universal order for creation, the “law”, the structural order for reality. It is this order which constitutes the proper object of study of scientific investigation. Accordingly, the *task* of science is “to describe and explain, with the aid of theories, the regularities and scientific laws that typify phenomena in reality” (Botha 1996, 333). Stafleu (1987, 152) argues in similar vein when saying: “we consider this aim to be the opening up of the law-side of nature, the discovery and development of law-conformity in reality”.

The basic shortcoming of realism has been to suppose that the object of scientific analysis must resemble concrete reality. Therefore the object of research has been “objectified”. This objectification or reification is for example easy to notice in Plato because his ideas become objects in a realm above reality. Aristotle’s reification is perhaps more subtle but it can be argued that the difference between the two solutions is not so radical. Plato’s universals are abstracted as something that does not belong to concrete objects, while Aristotle abstracts something that belongs to the objects. Both solutions, however, presuppose the universal as

existing in a space within or outside the concrete objects. This implies attributing an entity-like character to the universal, a solution that disqualifies concrete reality, which is individual. But science does neither reflect concrete reality nor describe or parallel such reality. The object of science should not be searched in what resembles reality.

On the other hand nominalism has missed a golden opportunity for finding this element which does not resemble reality. The major merit of nominalism is that it restores the importance of concrete, individual reality.<sup>41</sup> In nominalism, in fact, the individual side of reality has precedence over the universal. Unfortunately the nominalist's alternative to the entity-like character of universals is simply a definitely subjective view of the universal. The solution is not satisfactory because it does not solve the incongruence between individual reality and scientific knowledge which aims at the universal.<sup>42</sup>

The recognition of the law, in principle, solves the incongruence between reality and science. Scientific knowledge aims at the universal, at a law which is "valid for", which "obtains for". The universal order for reality is therefore the proper object of study of all scientific disciplines.

#### *2.6.b Knowing the universal and the individual*

Realism has given priority to the knowledge of universality to the detriment of the knowledge of individuality. Nominalism and conceptualism have stressed the knowledge of individuality to the detriment of the knowledge of universals (which do not exist). Reformational philosophy has promoted the view that both individuality and universality are traits of everything that exists and are both knowable (e.g. Hart 1984, 72-83; Taljaard 1976, 80-87). Knowledge of the individuality and knowledge of universality are actually linked. In this philosophical tradition universality and individuality are not regarded as entities but as traits of everything that exists (Taljaard 1976, 172, Hart 1984, 19). Knowledge of the universals is typical of scientific knowledge, while knowledge of individuality is typical of pre-scientific knowledge.

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<sup>41</sup> Despite my rather critical attitude, in this chapter I have tried to underline in several instances that I do recognise that there are indeed "moments of truth" in both realism and nominalism. Concerning realism for example, it must be recognised that whatever exists must meet certain qualifications before we experience it as being of some or other kind. Frogs do not meet the qualifications for being swans. Qualifications of each kind apply universally to the individuals of that kind. Things in this world are not just individuals. On the other hand, concerning nominalism, it must be recognised that our world does not contain any things or entities like universals. We sometimes use words to refer to complex things, events or actions. In our world, however, there are individual things and entities like people, frogs and tables. They have characteristics, functions and traits and should be recognised in our ontology, as the nominalist demands.

<sup>42</sup> Alongside the solutions proposed by realists and nominalists there are also a few attempts at ignoring or evading the problem. According to Van Riessen (1992, 52) Habermas belongs to this group. However there is one modern philosophical school that on the contrary came very close to the recognition of the structural order for reality. Van Riessen (1992, 54) shows deep appreciation for De Saussure and structuralism in general. He writes: "it is really remarkable that in the present-day climate of philosophy

This is an idea that appeared quite early in the circles of reformational philosophy. According to Dooyeweerd (1984, I, 18) for example, science is characterised by the opposition between the logical aspect of our experience and the other aspects that constitute the field of study of a particular discipline. These aspects are abstracted from the cohesive relationship with the other modal aspects and thus become the object (*gegenstand*) of scientific enquiry (Dooyeweerd 1984, I, 38-39). Science therefore, according to Dooyeweerd, is characterised as thinking along modal lines, it requires abstraction and it aims at the universal. As the modal aspects correspond to modal laws, science explores the law and is therefore aimed at the universal and structural order for creation.<sup>43</sup>

A similar perspective is developed by Hart. In his opinion "rational knowing" is about "our understanding of structures, our grasp of general patterns, our insight into laws, kinds and properties" (Hart 1985, 155). Stafleu too distinguishes between theoretical (i.e. "artificial") thought and "natural" thinking (Stafleu 1981, 165). Although he would like to improve on the dooyeweerdian view of theoretical thinking (1981, 167), he acknowledges that "the logical objects of natural thought are concrete things, events and relations" (Stafleu 1981, 166). Theoretical thinking on the contrary is "abstracting thought, by forming concepts it focuses on a limited number of aspects of concrete things" (Stafleu 1981, 167). Without entering into the complex meanderings of this discussion within reformational circles,<sup>44</sup> the previous few examples aim at demonstrating that knowing the universal side of reality is taken seriously by reformational philosophers.

On the other hand, according to Dooyeweerd everyday experience is an experience of individualities. In naive thinking there is no abstraction of a particular aspect of our experience in order to make it a "*gegenstand*" of scientific thought (Dooyeweerd 1984, I, 34). In our naive experience we freely move through the ample variety of modal aspects, without concentrating on a specific one in particular. We nevertheless deal with structures of individuality.

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a philosophical school has risen that deals with the incongruence between science and reality by introducing a third element not located within man himself: that is, structure" (Van Riessen 1992, 54).

<sup>43</sup> This view of Dooyeweerd has undergone serious discussion by several reformational thinkers. For example, Hart (1985, 150 ff.) points to several problems in Dooyeweerd's "*gegenstand*'s" theory. Hart asks whether it is possible to suppose a structural difference between scientific and non-scientific thinking. Along this line of thinking it becomes difficult, according to Hart, to identify the "*gegenstand*" of philosophy. Philosophy is theoretical thinking, and therefore requires the theoretical dissociation of the modal aspects. Yet philosophy is presented by Dooyeweerd as dealing with the cohesion of the modal aspects, and even with the totality. One additional problem, according to Hart, is that Dooyeweerd gives the "*gegenstand*" a modal character in the first volume of his *opus magnum* (Dooyeweerd 1984) but in the following volumes the *gegenstand* seems to be given a structural character. Similar critical arguments are developed by Geertsema (1995) and Strauss (1984).

<sup>44</sup> See for example how Strauss, from an appreciative point of view, has recommended a few alterations for both the ontology of Hart (Strauss 1989, 103- 120) and the epistemology of Stafleu. Strauss (1995, 127-138) deals in particular with Stafleu's idea of theoretical analysis. However, Strauss' remarks are

Knowledge of individualities is therefore considered possible as well, by reformational philosophy (Dooyeweerd 1984, I, 41).

The link between theoretical and naive thinking (on the epistemological level), and the link between universality and individuality (on the ontological level), are to be considered at least as interesting resources that reformational philosophy has to offer as a potential framework to solve issues raised in modern philosophy of science. These resources can be utilised in view of a possible solution to debates that have ravaged Western philosophy for centuries and still continue to divide the philosophical community.

### *2.6.c Accessing the structural order*

Several reformational philosophers have also tried to specify more in details the role of theories, abstraction, logical analysis and the metaphorical use of language. These attempts can also be regarded as accounting for the fact that the structural order is accessible to scientific investigation. Certainly they have not understood this accessibility in a positivist way. The knower is not simply placed into a rather direct and unproblematic contact with an object that can always be adequately represented. Reformational philosophy has taken into account the artificial character of theoretical thinking and the role of the knower in the cognitive act. Yet the object of science has not become inaccessible. In this section we will pay attention to a few authors like Botha, Stafleu and Hart.

Botha (1984, 59-64) calls for attention to the role of linguistic metaphors in constituting scientific theories. In her opinion there are several types of metaphors (e.g. literary or poetic metaphors). The ones that are constitutive of theories function as speculative instruments which provide a hypothetical focus or perspective illuminating an object that is going to be scientifically examined (Botha 1984, 63). Such a metaphor represents a certain expectation concerning the structure of a certain aspect of reality. At the same time it helps shape “a certain frame of vision” (Botha 1984, 63-64). On the basis of this metaphor a theory is elaborated, the aim of which is “cutting the world at its joints”<sup>45</sup> (Botha 1984, 64). The latter is a logical act, but it is supported by a lingual theoretical network. These theory-constitutive metaphors, according to Botha, are based on a “root-metaphor” providing an overall view of reality. In this way Botha recognises the role of religious perspectives in science, while at the same time affirming the role of the language in the accessing of the structural order for reality.

Theories, in her opinion, have the potential of partially disclosing the nature of reality. They do not provide descriptions but metaphors that are nevertheless truthbearing. It is not possible

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important especially for a dialogue within reformational philosophy and are less crucial in view of our present interaction with late-modern humanist philosophy.

<sup>45</sup> On this point, Botha’s argumentation refers back to a previous dialogue between Boyd (1979) and Kuhn (1979).

to approach the structure of reality without linguistic supports like a conceptual frame of reference. Discoveries and new perspectives in science are not possible without metaphors or models. But the structures to which our language is accommodated exist quite independently of our conceptual schemes or theories. We do not construct the world when we adopt linguistic or theoretical frameworks. Botha disagrees on this point with Kuhn who insists that all we can know about the eventual "joints" of the world is our linguistic articulation of those joints (Kuhn 1979, 418).

Botha (1984, 62) argues, on the contrary, that we accommodate our language to the structures of the world. Semantic changes are conditioned by more than semantic realities (Botha 1986b, 85). Recognising these realities is essential and necessary to provide a framework for determining semantic conflation in scientific concept formation. In other words, to be able to recognise when our metaphors become myths (Botha 1986b, 86). Botha (e.g. 1994, 29) claims that in order to escape the relativistic dead-ends of postmodern philosophy of science, a new ontological model is needed. In this context, she appreciates Hart's ontological proposals, which appropriates Vollenhoven's systematic distinctions in a fruitful way. Vollenhoven (1961, 11) has pointed out with particular clarity the distinction between the universality of the law and the universal law-conformity of the entities that are correlated to the law. In order to be the type of individual entities that they are, they must relate to the law in a certain way.

Along this line, Hendrik Hart points to the existence of structural conditions. In fact he claims that the universals themselves constitute such structural conditions. The universals, in his opinion (Hart 1984, 19; 72-73), shouldn't be interpreted as universal "entities" but as structural conditions that provide the basis for the irreducible primary relations in concrete reality (e.g. similarity and difference). As a consequence, they also provide the basis for linguistic predication and nomination.

Individuals, in order to be what they are, must relate to universals in a certain way. This points toward the existence of certain nomic conditions (Hart 1984, 35). Universals and individuals, says Hart (1984, 83), are "both real, mutually irreducible and correlative". They are in fact "traits of the relationship between the nomic conditions (that hold universally for what exists) and the empirical individuals that are subjected to these conditions. Understanding what something is, is grasping in a concept what the (nomic) conditions for its existence are" (Hart 1984, 83). Concepts are therefore linguistic expressions of one's understanding of particular (individual) existents. We recognise them as belonging to a certain class or category on the basis of our own experiential knowledge. These conditions provide the basis for the general modes of experience that grant the possibility of both stability and the possibility of change.

One might still wonder whether the existence of such conditions can be proved. Some might argue that we still have no other access to these conditions than the linguistic conventions of our own culture. Botha's (1986b, 86) reply is that there are several possible avenues to respond to this argument. The first one is a retreat to *commitment*: one claims to believe that such conditions exist. A second possibility is an appeal to *experience*, that can be supported evidentially by research itself (Botha 1986b, 86). The two approaches need not be exclusive. According to Botha the evidences to substantiate such claim should be drawn from various disciplines but also from everyday experience of reality. In the end, all this would require the development of a new ontological model that could accommodate such evidences (Botha 1986b, 86).

I would add the comment that when it comes to nominalist philosophy, the assertion that nomic conditions or universals are simply names, concepts or conventions and do not have ontological status is equally presupposed and certainly not "proved". The two strategies indicated by Botha are not, therefore, specific to a reformational approach. The nominalist approach also has recourse to commitment and looks at possible supports coming from special-scientific investigation. Finally, also the nominalist position must be accommodated by a philosophical system that aims at supporting the nominalist strategy. I personally doubt that one of these approaches might be finally "proved" or disproved. They remain perspectives that are rooted in basic religious convictions.

For example, Van Riessen (1992, 55) says he is surprised that the nominalists, who were so closely associated with the developments of physics, did not consider exploring the idea of a universal order. After all, the knowledge of physics is expressed in laws. Why was this fact not sufficient to suggest the idea of a law-order? Van Riessen considers a few possible answers. "But it seems to me", he concludes, "that recognition of a law or structure (...) would run counter to the subjectivist program, for which reality and the scientific, thinking subject were sufficient and heteronomy was rejected" (Van Riessen 1992, 55). According to Van Riessen, therefore, the most profound reason why nominalism rejected the idea of a law-order, was the necessity to preserve the idea of human autonomy. This required rejecting a law that is external to the human subject and seeking autonomy in the attempt of constituting the human being as creator and law-giver of his own world.

In conclusion, in this chapter I have explored a few problems emerging within late-modern philosophy of science as a consequence of the anchoring of scientific certainty in the subject of knowledge (subjectivism or relativism). This anchoring is problematic because it leads to an "eclipse" of the object of scientific enquiry and the latter is gradually experienced as elusive and inaccessible. Furthermore, I have argued that, in addition to placing the anchor of certainty

in the subject an archimedean point for knowledge is sought within different aspects of the scientific activity itself (the linguistic, the historical or the logical). This leads to arbitrary absolutisations of modal aspects and the same time reveals that the search for the “elusive universal” is still active and no satisfactory solution has been provided yet. A final problem emerging as a consequence of the realism-nominalism dilemma is the problem of the incongruence between science and reality.

In the “diagnostic” phase I have indicated the roots of the difficulties affecting both the subjectivist and the objectivist approaches. I have suggested that these problems are connected to the cartesian division between the subject and the object of knowledge. In this dichotomy, nominalism privileges the knowing subject as the final foundation of knowledge, while realism privileges the object. Both nominalism and realism however, create an incongruence between science and concrete reality by missing the importance of the universal order for reality. However this “missing” universal, I have argued, is still sought via absolutisations of several aspects of reality, especially those aspects that are closely related to the knowing subject (linguistic, logical, historical, social). This strategy is both telling and contradictory. In fact on the one hand the nature of a universal order is either not discussed or taken for granted or not accounted for. On the other hand some or other aspect of reality is declared to be ultimate, is absolutised and becomes the anchor of sound certainty and truth.

In the final part of the chapter I have first of all tried to propose a viable alternative to the realism-nominalism dilemma in relation to the problem of the incongruence between science and reality. On the ontological level I have argued that the structural order for reality is the proper object of scientific investigation and the anchor of scientific certainty. On the epistemological level I have suggested that it is possible to know both the individual and the universal and the creational universal order is accessible to scientific investigation. As a consequence, it is not as elusive as it appears to be in many late-modern philosophies of science.

Having completed my analysis on the theme of the anchor of certitude of scientific investigation, the focus of the next chapter will be on the *subject* of scientific knowledge. In particular, I will deal with the nature and role of (non-scientific) presuppositions in late-modern philosophy of science. The knowing subject enters the postmodern period not as an autonomous and neutral observer, but as holding to presuppositions of various kinds. For some, this constituted another potential threat to the legitimacy of science, in particular to its objectivity.

### 3

## SCIENCE AND PRESUPPOSITIONS

“Scientific education, as we know it today (...) simplifies “science” (...). A person’s religion, for example, or his metaphysics, or his sense of humor must not have the slightest connection with his scientific activity”.

“One does not say: some people believe that the earth moves round the sun while others regard the earth as a hollow sphere. (...) One says: the earth moves round the sun - everything else is sheer idiocy”.

Paul Feyerabend  
(1975, 19 and 301).

### Introduction

The recognition of the role of presuppositions in science is perhaps the most significant feature of the philosophy of science of the 20th century. The two quotations above illustrate such increasing emphasis on presuppositions. In the first one, Feyerabend criticises the view that science should have no connection with religion and metaphysics. This is, in his opinion, a simplistic view of science. The reformational thinker might feel ready to applaud this initiative, but he would probably feel perplexed about the connection suggested in the next phrase, namely the link between science and “sense of humour”. When we move to the second quotation, Feyerabend tells us that there is no scientific truth any more. There are only “views” or beliefs, and they should not be imposed on others. Each individual should be free to endorse his preferred beliefs, accepting that others might disagree. No final scientific “proof” should be allowed to be decisive for our convictions.

The assessment of scientific objectivity certainly does have consequences for the assessment of the legitimacy and authority of the scientific enterprise. The received view insisted on the objectivity of science, which meant the exclusion of all metaphysical elements from scientific research itself (Botha 1988, 40ff.). But the growing awareness of the role played by frameworks, paradigms and interpretation in general, in the long run imposed a much more modest view of the objectivity and rationality of science. By sketching the historical

development of contemporary philosophy of science it will become apparent, I believe, that relativism and loss of confidence about scientific objectivity gradually increased in the late-modern era.

In the first four sections of this chapter (3.1 to 3.4) I will trace aspects of the latter historical developments in some of the most important philosophers of science of the 20<sup>th</sup> century. I will argue that the history of late-modern philosophy of science introduces us to growing emphasis on presuppositions accompanied by a growing relativistic attitude concerning scientific objectivity. The next step (3.5) will be to propose an analysis of the possible causes of this problematic loss of confidence in scientific objectivity. Finally (3.6), some possible resources to counteract the phenomenon will be indicated.

As clarified in the introduction (see 1.6.c) I will use the term *presuppositions* (without other qualifications) to indicate generically all kinds of non-scientific or pre-theoretical assumptions, beliefs, worldviews which might be related to science. Unfortunately different authors often attribute different meanings to terms like these. In Stafleu (1987, 85-86) for example, the term *presupposition* is reserved only for one specific type of *scientific* premise, with a specific function. We have the same problem with terms like axiom, theory, hypothesis: the meaning changes in the work of the different authors. One additional problem is that the border between scientific and pre-scientific is drawn differently by different authors. For example, philosophical (or metaphysical) considerations can be considered as scientific by some and pre-scientific by others. However, I will try to specify the meaning that I attribute to such words, as soon as it becomes necessary.

Of course, it is not my intention to discuss fully the philosophies of science of the numerous authors mentioned in this study. I will limit the exposition to those aspects that are relevant for the present research. By beginning with Popper I would like to explore briefly a philosophical system that is not fully implicated in the relativism of late-modern philosophy and for this reason constitutes a kind of background to more recent (and radical) developments.

### **3.1 Popper: horizons of expectation**

#### *3.1.a Popper and the demarcation problem*

Before exploring Popper's view of presuppositions and their role in science, it might be necessary to ask a preliminary question. Is there a clear boundary, in Popper's opinion, between science and non-science? Is there a so-called demarcation criterion? Is the influence of the pre-scientific upon science a legitimate one, or should it be resisted, as the positivists taught? These questions will also be asked of the other philosophers that we will encounter in this chapter. In fact, when dealing with the issue of the influence of presuppositions on science, it is necessary

to ask where the border between science and non-science is situated. It is also necessary to ask each author what differentiates scientific from non-scientific knowledge.

Popper worked for years on the problem of demarcating science from pseudo-science. In retrospect he realised that the problem of demarcation was the key to develop fundamental ideas of his system (Popper 1963, 42; 52). He did not like the confusion between scientific and non-scientific. The two domains remained separated rather neatly, in his system of thought, especially in the initial years. Yes, he conceded much more to imagination than his positivist predecessors. He actually brought imagination into science, as a legitimate and even necessary ingredient. In fact, in order to falsify theories someone has first of all to propose them (Popper 1963, 52). And to create these theories one needs imagination. The latter precedes rational criticism, and the proposal of theories comes before whatever attempt at falsification.

But for Popper science should not be based on any type of non-scientific presupposition. Most of the times, for Popper, "non-scientific" still means un-scientific or pseudo-scientific, with a negative connotation. There is a sharp demarcation line between presuppositions and science: the critical attitude is what marks the boundary between the two. Science is nourished by criticism, while non-scientific knowledge (although not meaningless) is un-critical. Scientific theories are falsifiable, while non-scientific statements, although not necessarily false, cannot be *proven* false (Popper 1963, 37-39).

This is the crucial difference between astronomy and astrology, for example. Astrological convictions and predictions can always be justified, but never falsified. Science does not begin, specifies Popper, with the abandonment of myth, or with the replacement of myth by something better. Science begins with the introduction of a critical attitude towards all myths (Popper 1963, 127). Science, therefore, should not be based on any non-scientific presupposition, conviction or expectation. Science is based on the strategy of trial and error or, more properly, on the production of "conjectures and refutations"<sup>46</sup> (Popper 1963, 52). Science is based on the possibility of criticism, open and mutual scientific criticism, through which the best (scientific) conjectures can be selected. This is the method of the natural sciences. This is what grants the natural sciences progress and credibility.

However, it is also important to note that Popper's position was modified during the years. It is true that non-falsifiable presuppositions continued to be regarded as unsuitable for scientific purposes. But then he could not simply ignore, for example, the possible relevance of philosophical ideas or metaphysical choices for science. These, he admitted, may be recognised as possible vehicles of important ideas. I agree with Botha (1986, 435ff.), however, that

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<sup>46</sup> In Popper's view "trials and errors" occur at three different levels, and there are clear similarities (with differences) between them. At the biotic level this process concerns the evolution of organisms. At the behavioural level it concerns the adaptation to external conditions through a learning process. At the scientific level it concerns scientific discoveries (Popper 1996, 1-32).

concerning his evaluation of the influence upon science of different kinds of metaphysical or philosophical assumptions, Popper remains ambiguous. On the one hand, he seems to recognise the influence of metaphysics in science. On the other he warns against the “bewitching” effects of metaphysical ideas. In general, he does not provide a criterion to distinguish between the “good” and necessary philosophical guidelines and the “confusing” influence of unwarranted (un-testable) presuppositions (Botha 1986, 436ff.).

Another important question, which needs to be raised when discussing each author’s position, is their views on *which disciplines are really sciences*. Science might be legitimate and credible, but which scientific disciplines does it include (or exclude)? The opinions on this topic changed rapidly in the course of the 20th century and the non-natural scientific disciplines were increasingly admitted into the realm of the “sciences”. But this was not yet the case with Popper. In his reply to Kuhn, he questions exactly the supposed insight that history, psychology or sociology might offer to science.

“To me, the idea of turning for enlightenment (...) to sociology or to psychology (...) is surprising and disappointing. In fact, compared with physics, sociology and psychology are riddled with fashions and with uncontrolled dogmas. (...) how can the regress to these spurious sciences help us? [I hope] you do not want to appeal to the sociological (or psychological or historical) lunatic fringe?” (Popper 1970, 57-58).

Terms like *spurious*, *regress*, *uncontrolled*, and even *lunatic* leave no doubt: for Popper, these subjects are not (strictly speaking) “scientific”.<sup>47</sup> It is not excluded that they might be put on the right track in future (Popper 1963, 37). But for the moment they are riddled with fashions and un-controlled dogmas. Philosophy, although a fascinating field of study, according to Popper (1963, 71-72) does not have problems of its own. Its problems are ultimately always derived from the special sciences. In other words philosophy, if “left alone”, would only debate pseudo-problems.

For Popper one of the worst consequences of the influence of (non-scientific) presuppositions on science is the proliferation of “schools”, movements and sects, which can be observed in the humanities. The natural sciences are relatively protected (as Kuhn himself tends to think) from such a proliferation of competing views. But the humanities show the poor spectacle of

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<sup>47</sup> In chapter 2 I have explained that according to Popper (1961, 144) the natural sciences focus on universal laws while the social sciences presuppose these laws but focus on individual phenomena. This structural difference must be kept in mind to understand Popper’s point of view on this topic. Admittedly, Popper still manages to find a certain unity of all sciences on the level of method. All sciences make deductive causal explanations and test them via predictions (Popper 1961, 130ff.). However, in my opinion, the fundamental dichotomy that Popper introduces at the very beginning of his evaluation of the different sciences remains an obstacle towards the admission of the full scientific

constant internal divisions and conflicts. Yet Popper was not simply a positivist thinker: he also knew that there are some presuppositions in science. In the next section we are going to see which ones.

### 3.1.b When the presuppositions are “shallow”<sup>48</sup>

In a rather direct dialogue with Kuhn, Popper (1970, 51) claims that the whole idea of paradigms influencing the scientific research was anticipated by himself already in 1934, in the preface to his book *The Logic of Scientific Discovery*. Speaking of the “structure of scientific doctrine” Popper claims that he provided an “anticipation of this central point of Kuhn’s”. Popper does not use the term paradigm, of course, but he goes as far as admitting that “we approach everything in the light of a preconceived theory” (Popper 1970, 52). Is Popper admitting that there are presuppositions for the natural sciences? In a certain sense he does.

According to Popper science starts from problems.<sup>49</sup> It does not start from a simple accumulation of impressions, according to what Popper (1979, 341ff.) calls the “bucket theory” of science. It starts from observations related to problems. Observations, admittedly, are also perceptions but they are *planned* and *active* perceptions. They are selective. Behind problems and observations one can still posit a “horizon of expectations”. The latter idea is what Popper considers to be his anticipation of Kuhn’s doctrine. This horizon is the framework which precedes both perception and observation, conferring meaning to both of them (Popper 1979, 345ff.). Expectations, therefore, come before the observations. Here Popper takes a definite distance from the positivist viewpoint.

However, our expectations can be disappointed (Popper 1979, 342). Then observation can test our single expectations and even our horizon of expectations (1979, 344). It can help us to falsify our expectations or hypotheses, so that they can be corrected. As for Popper’s horizon of expectations, it is by no means as demanding as Kuhn’s notion of paradigm: it does not impose itself on generations of scientists, demanding a committed loyalty. It does not resist the advent of a new paradigm. It does not prescribe problems, methods, theories and even instruments. It does not affect the scientist to the extent that the latter can only practice science from “within” a given paradigm.

Yes, we are “caught up” in paradigms, admits Popper, but we are free to leave them at any time. Yes, we then have to enter a new paradigm, grants Popper. But it will then always be “a

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character of the social sciences or humanities, as opposed to the “theoretical” (e.g. 1961, 130, 143) sciences.

<sup>48</sup> About the meaning of “deep” and “shallow” when referring to presuppositions, see 1.6.d

<sup>49</sup> H.G. Stoker, the famous South African reformed thinker seems to agree with Popper on this point and writes: “scientific knowing begins with (...) a problem revealing itself to the scientist” (Stoker 1970, 197). On this issue, a more critical analysis of the popperian line of thought, from a reformational

better and roomier one" (Popper 1970, 56). And we can leave it again at any time. Paradigms can always be critically discussed and compared (1970, 57). The dramatic picture offered by Kuhn is therefore dubbed by Popper as "the myth of the framework" (Popper 1970, 56). This phrase will later become the title of a whole book and of a specific chapter in which Popper (1996, 33-67) elaborates more in detail his criticism of the relativist approach by repeating and enriching theses that he formulated at an earlier stage.

It is quite evident that what Popper has in mind is a rather "shallow" type of presuppositions. One might even wonder whether Popper does not only have in mind the scientific type of presuppositions. In fact, his presuppositions do not seem to belong to the "deep" convictions of the scientist (e.g. worldviews or religious<sup>50</sup> ground motives). In several instances they are called "theories", which indicates something scientific, rather than pre-scientific. They don't look like worldviews, because the latter are not changed so easily, or "tested" so often. Nevertheless, as Popper compares his view with Kuhn's notion of paradigm, it is legitimate to suppose that Popper has in mind a framework that, like Kuhn's paradigm, does not completely exclude pre-scientific moments (cf. 2.3.a).

Does Popper also discuss the influence on science of presuppositions of a deeper level? Unfortunately not, or not openly enough. For Popper there are "extra scientific beliefs (such as a faith in the power of critical discussion) which a scientist need accept" (Popper 1963, 107). Such beliefs grant the progress of science and allow the scientific practice itself. They are non-negotiable and should be held in common by all scientists. But they are not discussed as hypotheses, and to a large extent they remain unquestioned. In this sense, Popper has again something in common with the positivists. He is not always prepared to acknowledge and to "test" his presuppositions.

Popper is more prepared to discuss philosophical issues, although there is a relevant degree of ambiguity in his attitude. On the one hand, as Stafleu (1987, 253) observes, it is clear that Popper has his "metaphysics". For example, he has convictions concerning the importance of the hypothetical-deductive method, about logic, realism, objective truth, the existence of natural

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perspective, would have been desirable. For a critique of this assumption (i.e. science starts from problems) see for example Feyerabend's (1975, 172-176) point of view.

<sup>50</sup> When speaking about *religious* convictions, beliefs or faith, reformational philosophers normally do not refer only to christianity or to the classical religions. It is possible to speak of the *religious* convictions of humanists, atheists, positivists, marxists and all those who "interpret" life in some way or the other (i.e. all human beings). Roy Clouser (1991, 17) observes that in some religions there are no temples, in others no prayers, or no public gatherings, no "holy books" or even no god(s) (in the traditional meaning of the term). He mentions, as an example, the Pythagoreans and reports the text of an ancient "prayer to number ten" (Clouser 1991, 17). On the other hand he observes that "a belief is religious provided that (1) it is a belief in something(s) or other as divine or (2) a belief concerning how humans come to stand in proper relation to the divine". Divine, according to Clouser should be defined as "having the status of not depending on anything else" (Clouser 1991, 22-23). According to the

laws which are inborn but independent of humankind. Popper argues with vigour in favour of his metaphysical convictions. On the other hand Popper often seems to shy away from “metaphysical” issues, as if they might be potentially dangerous or “bewitching”. On this point, the questions asked by Botha (1986, 436-438) are relevant and I agree on the necessity of a second criterion of demarcation, to distinguish between “good and bad” metaphysics.

### 3.1.c *Towards an evaluation*

For a brief preliminary evaluation, I would say that the role of (pre-scientific) presuppositions in science remains largely hidden to Popper. In general, presuppositions represent a threat to the appropriate scientific method, and they are the cause of the unreliability of sciences like history or theology. The existence of different schools of thought within a scientific discipline demonstrates the negative influence of “fashions” and “dogmas” (Popper 1970, 57-58). Where the method of conjectures and refutations is thoroughly applied, the best theories must prevail, and the weaker ones should give way. This view in principle opposes the possibility of “schools” holding to competing presuppositions. The differences of opinion among scientists are reduced to temporary disagreements: it might take some time before theories are tested and falsified. In the ideal context, all scientists hold to the same presuppositions, which are rather shallow. If we consider deeper beliefs, they are not necessarily meaningless but they do not concern science.

Popper (1963, 38) does not deny the fact that theories are often suggested by worldviews and even by myths. The history of the copernican revolution, for example, shows an intricate interplay of pythagorean, platonic and aristotelian beliefs, it reveals the role of the catholic, mechanist and occult world pictures. All these had a tremendous impact on scientific theories or on their rejection. Yet Popper would simply say that in the end the theories were tested and only the best ones survived, not as myths of course, but as scientific theories. Sometimes good scientific ideas can even initially be suggested by “stories” which are not really different from myths. Theories, like myths, are simply invented by humans. But the “scientific myths”, unlike other stories, are critically discussed (Popper 1963, 127), and this makes a world of difference.

By way of conclusion, it can be argued that Popper attributed a rather limited role to presuppositions. One of Popper’s aims in this respect was certainly to maintain to a large extent the legitimacy of science. Yet his position, although far from the relativism of the historical or the sociological<sup>51</sup> schools, represented an important break-through, compared to the positivist point of view. In comparison to previous views, in Popper’s philosophy the role of expectations

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reformational perspective therefore, religion is not an “option” that some choose and others reject, but all human beings are religious beings.

<sup>51</sup> H. Collins (1992) J.R. D. Barnes and B. Bloor (1995) and can be considered as representatives of the sociological school, while differing, however, in the radicality of their approaches.

and frameworks at least *started to* be mentioned in a positive way. They were no longer something that should be simply kept out of science.

The human subject was not seen anymore as a neutral observer collecting facts that speak by themselves. Popper argued that creativity and imagination are essential to propose scientific theories. He placed the human being and his *problems* in the center of science, while truth and objectivity (in comparison to the positivist understanding of science) were now shifted more to the periphery (cf. e.g. Popper 1976, 87-89). With Popper non-scientific does not automatically mean nonsensical or meaningless (1963, 38). In addition, there are beliefs (e.g. the “faith in the power of critical discussion”) that a good scientist needs to accept (Popper 1963, 107). The fact that Popper does not discuss those beliefs at length, does not mean that they are unimportant to him. It rather means that in his opinion those who believe in the importance of science usually accept them in a natural and preliminary way.

Although Popper cannot be called a subjectivist, he moved away from positivism by recognising a more active role to the subject of knowledge. Without his contribution it would have been impossible to elaborate the more relativist approaches which developed at a later stage, in the late-modern philosophy. Polanyi, for example, continued Popper’s journey and reached a more relativist position.

### 3.2 Polanyi on the premises of science<sup>52</sup>

#### 3.2.a Polanyi: the duality in his thought

With the Hungarian philosopher Michael Polanyi, we move a step forward toward the recognition of the important role played by presuppositions in science. I will argue that it is possible to discern two “lines” in Polanyi’s philosophy. The first one is more “conservative”<sup>53</sup> (and I suspect informally related to his catholic background) the second is more inclined towards postmodern<sup>54</sup> thinking (see 4.2.a, b and d). Such a “mixed” approach is recognised by Anastasiou, who analyses Polanyi’s philosophy from a reformational perspective. According to Anastasiou (1979, 104-105) Polanyi’s approach is constituted by an epistemology influenced by christian ideas, while his ontology seems to be more linked to vitalism, or *Lebensphilosophie*. The point of agreement between my analysis and Anastasiou’s one is that

<sup>52</sup> Polanyi spells “premisses” and of course I maintain this spelling in all the direct quotations.

<sup>53</sup> I define this line as “conservative” because it emphasises themes like scientific authority and tradition, the unity of the scientific community, the considerable agreement on basic presuppositions, the confidence in the scientific enterprise (cf. sections 3.2 and 4.2.e).

<sup>54</sup> I define this line as “late-modern” because it is more “tuned” to the developments of late-modern philosophy of science. While relativising the emphasis on authority and tradition, it brings to light with more determination the plurality of frameworks, the difficulties of mutual understanding and dialogue, the fragmentation and dissensus in the scientific community (cf. sections 4.2.a to d). This line is the special focus of *Michael Polanyi’s Postmodern Philosophy* (Gill 2000, 13-90).

we both recognise a kind of double “inspiration” in Polanyi’s thought. I define this duality as conservative and late-modern, Anastasiou defines it as christian and irrationalist. In my opinion it is also possible to observe that the more “conservative” line is present especially at the beginning of Polanyi’s career, while in his later years he reached a position which is more in line with the general direction of contemporary philosophy of science.

In this chapter I will present especially the Polanyi of *Science Faith and Society* (i.e. more influenced by his “conservative” line), while in chapter 4 I will comment more on *Personal Knowledge* (where the two lines of his philosophy are intertwined). I hope to be able, in this way, to illustrate better the “duality” contained in his philosophy and the development of Polanyi’s thought during the twelve years separating the two publications. Such development does not, in my opinion, amount to a radical change of direction. In other words, we will not find a totally different Polanyi in *Personal Knowledge*, which is the focus of chapter 4. Yet the development can enlighten the motives at work in Polanyi’s philosophy and their consequences for the theme of the legitimacy of science.

I need to add one more specification: it is particularly difficult, in Polanyi’s case, to separate his theory of presuppositions from his views about dialogue and communication in science (the topic of chapter 4). In his philosophy the premises of science are always linked to a certain community, holding to a certain tradition and *communicating* its own presuppositions to the next generation of scientists. Therefore, in Polanyi’s case I will not sharply separate the two topics (presuppositions and communication) as I do with the other authors. I will rather discuss the two topics together, both in this chapter and in the section dedicated to Polanyi in chapter 4.

### 3.2.b Demarcation criterion and tacit knowing

It is not immediately clear what creates the demarcation between scientific and non-scientific, for Polanyi. At first sight his approach seems to be more suitable to indicate the continuity between the two, rather than the difference. I’m referring for example to the passages where Polanyi says that the scientist must always start from some kind of pre-scientific interest, knowledge or concern for what he intends to investigate. For example he remarks: “the existence of animals was not discovered by zoologists, nor of plants by botanists” (Polanyi 1958, 139). Yet this continuity does not constitute the whole picture: he also suggests a criterion to distinguish between scientific and pre-scientific.

It is the distinction between focal and subsidiary awareness that informs the demarcation criterion between science and non-science. Scientific study is *focal* and integrates the (pre-scientific) awareness of particulars apprehended in a *subsidiary* way. For Polanyi (1974, 150-151) however, pre-scientific knowledge is not only tacitly bound up with science. It also provides, on its own level, an informal and tacit integration of the parts of an entity that science

may study (Polanyi 1974, 151). In other words, while the formal focusing of science is integrated with a pre-scientific subsidiary awareness, one finds on this subsidiary level further centers of tacit integration.

In Polanyi's works the term *science* still refers primarily to the natural sciences. Philosophy of science is thus especially related to the problems and history of the *natural* sciences, but Polanyi also offers lengthy discussions of mathematics, the humanities and even technology. The humanities acquire more emphasis and focus in Polanyi's view than in Popper's.

Before analyzing Polanyi's view of the role and nature of presuppositions, it will be useful to sketch his general view of tacit knowing. In fact, by exploring this specific aspect of Polanyi's philosophy the reader becomes aware of the constant link established by Polanyi between scientific (focal) and prescientific (subsidiary) knowledge. Every act of knowing consists of two dimensions or components. The implicit (tacit) component remains unnoticed, while the explicit one is consciously perceived and becomes the focus of our attention. A person relies on the tacit component to understand the explicit component (Polanyi 1966, 9-10). When we read a sentence, for example, understanding of the meaning of the single words is tacitly assumed and constitutes a series of clues to the meaning of the whole sentence, which is the focus of our explicit attention.

Polanyi (1966, 3-14) recognises four types of tacit knowing: functional, phenomenal, semantic and ontological.<sup>55</sup> Functional tacit knowing is related to knowing and applying skills. Phenomenal tacit knowing deals with the process of observation. The semantic aspect of tacit knowing deals with meaning or significance and ontological knowing involves understanding a complete entity. Each kind of tacit knowing, therefore, has crucial implications for the scientific activity. When scientists conduct experiments and manipulate instruments, they use skills involving a tacit component. Observation has a tacit component. Trying to find the meaning of experiments and observations implies a tacit dimension. Finally, we can only understand an entity by relying on subsidiary particulars constituting the tacit element of the act of knowing. *In short, all the major steps of the scientific enquiry are constantly tied up with implicit, subsidiary, pre-scientific knowledge.* Given this general starting point of Polanyi's epistemology, we can now move to a more specific analysis of the nature and role of presuppositions.

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<sup>55</sup> Joldersma (1983, 85-87) asks the interesting question whether these four aspects of tacit knowing are separate acts of knowing or whether one act contains all four aspects. Polanyi's exposition seems to allow both interpretations. However Joldersma (1983, 86-87) recommends the second interpretation because it avoids a hierarchical understanding of the four acts, in which the phenomenal and functional types of tacit knowing would become the basis of the other types. According to Joldersma, the second alternative also takes more into account the fundamental role of beliefs in Polanyi's philosophy.

### 3.2.c When the presuppositions create a common ground.

Polanyi is among the first philosophers of science<sup>56</sup> to acknowledge the influence of pre-scientific premises on science (Polanyi 1946, 42ff.). When comparing his work with Popper's, an increased awareness of a broader role played by presuppositions in the scientific enterprise is quite apparent. But for Polanyi this doesn't mean that science must automatically become less credible or less legitimate.

Polanyi (1946, 42) distinguishes two classes of presuppositions: *general* and *particular assumptions*; but the distinction is not very systematic or even precise. In addition he sometimes speaks of "ideals" (e.g. 1946, 71). These three types of presuppositions are pre-scientific in nature, while the "ultimate presuppositions" (Polanyi 1946, 85) belong to the scientific level and will be discussed in 3.2.d. Ultimate presuppositions, according to Polanyi, are the only type of premises that create divisions (though to a limited extent) within the scientific community.

The premises that Polanyi calls "ideals" look mainly like moral values. Science begins when a young student is prompted by a "love of science, and a faith in its great significance" (Polanyi 1946, 55). In joining his master he will "give him freely his admiration and trust". He will be passionately involved in solving problems, while "fighting against self-deception". He must strive "for a true feeling of reality". As he advances in life and in his career, he has to form judgments that are ultimately guided by the ideals of science. This will constitute his guideline in publishing papers, in criticising the texts, in lecturing to students, in selecting candidates for appointments (Polanyi 1946, 55).

The presuppositions of science are closely linked to the practice of science. According to Polanyi it is difficult to describe or discuss them in abstract. Yet in the *concrete* tradition of science, not only scientific knowledge is transmitted but also certain basic "presuppositions" with it. The practice, according to Polanyi (1946, 44), resembles a kind of "Apostolic Succession". Only by recognising the authority of a master, by undergoing apprenticeship and instruction, the young scientist is guided through the mysteries of science. This is the first step constituting the community of scientists.

Now, all the ideals, beliefs and presuppositions indicated up to this point, are to a large extent *shared by all scientists*, irrespective of their faith and other commitments. At this stage of his career, this is an essential element in Polanyi's thought: presuppositions exist and are real, they influence science and even make it possible. But, most importantly, they are a common

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<sup>56</sup> It is possible however to find precursors of this acknowledgment. See for example Duhem (1954) and Koyre (1978). Dooyeweerd too anticipated Polanyi's insight already in 1935, in *De Wijsbegeerte der Wetsidee* (Dutch: The Philosophy of the Idea of Law).

property of all scientists. They do not divide the scientific community. On the contrary, they unite it.

“The knowledge that the same obligations to scientific ideals are generally accepted by all scientists effectively confirms their faith in the reality of these ideals. (...) There exists then a community of consciences jointly rooted in the same ideals recognized by all” (Polanyi 1946, 55-56).

Concerning the problem of communication, the “premises of science, are transmitted to the next generation by a concrete community, upholding the tradition of science and preserving its authority. This means that presuppositions are not to be regarded primarily as a threat to scientific communication and dialogue. The premises must also be communicated and are necessary to communication. The community is constituted by the premises, and at the same time contributes to the transmission and re-elaboration of the premises. The latter, held in common by the scientific community, must also be subscribed to by each individual scientist “by an act of devotion” (Polanyi 1946, 54). These values form “not merely a guide to intuition but also a guide to conscience; they are not merely indicative, but also normative”. For the scientists “it is a spiritual reality which stands over them and compels their allegiance” (Polanyi 1946, 54). Legitimate science, therefore, is characterised by legitimate presuppositions.

This is not yet the complete picture, however. Yes, there are presuppositions which create a common ground. But Polanyi does not ignore that there are also premises which create differences and hamper the dialogue among scientists. They are the ones creating new epochs in the history of science and causing the end of previous eras. It is interesting to observe that, in *Science Faith and Society*, in order to find presuppositions that relativise the unity of the scientific community we have to reach the most “shallow” level of *scientific* models and presuppositions. We are going to analyse these presuppositions in the next section.

### 3.2.d *When the presuppositions create differences*

It is sufficient to look at the history of the natural sciences to see that different models of thought have influenced the natural sciences and have shaped the scientific research in a certain way. Polanyi gives a brief survey of the main models adopted by scientists in different epochs. Among others, Polanyi discusses Copernicus and his pythagorean inspiration, Galileo, and then Faraday. He also deals with Maxwell abandoning the mechanistic model. Among the moderns Mach is considered as particularly important (Polanyi 1946, 85ff.).

Closer analysis shows that these premises may be considered as scientific presuppositions of science. Although Polanyi does not use this distinction, it is clear that he is referring to models (like mechanism, empiricism and so on) that emerged in philosophy and in the special (natural) sciences. These premises belong to a category that is less relevant for the present study (see

1.6.c). Nevertheless, it will appear that for Polanyi they are closely linked to the pre-scientific beliefs that he has explored in advance.

Polanyi admits that some presuppositions create differences among scientists. This raises a question: do such presuppositions not eliminate the common ground that Polanyi considered so important? Do they not constitute a threat to the unity (and therefore to the authority) of the scientific community? Polanyi insists: although the “ultimate presuppositions”<sup>57</sup> adopted by scientists are different, they are “fundamentally based on common ground” (Polanyi 1946, 85).

Although scientists constantly change their theories and improve them, they still accept a great part of the past knowledge as true. Every scientific discovery, in this sense, is not a “revolution”. It does not cancel the previous theories. It rather improves on them, while still recognising their essential validity and truth (Polanyi 1946, 89). According to Polanyi (1946, 90) “the modern premisses of science include a great deal of the old premisses”. In this way, the “apostolic succession” is still guaranteed, science remains a legitimate enterprise, and scientists can still communicate with and respect each other.

In *Science Faith and Society* Polanyi does not ignore the conflicts within science, the schools and currents opposing each other, not only in different epochs but within each epoch (cf. Polanyi 1946, 88). He knows that the models behind science (mechanism, rationalism, empiricism and so on) were not just considered complementary to each other, by their proponents. Yet even the conflicts, according to Polanyi, confirm the fundamental unity of the scientific community at a deeper level, and the possibility of dialogue and communication is therefore not dramatically challenged.

In times of conflict, for example, each party becomes eager to convince the interlocutor. This proves that scientists consider each other as competent. When the dialogue fails, they are ready to accept the judgment of a third party, the arbitration of the scientific community, and to submit to it. This shows the conviction that their fellow scientists are sincere and capable of understanding their point of view (Polanyi 1946, 50-51). It also shows that communication is always possible and necessary.

Nowadays, many would say that Polanyi’s view constitutes a rather idealised picture. The common ground, considered so vast by Polanyi, now seems to have shrunk. The appeal to a third party, when it is issued, does not often solve the disputes, and different schools continue to elaborate their particular views even in the natural sciences. It might be that all scientists love science and believe in it. But to what extent are they prepared to grant legitimacy to the

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<sup>57</sup> The term “ultimate” can have different meanings and be a bit confusing. For example, Collingwood’s (1998) “ultimate presuppositions” (in terms of my scheme in 1.6.d.) refer to the deepest levels of presuppositions. In Polanyi, on the contrary, “ultimate” refers to the most superficial or “shallow” level. It is however only a matter of understanding whether in the scheme of a certain author “ultimate” refers to the upper level (scientific) or to the bottom levels (pre-scientific) of his list of premisses.

views of their opponents? Do they still pursue that truth which, according to Polanyi, is closely associated with freedom and should be the ideal of all scientists? Nowadays, the idea of truth itself is quite problematic. Many have proceeded a long distance from Polanyi's conviction that the truth is one, even when the opinions about truth are many (Polanyi 1958, 315). Besides, what happened in the Soviet Union? Would Polanyi maintain that science under the communist regime was still part of the legitimate tradition of science? To answer these questions we have to go back with Polanyi to the deeper level of pre-scientific beliefs.

### 3.2.e *When the presuppositions are wrong*

Science under the Soviet regime was not always bound to the principles of materialism, and not in all periods. The natural sciences, at a certain stage, enjoyed more freedom than the humanities. Nevertheless, in general, Polanyi (1946, 8) could never share the naive optimism of his Muscovite friend Bucharin. For Polanyi, science in the Soviet Union was conducted under false premises. The pathology was caused by a wrong understanding of freedom, implying in fact the rejection of all authority and the absolute autonomy with respect to the transcendent ideals of science. Such distortion also had its roots in the rejection of tolerance and free discussion. In particular, four specific ideals of science were denied by civil society:

1. that there is such a thing as truth
  2. that all members love it
  3. that they feel obliged and
  4. are in fact capable of pursuing it
- (Polanyi 1946, 71).

Now, says Polanyi, there are nations in which the ideals of science are embedded in a long tradition of civic liberties. In Britain, says Polanyi (1946, 69), there are for example institutions like the House of Parliament, the universities, courts of law and the media. They all support and even enforce the tradition of tolerance and free discussion. "When a child is born to a national community, the Social Contract is imposed on it by force. (...) The premisses of freedom will thus be secured by compulsion" (1946, 71-72).

It could be asked, says Polanyi, "what the grounds are on which we hold the conviction that truth is real, that there is a general love of truth among men and a capacity to find it" (Polanyi 1946, 73-74). For Polanyi, it is "an act of ultimate conviction" (1946, 81). Unfortunately these convictions have lost credibility in our societies. On the other hand it is clear that for Polanyi the christian religion has a fundamental importance for the support of the four beliefs listed above. These beliefs, however, are also supported by the humanist tradition in the West. For example, it was through a revival of Greek thought, in Europe, after the "Dark Ages" that a

society emerged “dedicated to the pursuit of truth by the methods of objectivity and tolerance “ (Polanyi 1946, 74).

However, it is in the humanist tradition itself that a false view of freedom developed, with fatal consequences for science and its legitimacy. Polanyi does not say when exactly the pathology started, but he observes with tact that “both Descartes and Locke maintained their belief in the revealed Christian doctrine” (1946, 75). Some of their successors were deists or atheists, but still “held the conviction that the critical faculties of man (...) could establish the truth of science” (1946, 75).

Then the “extreme empiricists” appeared on the scene. They were convinced that “our main troubles still come from our having not altogether rid ourselves of all traditional beliefs” (Polanyi 1946, 76). These philosophers “set their hopes on further applications of the method of radical scepticism and empiricism” (Polanyi 1946, 76). According to them, freedom means the rejection of all obligations and is incompatible with the prescriptions of any authority. This is a deviation from “the process by which liberal intellectual life was in fact established” in Europe (1946, 76). The method of disbelieving every proposition that cannot be verified, says Polanyi, “would destroy all belief in natural science”. The method indeed “leads to nihilism” (Polanyi 1946, 76).

How then is it possible to continue to speak of one tradition and one community of science? It seems as if there no longer is a common ground. Do we not have two sciences (as Kuyper himself was accused of believing)<sup>58</sup> and two scientific communities? By no means: according to Polanyi the sceptics are still practicing the old tradition, though in the light of a false theory!

“It only shows that people can carry on a great tradition even while professing a philosophy which denies its premisses. For the adherents of a great tradition are largely unaware of their own premisses, which lie deeply embedded in the unconscious foundations of practice. These premisses can therefore remain long immune against their theoretical denial by those practicing and transmitting the tradition” (Polanyi 1946, 76).

This is not intended to downplay the gravity of the situation. These movements justify themselves “by the support of supposedly scientific theories” (1946, 77). But this is not the real

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<sup>58</sup> For example Wolterstorff (1989, 59) claims that Kuyper creates two different types of science, produced by two types of people (regenerated and unregenerated). Kuyper’s “twofold development of science” (as it is called in his *Principles of sacred theology*) is re-named by Wolterstorff (1989, 61) as the idea of “two distinct sciences”, or “two kinds of science” (e.g. 1989, 59). Such an idea, in Wolterstorff’s opinion (1989, 66) is still maintained today by the “Reformational movement” (meaning the section of neo-calvinism more influenced by Vollenhoven – see fn 17). In a similar vein, Klapwijk (1986, 141) quotes Kuyper’s sentence “parallel lines never intersect” to demonstrate that Kuyper acknowledged two systems of science. But in this passage Kuyper (1978, 134) was referring to the “origin” or the “starting points” of the different “systems of science”, not to the systems themselves. As will be observed in 4.6.c one should take into account the different levels of antithesis. Antithesis at the religious level does not automatically mean complete or absolute antithesis at the theoretical or practical level.

source of their strength. These movements “owed their success altogether to their hidden spiritual resources (...) their ultimate reliance and all their love and devotion are then attached to (...) the power of the chosen party” (Polanyi 1946, 78). This faith brings an inevitable regress into society and science. In the long run, it could lead to a denial of the ideals of science themselves.

Presuppositions are therefore essential for science, although their pathological deterioration can lead to the destruction of both science and society. Nevertheless, Polanyi remains hopeful: “totalitarianism has never been fully established in any place” (1946, 82). It is not possible for persons to hold just any belief. Destructive premises will ultimately dissolve themselves. The return to freedom and truth always remains a possibility. Polanyi can already see it: “in Soviet Russia we see pure science once more recognised, literature freed from Marxist interpretation, religion reinstated” (Polanyi 1946, 80).

With this, Polanyi offers an important and new contribution to the discussion: the premises not only do exist but can be positive or negative. This distinction concerning the ethical or religious quality of presuppositions points towards his christian background. Polanyi believes in the solid foundations of truth and freedom. His philosophy is a tremendous reminder that our philosophical views can be designed in accordance with our deepest beliefs, provided we have the dignity to take them seriously...

### *3.2.f Science Faith and Society: towards an evaluation*

If we compare it with Popper’s position, Polanyi’s view represents an important step forward in many respects. In his work the premises of science are taken seriously and explored. It is understood that these premises have an influence on the scientific process. Most of the premises are shared by all scientists: nobody can practice science, for example, without believing in its value. Certain (scientific) presuppositions are shared only by a particular community, or in a certain epoch (e.g. the premises behind classical physics). But all premises are somehow linked to the sphere of belief, they refer to a “spiritual reality” (Polanyi 1946, 54), to a faith and to certain ideals.

These are not the shallow presuppositions of Popper, which can be abandoned at any moment with the help of criticism. In Polanyi’s philosophy scientific premises direct even our criticism, our experiments, our tests (Polanyi 1946, 90-92). All this takes Polanyi away from Popper and closer to Kuhn’s idea of paradigms. But presuppositions in general, in Polanyi’s system, do not threaten science or communication because they are held in common, to a large extent, by all scientists. Polanyi links the presuppositions to the scientific tradition and community, thus granting science the possibility of dialogue and to retain a considerable authority and legitimacy.

Polanyi does not place all his presuppositions into a single “container” (as Kuhn does, at least initially, by grouping them within a paradigm). In Polanyi’s writings (although not precisely classified) one can distinguish different types of premises, with different characteristics and “depth” (see 3.2.c). Polanyi recognises the “ultimate presuppositions” as belonging to the scientific level. They are different for diverse scientific schools. On the contrary, general and specific assumptions (and ideals) belong to the prescientific level and are basically held in common among scientists (see 3.2.c).

In Polanyi, presuppositions reach “deeper” levels (see 1.6.d) than in Kuhn: they point towards the area of personal belief. Polanyi comes very close to the notion of religious ground motive (see 1.3.b), which was indicated as the root of scientific thought in the neo-calvinist tradition. In fact he often mentions concepts like “spiritual reality” and “spiritual resources” (1946, 54), “act of devotion” (1946, 54) “faith” (1946, 15, 55, 76). Another important contribution of Polanyi is the differentiation between positive and negative presuppositions, conditioning the development of science and even society.

Unfortunately, by focusing especially on the common ground between scientists of all epochs and persuasions, Polanyi (this is true especially in *Science Faith and Society*) tends to overlook the ideological plurality of the presuppositions (see 3.5.c). He does not realise fully that the scientific community is divided into numerous schools which do not share the same worldview. It is not possible to impose on this community the ecumenical call *ut unum sint*, as it is done for the community of believers. The tradition, in the scientific world of today, is constituted by a variety of traditions, sometimes by the conflicting traditions of rival schools. There are not only the christian scientist and the “extreme empiricist” who rejects the faith in “traditional beliefs” (Polanyi 1946, 76). The different religious motives and worldviews, create different understandings of science itself, of its ideals, of its aims and possibilities.

The neo-calvinist thinker might agree that certain moral values apply to all scientists and to all schools. He may also agree that the created order remains the same for all. This order will “contradict” all views, theories and attitudes that are not in tune with reality. But the recognition of the differences, of antitheses and conflicts within the scientific community, should be given more relevance than Polanyi concedes in *Science Faith and Society*.

We will see that *In Personal Knowledge* he attempted to acknowledge to a larger extent the consequences of different premises. This will be discussed in chapter 4. For the moment, we can say that in *Science Faith and Society* Polanyi follows a “conservative” line, characterised by a defense of scientific unity, authority and tradition. In order to learn more about the *differences* created by presuppositions, we can turn to Thomas Kuhn. In his work, the radical fractures created by paradigm changes are recognised more openly.

### 3.3 Kuhn: the paradigm

#### 3.3.a *Scientific and non-scientific: the boundary*

In this whole section dedicated to Kuhn I will argue that he continued the process of recognition of the role of presuppositions by introducing the notion of paradigm. This recognition is in itself a positive process, yet it was caught in the fundamental shift of late-modern philosophy towards irrationalism which compromised the process itself. It must be admitted from the beginning that paradigms are not constituted only by pre-scientific presuppositions but by scientific ones as well. Kuhn also tried to avoid the extreme relativism that the emphasis on “frameworks” might have produced. But before paying attention to his view of the role of paradigms we should pay some attention to Kuhn’s criterion of demarcation between science and non-science and to his opinion about the eventual scientific character of the humanities and of the social sciences (3.3.b)

On the problem of demarcation between science and non-science, Kuhn (1970a, 6) differs from Popper and argues that the critical attitude is typical only of the revolutionary moments of scientific research. If we want to define science, we must keep in mind normal science (Kuhn 1970a, 6) and there criticism is not the main ingredient. On the contrary, “it is precisely the abandonment of critical discourse that marks the transition to a science” (1970a, 6). What is essential to normal science, in Kuhn’s opinion, is rather “puzzle-solving”, which is “more fundamental” than Popper’s testing (Kuhn 1970a, 7) and constitutes also the demarcation line between scientific and non-scientific (1970a, 9). The phrase “puzzle-solving” should not be taken as pejorative. Puzzle-solving is an activity aiming at solving scientific problems generated within a certain paradigm. The metaphor of a puzzle appeals to the fact that in normal science both the boundaries of the problems to be pursued and the outcomes of research are delineated in advance (Kuhn 1963, 361-362).

Yet Kuhn’s solution of the demarcation issue seems to privilege the normal phase of science, leaving revolutionary science out of the definition. It is true that he sometimes includes other aspects in his definition, in addition to puzzle-solving. For example he says that Popper’s “testing” does also play a role in scientific revolutions (Kuhn 1970a, 7ff.). In other instances he speaks of both puzzle-solving and *invention* (in dialectical tension) as characterising science (Kuhn 1963, 368-369). But Kuhn is inclined to find the essence of science in normal science and more precisely in puzzle-solving (Kuhn 1970a, 6 ff.). In fact, at least in Kuhn’s initial understanding, science is born as normal science, after abandoning the pre-paradigm period characterised by creativity and proliferation of theories. In addition, normal science is by far the most common occupation of the scientific community and revolutionary moments are quite rare.

The problem with puzzle-solving, as a demarcation criterion, is that it characterises not only science but ordinary life in general. Feyerabend (1970, 200) rightly observes that even a group of criminals might very well be involved in solving “puzzles”, in order to realise their purposes. The same critique, it seems to me, is applicable to Popper’s criterion of demarcation: the critical attitude and the attempts at falsification do not occur only in science but also in parliaments, in business and in everyday life in general.

### 3.2.b Which sciences are scientific?

Kuhn seems to value the contribution of the social and human sciences more than Popper. In particular, history (of science) acquires great importance. As the psychological and social aspects of the scientific community become relevant for Kuhn, it might be argued that this is in itself a recognition that sociology and psychology may help clarifying the mechanisms of the scientific research. The history of (the natural) sciences is the basis of Kuhn’s system of thought. Philosophy also acquires a new legitimacy<sup>59</sup> and theology in his view is not more dogmatic than the natural sciences (Kuhn 1963, 147ff.). According to Van der Merwe, the new perspectives derived from pedagogics, philosophy, history, psychology and sociology constituted the backbone of Kuhn’s system of thought (Van der Merwe 1975, 333-336).

Kuhn reverses a time-honored prejudice concerning the open mindedness and objectivity of the natural scientist as opposed to the dogmatism of the scholar involved in the humanities. In his view, the type of education imposed on students of the natural sciences is likely to cause dogmatism and a narrow-minded propensity for puzzle-solving. The human sciences on the contrary, show a rich variety of paradigms which probably keeps the mentality of the scholar more open (Kuhn 1963, 350-351). All this, as we have seen, could only provoke the irritation of Popper (1970, 57-58).

Kuhn’s revaluation of the humanities is certainly valid to a large extent (cf. Kuhn 2000, 216-223). And yet the natural sciences maintain some kind of “superiority” in Kuhn’s system of thought. For example, the philosophy of science remains for him the philosophy of the natural sciences. At the end of the *Postscript to The Structure*, the natural sciences are repeatedly called “the sciences” while there are disciplines which are indicated simply as “activities” or “fields” (Kuhn 1970, 208-210). The fact that Kuhn (1970b, 144ff.) defines philosophy and the “arts” as “proto-sciences” betrays the conviction that (at least in the present situation) the social sciences are missing something in comparison with “mature science”, or have been left behind in the course of historical development.

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<sup>59</sup> But see the remarks by Van der Merwe (1975, 350-352), who would like Kuhn to pay more attention to the role of philosophy.

### 3.3.c *The Structure of Scientific Revolutions: different paradigms behind science*

In this section we will pay some specific attention to *The Structure of Scientific Revolutions*. In this specific contribution Kuhn elaborates in detail his initial view of paradigms and their impact on science. A presuppositional framework is finally recognised as the cause of substantial differences in the practice of science. However, paradigms create differences between different scientific epochs, not within the same epoch. In fact, every period (of normal science) is dominated by only one paradigm, which also provides the necessary unity of a certain scientific community. In this sense, Kuhn's view is not totally different from Polanyi's: under the same paradigm, a discipline is granted the necessary homogeneity. Its experts can work as a body, can forget about foundational questions and can solve very specialised problems. This is normal science.

In its initial version the concept of paradigm "contained" three basic moments or aspects.<sup>60</sup> One could discern a (1) metaphysical paradigm, pointing to the fact that, in the light of a certain paradigm, reality is seen in a certain way. Then we can discern a (2) sociological paradigm, which points to the scientific *community* that recognises the new paradigm as a valid scientific achievement. Finally we can speak of an (3) artifact or "construct" paradigm. This aspect refers to the fact that the paradigm provides the scientist with a set of problem solutions (exemplars) which function as a new set of tools to conduct his research. Initially, this was the basic constitution of the kuhnian paradigm.

Of course paradigms are not eternal. From time to time they are substituted by new ones. This is due to relevant anomalies which are experienced in a specific discipline. As familiarity with Kuhn's views can be assumed, I will only focus on the aspects of his philosophy relevant for the argument. I would like to underline only one fact: in *The Structure* the successive paradigms contradict each other to a large extent.

This is due to the fact that each new paradigm is supposed to solve problems which were not solvable under the previous paradigm (Kuhn 1970, 92). The old paradigm is not simply modified, it is substituted by a new one which is supposed to provide the means to solve the crisis.

In this sense, Kuhn has seen something important, namely that paradigms (which include some pre-scientific presuppositions) do create differences in the disciplines themselves, and one could even speak (borrowing a term from reformational philosophy) of a certain "antithesis" (see 4.6.a). For Kuhn, in fact, the new paradigm is at least antagonistic to the one which is

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<sup>60</sup> Masterman (1970, 66 ff.) was the first to propose the basic distinction of three aspects in Kuhn initial version of the paradigm concept. Masterman's idea has been basically accepted by Van der Merwe (1975, 343) and Botha (1988, 47).

substituted. This introduces the idea of (theoretical) antithesis, which is a new and important idea provided by Kuhn.

Yet here we can also start to observe a few inconsistencies in Kuhn's doctrine of paradigms. It seems to me that paradigms are sometimes attributed characteristics and functions which conflict with each other. For example: the creation of a radical "antithesis" (after a revolution) between an old and a new "school", would indicate that Kuhn attributes a fundamental role to the paradigms. This is one of the reasons why Kuhn's views obtained special attention from the academic community. In some instances paradigms seem to prescribe the legitimate problems, to indicate the methods, to affect observation. Even the instruments to be used for experiments seem to be "suggested" by the prevailing paradigm.

Yet in other instances paradigms seem to allow large freedom to the scientific community. In this connection Kuhn (1970, 49) suggests that "there can be small revolutions as well as large ones". The paradigm of quantum mechanics has "different applications" (1970, 50) for different groups of physicists:

"It follows that, though a change in quantum-mechanical law will be revolutionary for all of these groups, a change that reflects on only one or another of the paradigm applications of quantum mechanics need be revolutionary only for the members of a particular professional subspecialty. (...) though quantum mechanics is a paradigm for many scientific groups, it is not the same paradigm for them all. It can simultaneously determine several traditions of normal science. A revolution produced within one of these traditions will not necessarily extend to the others as well" (Kuhn 1970, 50).

In the passage above we are told that revolutions can be small or large, and they can change the *applications* of a paradigm rather than the paradigm itself. Now, I believe this is not the kind of influence that the term paradigm still evokes nowadays, when it is mentioned in the broad academic community.<sup>61</sup> Uncertainties like these, I believe, required the subsequent modifications of the paradigm concept. Such modifications were going to appear in the following phase of Kuhn's philosophy. It is necessary at this point to give at least a brief summary of these subsequent developments of Kuhn's view of paradigms.

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<sup>61</sup> For Hans Kung (1988, 123-169) who applies Kuhn's insights to theology, a paradigm is supposed to support the consensus of huge theological traditions. The catholic paradigm or the orthodox one, have created the agreement of millions of church members, for centuries. In the whole history of theology, according to Kung, only five such paradigms have appeared. This is how Kuhn's paradigms are sometimes imagined to work. The reason, as I have tried to show, is that there are precise instances in Kuhn, when this kind of picture is suggested, but others when it is denied.

### 3.3.d Further developments of the paradigm concept

Kuhn provided his first definition of the paradigm concept in 1959 (Kuhn 1977, 229-238)<sup>62</sup>. In its initial formulation the paradigm is a generally accepted problem solution, of the type we find in scientific textbooks. However, by the time the concept was introduced in *The Structure*, it had been broadened to a considerable extent. Masterman's (1970) study showed that Kuhn used the concept in too many different ways. After reflecting on the critical remarks of several colleagues, Kuhn started re-modifying the paradigm concept in 1970a (*Logic of Discovery or Psychology of Research?*) and subsequently introduced the distinction between paradigm and disciplinary matrix. Now "disciplinary matrix" replaced "paradigm" in its broadest sense, while "exemplars" replaced paradigm in its narrowest sense. The disciplinary matrix, as the totality of objects of scientific consensus, contained also the exemplars, to which the term paradigm refers in a more particular sense.

The choice of the word "matrix" suggests that the latter consists of several elements. With no claim to completeness, Kuhn (1974, 463 and 1970, 182-187) mentions at least four components: 1) symbolic generalizations, 2) models, 3) values, and 4) exemplary problem solutions. A short description of these four components will have to suffice. Symbolic generalizations are the formalised universal propositions, regarded as natural laws by a scientific community (or as the fundamental equations of theories). As part of a disciplinary matrix these propositions (unlike in scientific practice) must be read as purely formal, i.e. as divorced from their meanings (Kuhn 1974, 464). In this sense, they are thus artifacts constructed by the philosopher of science.

By models Kuhn means both heuristic models and ontological models. The reason for grouping these two rather heterogeneous moments of consensus under the same definition, lies in the fact that they perform similar functions for the scientific community. They are a source of similarity relations (Kuhn 2000, 13-32). In *The Structure* (1970, 4-5 and 7) basic agreement on ontological issues was deemed very important, but the idea was later "softened" to a considerable extent. The models provide a scientific group with the "preferred analogies" (which are sufficient for the group). The provision of a common ontology is not excluded when the models are "deeply held" (Kuhn 1974, 463-64), but it is not absolutely necessary.

Values (the third component of a disciplinary matrix) are e.g. important for the evaluation of theories and form an important moment of the consensus of a scientific community. Some of these values are: accuracy, consistency, scope, simplicity, fruitfulness (Kuhn 1977, 321-22). Occasionally Kuhn cites also "explanatory power" (Kuhn 1970a, 20) and the capacity of solving as many problems as possible, from a quantitative point of view (Kuhn 1970a, 20-21).

Finally, the exemplary problem solutions are the examples studied by students and showing them how to solve the problems they meet on the basis of previous solutions. They are paradigms in the narrow sense.

Now, these components can be conceived as separable “parts” or elements within a disciplinary matrix. But they can also be conceived as moments of a single and non-separable unity. If the latter solution is accepted, paradigms (in the sense of concrete problem solutions) then emerge as the foundation of the consensus of the scientific community, while rules, laws or theories (conceived apart from concrete application) do not make up such consensus. And in fact exemplary problem solutions (or exemplars) became Kuhn’s new elaboration of the concept of paradigm (Kuhn 1974). Summarising, from the initial paradigm concept Kuhn moved to the disciplinary matrix containing several elements. Then gradually, exemplars substituted the previous elaborations and emerged as the true basis of scientific consensus.

After this overview of Kuhn’s development of the paradigm concept, we can now try to provide some evaluation of the role played by presuppositional frameworks in the philosophy of Kuhn.

### 3.3.e *The weight of paradigms*

Given the many and complex changes introduced by Kuhn in his view of paradigms, it would be very demanding to comment on his philosophy<sup>63</sup> by following its historical development step by step. In addition, such an effort would not be particularly fruitful. I will rather try to individuate a few characteristics of Kuhn’s idea of a paradigm (and its modified versions) that are sufficiently stable through the changes. On this basis, I will try to compare Kuhn’s view with the views of his predecessors, in particular Popper and Polanyi.

My main argument in this respect is that Kuhn, by introducing the paradigm concept, has recognised a broader role and influence of presuppositions. Though the paradigm contains not only pre-scientific presuppositions, Kuhn’s notion of paradigm contained a more elaborate exposition of the role of frameworks in science and far surpassed the influence attributed to presuppositions in both Popper’s and Polanyi’s work.

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<sup>62</sup> I quote this section from *The Essential Tension* (Kuhn 1977), but it was first published with the same title in: *The Third* (1959) University of Utah Research Conference on the Identification of Scientific Talent. Salt Lake City: University of Utah Press.

<sup>63</sup> Kuhn’s philosophy has indeed undergone many changes. According to Newton-Smith (1981, 103) “Kuhn has so modified and oblique [sic] or re-interpreted the position advanced in the first edition of *The Structure of Scientific Revolutions* that it is no longer clear whether a rationalist is committed to denying anything that Kuhn asserts”. The remark is indeed a bit provocative but not without good reasons.

For example, paradigms (but also matrixes and exemplars) while accounting for the consensus of a certain scientific community<sup>64</sup> also make more room for the recognition of dissensus than in the past. For Popper dissensus is basically exercised by dialogue and mutual criticism: in the end the best theories will gain universal consensus. For Polanyi the scientific community is basically united in its fundamental premises, though occasional differences do appear in the surface. For Kuhn the scientific community is united by a paradigm, but as the consensus seems to unite smaller and smaller communities, the expectation of divisions seems to become inevitable.

The scientific community (as Kuhn admits) experiences divisions during revolutions, when new paradigms compete for the control of scientific research. But in addition, the communities holding to a certain paradigm seem to become quite small in Kuhn's view. These communities, therefore, in practice adopt different paradigms and dissensions might certainly arise among these groups. If the conflict seems to be at least conceivable at the intra-disciplinary level, it becomes even more predictable at an inter-disciplinary level, among scholars dedicated to different fields of research. The image of science that Kuhn provides us, therefore, is characterised by more conflicts than Popper or Polanyi would have admitted.

Secondly, paradigms and their subsequent elaborations change the world they constitute or help constituting. Since *The Structure* Kuhn introduced the idea that paradigms are "constitutive of nature" (Kuhn 1970, 110). They are part of that phenomenal world which is accessible to a specific community. Now, neither Popper nor Polanyi would ever have pushed the role of presuppositions thus far. For Polanyi the world is one and the truth is one, although the complexities of both require a great deal of openness and humbleness from the scientist. But for Kuhn paradigms become part of the phenomenal world that we explore scientifically. This is a world that we constitute (see 2.3.b) and because we hold to different paradigms we constitute a plurality of phenomenal worlds. In this sense then, the weight of paradigms and their subsequent elaborations is much heavier than previous forms of presuppositions recognised by several philosophers of science. Once again, this weight (being not balanced by the recognition of the structural order for reality) pushes the philosophical reflection towards a more skeptical and relativist position.

Thirdly, this constitutive element of the world, though not chosen in a completely arbitrary manner,<sup>65</sup> is nevertheless strongly characterised as a construction. Certainly a paradigm is not

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<sup>64</sup> According to Hoyningen-Huene this is the reason why they were first introduced. Paradigms were supposed to account for the consensus existing within a scientific community (Hoyningen-Huene 1993, 132ff.).

<sup>65</sup> Hoyningen-Huene is determined to defend Kuhn against the many who saw in his view of paradigm-change a sort of proof of his irrationalism and relativism. He insists that paradigm-choice was never intended to be an arbitrary choice. Others would maintain that the arbitrary element in that choice was not so irrelevant, especially in Kuhn's initial formulations, and I am inclined to think that there is much

supposed to reflect any world in itself. It might represent to a certain extent the phenomenal world of a certain community, but this is at the same time a world partially constituted by the paradigm. It is true that anomalies can present themselves and lead to the abandonment of a certain paradigm. But there are no knowable joints in the world out there, and our science is at least in part an accommodation of the world to our language (Kuhn 1979, 418). The constructivist element is therefore far stronger in Kuhn's view of presuppositions than in Popper's or Polanyi's. This introduces an element of further weakening of the notion of objectivity.

Fourthly, paradigms and all their subsequent elaborations determine science to an extremely large extent. As they are constitutive of the world, they constitute *science* even more. In *The Structure* paradigms determine both the form and the content of scientific research, the problems, the questions, the methods, the instruments, the solutions! But even exemplars shape and determine the convictions and the results of the scientific community. They are not formal rules, they are rather learned by paradigmatic examples and "ostensions". But their effects are still very similar to those of the initial paradigm: scientific research is determined to a large extent by these frameworks. Exemplars determine the similarity relations that are at the basis of our way of exploring the world (Kuhn 1974, 482). In the previous section we have observed that Kuhn's pronouncements on this topic have not always been consistent. Yet there remains sufficient ground to say that in his view paradigms determine science much more than in the philosophies of his predecessors in the humanist tradition.

All these considerations bring us to the conclusion that in Kuhn's philosophy of science the role of presuppositions in the shaping of scientific research was recognised more than in the past. Admittedly, paradigms are not only constituted by pre-scientific presuppositions and also seem to gradually harbor a more shallow type of presupposition in the later phases of the elaboration of the concept (cf. Van der Merwe 1975, 348 ff.). But those presuppositions are never absent and continue to play a crucial role in determining our understanding of facts, our observations and theories. At the same time, the objects of knowledge seem to play a more limited role. (This theme of the conflict between reality and presuppositions will be explored further in the section dedicated to Feyerabend).

Today it is impossible, as Masterman maintained in the early seventies (1970, 89), to return to a previous era, to pretend that Kuhn's doctrine of paradigms has not made a long-lasting impact on our intellectual environment. Kuhn has taught us that presuppositions have a deep influence on our scientific activities. Paradigms (and their further developments) shape our science and

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truth in the latter view. But for our specific purpose, these differences of opinion among Kuhn's critics can be ignored. In fact, even if Hoyningen-Huene's argument can be granted, the paradigm and its

also create deep differences between scientific schools. Paradigms create the conditions but also constitute the limits of our scientific endeavours. Both theorising and evidence are determined by the paradigm.

The positive contribution of Kuhn's conception of paradigm is that it provided a way to abandon the positivist notion of scientific objectivity. Unfortunately, this positive development was not accompanied by a recognition of the structural order for reality and of the constraints provided by this order. In this respect, one significant constraint is that the archimedean point must not be sought within the horizon of temporal experience. It must rather transcend the modal diversity and be found in *the totality and radical unity* of the latter (Dooyeweerd 1984, I, 99). By transgressing this requirement and by placing the archimedean point in the social and historical aspects of reality, Kuhn has absolutised those aspects and has placed the *locus ordinis* in the human subject, a move paving the way to relativism.

When considering Kuhn's philosophy as a whole and comparing it with the philosophy of Polanyi or other predecessors, we must acknowledge a further shift towards relativism. Kuhn tried to avoid complete relativism, but had to admit at least that his philosophy promotes "partial relativism" (Kuhn 1970b, 264-265). He was aware of the threat of complete anarchism and tried to protect the reliability of science from it. Feyerabend, on the other hand, saw anarchism as a wonderful opportunity, leading towards a new conception of science.

### 3.4 Feyerabend: science and worldview

#### 3.4.a *Scientific and non-scientific: the demarcation*

According to Feyerabend Kuhn's criterion of demarcation between science and non-science is not plausible: puzzle-solving can be fundamental even for a gang of robbers! They surely might have "puzzles" to solve, and might in certain cases even change their strategies in "revolutionary" ways! (Feyerabend 1970, 200). He proposes a new criterion of demarcation: the "interaction between tenacity and proliferation" (1970, 211), the initial formulation of which he attributes to Lakatos. Science implies tenacity in defending older views and theories, and the proliferation of new theories as well. The two ideas are not set in conflict (as in Kuhn) but dialectically united to account for the real practice of science. Feyerabend also avoids dividing science according to the dichotomy proposed by Kuhn's division between normal or revolutionary science. The two categories become two phases within the same science: Feyerabend says that the "philosophical" and the "normal" phases of science co-exist in every segment of research (Feyerabend 1970, 212).

But once again, in my opinion, the proposed criterion does not really tell us what science is, and it does not necessarily exclude non-scientific ideas. Tenacity and proliferation can be

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modified versions basically remain human constructions.

observed e.g. in parliaments and in church life as well. I even suspect that perhaps this was done on purpose by Feyerabend: although he provided some sort of demarcation, in a context of high academic discussion, he did not believe in a real separation between scientific and non-scientific, between science and life (Feyerabend 1975, 19).

What then possesses scientific status? Which disciplines? Feyerabend (apart from a few bitter remarks on theology and philosophy) has no problem in granting scientific status to a large variety of disciplines. But this is not the main issue. It is the demarcation itself, it seems to me, that cannot be allowed permanent residence in his system. "The separation of science and non-science" he writes in one instance, "is not only artificial but also detrimental to the advancement of knowledge" (Feyerabend 1975, 306). He complains that a rigid demarcation is introduced between sciences: physics is distinguished from theology and from philosophy (e.g. 1975, 19). Then science is distinguished from life and from belief. According to Feyerabend it is not understood that science is also a belief, a demanding practice perhaps, but still a belief that cannot be completely justified.

Feyerabend considers science as a *belief* system, while he can also speak of the "scientific content of some myths" (1975, 49, fn. 7). The belief in science should be treated as any other belief. There are other options available: one could prefer to believe in voodoo or some ancient myths. Science should not be compulsory for our education and there should be a "separation of state and science" (Feyerabend 1975, 301), as the state has already separated itself from any specific church (1978, 106). Feyerabend complains, for example that the belief that the earth rotates around the sun is considered as an absolute truth, instead of saying "*some believe* that the earth rotates around the sun"! (Feyerabend 1975, 301).

In his work there remains a sense in which myth and scientific elaborations can be distinguished. Scientific elaborations can be seen as articulations of a basic myth. Some myths are elaborated into all types of theories. Others are never elaborated, although the possibility remains always available. In any case, science is not more rational than other activities. Its supposed objectivity and rationality are unmasked as pretensions the moment we analyse the concrete ways in which science has progressed through history. These ways do not exclude *ad hoc* adjustments, propaganda and so forth. Science is like any other human affair: it progresses irrationally, it needs to resist common sense and to proceed counter-inductively.

In this view, it is possible to compare beliefs with beliefs, and scientific theories with scientific theories. But we can also compare myths with science, or scientific with non-scientific activities! For example, Feyerabend asks (1975, 49-50, esp. fn. 7 and 8), is modern science "superior" to ancient *achievements*? Our trust in science is often based on the conviction that science has achieved incredible practical results. Well, primitive and ancient

activities testify to many achievements as well;<sup>66</sup> sometimes more modest, technically speaking, but much more gratifying from a psychological point of view! (Feyerabend 1975, 306). They satisfied both physical and social needs, which cannot be said of modern science. We should then decide, once again, which ones are preferable. Terms like *desirable* and *preferable* become key-words in Feyerabend's worldview. While the previous authors struggle to decide what is more appropriate in science, or true, or useful, Feyerabend seems to be interested in what provides more pleasure!<sup>67</sup>

#### 3.4.b Towards a "loss of the real"?<sup>68</sup>

In this system of thought, the structural order for reality does not enjoy a very relevant status. While presuppositions are extremely important and are studied deeply, the structural order seems to be dependent on beliefs and theories (Feyerabend 1978, 70). This is not just an extreme invention by Feyerabend.<sup>69</sup> It is the result of a process that has gradually increased the weight and importance of presuppositions. Feyerabend's philosophy seems to be just a radical version of a trend inspired by a growing quest for freedom. In this sense, it is an extremely consequent system. It finally tries to get rid of the remaining ties with reason, objectivity and evidence.

As a consequence, reality seems to have less importance, for example in the evaluation of theories. Kuhn has suggested that theories cannot simply be tested against nature but are normally also compared at the same time with other (rival) theories (Kuhn 1970, 77). Feyerabend (1970, 35-46) takes the idea a step further: theories should only be tested (at least in some specific cases) against alternative theories! The latter can bring into the picture facts that have been neglected by a theory. Then one should decide which theories are more desirable.

In fact, according to Feyerabend facts are interpreted differently on the basis of different convictions. As a consequence, Feyerabend has no difficulty in admitting the presence of numerous "schools" within the same discipline and within the same epoch. One of his objections to Kuhn, for example, is that there must have been at least three paradigms acting

<sup>66</sup> Feyerabend mentions the knowledge of the therapeutic effects of certain plants, the building of the pyramids, Polynesian travels, the astronomy developed in the old Stone Age, rotating agriculture and so on (1975, 306).

<sup>67</sup> In Feyerabend 1970, the title of section 7 is: "a plea for hedonism". The pragmatic undertone which can be observed in both Popper and Kuhn (cf. 5.1.b and 5.3.c), in my opinion becomes in Feyerabend a hedonistic undertone. In his philosophy, what "works" is especially what "satisfies".

<sup>68</sup> The phrase has been devised by Baudrillard (1984, 133).

<sup>69</sup> See for example the discussion, in philosophy of science, concerning the "underdetermination of theories by data". The thesis has a strong and a weak version. In its strong version the thesis claims that for any theory about a given subject matter, there is an incompatible rival theory which explains the available evidence in an equally satisfactory way. In its weak version the thesis claims that there *can be* such a rival and evidentially equivalent theory (cf. Newton-Smith 1981, 40-43).

simultaneously on the scene of physical science after 1860. There were in fact at least three distinct “schools” in this period, and it was certainly not a pre-paradigm period (Feyerabend 1970, 207-8).

Feyerabend is certainly right on this specific point, as in many others, but these insights are inscribed into a system which seems to be more interested in worldviews and scientific beliefs than in reality (Feyerabend 1978, 70). For example his suggestion that science should proceed counter-inductively, against evidence, without indicating the limits of this attitude is a telling example of the reduced importance of real states of affairs. The anarchistic life-view tends also to overwhelm every methodology. The motto *anything goes* becomes the banner under which all methods and non-methods are equally acceptable.<sup>70</sup> To mention a last example, for Feyerabend science progresses by way of propaganda, *ad-hoc* hypotheses, all kinds of adjustments. It is his worldview that imposes such solutions.

Once again, there is a certain amount of wisdom, for example, in the demand for a more free methodology. Reformational thinkers often also complained that certain methods are absolutised in science, while others are unwisely neglected (Venter n.d., ch. 7, Stafleu 1987, 152). This can be clearly detected in the history of philosophy. It is often claimed that a wise use of different but appropriate methods should be recommended and would improve scientific achievements (e.g. Stoker 1970). But in Feyerabend it seems not to be important that the method be appropriate to the *object* of study, or respond to certain criteria at all, and the motto *anything goes* illustrates this position vividly.

Finally, in order to realise that in this philosophical system the role of the structural law-order is indeed neglected, one can also consider the idea of incommensurability. When two systems are incompatible and “suspend” (Feyerabend 1975, 276) their respective basic axioms, there is no recourse to a common ground on which we can solve the conflicts. As the knowledge of the structure for reality is dependent on theories and on subjects, we are condemned to a position of non-communication. The discussion of this point will be continued in chapter 4, which is dedicated to problems of communication.<sup>71</sup>

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<sup>70</sup> Yet note how this attitude becomes totalitarian as well when he says: “the anthropological method is the correct method for studying the structure of science and, for the matter, of any other form of life” (Feyerabend 1975, 252). The plurality of methods cannot simply become an established “truth” in his system. It needs contradiction!

<sup>71</sup> There is a large amount of literature exploring the themes of incommensurability, incomparability, untranslatability. The contributions are so many that it would be impossible to offer a reasonable account of it by selecting just a few titles. One of the central tendencies that seem to emerge from recent studies is a certain resistance against the idea that radical incommensurability may be desirable or even possible. This is the direction taken (e.g.) by Xeng Chiang (2002, 1-21; 2003, 962-974), Hung (2001, 275-292), D’Agostino (2003) and Douwen & De Recht (2002). The same inclination to relativise incommensurability is observable beyond the borders of philosophy, in other research fields. This is, for example, Povinelli’s (2001, 319-35) attitude in anthropology. Carlson (2004, 220-224) and Qizilbash

I think this short exposition gives us a sufficient picture of Feyerabend's system, also in relation to the philosophies of science discussed before. We are also, I hope, in a position to realise the gradual move of these philosophies away from the traditional "standard view", towards an irrationalist position. The continuity between the systems explored above should not be missed. Feyerabend's philosophy can thus be considered a mature expression of themes and views which were followed from Popper's, Polanyi's and Kuhn's views. In the next section, I would like to provide an evaluation of Feyerabend's view of presuppositions.

### 3.4.c Feyerabend, worldviews and myths: an evaluation

With Feyerabend, we finally reach a point of the humanistic reflection in which not only the role of presuppositions is openly admitted, but they can almost be considered "deep" enough (see 1.6.d), even from a reformational point of view. Feyerabend comes very close to the recognition of the religious roots of theoretical thought, with the problem that religion, in his view, means "myth" more than anything else. In addition, in his system of thought religion is not really distinguishable from worldview, ideology or even philosophy. As already observed, in Feyerabend's philosophy science is not only based on belief, but is itself a belief.

Unfortunately, a belief or a worldview is not required to possess certain characteristics in order to be a suitable basis for science. Such characteristics were indicated for example by Kuyper: a life-system must be able to provide certain essential answers, in order to support a certain culture. It must clarify "our relation to God, our relation to man, and our relation to the world" (Kuyper 1978, 16ff.). With Feyerabend *anything goes*: voodoo is as good as islam. Not only can a religious motive form the basis of science, but any myth can. In one of his memorable passages, he says that science might be connected to "a person's religion, for example, or his metaphysics, or his sense of humor" (1975, 19). Although Feyerabend has seen better than others that the root of beliefs is quite deep, he has not explored their nature, their characteristics and functions. In his philosophy there is no difference between well-matured worldviews, supporting entire civilisations (including their scientific activities), and cheaper surrogates, which can support only sub-cultures. All this certainly causes a certain devaluation of science.

It might be asked whether such beliefs and worldviews can be easily changed, as in Popper, or are changed only with great difficulty, as in Kuhn. In this respect Feyerabend assumes a peculiar position. He realises that the deepest beliefs are not changed easily (this is why his fight against conservative worldviews meets such a resistance). Also the anarchist worldview is not changed easily. Is this fact not a hindrance to freedom? For the non-believer (in anarchism),

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(2000, 223-240) seek a shelter against incomparability in ethics. Agassi goes back to Duhem to defend a mild version of incommensurability for the field of aesthetics.

the answer is yes. But anarchism allows (within itself) a huge variety of choices, attitudes, changes of opinion. In other words it preserves freedom. "To be a true Dadaist one must also be an anti-Dadaist" (1975, 189. See also 21, fn. 12). His aims remain stable or change as a result of argument, or of boredom, or of a conversion experience, or to impress a mistress, and so on" (Feyerabend 1975, 189). This brings us to a few more comments on the problems implicit in Feyerabend's worldview.

Although all worldviews are legitimate for Feyerabend, anarchism is superior to all. While many worldviews are legitimate, anarchism is *necessary* for the development of our culture in its totality (Feyerabend 1975, 180). Now, in such a pluralistic and open-minded position, is this not a dogmatic, aristocratic and even authoritarian element? In addition, is it really possible to adopt a position which is a non-position? A worldview which escapes every fixed identity? Is it possible to believe everything, and the contrary of everything? Is it really different from believing nothing? Feyerabend's position seems to be superior because it eludes all positions. But how can it do this? By including in itself all positions, and then by relativising them all, so that it remains possible to choose any position and yet to change it again on a next occasion. In this way, one endorses all views, but none in particular.

Yet it might be asked: is this not just another view, or strategy, among others? Perhaps a different view, unusual, but nothing else than a view alongside the others. This is in fact what Feyerabend's metanarrative is: the dream of a worldview including all worldviews, the dream of a non- or a super-worldview. Yet it remains a worldview, with its likes and dislikes, with its rules and prescriptions. It does not allow absolute freedom and it must oppose each idea that conflicts with its own prescriptions.

Every doctrine, idea or belief that smells conservative must be opposed. Take for example Feyerabend's critique of puritanism, which he considers an ally of modern science (1975, 46). Associated to other forms of "empiricism", puritanism is condemned without appeal. So are many other beliefs and systems of thought condemned on the basis that they clash with the true "humanitarian attitude". Feyerabend's system, therefore, does not include everything and the contrary of everything. It is as prescriptive and restrictive as any other system, with the additional arrogance that it is believed to be superior (while fighting the superiority feelings of everybody else).

If we take a look at more recent times it is not surprising, therefore, that (e.g.) Lyotard (1984) proclaimed the abandonment of all metanarratives (presumably including anarchism) and the birth of a new science, based on local narratives. And yet, I believe Feyerabend's approach has not been completely dismissed. Actually, even Lyotard appears to continue Feyerabend's line of thought when he tells us (e.g.) that the only legitimate science is science without legitimation (or rather self-legitimated science), that the only authorised foundations are those with little

authority (i.e. narratives) and so on. Lyotard's philosophy just takes the irrationalist line that has characterised the late-modern reflection on scientific knowledge a bit further. Even nowadays this line is still alive and in good health.

With this brief comment on Lyotard I have concluded my analysis of several contemporary philosophies of science. In the next section I will propose a "diagnosis" of the causes of the humanist drift towards a de-legitimation of science and towards an increasing skepticism about its authority. The diagnosis and the following proposals (see 3.6) are provided from a reformational point of view. I will attempt to demonstrate that this school of thought can provide relevant resources to understand the reasons of the increasing loss of legitimacy of science in the contemporary philosophical reflection.

### 3.5 Attempting a diagnosis

#### 3.5.a From nature to freedom

Popper himself once said that "scientific knowledge may be regarded as subjectless" (Popper 1970, 57). But in his system the pole of freedom, with its ideal of personality *starts* to impose its demands. A new epoch begins. Rationality is the same for all, but presuppositions are not. In the new dispensation they are therefore discovered as the new foundations of science: no more universal foundations, but rather related to groups and communities. The process is gradual and very slow, it passes through many phases and thinkers. But its general direction is towards subjectivism and irrationalism. The role of reason as universal common ground for science is criticised and reduced. From this point of view, reason is not the main factor leading history and humanity, there are other factors that have been underestimated by rationalists. The rationalist emphasis on the universal is replaced by the irrationalist emphasis on individuality (see fn. 1).

Dooyeweerd's theory of religious motives remains, I believe, one of the most suggestive ways of looking at the developments described up to now. Dooyeweerd could not write very much on the philosophers of science mentioned above (he died in 1977). Yet he provided a framework for the understanding of humanistic thought, including philosophy of science. In his view, humanist thought is, like all theoretical achievements, regulated by a religious ground motive, a spiritual motive directing culture in a humanistic direction.<sup>72</sup>

This humanist religious motive (see Introduction 1.3.b) "contains" two poles: the pole of *nature* and the pole of *freedom*, which are in dialectical tension. The pole of nature is dominated by the ideal of science, implying the scientific control of nature by man. The motive

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<sup>72</sup> A basic explanation of the nature and function of a religious ground motive, as presented by Dooyeweerd, has been provided in the Introduction (cf. 1.3.b).

of nature, aiming at the control of nature, soon came into conflict with the pole of freedom. In fact, control over nature also means control over the human being, who is after all part of nature. Once rigid control is exercised over all nature, there is no more space for human freedom (Dooyeweerd 1979, 152-153). The conflict can be illustrated as follows. The motive of autonomous and creative freedom of the human person is incompatible with the acceptance of an order of nature, constraining human freedom and revealing its heteronomous character. But the motive of nature, promoting the idea of the dominion of nature through science, requires a deterministic and mechanistic conception of the world, which leaves no room for the free and autonomous human subject. In Dooyeweerd's words therefore, "nature is revealed as the relentless enemy of freedom" (Dooyeweerd 1959, 52 translation mine).

It is not possible to find a synthesis or a reconciliation between the two opposite poles of the same humanistic ground motive.<sup>73</sup> The only solution, according to Dooyeweerd, is to attribute the primacy to one of the two and simultaneously depreciate the other. But the depreciated pole, usually, in due time finds a way to regain the primacy and to overcome the opposing pole. The reason is that the world is an integral and coherent reality, which resists the absolutisation of one aspect to the detriment of the others. Declaring one dimension of reality as primary, inevitably leads to a tension in which the other dimensions call for recognition. In sociology for example, initially humanism was directed (by the pole of nature) towards the absolutisation of the method of the natural sciences. When it became clear that this was gradually eliminating human freedom from the scene, the ideal of freedom regained strength and imposed the absolutization of the "historical method" (Dooyeweerd 1979, 207).

Historicism viewed all of reality from the perspective of historical development. Just like human culture, also the planets, plants and animals were products of development. This seemed to suit better and to strengthen the ideal of the free personality, which is closely linked to the freedom pole of the humanistic ground motive, and dialectically opposed to the ideal of science of the nature motive. The ideal of the free personality, previously "defeated" by the ideal of science, gradually started to claim its rights. The mechanistic world picture of the previous phase was abandoned in favour of an organismic world view. The natural scientist used to divide a complex phenomenon into its simplest elements. The new method took its departure from the individual whole, and tried to understand the function of the parts in terms of the whole. In the next section we will observe more in detail the effects of the ideal of freedom on philosophy of science and more in particular on the role of frameworks and presuppositions.

### 3.5.b *Presuppositions and the ideal of freedom*

At this point it is possible to observe the basic conflict within the humanistic ground motive. On the one side we have the control of nature through science. On the other we have the freedom of the personality. The first ideal stresses science, the second one freedom. The first stresses *nature*, the second *man* and his creativity. It is enough to keep in mind these basic facts, in order to approach the problems discussed so far from a new perspective. We have observed a rather short segment, historically speaking, of the development of humanistic philosophy of science. Yet it appears likely to suppose that positivism, which is not analysed in this thesis, was inspired by the theme of the control of nature via natural science. Great importance was attributed to nature itself. The intervention of the human personality was experienced as an intrusion. The role of the scientist was reduced to a minimum and characterised by objectivity and neutrality. Facts were supposed to speak for themselves, while the subjectivity of the scientist (which includes his religious and metaphysical presuppositions) was ruled out of bounds.

Popper's philosophy inaugurated a transitional phase. Popper was still trying to create a synthesis between the two poles of the humanistic motive (cf. Stafleu 1987, 255-256). He was trying to save both scientific control and human creativity, but was still rather ambiguous about the role of presuppositions (see 3.1). With Kuhn, the emphasis falls more directly on human creativity. The importance of history emerges with new force. The observer creates a new world by his own way of seeing. Paradigms, the ultimate foundations of science, become the central notion of a new understanding of science. With Feyerabend we see the triumph of the free personality, unrestrained by any heteronomy: theories and worldviews should simply proliferate.

This strategy is continued among others by Lyotard.<sup>74</sup> Paradoxically, in his philosophy it gradually becomes clear that the freedom offered by small narratives is in reality a new cage. As Kuhn had already shown, paradigms, worldviews and other "foundations", while allowing us to interpret, also determine our interpretation, at least for a period. We are left, so to speak, with "reason within the bounds of a framework"! We can leave a certain paradigm, but we have to enter another one. To the thinker inspired by the motive of freedom, such heteronomy is in sharp contrast with what he hoped to achieve. Therefore it does not come as a big surprise that, after Feyerabend, Lyotard came very close to the idea of a foundation-less science, as free as possible from external constraints.

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<sup>73</sup> A religious dialectics, according to Dooyeweerd, cannot be solved by way of theoretical synthesis. He does not only affirm the idea, but the idea itself is the result of a thorough analysis of Western philosophy.

<sup>74</sup> See fn. 35.

We are now able to provide an answer to the general question concerning the causes of the legitimacy crisis in late-modern philosophy of science. A reformational answer would point to the dialectical tension within the humanistic ground motive of humanistic philosophy. The depreciation of the ideal of science and the hegemony of the ideal of freedom of the autonomous personality demand a new conception of science where objectivity and rationality are gradually downplayed. They demand a science in which presuppositions play a larger role, a science which does not impose its conclusions. But with the radical reduction of objectivity, authority and universal validity, the legitimacy of science is also gradually endangered. In the next section I will propose my evaluations of the analysis of presuppositions presented by the authors that we have consulted in the previous sections.

### 3.5.c *Presuppositions: the fragmentary picture provided by recent philosophy of science*

Concerning the role of presuppositions, one might say that the picture presented by the philosophers of science consulted above is rather fragmentary, not developed in depth, sometimes contradictory. The reformational school proposes the recognition of a wider variety of presuppositions. At the same time it claims that presuppositions of a thinker are expressions of a deeper dimension, within the thinker, than the humanist philosophy of science is prepared to admit. From a reformational point of view, a plausible picture of presuppositions should take into account at least the following factors:

1. There is a *confessional*<sup>75</sup> or *ideological plurality* of presuppositions: e.g. christian, humanist, islamic (and concerning science also positivist, pragmatist and so on).
2. There is also a *typological plurality*: presuppositions are differentiated as (e.g.) worldviews, religious motives, beliefs, world pictures, etc. Each type of presupposition has certain specific characteristics and functions, and also influences scientific thinking in a particular way. Presuppositions have a certain “depth” (1.6.d) and are always related to the religious root of theoretical thinking.

In some recent philosophies of science, as noticed before, presuppositions are rather shallow, in the sense that the point of contact between theoretical thought and the religious or worldviewish root in the human person has not been acknowledged. We are left with shallow hypotheses, “expectations” which can be abandoned at any time (Popper), or with paradigms in which the role of the deepest presuppositions is insufficiently explored (Kuhn). The presence of deeper levels of presuppositions has not been consistently affirmed by Popper and constantly maintained by Kuhn after the initial phases of his philosophy.

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<sup>75</sup> In this case “confessional” indicates that the basis of this type of variety is a plurality of religious commitments. (For the meaning of “religious” see fn. 48).

In addition, late-modern philosophies seem to provide a rather limited understanding of the nature and functions of the presuppositions that they do recognise. The insight is surely limited in the case of Popper, who considered pre-scientific presuppositions as largely irrelevant for scientific thought (cf. 3.1). Kuhn, although elaborating his view of the impact of paradigms on science in greater detail, has sometimes attributed contradicting roles and characteristics to paradigms and their modified versions (cf. 3.3.c).

Some authors were closer to the discovery of the religious root of thinking. Polanyi speaks of beliefs behind our theories, of faith and “spiritual resources”. Feyerabend points to the convictions deriving from worldviews, even though he uses colourful terms like “myth” and so forth. Unfortunately, the demarcation between scientific and pre-scientific is blurred in Feyerabend, so that it is never easy to determine what functions as a prescientific belief and what is scientific. Science itself becomes a belief among others, and there is no possibility of distinguishing clearly between theoretical and pre-theoretical thought. The reason is that Feyerabend does not offer an elaborated theory of presuppositions where the different layers are indicated properly, their nature identified, and their functions distinguished.

Polanyi is perhaps the one that comes closer to a reformational position. He recognises the influence on science of presuppositions and commitments that point towards the religious sphere. He also recognises, although a bit confusedly, a typological plurality of presuppositions (he explicitly mentions *ideals*, *general and particular assumptions* and *premisses*). Although the specific characteristics of each element are not always clear, he seems to be aware of a typological plurality of presuppositions with different functions. Unfortunately Polanyi (especially in *Science Faith and Society*) does not clearly grant their *confessional plurality*, or *diversity* which is an important element to understand the different scientific schools and tendencies.

The typological and confessional plurality of presuppositions, therefore, needs to receive further attention from contemporary philosophy of science (I will sketch a few fundamental reformational suggestions in 3.6.b). Another much needed improvement (and related to the previous argument) would be the recognition of the religious root of theoretical thinking. The humanist tradition has been ready to explore the influences that a large variety of factors have on science. Psychological factors, social, economic, cultural factors have been shown to have a meaningful influence. But the eventual influence of the religious dimension has been carefully kept out of the interests of humanistic philosophy of science. To avoid asking questions about the connection between scientific beliefs and religious beliefs, betrays the presence of a kind of “taboo”. Ironically, this taboo points towards the presence of deep commitments that are religious, in the sense indicated by Clouser (1991, 17-23) and further clarified by Wykstra (n.d., 14).

One would not expect to find this taboo in a tradition proclaiming its openness to critical discussion, to the recognition of the human influence on knowledge, its openness to creative freedom. The humanistic tradition should open itself to the whole human being and to all types of presuppositions. Botha (1994, 27) has expressed the hope that a certain trend within contemporary philosophy of science (the cognitive-historical school of a.o. N.J. Nersessian) might “provide interesting new perspectives (...) in which the role of religious factors is not excluded”. But for the moment this seems to remain just a hope.

In summary, one can recognise fragments of truth in all these systems, but the fragments are often part of a rather confused or even contradictory picture. Sometimes the link between science and the deepest presuppositions is missed. Sometimes the plurality of religious standpoints remains hidden, sometimes the typological plurality and the different roles of presuppositions are neglected. In general, one might conclude that presuppositions, in the authors considered, have been understood only in a limited sense. From a reformational point of view it would be necessary to make more room for the recognition of the confessional and typological plurality, as well as the link between the deepest presuppositions and theoretical thinking. But at this point our diagnosis is confronted with an important question.

#### 3.5.d *Towards more relativism?*

The question is: will this broader recognition of a typological and confessional diversity not lead to even more relativism? In fact, in contemporary philosophy of science, presuppositions appear to have gradually endangered scientific objectivity *even though* they were often attributed a rather “shallow” nature. In the mature expressions of late-modern humanistic thought, reality already seems to be the result of our presuppositions. Would not the recognition of deeper and more diversified presuppositions simply introduce more relativism and delegitimise science to a larger extent? On the other hand, a more optimistic perspective is offered by the realisation that the reformational school was not brought to a relativistic position,<sup>76</sup> *even though* it has recognised the link between science and religion, the deeper levels of presuppositions and the very relevant role they have in science. How was this possible?

This seeming paradox has to do with the acceptance (or non-acceptance) of a structural order for reality. While reformational philosophy accepts the existence of an order regulated by laws, which is not dependent on the human personality, humanist philosophy (especially in its irrationalist versions) tends to erase this order and to consider it just an elaboration of the human subject, a convention accepted by the community, and so on. Therefore, the process of

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<sup>76</sup> In the previous chapter I have defined relativism as placing the locus of order in the human subject of knowledge. The reformational school of philosophy cannot be regarded as a relativist movement because of its emphasis on the “cosmomic idea”, the idea of law as the proper *locus* of order for scientific analysis..

increasingly attributing more importance to presuppositions has been a key factor in relativising the legitimacy of science, but only insofar as it has been caught in the relativism required by the freedom motive. Of course the recognition of the variety and depth of presuppositions contains a positive element which reformational philosophy has supported and endorsed. But when this positive element is divorced from the recognition of the structural order for reality it becomes the key factor for a relativist view of scientific objectivity. This is why we have analysed with attention the process of attributing growing relevance to frameworks and presuppositions of various kinds in the late-modern philosophy of science.

The discussion on this important topic, namely the existence of a creational order, will be continued in the next sections as well (cf. 3.6.d), when we will look for resources to counteract the objectivity crisis. But, as in the analysis of several philosophers we have mentioned the problem of the demarcation criterion, I would like to start the following section by analysing Dooyeweerd's criterion of demarcation between scientific and pre-scientific.

### **3.6 Reformational contributions**

#### *3.6.a Dooyeweerd and the demarcation criterion*

The criteria of demarcation proposed by the authors that we have analysed in this study, do not seem to hold water completely. Popper proposed falsification, Kuhn puzzle-solving (eventually integrated by innovative exploration) and Feyerabend the dialectics of tenacity and proliferation. On their respective views I have reported the criticism the authors provide to each other and I have offered my own comments. Dooyeweerd proposed a more sophisticated criterion, based on the analysis of the structure of theoretical thought. The latter is connected to religious motives, but the boundary between the two is not blurred by this connection. Thinking becomes theoretical when it explores reality according to a certain modal perspective and when it is characterised by logical abstraction.

In pre-scientific thinking we do not focus on one particular modal aspect in our thinking (see 2.6.b). One might say that we concentrate rather on "individual things and concrete events" (Dooyeweerd 1984, I, 41) while we move freely from one modal perspective to the other. For example, while attending a party, in the pre-scientific attitude we do not focus particularly on the economic aspect, or the social aspect, or the linguistic aspect. Our thoughts do not remain within the borders of a given aspect. The latter attitude is rather characteristic of the economist, the sociologist, the linguist.

Hart (1985) and others (see fn. 42) have revised the Dooyeweerdian criterion of demarcation by linking it not to the distinction between modalities and entities, but to the distinction between individuality and universality (both traits are to be found in modalities and in entities – Hart 1985, 155). Hart's solution does not disrupt Dooyeweerd's proposal, but rather brings to

light elements that are already present in Dooyeweerd's philosophy. After all, Hart (1985, 154) reminds us, Dooyeweerd (1984, II, 469) himself said for example that structures of totality may become the object of scientific thought as well. According to Hart therefore, scientific thinking is different from non-scientific thinking mainly because it is characterised by analysis and abstraction and it focuses on the law side of reality.

Whatever specific solution one prefers, the reformational attitude allows all sciences (including the humanities, social sciences etc.) to be recognised as fully scientific. They all study reality according to a certain modal point of view<sup>77</sup>: theology according to the pistic point of view, and biology from a biotic point of view. This allows Dooyeweerd to recognise the scientific status of all academic disciplines, without exception. In this regard he takes a distance from Popper, who recognises the existence of problems rather than the existence of sciences (Popper 1963, 66-67).

For Dooyeweerd the natural sciences are not "more scientific" than the humanities.<sup>78</sup> Of course Dooyeweerd might have had a particular sympathy for philosophy, and when we look at the relationship between sciences perhaps philosophy remains particularly important (Coletto 2002, 15-20). But this is another problem. What is important for the present context is that his demarcation criterion allows a distinction without separating naive and scientific thought, while affirming the full scientific status of all academic disciplines.<sup>79</sup>

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<sup>77</sup> There are however exceptions in Dooyeweerd (1984, II, 55) himself to the "rule" that each science selects only one modal aspect as its own perspective. As Botha (1971, 62) observes, anthropology and sociology are two exceptions. More recently Skillen (1988) observes that politics studies state relationships from more than one point of view, or modal perspective. These exceptions, however, do not abolish the basic intuition of Dooyeweerd that scientific thinking occurs along the lines of modal aspects.

<sup>78</sup> See how Strauss (2001) for example, endorses this attitude and considers the distinction between natural sciences (as "proper" science) and humanities (as "surrogate" sciences) as untenable. Strauss (2001, 25-28, 33-34) revisits several traditional arguments used to suggest the superiority of the natural sciences and concludes that they are in fact incorrect.

<sup>79</sup> The reformational position is much preferable, in my opinion, to the positions emerging in other recent christian movements. The latter often show an unfortunate inclination either toward an over-estimation of theology and its possibilities or to attribute enormous relevance to the natural sciences while devaluating (at least in part) theology. As an example of the over-estimation of theology I would indicate the "vantilian" movement (Coletto 2002, 49-69). In this reformed circle theology is still considered the "queen of sciences" (Frame 1987, 316) and christian scholarship in practice consists of a sort of "theologisation" of all disciplines (Coletto 2002, 58-62). On the opposite front we have several authors who recently have called attention to the possibility of a renewed relationship between theology and (natural) science. For Peacocke (2001), for example, theology should be reshaped according to the recent findings of physics (and to a panentheistic ontology). Van Huyssteen (1998) would like theology to be shaped according to the insights provided by biology, in particular by the theory of evolution. Theology must be founded on an "evolutionary epistemology", because (biological) evolution is the basis of our rationality and rationality is the basis of all sciences (Van Huyssteen 1998, 38). Murphy, who is more positive on the role of theology, nevertheless deems it necessary to test theology against the canons of Lakatos' philosophy, to see if it can be considered "scientific" (Murphy 1990, 51-87 and 174-212). The whole project, which attracts considerable attention internationally, seems not to take into account the possibility of an inner reformation of science (e.g. Geertsema 1995), as advocated for decades in reformational circles. Instead, the movement adopts an "integration strategy" (cf. Sinnema 2001) which

### 3.6.b Presuppositions: a few suggestions on typological plurality

In itself, a better understanding of the nature and functions of different presuppositions would not solve all the problems related to scientific objectivity. It might, however, constitute a preliminary step towards a better understanding of objectivity. Studying the role of presuppositions in science has been crucial for the neo-calvinist reflection on this matter. The realisation that science is shaped by ground motives allowed reformational scholars to propose and elaborate christian perspectives in the arena of the sciences with much more confidence. As a consequence, presuppositions have been an object of intense investigation and their nature and functioning have been subjected to careful examination. In the following paragraphs I would like to mention briefly a few aspects of the reformational reflection that would constitute useful resources towards a better understanding of the nature and role of presuppositions in theories.

The reformational school has suggested that there are different types of presuppositions: worldviews, world pictures, beliefs, religious ground motives, doctrinal convictions, ground ideas and so on. The belief that these elements are constitutive of the scientific reflection was there from the beginning. The background of this idea is certainly to be found in the neo-calvinist belief that all of life is religion, and the latter influences all areas of human existence, including scientific thinking. It was also clear that each element influences theoretical thinking in a different way. The different nature and function of each element has therefore been studied.<sup>80</sup> Of course it is not always easy to determine precisely the nature of (and relationship between) such entities (worldviews, ground motives, etc.). According to Klapwijk (1989, 42) these are delicate pets (he refers to worldviews in particular) that can be killed if grabbed too firmly!

Admittedly, the agreement was not complete on these points, especially on the proper relationship between these elements and on the type of influence that they should have on science (Coletto 2002). Some authors gave more importance to religious motives (Dooyeweerd 1984), others to worldviews (Klapwijk 1987), others to beliefs and commitments (Wolterstorff 1976 and 1989). Some excluded a legitimate or *direct* influence of some of these elements on science. Dooyeweerd, for example, maintained the view that philosophy, as a science, should

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is supposed on the one hand to "christianise" science and on the other to make theology more credible in today's world. For a recent, South African and sympathetic appraisal of such a movement cf. De Lange (2002).

<sup>80</sup> The literature on this topic is so abundant that I will mention only a few contributions. See for example Wolters' (1989, 21ff.) discussion on the understanding of the different roles played by worldviews and religious ground motives, within reformational philosophy. See also Klapwijk's contributions in 1987 and 1989, Olthuis 1989, Botha 1993 and 2002 (esp. pp. 210-216). A short but very interesting contribution on this topic has been provided by Duvenage (1985), see fn. 101.

not be based directly on worldviews or constitute just an elaboration of a specific worldview (Dooyeweerd 1984, I, 124-164, in particular p. 128). Dooyeweerd however, maintained the direct influence of religious ground motives on philosophy and science. A similar view is defended by Clouser who argues that *religious beliefs* shape scientific theorising by “acting as presuppositions” for the theories, and by providing “presupposed perspectives” guiding our theorising (Clouser 1991, 74-107).

But in general there was a definite awareness of and consensus about a typological plurality of pre-scientific presuppositions with different characteristics and functions (e.g. Olthuis 1989). Reformational philosophers did not try to reduce everything to a generic “framework” or paradigm. The plurality of elements was indeed recognised, and although it was not always possible to agree completely on the most appropriate *relationship* among them, there was considerable consensus on the specific characteristics of each element (worldview, beliefs, etc.).

In general it is agreed, for example, that religious ground motives express a deeper dimension (within the human subject) than worldviews or world pictures.<sup>81</sup> World pictures can often be substituted without changing our religious commitment. It is often painful, on the contrary, to modify or even substitute our worldviews, and in the process we might even experience a personal crisis (Olthuis 1989, 33-36). It can be observed that different worldviews can be compatible with the same religious motive, and so on. In other words, reformational philosophy has recognised a typological plurality of presuppositions and has seriously tried to recognise their specific characteristics (e.g. Olthuis 1989, 26-38), their different functions (e.g. Klapwijk 1989, 46-55) and their respective significance for scientific thought.

### 3.6.c *Presuppositions: a few suggestions on confessional plurality*

Reformational philosophy has also recognised the (confessional) plurality of religious standpoints, as a consequence of the inner link between religion and theoretical thinking.<sup>82</sup> I have already tried to illustrate (see 1.6.c and 1.6.d) the link between presuppositions and the deepest levels of personal and communal commitment. I have tried to capture in a schematic way, what reformational philosophy has been debating for decades. In my own “summary” different presuppositions originate at different levels of reflection that may be more or less

<sup>81</sup> For the difference between world picture and worldview see Venter (1991, 130). World picture is a representation of the origin and structure of the world, its creatures and even social relationships. A scientific world picture is e.g. the Newtonian representation of the world, with an emphasis on the physical, astronomical universe. On the other hand “worldview” refers to a total vision of life, implying certain categorical distinctions, a basic and practical orientation in life, commitment and a total vision.

<sup>82</sup> The reformational literature on this essential theme is too broad to be quoted. On the historical roots of the debate see Dooyeweerd (1984, vol. I). For a general and not too technical introduction to the topic see

“deep”. The deeper presuppositions constitute a kind of necessary “basis” for the elaboration of presuppositions of a more shallow character. In this scheme, however, it is not necessary to imagine any hierarchic type of relationship, as if the presuppositions could be like the rings of a chain (i.e. each ring being related only to the previous and to the following ring).

Instead, I have proposed (Coletto 2002, 114-116) a model in which the recognition of different “levels” (or “rings”) in a certain sequence, is accompanied by the recognition of a reciprocal and complex interaction, like in a web. For example, concerning the sequence of levels, we are allowed to say that a certain (scientific or pre-scientific) presupposition is placed (e.g.) on the 4th level. But we should also recognise that such presupposition may influence (or be influenced by) level 6 or 10, or level 2. It is not necessary to postulate the “mediation” of the intermediary levels and the influence is often reciprocal, though it occurs according to certain limits and according to the specific characteristics of each presupposition (see fn. 15).

Dooyeweerd’s (1984, I, 3-67) transcendental critique of theoretical thinking was a decisive step in the recognition of the confessional plurality of presuppositions. His critique allows to maintain that a plurality of religious motives are the basic pre-theoretical starting points of philosophy and culture in general. They influence theoretical thinking by providing a triplex fundamental idea, which Dooyeweerd (1984, I, 68) calls a transcendental or ground idea. The first “side” of this idea provides an answer to the question concerning the *origin* of meaning. The second side deals with the *unity* of meaning and the third one deals with the relation of *coherence and diversity* between the different aspects of created reality (Dooyeweerd 1984, I, 93-102).

The three questions are inter-related. According to Dooyeweerd (1984, I, 69), the acceptance of a unique Origin of all meaning (or of two original principles opposed to each other) determines whether one accepts or not (see second question) the integral unity of meaning at the root of the modal aspects. And the answer given to this second question determines how one understands the mutual relation and coherence of meaning of the modal aspects (first question). All this influences philosophical thinking. However, the special sciences rely on philosophy for their theoretical conception of reality and for their method of forming concepts and problems. The specific meaning of analogical concepts is different between different sciences because their modal points of view differ and their basic ground idea differs. It is therefore clear that the triad of transcendental ideas, on the basis of a religious ground motive, influences both philosophy and the special sciences.

However, the recognition of the fundamental role of presuppositions, of their typological and confessional diversity, is only one side of the coin, only one part of the therapy that

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e.g. Clouser (1991, 74 ff.). On the influence of religious commitments on biology see Strauss (2001b). For the influence on mathematics, psychology, physics and other disciplines see Clouser (1991, 111 ff.).

Reformational philosophy wants to propose. In the following section I'm going to discuss the other side of the issue: the importance of recognising the structural order for creation.

#### *3.6.d Recovering the real world*

I am not simply suggesting that a better understanding of the nature and role of presuppositions would be sufficient, in itself, to heal the widespread philosophical skepticism concerning the objectivity of science. The recognition of the existence of creational structures is equally relevant and even crucial for this purpose. Reformational philosophers have never abandoned the idea that, apart from our frameworks, there must be some type of real world out there: a reality which is not changed by our views. And as we human beings are part of this created reality, the latter must be accessible to our enquiries, to a considerable extent. Here we meet the fundamental theme of creation, so characteristic of the reformational approach.

The created order is not simply constructed by the subject. It cannot be “boxed” into just any view or theory. It is not just what we want it to be. But at the same time it is accessible to us. This is the fundamental insight that can be suggested by reformational thought in this context, a theme that I have started developing and clarifying in chapter 2. One of the consequences of this basic insight is that our theories do not have the power of shaping or determining our scientific observations of reality completely. Reality is to a certain extent independent from our theories and frameworks. Stafleu offers a good summary of this important theme when he says:

“Some logical-empiricists thought (...) that the observational results, the “sense data”, are completely independent of any theory. But this view seems to be untenable. Some modern philosophers tend to adhere to the other extreme by assuming that any observational result is completely determined by a theory. An intermediate position seems to be more in accord with scientific practice. We argued that observational results can only be acquired in a theoretical context, but nevertheless have a certain autonomy (...) They can be transferred from one theory to another one. If this were not the case theories could not be tested and competing theories could not be compared” (Stafleu 1987, 125).

Accepting the existence of a universal created order has another important consequence: science does not develop primarily in accordance with our own frameworks, but in accordance with the clues provided by the created order. In this context Stafleu (1987, 151-57) suggests an explanatory model. This model will be discussed in more detail a bit later, in the chapter dedicated to scientific progress (cf. 5.6.b). For the moment I will only briefly mention the fact that such a model tries to account for the empirically discovered natural order (cf. Stafleu 1987, 157).

In this model, Stafleu identifies three fundamental “axes” of scientific research. They are linked to ontological distinctions such as the theory of modal aspects, the distinction universal-

typical and the distinction between law side and the subject side of reality (see fn. 5). These axes of research allow the fourfold scientific search for objectivity, application, universality and structure. This shows that scientific research does not proceed in a chaotic manner, but rather follows a natural order. In the past few decades, several philosophers have argued that science develops under the guidance of a paradigm (Kuhn) a research programme (Lakatos 1970, 91-196) or a research tradition (Laudan 1977, 70-120). They have shown the intimate relation between a worldview (or framework) and scientific work. But, as Stafleu observes: “none of them has recognized the natural order of these various directions of scientific research” (Stafleu 1987, 157).

Now, these suggestions by Stafleu can probably be criticised and improved. It is not my intention to defend them as the only possible solution to the problems we have been discussing in the present chapter. Yet Stafleu’s suggestions are important, especially for the direction they indicate. They try to supply something which is definitely missing in the most recent philosophies of science. Indeed, in the late-modern humanist reflection there seems to be a shortcoming in the recognition of creational structures (cf. 1.3.c, the notes on chapter 2).

The reformational tradition recognises to a larger extent the reality of both presuppositions and conditioning structures. It does so, not by searching a fine balance but by placing them into a new context. This context is the biblical view of the world, in which the human subject is a religious being and the world is a creation including man himself. In this perspective, the different subjects and objects are not autonomous but are subjected to a “Law” or laws. The law does not constitute an inhibition of the freedom of the subject but rather constitutes the possibility itself of this freedom.

We have already discussed the importance and necessity of acknowledging the creation order in the first chapter. Unfortunately, the late-modern philosophy of science has very often been nominalist and subjectivist, and therefore has been inclined to deny any order external to the subject. This has caused several anomalies. In particular, the denial of an order external to the subject has gradually contributed to placing the *locus ordinis* within the subject himself. In this way the presuppositions of the knowing subject, his theories and views, have increasingly acquired an extraordinary importance to the detriment of another fundamental epistemic factor: the universal order for creation.

In conclusion, we can say that late-modern philosophy did something positive when rejecting the notion of objectivity promoted under the “received view” of science. Objectivity should not be understood as correspondence to the facts. Theories are underdetermined by facts and facts are selected in a way that makes them at least partially theory-laden. But then objectivity cannot be interpreted as correspondence to the knowing subject either. *The notion of objectivity that should be defended is correspondence to the law, or law-conformity.* According to Stafleu

(1987, 241) it was precisely this type of respectful “submission” to the law that guided for (example) Kepler to the acknowledgment of laws that contradicted all hypotheses conceived up to 1600.<sup>83</sup> And it is this type of search for objectivity that can allow us to avoid the relativistic tendencies of postmodern philosophy of science.

In conclusion, in this chapter I have explored the role of the pre-scientific presuppositions of scientific research. I have argued that the objectivity crisis in late-modern humanist philosophy of science is characterised by a growing emphasis on the role of frameworks, premises, paradigms. Such emphasis has positive sides and has counteracted the positivist view that science should get rid of non-scientific presuppositions. However, under the dominance of the freedom motive, this emphasis on presuppositions results in a denial of the structural order for reality and scientific research is made too dependent on the preferred paradigms and worldviews. Such presuppositions then, are believed to determine science and to dictate problems, methods and conclusions.

In the diagnostic part of this chapter Dooyeweerd’s concept of religious ground motives was introduced to interpret the crisis of humanist philosophy of science and the swing (from positivism) towards relativism and subjectivism. While positivism was inspired by the theme of the control of nature via natural science, late-modern philosophy of science was inspired by the ideal of the free human personality. Under the control of this new motive, the presence and the role of various types of presuppositions were recognised as a fundamental component of scientific research. Unfortunately, this positive approach was not balanced by the recognition of law structures conditioning and constraining the knowing subject.

In the last part of this chapter I suggested an alternative to counteract the unilateral emphasis on presuppositions. The recognition of a created order provides the needed external anchorage for scientific knowledge. The admission that presuppositions are not all in science and we are constrained by a created order partially independent of the knower, counteracts relativism and provides a more balanced picture of the scientific analysis. In principle, the recognition of various levels of presuppositions (even “deeper” than the ones we encounter in late-modern philosophy) is not necessarily to be considered a threat to scientific objectivity. But it is inappropriate to emphasise the role of presuppositions to the detriment of the structural order that the different sciences are supposed to investigate.

In the next chapter we are going to explore one of the consequences of this emphasis on presuppositions. In particular, we will study the effects of this emphasis on scientific dialogue and communication.

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<sup>83</sup> For an assessment of the relevance of the “Reformed view of the law” for modern science see Stafleu 1987, 239ff.

## 4

# THE DIFFICULTIES OF COMMUNICATION

“A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die”.

Thomas Kuhn (1970, 151)

### Introduction

In the previous chapters I have outlined the difficulties of humanist philosophy of science in identifying the object of study of science (ch. 2) and in believing that it can be studied with a sufficient degree of objectivity (ch. 3). In the 20<sup>th</sup> century, not only in philosophy but also in everyday life, the impression that it is not so easy to understand the real world has become common. The knowledge which in previous centuries was considered innate, evident or immediately accessible, now seemed to become rather a process, even a slow and problematic process.

The feeling is widely illustrated in English and international literature. I have in mind, for example the protagonist of Joseph Conrad's novel *Heart of Darkness*.<sup>84</sup> The English sailor travelling by boat on the Congo river is suddenly attacked by the natives. Initially he realises only that “sticks, little sticks were flying about, thick: they were whizzing before my nose”. He needs time before realising: “Arrows, by Jove!” Now, if knowing and perceiving become laborious processes, how difficult will it be to *communicate* to others what we can barely access ourselves?

The problem of communication, as a theme, is linked to the elusive character of the object of study and to the role of presuppositions, in the philosophy of science of the 20th century. Humanist science started with considerable confidence in the possibility of communication.

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<sup>84</sup> Conrad 1989. The following episode is narrated on page 85.

The works of Galileo often contained words like *Dialogo* or *Discorso* in their titles.<sup>85</sup> Communication was a precious possibility, often taken for granted both in philosophy and in science. It was one of the elements that granted credibility and legitimacy to science. How did it happen that even its possibility is questioned in the most recent philosophies of science?

In the first part of this chapter (4.1 to 4.4) I analyse the gradual move towards skepticism concerning the issue of scientific communication in contemporary philosophy of science. After a survey and description of this gradual process I try to analyse (4.5) the possible causes of this loss of confidence. The humanist ground motive (especially its ideal of freedom) and its incompatibility with the idea of a creational order is indicated as the cause of this gradual loss of confidence in the possibility of scientific communication. In the final section (4.6), I indicate the possible resources that might help counteracting the present tendency towards relativism and pessimism.

First, I will sketch a basic reformational approach to dialogue and antithesis and I will argue that (on the theoretical level) complete incommensurability does not occur. Secondly I will defend the thesis that concepts are only relatively dependent on theories. Furthermore, I will argue that theories are only partially dependent on paradigms or frameworks. The purpose of this final discussion is to counteract the idea that it is not possible to compare and critically discuss rival theories and concepts.

#### **4.1 Popper: the dialogue must be possible**

##### *4.1.a Legitimacy and communication*

In Popper's philosophy criticism is an essential ingredient. Criticism, together with the proposal of new theories, constitute the essence of science. The method of trial and error, used also by living organisms for their survival, is refined in science as the method of conjectures and refutations (Popper 1963, 52). This method implies (or even takes for granted) the possibility of effective communication between scientists.

If science is characterised by the testing of our hypotheses, by the proposal and critical evaluation of theories, it must be possible to communicate even among scientists of different persuasion. Theologians might not be able to test their ideas, says Popper (to Kuhn), but in science (and in science only) can we criticise our points of view (Popper 1970, 57). Part of the credibility and authority of the scientific enterprise rests on its ability to communicate. The possibility of communication is mostly just taken for granted in Popper's work. Of course he

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<sup>85</sup> For example his *Dialogue concerning the two chief world systems* (1632). See also the *Dialogue concerning two new sciences* (1638), and *Discourses on the new sciences* (1638). Also in the title of his *Sidereus nuncius* the idea of communication is pre-eminent.

discussed the philosophies of several skeptics and irrationalists, but for Popper the negative consequences (for science) of their positions spoke for themselves.

On the other hand, Popper did not simply appropriate the positivist approach. The positivists had been looking for a kind of neutral language, a scientific language capable of eliminating the problems implicit in ordinary language (the latter in fact harboured religious and philosophical notions). Along the same line, they tended to identify verifiability with meaningfulness: linguistic statements that are non-verifiable do not have meaning. But Popper softened this hard-line: statements and theories can be non-scientific without necessarily being “meaningless, or nonsensical” (Popper 1963, 38-39).

However, it is only with the appearance of the works of Kuhn that the issue of incommensurability is faced with particular attention (and with a certain irritation) by Popper. To him, the whole issue boiled down to “transforming a difficulty into an impossibility” (Popper 1970, 56-57).

#### 4.1.b *The issues at stake*

In the discussion of incommensurability Popper accepts the metaphor of translation. Is it not true, he asks, that concepts and ideas can always be translated into English, for example from Chinese or Hopi? (Popper 1970, 56).

But he realises here that there is a more fundamental issue at stake, namely the “myth of the framework”: the conviction that ideas can be debated only if certain basic assumptions are shared. If theories and ideas can be conceived and designed only within certain frameworks (he refers explicitly to Kuhn’s paradigms) then it becomes plausible to think that our theories are understandable only when a certain framework is shared (Popper 1970, 56). Those who don’t know or don’t share the framework, cannot be told about the theories and the facts generated by the framework. Popper has no doubt that such an approach is wrong. It is a form of relativism, actually the central bulwark of relativism (1970, 56).

Popper, on the contrary, is not a relativist. Theories must be understandable by the whole scientific community, unless we decide to abandon (at least in principle) the whole system of falsifications, tests, conjectures and refutations, even the whole tradition of critical discussion. Therefore he declares that not only particular theories should be compared and criticised, but even our sacrosanct “frameworks”, which are often placed beyond the reach of critical discourse (Popper 1970, 57). In the dialogue between Popper and the historical school in philosophy of science, we are therefore confronted with two rival views of scientific *dialogue*. For Popper the dialogue is always possible while for the historical school it is not. These differences have important implications for the assessment of the legitimacy and authority of science. Should we have to admit that scientists holding to different frameworks do not

understand each other, it would not be possible to avoid the doubts on the excellence of scientific knowledge. And Popper was not prepared to move even a single step in that direction.

There is another issue at stake: the idea of progress. The idea that scientific dialogue must be possible (Popper 1970, 57) supports a positive view of scientific progress. In Popper's view there remains considerable common ground between the different successive theories. The new theories are always better than the falsified ones. They constitute an improvement, they have more explicative power and can predict better. Yet the gap between old and new theories remains relative. The new theory cannot be said to be "true" while the old one is "false". They are both footsteps in the way towards the approximation of truth. Also the gap between different generations of scientists remains relative. We are still capable of appreciating the theories of Newton, Galileo or Kepler. We can fully understand their points of view. All this means that progress is possible if communication is possible. Popper's view of communication still contained much of the confidence of the "received view".<sup>86</sup> Yet the times were changing rapidly, and part of the change was due to the contributions of Michael Polanyi.

## 4.2 Polanyi on scientific communication

### 4.2.a *The Polanyi of Personal Knowledge*

In the previous chapter I analysed Polanyi's view of presuppositions and communication especially as they are presented in *Science Faith and Society* (see 3.2). In this chapter I will explore the same topic (both presuppositions and communication) as it is presented in *Personal Knowledge*, which was written 12 years later.

In the previous chapter (see 3.2.a) I argued that Polanyi's philosophy presents two lines, which I defined as a conservative and a late-modern line.<sup>87</sup> Having illustrated in the previous chapter the influence of the conservative line in *Science Faith and Society*, I will now focus on the second or late-modern line. The latter is especially present in *Personal Knowledge*. In his systematic survey of Polanyi's philosophy, Anastasiou (1979) emphasises especially (what I have called) the late-modern line" in Polanyi's philosophy of science. The Polanyi of *Science Faith and Society* is left a bit more in the shadow. This happens especially when Anastasiou analyses issues like the role of "frameworks", pre-scientific knowledge and communication

<sup>86</sup> For an introduction to the "received view" of scientific theories see Suppe 1974, 6-118. A shorter introduction is provided by Botha (1988, 35-43).

<sup>87</sup> In the previous chapter (see fn. 51) I have defined the "conservative line" as emphasising themes like scientific authority and tradition, the unity of the scientific community, the considerable agreement on basic presuppositions among scientists, the confidence in the scientific enterprise (cf. 4.2.e). The "late-modern line" (see fn. 52) is more "tuned" to the developments of late-modern philosophy of science. While relativising the emphasis on scientific authority and tradition, in this line Polanyi is more prepared to recognise that different basic presuppositions divide the scientific community. He also admits more openly the difficulties of mutual understanding and dialogue and the reality of fragmentation and dissensus in the scientific community (cf. sections 4.2.b and d).

between different schools (cf. Anastasiou 1979, 35ff. and 63ff.). In these cases, the new developments are more or less regarded by Anastasiou as *the* position of the Hungarian scholar.

I tend to consider these new elements as developments which do not cancel the previous convictions but are rather *united* to them in a kind of synthesis. In fact, I believe in *Personal Knowledge* the two “lines” co-exist in the same text. In my opinion they are to a certain extent in conflict, yet they are united in a synthesis. The second line is the dominant one in *Personal Knowledge*. But in this same text there are also clear reminiscences of (and even returns to) the conservative line.

The purpose of the following two sections is to analyse especially the second line in connection with Polanyi’s view of presuppositions and scientific communication. My aim will be to show that in the second line Polanyi’s views are more inclined to a certain relativism than in the first line. One of the consequences is that the role of presuppositions becomes much more pronounced while the possibility of scientific dialogue<sup>88</sup> is regarded with more skepticism. Yet Polanyi does not abandon completely his “conservative” line of thought. He rather tries to combine the two lines in a position which represents (when compared with Popper’s one) a step further towards irrationalism. In 4.2.f I will try to assess what the consequences of this approach are for the legitimacy of the scientific enterprise.

#### 4.2.b Developing a “late-modern” line: frameworks and presuppositions

The irrationalist/vitalist line emphasised by Anastasiou (1979, 35ff. and 63ff.) is certainly present in *Personal Knowledge*. Concerning communication, this line presses Polanyi into a position which is not far from Kuhn’s. Following Polanyi’s new approach we learn that the frameworks behind science can indeed be very different. Instead of creating a common ground they create controversies (see e.g. the ones on fermentation or hypnotism) which are *only apparently* centered “on questions of factual evidence” (Polanyi 1958, 167). In fact, looking at these disputes more closely, it becomes clear that the two sides do not accept the same “facts” as facts, or the same “evidence” as evidence. “For within two different conceptual frameworks the same range of experience takes the shape of different facts and different evidence” (Polanyi 1958, 167).

According to this new line of thought, frameworks do not necessarily create consensus, even when they are shared. In addition, frameworks are radically different in different societies and in different historical epochs. “Even people whose conception of the nature of things otherwise coincide”, says Polanyi (1958, 240), “may be fundamentally divided in respect to the reality of certain facts”. The antagonists on either side of a great scientific controversy do not accept the

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<sup>88</sup> As anticipated in section 3.2, for Polanyi I am not going to separate the discussion of presuppositions from the discussion of communication. The latter will be discussed in 4.2.d

same facts as real. This is also true on the social level. A society believing in magic will agree on a whole system of facts which modern man regards as fictitious. And “similar logical gaps can be found (...) in different periods of European history”, says Polanyi (1958, 240).

The attitude is quite different from the one pervading *Science Faith and Society*. Now scientific thinking seems to be “locked up” within a certain framework. It is difficult to criticise our own frameworks because they all have an “epicyclical structure” supplying subsidiary explanations in difficult situations (1958, 291). According to Polanyi:

“They have power to control our thought. They speak to us and convince us, and it is precisely in their power over our minds that we recognize their justification and their claim to universal acceptance” (Polanyi 1958, 265).

“We must now recognize belief once more as the source of all knowledge. Tacit assent and intellectual passions, the sharing of an idiom and a cultural heritage, affiliation to a like-minded community: such are the impulses which shape our vision of the nature of things (...). No intelligence, however critical or original can operate outside such a fiduciary framework” (Polanyi 1958, 266).

It is not possible, says Polanyi, to “prove” that our basic beliefs are reasonable, or logical, or even acceptable. While the acceptance of a basic framework is the condition for having any knowledge, the framework itself can claim no self-evidence. Our basic beliefs are indubitable only in the sense that we believe them to be so. Our liberation from objectivism, says Polanyi, is therefore to realize “that we can voice our ultimate convictions only from within our convictions” (Polanyi 1958, 267).

In *Personal Knowledge* the premises of science also become rather indefinite, inarticulable, constantly changing within history and more elusive. Perhaps every particular discovery, says Polanyi, was born of different premises. Therefore, in order to identify the premises of science, we would need an extensive research. It would be necessary to examine at least all the great discoveries of the 20<sup>th</sup> century. “But even so, such an account would only reveal the premisses of past scientific achievements. The actual premisses of science (...) are present only in the yet unformed discoveries” (Polanyi 1958, 165).

#### 4.2.c Interlude: what type of presuppositions?

The hypothesis might be advanced that perhaps in these statements Polanyi refers only to the “ultimate premisses” that he mentions in *Science Faith and Society* (p. 85 ff.). We have already noticed (see 3.2.d) that those premisses should be classified as scientific, and Polanyi admitted that they in fact are the ones responsible for divisions within the scientific community. If this hypothesis is true, then the Polanyi of 1958 may not be different at all from the Polanyi of 1946 and the distinction of two lines would be somehow weakened.

A few observations are however needed. First, even if the hypothesis is taken to be true, the fact that Polanyi now insists much more on the divisions among the scientific community shows a change of attitude. In fact, in *Science Faith and Society* the discussion was about the fact that even the “ultimate premisses” are based on common ground (1946, 85 ff.). The change of emphasis is significant in itself.

Secondly, it is not so certain that Polanyi has in mind only scientific presuppositions. On the contrary, the way he describes them does not confirm that impression. The references to “beliefs” (Polanyi 1958, 266), “passions” (266), “convictions” (267), “fiduciary frameworks” (266), the idea that it is almost impossible to justify them and even identify them (265), the example of “believing in magic” (240), point rather towards the pre-scientific type of presuppositions.

Thirdly, it is quite possible that here Polanyi has in mind both types of presuppositions, scientific and pre-scientific, without distinction. It would nevertheless mean, in this case, that (unlike in *Science Faith and Society*) now Polanyi would not consider the pre-scientific premises (included in the discussion) as a common ground for scientists of different persuasions. Those premises would now be regarded as similar to the “ultimate presuppositions”, i.e. as being different and creating different positions among scientists. And this would of course constitute a considerable change of attitude, in comparison with *Science Faith and Society*.

In the previous paragraphs I have tried to sketch briefly the new developments emerging in *Personal Knowledge*. However, Polanyi does not simply abandon his “conservative” line, which was analysed in some detail in the previous chapter and concerns especially his *Science Faith and Society*.

#### 4.2.d Developing a “late-modern” line: the possibility of dialogue

All this has immediate consequences for the possibility of scientific dialogue: the latter cannot be taken for granted any more. According to Polanyi, formal operations relying on one framework of interpretation cannot demonstrate a proposition to a person who relies on another framework. Those who support a certain framework may not even succeed in being understood by the supporters of a different one, because “they must first teach them a new language” (Polanyi 1958, 151). And even the new “language” cannot be learned without a previous “decision” (by the audience) to accept the new hypothesis with sympathy. It is not just a matter of proving something scientifically or rationally.

“Proponents of a new system can convince their audience only by first winning their sympathy for a doctrine they have not yet grasped. (...) Such an acceptance is (...) to this extent a conversion” (Polanyi 1958, 151).

Such “conversions” divide the community of the scientists. Polanyi uses the language of religious experience when speaking about “*disciples* forming a school, the members of which are separated, for the time being, by a logical gap from those outside it. They think differently, speak a different language, live in a different world” (Polanyi 1958, 151-2, italics mine).

Even the authority of science, so important in Polanyi’s conservative line, now seems to be subjected to more specifications:

“Whenever I submit to a current consensus I inevitably modify its teaching: for I submit to what I myself think it teaches and by joining the consensus on these terms I affect its content” (Polanyi 1958, 208).

“I do not enter this commitment unconditionally (...) I refuse to follow (...) the objectivist ideal in psychology and sociology. I accept the existing scientific opinion as a *competent* authority, but not as a *supreme* authority” (1958, 164).

Generally speaking, in his “second line” Polanyi introduces considerably more skepticism and relativism towards the possibility of consensus. In other words, the (prescientific) frameworks are more differentiated and control our research to a larger extent. Apart from creating a common ground they also create controversies and cause facts to be seen differently. It is almost impossible to evaluate critically our frameworks and we can only pre-suppose them. Communication, as a consequence, is more difficult: it is like learning a new language. The eventual dialogue normally does not convince the interlocutor, unless he accepts the new language with sympathy.

#### 4.2.e Moving back to the “conservative line”

The rather extensive quotations of the previous section had the purpose of substantiating the view that there are indeed new developments in *Personal Knowledge*. Yet the more “traditional” line is not simply eliminated. I will try to substantiate this claim before attempting a more general evaluation of Polanyi’s contribution.

Let us start from the idea that although there are many frameworks, they are not all true. Depending on the particular framework adopted, the formal rules of scientific procedure will be interpreted quite differently. The same will apply to the conception of the nature of things by which the scientist is guided. But “his chances for reaching true and important conclusions will depend decisively on the correctness and penetration of these conceptions” (Polanyi 1958, 167). In other words, according to the well-known sentence by Polanyi: “though every person may believe something different to be true, there is only one truth” (Polanyi 1958, 315).

In addition, there is much in science which is shared:

“science is not established by the acceptance of a formula, but is part of our mental life, *shared* out for cultivation among many thousands of specialized scientists throughout the world, and *shared* receptively, at second hand, by many millions. (...) the reason for which we too *share* in this mental life must necessarily be given as part of this life” (Polanyi 1958, 171; italics mine).

When comparing the domains of the arts, the humanities or religion with the practice of science, it is evident that “in all these domains there is much greater divergence of views than there is between scientists” (Polanyi 1958, 220). But even in these domains we can recognise a unity within each particular culture and tradition. The adherents of one persuasion may refuse to recognise the intellectual merit of adherents of a rival persuasion. Yet the fact that most rival leaders share the same status in a pluralistic society, demonstrates a measure of consensus in according some intellectual merit to most of them. This implies the acknowledgment of “standards which though manifestly disparate, are descended from a common inheritance of values and beliefs” (Polanyi 1958, 222).

If this communication and agreement are possible even “between different philosophies, religious or artistic movements”, they are even broader among scientific schools. In his view, “rival schools of thought, which are essential to a vigorous cultivation of modern art, in science are *infrequent and transitory*” (Polanyi 1958, 220; italics mine). The idea of a scientific community (parallel to the church) called to ecumenical unity tends to re-emerge, and it is certainly a telling detail that the division among scientific schools is repeatedly called “schism” (Polanyi 1958, 157 and 159). As a consequence, the “dialogue” between different periods of science is not so problematic:

“The giants of the past, if they returned today, might not readily accept relativity and quantum mechanics (...) yet so much of earlier science has remained true (...) that the pioneers of science have kept growing through the centuries in our respect” (Polanyi 1958, 165).

Communication, as a possibility, remains available, notwithstanding the differences of frameworks. “I believe” says Polanyi (1958, 205), “that even though people may conceivably misunderstand any particular words addressed to them, they can, as a rule, convey information to each other reliably enough by speech”. The reason for this belief, according to Polanyi, is that in the process of denotation, the tacit judgments involved tend to coincide between different people. In addition, different people also tend to find the same set of symbols manageable for the purpose of reorganising their knowledge (Polanyi 1958, 205).

However, the possibility of dialogue is not simply founded on linguistic skills. It is founded on common ideals, shared by all. The idea that scientists share the good old tradition of science

even while verbally denying its principles (which was proposed in *Science Faith and Society*-p. 76) re-emerges again in *Personal Knowledge*:

“Men may go on talking the language of positivism, pragmatism and naturalism for many years, yet continue to respect the principles of truth and morality which their vocabulary anxiously ignores” (Polanyi 1958, 233).

In addition, the importance of the community and tradition, and of submission to authority is strongly re-affirmed. A certain organismic model of thought seems to re-emerge, where the whole is more important than the parts and entails “the existence of a value that is absent from the constituent particulars” (Polanyi 1958, 327). One is brought to remember his view (in *Science Faith and Society*<sup>89</sup>) that freedom should be imposed on individuals by certain institutions, an idea which follows an old suggestion of Rousseau (1988, 195), one of the outstanding representatives of the organismic tradition of thinking.<sup>90</sup>

Coming back to our main topic, i.e. communication, I agree with Rutledge (2003, 23), when he says that there are instances in Polanyi’s works, where he seems to affirm that dialogue between rival views is merely a matter of “conquer or die”. In other words, we would only communicate with those who hold our own position. But in his philosophy there is also another line of thought, which allows for dialogue and mutual understanding to a larger extent.

#### 4.2.f Polanyi’s contribution: towards an assessment

I have tried to demonstrate that one can find something like a “dual line” in Polanyi’s philosophy, and in particular in *Personal Knowledge*. Within the “conservative” line certain notions prevail (tradition, authority, community, the uniqueness of truth). This line also seems to create the sense of balance and measure which permeates Polanyi’s system in general: the personal fiduciary act (Polanyi 1958, 274, 294) is balanced by a claim to universal validity. The importance of the framework in science is balanced by the recognition of real facts (but on this point see my critical remarks in 2.2.c). The admission of (superficial) conflicts is accompanied by the recognition of the fundamental unity of the scientific community.

Yet one can notice a second line, influenced by the developments of the humanistic philosophy of science of the time. These developments cause a pressure on Polanyi’s

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<sup>89</sup> For example in England, according to Polanyi, institutions like the House of Parliament, the universities, the courts of law, the media, support and even enforce the tradition of tolerance and free discussion. “When a child is born to a national community, the Social Contract is imposed on it by force. (...) The premisses of freedom will thus be secured by compulsion” (Polanyi 1946, 71-72).

<sup>90</sup> Rousseau says that “whoever refuses to obey the general will shall be compelled to do so by the whole body (...) he will be *forced to be free*” (1988, 195, italics mine). For an introduction to and historical overview on the organismic way of thinking (and a comparison with mechanistic thinking) see Venter (1992).

conceptions and pull his philosophy towards a more open relativism. This is why, I believe, in some instances Polanyi is tempted to absolutise the role of the framework, to admit the complete fragmentation of science into rival schools, to renounce the possibility of real communication, to foresee a bleak future. From there, we also have a movement of "return" to the more conservative line of thought, and all this creates a certain oscillation, a dialectic within his system of thought.

Probably the relationship between the two lines could be interpreted in many different ways. What one could consider a series of contradictions, can be regarded as a dialectical unity by a more sympathetic reader. There can be a different answer to the question whether the two lines integrate each other, constitute an unresolved tension, or are even necessary to each other.

Yet it seems to me that at least a few basic *hypotheses* can be proposed following the dooyeweerdian idea of ground motives. In this perspective one may argue that in *Personal Knowledge* Polanyi has moved beyond Popper's attempt of a synthesis<sup>91</sup> between the poles of nature and freedom of the humanistic religious motive. Polanyi gradually prefers the freedom ideal. Hence the necessity of overcoming objectivism because it denies the role of the human personality in science. According to Anastasiou's (1979, 104-105) analysis, Polanyi's philosophy contains an ontology which shows traces of *Lebensphilosophie* and vitalism, while his epistemology discloses a christian orientation. While basically agreeing with this conclusion, I would like to add that in the development of his philosophy Polanyi also shows a tendency to move gradually with more determination towards a subjectivist type of position, relying on the freedom motive. His later works are therefore more oriented to the freedom motive.

In general, we can say that Polanyi provides a system which is more open than Popper's to the recognition of irrational factors shaping science. Obviously his conservative line of thought opposes this tendency. When the conservative line prevails we are told that the common ground of presuppositions and the unity of the scientific community create the ideal conditions for communication. But when we consider the "late-modern" developments of his thought, we can notice that scientific communication and dialogue are threatened by the adoption of different frameworks, by the fragmentation of science in rival schools, by the strength of theories and the weakness of evidence. All this had consequences, of course, for the assessment of the authority and credibility of science.

The philosophers that we are going to analyse in the following sections welcomed the "second line" of Polanyi's philosophy in their own systems of thought and continued to uphold the ideal of freedom. Unfortunately, this resulted in the further reduction of the legitimacy and credibility of science. In the following section I will show that the same tendency is present in

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<sup>91</sup> Popper's attempted synthesis is illustrated by Stafleu (1987, 255-56)

Kuhn, who was a student of Polanyi and explored the important notion of incommensurability. I will begin with an assessment of his views of communication in *The Structure of Scientific Revolutions*.

### 4.3 Kuhn and incommensurability

#### 4.3.a Radical views in *The Structure of Scientific Revolutions*

Compared to Polanyi, the possibility of communication is more problematic for Kuhn, especially between scientists holding to different paradigms. In *The Structure*, the prevailing paradigm provides on the one side the common ground for communication between adherents of the same paradigm. But on the other side it makes it almost impossible (at least considering some of Kuhn's statements) to communicate with scientists who do not accept the same paradigm. Fortunately, according to Kuhn, only one paradigm can dominate a certain field of study in a certain period. This preserves a considerable degree of communication. The problem of communication rather arises in times of crisis, in transitional periods, when revolutions divide the scientific community between an old and a new generation.

Kuhn indicates clearly both the presence of paradigms and the consequences for communication. For example he says that two scientific schools "will inevitably talk through each other when debating the relative merits of their respective paradigms" (1970, 109). The new paradigm must be incompatible with the previous one because it promises to solve the anomalies that caused the crisis of the old paradigm, and it also causes different predictions (Kuhn 1970, 97). One of the most striking examples of incommensurability, according to Kuhn, is the famous debate between the French chemists Proust and Berthollet. The first claimed that all chemical reactions occurred in fixed proportions, the latter that they did not. "Each collected impressive experimental evidence for his view. Nevertheless (...) their debate was entirely inconclusive" (Kuhn 1970, 132).

Every paradigm is, in any case, "constitutive of science". Paradigms "are the source of the methods, problem-field and standards of solution" (1970, 103). But that is not all: paradigms are "constitutive of nature as well" (Kuhn 1970, 110). As a consequence "during revolutions scientists see new and different things (...). We may want to say that after a revolution scientists are responding to a different world. (...) what were ducks in the scientist's world before a revolution are rabbits afterwards" (Kuhn 1970, 111). It is not only the perspective that changes, but the (phenomenal) world itself. Therefore, Kuhn can ask himself: "what sorts of transformations in the scientist's world" could be discovered by the well-informed historian? (Kuhn 1970, 115). Kuhn wishes to say that "after Copernicus astronomers lived in a different world" (1970, 117), and after discovering oxygen "Lavoisier worked in a different world" (1970, 118). But here he pauses for a moment. He asks himself: "did these men really see

different things when looking at the same sorts of objects? Is there any legitimate sense in which we can say that they pursued their research in different worlds? Many readers will surely want to say that what changes with a paradigm is only the scientist's interpretation" (Kuhn 1970, 120). We may want to add the question: what are the implications for the possibility of dialogue?

But Kuhn maintains his position. Surely, Kuhn says, the epistemological tradition in which we are merged would advise us to go back to the usual conclusions. And there is not yet an epistemological alternative that has been properly elaborated. About the traditional epistemology he says: "I am unable to relinquish entirely that viewpoint" (Kuhn 1970, 126). He is "acutely aware of the difficulties created by saying that when Aristotle and Galileo looked at swinging stones, the first saw constrained fall, the second a pendulum" (1970, 121). Yet he will not surrender easily and he adds that "we must learn to make sense of statements that at least resemble these" (Kuhn 1970, 121).

All this has important consequences for communication. "Before they can hope to communicate fully one group or the other must experience the conversion (...) a transition between (...) competing paradigms cannot be made a step at a time, forced by logic and neutral experience" (Kuhn 1970, 150). But how then are scientists brought to make this transposition? Kuhn's answer is that in fact "they are often not" (Kuhn 1970, 150). And here he reminds the reader that copernicanism made few converts for almost a century after Copernicus' death. The examples could be multiplied. Therefore Kuhn concludes: "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die"! (Kuhn 1970, 151).

In *The Structure* Kuhn, if compared with Polanyi, shows more skepticism in his view of communication. However, Kuhn does not take a fully straightforward position. On the contrary, his position contains many nuances and uncertainties. In the section that follows I will try to consider these uncertainties and nuances as they are found in *The Structure of Scientific Revolutions*.

#### 4.3.b Softening the radical views in *The Structure of Scientific Revolutions*

After all, it is not easy to affirm that we simply don't understand what scientists discussed in previous times, simply because they adopted a different paradigm, or worked in a pre-paradigm situation. Perhaps Kuhn had already considered one objection later expressed by Watkins, which sounds far from superficial: if the new and the old paradigms must be incompatible, how can they at the same time also be incommensurable? (Watkins 1970, 36-37).

Attenuations are needed. Therefore Kuhn introduces another idea: the new paradigm always incorporates something of the old one (Kuhn 1970, 169)! We are also told that "the scientist

after a revolution is still looking at the same world" (1970, 129), and "most of his laboratory instruments are still the same" (1970, 130). Kuhn specifies that his more radical statements do not mean that two rival schools "can see anything they please. Both are looking at the world and what they look at has not changed. But in some areas they see different things and (...) in different relations" (1970, 150). Kuhn is not trying to deny the previous doctrine of incommensurability:

"Two men who perceive the same situation differently but (...) employ the same vocabulary in its discussion must be using words differently. They speak, that is, from (...) incommensurable viewpoints" (Kuhn 1970, 200).

But in the *Postscript* one can find a few significant adjustments. Incommensurability exists, but it does not necessarily mean that communication is totally impossible. Scientists seem to be again able to listen to each other, at least in some sense. The stimuli that impinge upon them are the same, says Kuhn, and their "general neural apparatus" is the same. They share a history and in addition both their everyday experience and most of their scientific world and language are shared (Kuhn 1970, 201).

At this point the metaphor of translation dominates the discussion. Different views are like different languages. But according to Kuhn there are strategies available for those who want to learn a foreign language:

"What the participants in a communication breakdown can do is to recognize each other as members of different language communities and then become translators" (Kuhn 1970, 202).

The required techniques are not, to be sure, "straightforward or comfortable" (Kuhn 1970, 201), yet they can first of all try to individuate the terms that are used unproblematically within each community but do cause problems in the inter-group discussion (Kuhn 1970, 202). They can also try to imagine what the other would see and say. In this way each group becomes a good predictor of the other's behaviour. Since the dialogue allows them to see the merits and defects of each other's point of view, it constitutes the possibility of persuasion and conversion (Kuhn 1970, 202).

Having realised the uneasiness of some of his readers, Kuhn (1970b, 267) informed them that by using the term incommensurability he did not mean to indicate the impossibility of comparing two paradigms. Translations are difficult but not impossible. Did he not speak then,

of impossible communications during revolutionary periods? According to Toulmin he did, but then he gradually modified the idea (Toulmin 1970, 43-44).<sup>92</sup>

#### 4.3.c *The further development of Kuhn's views on scientific communication*

In *The Structure*, Kuhn's presentation of incommensurability oscillated between radical pronouncements and moderate resolutions. In his further development Kuhn somehow narrowed and limited the incommensurability concept. During the 1960s the incommensurability of two theories touched three aspects:

- a) differences in the phenomenal world,
- b) differences in the selection of problems and standards for their solution,
- c) differences of meaning.

At the end of the 1960s and beginning of 1970s the first two aspects were abandoned and incommensurability focused only on meaning change (Kuhn 1970b, 267-268). In addition, Kuhn specified that his incommensurability thesis never implied that all concepts change in the transition to a new theory (e.g. 1970b, 267 and 269). Therefore two theories were now called incommensurable when there is no "language into which at least the empirical consequences of both can be translated without loss or change" (Kuhn 1970b, 266). This language would be a neutral observation language, but there is no such language.

Kuhn's further re-elaboration of the concept at the beginning of the '80s focuses on the notion of translation. The argument concerning a neutral observation language now disappears and untranslatability is involved in explaining incommensurability. "If two theories are incommensurable they must be stated in mutually untranslatable languages" (Kuhn 2000, 34).<sup>93</sup>

At this point Kuhn's discussion of the topic becomes quite intricate. One of the factors creating confusion is that during the 1960s and 1970s Kuhn works with an everyday concept of translation (Kuhn 1970b, 267),<sup>94</sup> while in the 1980s translation consists of two heterogeneous moments. Translation in the narrow, technical sense consists in replacing single words (or groups of words) by words available in the target language. In translation in the broader sense

<sup>92</sup> Toulmin was not the only critic who understood some of Kuhn's passages as purporting a breakdown of communication Newton-Smith (1981, 12) and Popper (1970, 55-57) had the same impression. Even Hoyningen-Huene (1993, 254, fn. 173) is compelled to admit that one may find passages in *The Structure* that seem to suggest a complete breakdown of communication. But he defends Kuhn by saying that this can only happen if those passages are "taken in isolation" (Hoyningen-Huene 1993, 254, fn. 173).

<sup>93</sup> The paper *Commensurability, Comparability, Communicability* (published in Kuhn 2000, 33-57), was first published in 1982 in the 2<sup>nd</sup> volume of the Proceedings of the [1982] Symposium of the Philosophy of Science Association (East Lansing, MI: The Philosophy of Science Association).

<sup>94</sup> For example this everyday concept of translation allows Kuhn to say that translation "can present grave difficulties even to the most adept bilingual. He must find the best available compromises between incompatible objectives. Nuances must be preserved but not at the price of sentences so long that communication breaks down. Literalness is desirable, but not if it demands introducing too many foreign words, which must be separately discussed in a glossary or appendix" (Kuhn 1970b, 267).

(interpretation) the unintelligible must be made intelligible. This requires learning to a certain extent the language in which the unintelligible material is articulated (Kuhn 2000, 37-40). Two theories are now called incommensurable just in case they are formulated in languages not translatable in the narrow, technical sense (Kuhn 2000, 40). In any case this type of “local incommensurability” allows Kuhn (2000, 35-37) to declare that “the claim that two theories are incommensurable is more modest than many of its critics have supposed” (Kuhn 2000, 36).

The “taming” of incommensurability requires the adoption of two other important ideas that certainly have been cultivated by Kuhn in previous writings.<sup>95</sup> The first one is that incommensurability does not imply incomparability (Kuhn 1979, 416). As a consequence (and this is the second idea) there remains a *continuity* between different traditions of normal science. The empirical potential of incommensurable theories can indeed be compared. Such theories have empirical interconnections. There must therefore remain a certain type of continuity even between incommensurable theories (Kuhn 2000, 36).

We see Kuhn gradually reducing and limiting some of the radical claims he presented in *The Structure*. This move seems to be much appreciated by many recent commentators and contributors who feel much more at ease with Kuhn’s latest conception of incommensurability and are determined to build on it. According to Xiang Chen (1997, 257-273) Kuhn’s latest version of incommensurability is an attempt at reducing relativism. The same author argues that incommensurability can be only a “local phenomenon” and does not necessarily imply incomparability (Xiang Chen 2003, 962-974). On the contrary, says Xiang Chen (2002, 1-21), even so-called incommensurable taxonomies can be rationally compared. Hoyningen-Huene (2002, 61-83) points out that even Feyerabend, towards the end of his life, re-evaluated in more positive terms Kuhn’s latest pronouncements on incommensurability. Sankey (1998, 7-16) would even like to rescue Kuhn from anti-realism and to show that his latest version of incommensurability is compatible with “a reasonably robust scientific-realist framework”.

Newton-Smith remains a bit more ironic. He summarises the development of Kuhn’s philosophy by saying that he started as a “revolutionary” and gradually became a “social democrat”! (Newton-Smith 1981, 102-107). This slightly humorous observation indeed contains a truth. In the next section I will attempt an assessment of Kuhn’s contribution to the contemporary understanding of scientific communication.

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<sup>95</sup> Hoyningen-Huene, always busy underlining the continuities in Kuhn’s writings, traces the roots of the above-mentioned ideas in *The Structure* and other texts (Hoyningen-Huene 1993, 218-22). In my opinion, however, should Kuhn have adopted much more radical views in his later developments, it would still have been equally possible to trace the roots of such radical ideas in his previous works.

#### 4.3.d Kuhn between dialogue and incommensurability: attempting an evaluation

In *The Structure* Kuhn often takes a radical position on incommensurability. Scientific dialogue appears to be highly problematic. Yet he also tries to show that his system does not amount to complete relativism. He rather defines his views as a “partial relativism” (1970b, 264-65). Some forms of communication and some translations are possible, at least in part. It is not necessary to fear the complete disruption of rationality and dialogue. Kuhn is determined not to present himself as a promoter of anarchism. His style is often nuanced and cautious. It would actually be interesting, from a linguistic point of view, to observe his clever use of adverbs, when he tries to “soften” ideas that would otherwise sound quite radical.

In the further development of his thought the concept of incommensurability is narrowed down. The continuity between theories, the possibility of translation, the idea that theories remain comparable are more and more acknowledged in his later writings. All this shows that the gradual move towards relativism of contemporary philosophy of science is not simply a cumulative process. It is a process that knows pauses and even contradictions.

Nevertheless we should also acknowledge that Kuhn takes us a step further from where we had been left by Polanyi. Polanyi built the image of a united scientific community where the dialogue was basically possible. From there he occasionally admitted the challenge of fragmentation, the obstacles and the divisions among scientists. With Kuhn the impossibility of communication, at least in certain circumstances, is clearly admitted (at least initially). Incommensurability now becomes an established concept, entailing a potential threat to the legitimacy of science and its reliability. This is certainly one of the reasons why Kuhn sometimes seems to hesitate or to look back. In order to maintain the credibility of science he tries to indicate a few possible antidotes to complete relativism.

These hesitations will be abandoned by Feyerabend, who accepted the challenge of relativism and pluralism and even welcomed them as the basis of his system. Apparently, all this did not lead Feyerabend to a pessimistic approach: on the contrary it seemed to open new and exciting possibilities. Not for the legitimation and authority of science, to be sure. But for the abolition of authoritarianism.

### 4.4 Feyerabend and beyond: incommensurability and freedom

#### 4.4.a Incommensurability is real

It can be said that Kuhn and Feyerabend are very similar in their reflection on incommensurability, yet Feyerabend reaches a more radical position. The theme of incommensurability is introduced, in *Against Method*, as something that anticipates the future radical character of philosophy of science (Feyerabend 1975, 214). He admits that times are not completely mature to propose anarchism, “that is liable to paralyse the brains of almost

everyone" (1975, 214). On the other hand, one can already try to "anticipate the next stage". Incommensurability is already here. It has to do with the "covert classifications" implicit in every language.

Later Feyerabend provides another definition of incommensurability:

"Let us call a discovery, or a statement, or an attitude *incommensurable* with (...) a theory, [or] framework if it suspends some of their universal principles" (Feyerabend 1975, 269).

With these declarations, Feyerabend switches the focus from linguistics and translation to the worldviews, principles, beliefs and ontological convictions supporting our theories. The linguistic issues are not simply abandoned (see 4.4.b), but Feyerabend is convinced that statements and theories are dependent on some fundamental framework. When the basic assumptions of such a framework are denied ("suspended") by another framework or by statements or by a new theory, the two competing systems become incommensurable. Feyerabend dedicates a long section to establish the fact that incommensurability exists, perhaps feeling that Kuhn's work was not decisive on this point. His arguments refer to studies on children's perception (1975, 227 ff.) but also to different styles in painting (230 ff.). Analysing the Greek archaic style, Feyerabend outlines the basic view of the world underlying such painting. Without entering into the details, we can say that, according to Feyerabend, the cosmology represented by these paintings is incompatible with our modern conception of the world and of man (Feyerabend 1975, 230).

When this happens in science, it is not possible to institute an experiment which will refute one of two theories (Feyerabend 1975, 282). It is also not possible to find a neutral position from which it would be possible to compare for example the theory of relativity and classical physics.

"The theory of relativity (...) does not just deny the existence of classical states of affairs, it does not even permit us to *formulate statements* expressing such states of affairs. It does not, and cannot, share a single statement with its predecessors" (Feyerabend 1975, 275-76).

This is why "the views of scientists are often so different from each other as are the ideologies that underlie different cultures" (Feyerabend 1975, 274). It is an illusion even to suppose that incommensurable theories deal "with the same subject matter". This view is born from the wrong supposition that facts can be relatively independent from theories. But there are no facts without theories. Therefore incommensurable theories deal with totally different facts. To hold a different hypothesis:

“would be as laughable as the statement “whenever there is possession by a demon there is also discharge in the brain”, which establishes a connection between (...) a possession theory of epilepsy and more recent “scientific” terms. (...) But in the case of relativity vs. classical mechanics a hypothesis of this kind *cannot even be formulated*. Using classical terms we assume a universal principle which is suspended by relativity” (Feyerabend 1975, 276).

Does all this mean that communication between scientists, in such cases, is simply impossible? The answer must be “Yes”, to a large extent. Yet according to Feyerabend it is still possible, for example, to refute incommensurable theories. It is not possible to do this by comparing their respective content. But they can be refuted by discovering their internal contradictions. Feyerabend admits that in the absence of commensurable alternatives “these confutations are quite weak, however” (Feyerabend 1975, 284), and referring to his previous evaluations, he must also admit that in this case “refutation is greatly reduced in strength” (1975, 285). Nevertheless incommensurable theories are not totally immune to criticism.

Another interesting reshaping of the concept of incommensurability is implied in the following idea. Feyerabend cannot be satisfied by just affirming the existence of a certain state of affairs without trying at the same time to relativise it. This is valid for incommensurability as well. I believe this is the reason why in his *Consolation for the Specialist* (1970, 220) he decides that incommensurability cannot be just an incontestable fact, and he observes that it is necessary also to ask: “incommensurable according to which interpretation”? Certain systems will be incommensurable according to certain views, but not according to others. Instrumentalism, for example, makes comparable all theories related to the same observational language. Implicitly, another important question about incommensurability should be asked, namely: “is it desirable”? Does it lead to a new authoritarian science or does it liberate us from oppressive science?

#### *4.4.b Incommensurability is desirable*

In a system like Feyerabend’s, incommensurability is fundamentally desirable because it contributes to diminishing the role of rationality in science. Incommensurability shows us that rationality cannot be a common ground underlying all systems. It tells us that rationality functions only on the basis of a certain framework, and that there are, in a sense, many versions of rationality. One must choose which form is more suitable in one’s opinion. But in this case the choice is not determined by logic (Feyerabend 1975, 190-196).

Incommensurability implies that the different choices are all equally plausible, that we cannot see the one as correct and the other as “mistaken”. This is openly declared also by Kuhn (1970a, 12), in a famous challenge to Popper. Ptolemaic science, says Kuhn, was not simply “wrong”, it was a plausible choice within the context where it developed. Nor can we say that

the successive systems are correct in any conclusive sense, we must remain open to new developments and improvements.

It can be observed that Popper too, who does not accept incommensurability, supports the view that systems are not simply correct or mistaken (Popper 1963, 235). He sees theories in an evolutionary line in which improvement is the key-concept. From a systematic point of view, therefore, it is not strictly necessary to accept incommensurability in order to recognise the relative merit of all theories or systems. Yet incommensurability has the advantage of stressing more radically the existence of parallel convictions which can be equally justified. It is therefore welcome in Feyerabend's pluralist system.

For Kuhn there is in any case a mechanism of selection attributing the victory to *only one* paradigm over all others in a certain period. But Feyerabend radicalises the view and posits the existence of incommensurable views co-existing side by side in the same epoch. This can be called a fragmentation and a chaos, admits Feyerabend, provided we also recognise that it is the only condition in which we can hope for fruitful developments. The domination of only one paradigm can easily lead to the exclusion of fertile hypotheses and new solutions. The danger is the sclerosis of scientific research, the inability to imagine alternatives. Science can only progress when alternative roads are explored, when the un-imaginable is given credit, when traditional beliefs are challenged.

It must be understood that behind the arguments in favour or against incommensurability, there remains a person with a certain aim. As for the arguments, it must be recognised that those proposed by Feyerabend are not simply arbitrary. He accepts the metaphor of translation, and places the discussion in the context of language. Then, precisely from this context, he elaborates new arguments in favour of incommensurability.

For example he argues that the view according to which translation is always possible (Popper) does not necessarily imply that incommensurability does not exist. Firstly because translations imply some degree of violence to the translated language. Secondly people might learn to move in two incommensurable frameworks without abolishing incommensurability (Feyerabend 1975, 273, fn. 130). He also objects to the idea that before one can learn a certain worldview (e.g. of a tribe) one must learn the language of the specific group. This is not, says Feyerabend, the way (e.g.) anthropologists are supposed to become acquainted with a certain culture. It is only when the framework of a new culture is assimilated that one can properly understand its language. Piaget's studies demonstrate that the child's perception also passes through phases which are incommensurable (Feyerabend 1975, 227). Incommensurability is desirable. Otherwise, asks Feyerabend, should we be happy living in a static, immobilised perceptual world?

“Should we welcome the fact (...) that an adult is stuck with a stable perceptual world and an accompanying stable conceptual system (...) whose general outlines have forever become immobilized?” (1975, 229).

Like Kuhn, he insists that we don't need to fear relativism and the disruption of all rules. His apology is a bit different though: science has never been as rational as it claimed to be. But is this enough, one might ask? Can science still maintain any claim to universal validity? The impression is that with Feyerabend we have reached a loss of communication. Feyerabend uses the arguments of rationality in order to defy rationality. One cannot say that it was a hidden agenda. He declares: “an anarchist is like an undercover agent who plays the game of reason in order to undercut the authority of Reason” (Feyerabend 1975, 33). Is he not also using communication in order to prepare (at least in part) the breakdown of (scientific) communication? If the answer is positive, then we have also reached a stage where the legitimacy of science is to a certain extent undermined. Although this seems to be a positive result for Feyerabend, those who do not hold his anarchistic views have legitimate reasons to be concerned.

#### 4.4.c Baudrillard: towards a postmodern implosion?

With Feyerabend the possibility of incommunicability finally becomes a real threat. The late-modern emphasis on pluralism paves the way to fragmentation. The latter is accepted, in postmodern times, with an attitude that is a mixture of celebration and anxiety. Some (like Feyerabend) celebrate, but others (e.g Baudrillard) cannot avoid being concerned.

Late-modern philosophy has insisted very much on the importance of language, and in some cases has also emphasised the importance of a rationality of communication (Habermas 1984). Philosophy of science has also known a *sociological* turn (Collins and others) which in theory could have established a more optimistic view of dialogue within the society of scientists. But for some reason, this was not possible. Perhaps the emphasis on pluralism has undermined the possibility of true dialogue and, after Feyerabend, has opened the door to radical and even apocalyptic views.

This is certainly the case with Baudrillard. Though communication within scientific circles is not the primary focus of his criticism, it is included in his general analysis of representations, reproductions, hyper-reality etc. In his view science and technology have caused the implosion of communication in general, exactly by exploiting its possibilities. His later works are especially dedicated to the media, but earlier he had conducted a thorough analysis of language and communication (Baudrillard 1981).

According to Baudrillard in our epoch we have entered the “third order of simulacra”, which is an order of simulation and is “controlled by the Code”. Communication becomes mis-

information and invades both public and private space (Baudrillard 1984, 129). What is left is only “obscenity”, in the sense that all is “transparency and immediate visibility when everything is exposed to the harsh and inexorable light of information and communication” (1984, 130). This leads to “a state of terror, proper to the schizophrenic: too great proximity to everything” (1984, 132). But it leads especially to “the loss of the real” (Baudrillard 1984, 133). Communication was supposed to constitute the real, but exactly through its reproductions it has created the hyper-real, which is just a simulation without origin or relation to any reality. Now, technology and the media are certainly to be blamed for this. But the media at least share their responsibilities with misguided science and technology. Science has not only experienced the “internal” difficulties of communication, but has also prevented the possibility of true communication in contemporary society.

Observing his philosophy from the point of view of the long conflict between nature and freedom (or culture and nature <sup>96</sup>) we might say that with Baudrillard we have reached a point where culture overwhelms nature and even eliminates it. Nature (the real) is then substituted by a cultural simulation, a copy. The original is lost and we are left with a simulation, the hyper-real. Baudrillard’s philosophy has already reached the point of despair. He has realised, much better than others, the ultimate implications of the loss of the real. While many continue to play around with various forms of nominalism, constructivism and conventionalism, Baudrillard prophetically announces the despair of living in a world of our own construction.

To counteract this situation Baudrillard has sometimes proposed eccentric solutions, culminating in the suggestion that we might as well “join the objects”! In other instances Baudrillard suggests that, given the hopeless situation, we should even “cooperate with the Code” (Baudrillard 1988, 185ff.) in order to subvert the present order of simulation. These are certainly provocative and paradoxical suggestions, aimed at denouncing a situation that has become unbearable. Yet it is precisely in these paradoxical and dramatic pronouncements that Baudrillard’s philosophy represents the postmodern anxiety towards communication very well.

#### *4.4.d Lyotard: towards a postmodern euphoria?*

Admittedly, not all voices are as pessimistic as Baudrillard’s. Lyotard, for example, invites us to stop longing for a science in which one of the most important values is the creation of consensus. According to Lyotard, postmodern science should not seek to create consensus but rather dissensus. In his view, Habermas’ philosophy of consensus obtained through dialogue, is based on the meta-narrative of emancipation (Lyotard 1984, 60). But according to Lyotard

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<sup>96</sup> Schuurman, among others, prefers to define the humanist ground motive of western society as the motive of “nature and culture” (e.g. Schuurman 1995, 185-190). The dialectical conflict between nature and culture is also regarded as a most crucial theme of the humanist period by Venter (n.d. ch 7).

(1984, 65), Habermas is wrong in identifying emancipation and consensus. Consensus is only a particular state of discussion, not its end. Also Lyotard believes in the necessity of emancipation, but he hopes to achieve it via the power of dissensus. Consensus can be the end of freedom and of debate. It is dissensus that allows the experience of liberation.

Lyotard therefore, replaces consensus with “general agonistics”. “To speak is to fight in the sense of playing” says Lyotard, “and speech acts fall within the domain of general agonistics” (Lyotard 1984, 10). This does not mean that one always plays in order to win, a “move” can be made for the sheer pleasure of invention. The moves keep the language-game going, while at the same time bringing pleasure to the players. But there are moves that are not permitted: the moves of “terror”. By this, Lyotard (1984, 63) means the moves that aim at eliminating or threatening one of the players from a certain language game (science, politics etc). This would in fact lead to forced consensus, to the end of dissension and, as a consequence, to the end of invention. It is not clear, however, how Lyotard can afford to introduce this meta-rule against terror, an “absolutist” ban excluding the experimentation that he so often recommends. In the opinion of some (e.g. Bertens 1994, 129) this meta-rule seems difficult to justify (in a philosophical sense) and constitutes a contradiction within Lyotard’s approach.

In Lyotard’s view of science, I would say in conclusion, dialogue and communication do have a legitimate place, but it does not matter whether they are “effective”, in any traditional sense. Actually, it is better if such dialogue *doesn’t* reach any result, if consensus is avoided: this will keep the game going. Postmodern science, in his view, can open up new and exciting perspectives exactly by resisting consensus. A few questions impose themselves: is the role of scientific communication not truncated in this case? If consensus (as Lyotard rightly observes) is not the aim of science, can dissensus be the aim? And will it be possible to the human being who occupies himself with science, to translate into daily practice Lyotard’s highly ideological recommendations, without experiencing a sense of isolation and even lack of purpose?

The analysis of the developments of humanistic philosophy reveals a gradual move towards skepticism and irrationalism. For the scientific enterprise, this implies a loss of credibility, in the eyes of contemporary philosophy of science. In the following section we will begin to look at a diagnosis in order to understand the symptoms of the disease and also to offer, in a next phase, a viable remedy.

#### **4.5 Understanding the symptoms: why is postmodern science becoming voiceless?**

##### *4.5.a The road to silence: historical summary*

The philosophies of science that we have surveyed reveal a gradual loss of confidence in the possibility of communication. Postmodernity seems to have gradually created a voiceless

science, i.e. science that has lost its ability to communicate.<sup>97</sup> For Popper communication had to be possible, but in Polanyi's philosophy a few doubts start surrounding the possibility of mutual understanding among scientists. Some of Kuhn's statements (at least initially) introduced a deeper sense of difficulty concerning the possibility of dialogue between different schools. Kuhn was cautiously trying to avoid complete incommensurability and introduced many nuances in his system of thought. But with Feyerabend incommensurability and the consequent possibility of a black-out of communication become a reality.

Looking at more recent developments (cf. 4.4.c and d) Lyotard's promotion of the revolutionary role of dissensus and "general agonistics" sounds too "heroic" and elitist, while Baudrillard prophetically announces the end of true communication and the advent of hyper-reality, not only for the scientific community but for society in general. Baudrillard's philosophy offers an even more dramatic perspective: representations (including scientific theories) swallow the real and substitute it with the hyper-real, a copy of their own construction.

The issue of the difficulties of communication is certainly linked to the problem of presuppositions. If the latter are all-powerful and increasingly important, how can scholars holding different positions communicate? In some cases it is argued that different presuppositions create even different worlds, change the meaning of the words we use and so on. By weakening the relevance of creational structures and the common ground that they provide for all, by seeing science and the world as a "creation" by the subject (or community) any common reference point seems to be lost, and dialogue with it.

What is the diagnosis of reformational thinkers? Is it possible to interpret these symptoms correctly? Is it possible to trace the roots of these problems and then eventually find a way out?

#### *4.5.b Autonomous freedom and universal order*

I am convinced that it is possible to relate the problem of communication to the influence of the humanist nature-freedom ground motive. The ideal of the control of nature requires a world which is the same for all, and a language that is understandable by all. The motive of freedom on the other hand might suppose the existence of totally different worlds, experiences and languages. Under the hegemony of the freedom motive, states of affairs are understandable only within certain theories. These theories are like a certain language. If the language is not

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<sup>97</sup> Of course this process has been gradual and not simply "cumulative". Some philosophers have insisted very much on the possibility of communication. Habermas in particular sees in communicative rationality (1984, 7) the key to rescue the original project of modernity, and with it a meaningful role for science itself. For him modernity has faced a legitimation crisis (1976, 61ff.) which involves science as well, but can be counteracted precisely by a rationality of communication, creating consensus and solidarity.

shared, there is no real possibility of dialogue. We are now free to develop our own idiom but, as in the case of paradigms, language becomes a cage as well. It prevents the communication with those convicted in a different cage.<sup>98</sup>

The symptoms that we have observed in late-modern reflection on science point to another problem. What is increasingly missing is the recognition of the conditioning role of the order for creation and the explicit statement of what has been chosen as archimedean point. When theories are compared especially with other theories, states of affairs depend on theories and we deal only with the *hyper-real* (i.e. reproductions and simulations) we are approaching a phase where communication becomes highly problematic. If a solution is to be sought, the theme of the access to a created order must be considered as very important, even for communication. By created order I mean a common ground, epistemological or ontological, an order which is stable and not totally dependent on the assessment and agreement of the subject or community.

The lack of the recognition of this order has obliged some philosophers to look at the subject himself, trying to find in it some stable and common ground. Society and language then become the *locus ordinis* (the foundation of order). But when the languages are plural and the communities are divided by different paradigms, the common ground seems to disappear again.

Two themes are important for a reformational diagnosis: the impact of a dualistic religious ground motive on late-modern philosophy, and the necessity of linking the theoretical reflection to the universal order. While the first theme helps us to understand the tensions of humanist philosophy the second suggests what is missing. The second theme also points towards possible ways out (which will be discussed in 4.6.b and c).

Reformational philosophers in general have distanced themselves both from the “received view” of scientific theories and the relativist developments of late-modernity. They argued from the beginning that dialogue is necessary and possible even in the presence of the deepest antithesis between rival systems. Our first example of a reformational approach will be one provided by Herman Dooyeweerd. But instead of dealing immediately with the most crucial issues of philosophy of science, the next section will simply “set the tone”. In other words it will help us to understand the general philosophical background and the worldview underlying the reformational reflection on scientific communication. After this, we will move to more specific themes of philosophy of science (cf. sections 4.6.c and d).

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<sup>98</sup> In philosophy of science the problem is known as the “meaning variance” (and invariance) of terms and is also related to the theory-observation discussion. In 5.6.c I will return to this topic.

## 4.6 Reformational alternatives

### 4.6.a Dooyeweerd and antithesis

Dooyeweerd is sometimes accused of having insisted too much on the impact of the “religious antithesis” (cf. e.g. Klapwijk 1986, 138-142) in theoretical thinking. However this antithesis, in his view, was not supposed to constitute an obstacle to the dialogue between scientists of different persuasions (Dooyeweerd 1959, 70-72). There is an equilibrium in Dooyeweerd’s position, which is the result of a double acknowledgment in his epistemology. On the one hand he recognised the importance of the religious ground motives, which indeed create a difference in the interpretation of creational data. This is true for philosophy as well as for the special sciences. But on the other hand Dooyeweerd acknowledged the reality of the order for creation (Dooyeweerd 1959, 72-73) constituting a common ground among scientists and thinkers.

In this respect it is interesting to mention that Dooyeweerd’s second way of transcendental critique (1984, I, 22-57) begins from the structure of theoretical thought, which is a common ground for all thinkers. The issue of the “antithesis” is introduced only in the third and final “step” of his critique. It is also interesting to notice that Dooyeweerd’s critics had strong differences of opinion in the evaluation of his idea of antithesis. For authors like Wolterstorff (1989, 64-65) or Klapwijk (1986, 141-143), Dooyeweerd’s view of antithesis was too radical. In the opinion of those authors the dooyeweerdian idea of antithesis would divide the scientific community and hinder the cooperation among scholars. For others however, the dooyeweerdian idea of antithesis was too weak.

For example, the critiques by Cornelius Van Til (1971, 99) centered on the impression that the theme of religious antithesis was not sufficiently emphasised by Dooyeweerd. According to the famous apologist of Westminster Seminary, Dooyeweerd should have started from “a confession of faith” (Van Til 1971, 99), not from supposedly neutral states of affairs. Dooyeweerd’s reply was that although he wanted to recognise the relevance of the religious antithesis, the states of affairs are founded in the cosmic law-order, not in the subjective consciousness, and therefore they are accessible to all thinkers, irrespective of their orientation (Dooyeweerd 1971, 80-81).

No matter what the convictions of the scientist are, according to Dooyeweerd he will have to deal with laws and structures that are not simply dependent on his or her views. By “states of affairs” Dooyeweerd did not indicate a reality simply disconnected from mankind, a reality that could, as such, provide the basis for scientific communication. His view of naive experience, does not wish to imply a naive realistic conception of reality (Dooyeweerd 1984, III, 34). And this is even more the case with theoretical thought.<sup>99</sup>

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<sup>99</sup> On this point Dooyeweerd had important discussions with the Dutch philosopher C.A. Van Peursen, who does not recognise the existence of states of affairs. He only recognises dynamics events and affairs

Nevertheless, Dooyeweerd believed that when the creational data are neglected or interpreted incorrectly, theoretical thought itself will end up in antinomies and contradictions (Dooyeweerd 1959, 74). The views of the different scientists therefore, both in philosophy and special science, are not considered “incommensurable”. Creation itself is the judge of our theories and it will continue to remain a normative source for all knowers (Dooyeweerd 1959, 72). It is precisely this intuition and acceptance of a created order which seems to be missing from humanist philosophy. This created order, on the other hand, seems to be accepted much more naturally by christian systems of thought.<sup>100</sup>

#### 4.6.b Dooyeweerd on scientific dialogue

Dooyeweerd believed the biblical ground motive to be preferable to all the others. However the religious antithesis does not magically divide the scientific community into two well-delimited groups. This antithesis cuts through the life of the christian scientific community as well as through all other communities (Dooyeweerd 1984, I, 524). Scientists who are christians sometimes adhere to unbiblical ground motives when it comes to science and, apart from that, they are still under the influence of the Fall. In addition, the religious ground motive is of course not the only reference point for scientific knowledge (Dooyeweerd 1959, 69). Creation is the God-given common denominator. Therefore the “states of affairs” can be discovered by all.

As a consequence, Dooyeweerd firmly believed in the possibility of cooperation between scientists of different persuasions. He augured for example a “healthy and noble emulation” between all philosophical schools (Dooyeweerd 1959, 73). Christian scholarship is not bound to an oppositional attitude. The challenge for all schools of thought is to provide a reliable account of creational data, not jealously to defend their own views.

Dooyeweerd believed in the possibility of real dialogue, and his transcendental critique was intended to promote this dialogue.<sup>101</sup> He contended that it is exactly the “dogma” of the autonomy of theoretical thought that had made genuine dialogue impossible. The different schools, in his opinion, were not sufficiently aware of the deeper causes of their disagreements,

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that do not “stand” but are also constituted by the interaction with the knowing subject. Cf. Van Peursen (1959).

<sup>100</sup> The idea of a structural order is of course not exclusive to reformational circles. Troost (1994, 2-16) demonstrates that the theme of a “creation order” has been present in christian thinking from the patristic era up to the present. However Troost (1994, 8-16) also points out that in the christian tradition this crucial idea has often been worked out especially from a theological perspective and has been shaped rather naively from a philosophical point of view. As a consequence, many types of alien influences (e.g. stoicism, neo-platonism) have been welcomed into various christian conceptions of the creation order, thus preventing a more original christian contribution in both catholic and protestant thinking.

<sup>101</sup> Of course there are, on this point, different evaluations of Dooyeweerd’s endeavour. See for example the doctoral thesis of Conradie (1960) and the recent one by Choi (1999).

which are often related to religious motives. Once these motives are uncovered, it should be easier to come to a more open comparison of ideas and theories (Dooyeweerd 1959, 71).

There was, for Dooyeweerd, a common call that was directed to all scholars and scientific communities. This view was a consequence of his reformed worldview. Notwithstanding the apostasy of humankind, God's cultural mandate to humanity is not cancelled. All scientists are exposed to the same risks and called to the same task. Even apostate thought contributes to the "fulfilment of the Divine plan" (Dooyeweerd 1984, I, 119). In the present era it is not possible to gather the "faithful servants" (of the parable of the talents) in a separate "school" (1984, I, 524). It is the common task that imposes the necessity of humility and dialogue.

True, the different schools see things differently. But this difference does not cancel the reference to creational data. Dooyeweerd's classical example centers on the proposition  $2 + 2 = 4$  (Dooyeweerd 1959, 72ff.). The judgment  $2 + 2 = 4$  corresponds to a state of affairs which is independent from every subjective theoretical view. Yet a difference of interpretation is manifested between the different currents of the philosophy of mathematics. The logicist tendency is thus opposed to the intuitionist, the formalist, the empiricist or to the sensualist tendency.

The conflict among interpretations is certainly real. However, according to Dooyeweerd, the structural order for the temporal horizon of experience, with all the states of affairs which are founded on it, is indubitably the same for every thinker, irrespective of the orientation of one's thought. Once they are discovered, the structural facts impose themselves on everybody and it would make no sense to try to deny them. All schools and currents of philosophy receive the common task of accounting for them in a philosophical way. These schools are inter-dependent, and must learn from each other. The philosophy of the Cosmonomic Idea, says Dooyeweerd, does not claim any privileged position among the other philosophical schools. (Dooyeweerd 1959, 73).

It seems quite clear that the theme of a religious antithesis, as far as Dooyeweerd is concerned, does not cancel the importance and necessity of dialogue. On the contrary, to discover this antithesis is the starting point to realise a meaningful dialogue. "Dialogue and antithesis", as the title of Choi's thesis (1999) reads, go hand in hand in Dooyeweerd's view.

#### *4.6.c Two levels: implications for dialogue and for framework-dependence of theories*

Reformational philosophy, according to Dooyeweerd, does not "attribute to its provisional and fallible results the infallible character of its religious starting point" (Dooyeweerd 1959, 73). This represents, in my opinion, another extremely important theme of the Dooyeweerdian view. The only radical kind of antithesis is the *religious* antithesis (1984, I, 123). Other forms of antithesis are therefore relative. When we speak of incommensurability in science (and the

same holds for antithesis) we must distinguish between the level of the deepest presuppositions and the level of the concrete theories. There is a definite difference between the two. In Dooyeweerd's words, the biblical religious motive is believed to be infallible by the Christian, but his own scientific theories can only be regarded as provisional and fallible. The point is very important.

By confusing or by not distinguishing properly the different levels, some philosophers of science tend to attribute the same degree of incommensurability to the starting points and to the theories. If the dialogue is not possible on the level of the deepest convictions, it cannot be possible at any other level, they would argue. There are of course differences between the different authors<sup>102</sup> but Feyerabend definitely shows a tendency to declare theories incommensurable because their pre-scientific frameworks are incompatible.

A sound distinction between different levels of presuppositions and of scientific research (e.g. Duvenage 1985, 31ff.)<sup>103</sup> would allow for a more nuanced position. It is true that there is always a connection between frameworks and concrete theories. A connection which is sometimes not immediately visible yet nevertheless real (Wolterstorff 1976, 79). But the framework does not completely determine the theories. The latter are not simply the product of the presuppositions of the scientist (Van Riessen 1992, 90). There is therefore a difference between the levels, and also a relative "independence" of theories from presuppositions of various depth and nature. This situation allows for a certain degree of communication.

Comparison and dialogue are therefore possible not because of the existence of a neutral language, or because scholars can enter a neutral position outside their paradigms, or because the paradigms are all the same, but because presuppositions are not decisive in a final sense and we live in a common (though not neutral) world. In the long run, the real world resists our reductions and distortions.

This entails another important implication for scientific dialogue: even our deepest frameworks can be criticised and evaluated. It might be impossible to compare two religious motives in abstract. But even in this case, according to Dooyeweerd (e.g. 1959), we can observe

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<sup>102</sup> In some cases (see Kuhn's paradigm) the frameworks are constituted by elements that belong partially to the scientific and partially to the pre-scientific level. As a consequence the framework itself can be regarded as partially scientific. In this case, from a dooyeweerdian point of view, the hypothesis of incommensurability simply becomes more remote because complete "antithesis" is not thinkable at the scientific level.

<sup>103</sup> According to Duvenage's account of scholarship three levels of scientific investigation should be recognised as different but connected to each other (1985, 31-36). He proposes the image of a spiral, in which we can distinguish three "levels" that are called: microfocus, mesofocus and macrofocus. In each level we find relevant "perspectives or visions" (Duvenage 1985, 33). "These visions cohere directly with the various levels which can be distinguished in the formation of science" (1985, 33). Now, in every level Duvenage distinguishes both scientific and pre-scientific "perspectives". In this way he recognises both the different assumptions and the common engagement of all scholars with the debates and theories of a certain field of study.

their influence on theoretical thought and we can criticise each other. When we realise, for example, that our philosophy and science are full of absolutisations, of reductionism and of consequent antinomies, we have a clue about the problematic nature of our deepest presuppositions. And this can certainly become a relevant topic for a dialogue among proponents of rival frameworks and theories.

Having argued in favour of a relative dependence of theories on frameworks and worldviews, in the following section I would like to defend the thesis that even concepts and statements are only relatively dependent on the theories in which they appear.

#### 4.6.d *Stafleu: relative theory dependence of concepts and statements*

Authors like Kuhn, Hanson and Feyerabend have criticised the empiricist position and have stressed theory-dependence of concepts and statements. In their philosophies, such theory-dependence is stressed to such an extent that the meaning of concepts and the truth of statements are completely determined by the theory in which they function. This means that, in principle, we cannot compare competing theories. This constitutes a powerful argument in favour of incommensurability. But obviously, if the basic assumptions of complete theory-dependence can be shown to be flawed, there is hope that incommensurability may be rejected and the dialogue between proponents of different views may be placed on more solid ground.

In this context I would like to refer to Stafleu's (e.g. 1987, 26-29) contribution, because he defends the idea of a relative theory-dependence of concepts and statements. Finding his starting point in Dooyeweerd's theory of modalities (see fn. 7) he suggests a position that "takes a middle course between two more extreme views, logical-empiricism, and historical relativism" (Stafleu 1987, 28). First he clarifies the term "intension" (= meaning) referred to concepts, and "extension" (the number of entities belonging to the class). After a short discussion of the concept *planet* he concludes that "the extension of a concept can be changed without changing its intension"<sup>104</sup> (Stafleu 1987, 26). This is for example the case with the discovery of the planet Uranus. The consequences drawn by Stafleu are far-reaching:

"Hence the meaning of a concept is partly determined by its theoretical context; it is theory dependent. But its meaning also has a certain autonomy with respect to the theory. A similar view can be held with respect to statements" (Stafleu 1987, 27-28).

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<sup>104</sup> On this specific point it is interesting to compare Stafleu's approach with Kuhn's. Kuhn (2000, 15ff.) analyses the sentence "in the Ptolemaic system planets revolve around the earth; in the Copernican they revolve about the sun". He concludes that "the sentence is incoherent. The first occurrence of the term planet is Ptolemaic, the second Copernican, and the two attach to nature differently. For no univocal reading of the term planet is the compound sentence true" (Kuhn 2000, 15).

In order to support his view of a “relative theory-dependence” of concepts and statements, Stafleu proposes a second argument, which takes into account once again the theory of modal aspects. Theories, says Stafleu (1987, 29) have a typical kinematic aspect, deduction being the logical *movement* from one statement to another. Similarly, statements have a typical spatial aspect, because they are dominated by the idea of *connection*. The structure of a concept refers to the logical unity and diversity, in other words to the numerical aspect of experience.

“Supposing the numerical, spatial, kinematic and logical aspects to be mutually irreducible, we now understand both why concepts and statements are theory dependent, and why they have nevertheless an irreducible autonomy. In this structural interlacement the meaning of concepts and the truth of statements are both relativized and opened up, if they start to function in a theory” (Stafleu 1987, 29).

The target of Stafleu’s arguments is the notion of incommensurability. He wants to propose a middle course between two more extreme views, namely logical empiricism, and historical relativism.<sup>105</sup> One of the fundamental assumptions of logical empiricism was the existence of observation statements and concepts independent of any theory (Suppe 1974, 14). These were called observation statements and observational (or empirical) concepts. The empiricists strongly relied on observation and assumed the possibility of finding purely empirical protocol statements, independent of any theory. But we cannot perform any observation outside the context of our expectations, frameworks and presuppositions in general. More in particular, for the observations performed in laboratories, sophisticated instruments are used that are often developed according to quite elaborated theories.

By finding a middle way between the two extremes, Stafleu not only proposes a view based on reformational ideas, but is in a position to invite humanist philosophers to welcome his own solution, to look back at the roots of their own tradition. Galileo’s *Dialogue* and *Discourses* for example, says Stafleu, “describe discussions among three persons, Salviati, Sagredo and Simplicio. Although the opinions of Salviati (the Copernican) and Simplicio (the Aristotelian) differ strongly, they are able to discuss all problems put forward” (Stafleu 1987, 28).

The agreement on the fact that observations cannot be made apart from any theoretical context should not blind us to the fact that observational results may be quite independent of some theories. As an example Stafleu (1987, 29) mentions the occasional backward motion of the planets, which played a considerable role in Copernicus’ heliocentric theory and was never disputed by any astronomer, copernican or not.

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<sup>105</sup> With this definition he refers e.g. to some of the works of Kuhn, Feyerabend, Hesse and others (Stafleu 1987, 28, fn. 34).

But, as already admitted in the introduction, not all the reformational attempts at clarifying the specific issue of communication in science are equally successful. In the next section I will present Klapwijk's view of the exchange and transformation of philosophical ideas. Although he focuses on philosophical incommensurability or dialogue, his views contain helpful elements to understand the process of communication between scientists or scholars holding to different prescientific frameworks.

#### 4.6.e *Less successful attempts: Klapwijk, communication and re-appropriation*

Klapwijk (e.g. 1986) has a very personal and original contribution to offer to the discussion concerning the possibility of dialogue between scholars, but I consider his reformational contributions as less successful than the others presented in this chapter. His views are nevertheless relevant for our topic, and though they focus especially on worldviews and philosophy, their implications for other sciences can be identified with ease.

In Klapwijk's opinion the founding fathers of reformed philosophy emphasised too much the difference in the starting points (e.g. worldviews) thus jeopardising the possibility of genuine dialogue and interaction with non-christian philosophical schools (1986, 138-143). On the opposite side we have the danger of synthesis, which Klapwijk does recognise as well. To avoid both dangers, he suggests the idea of a "transformation". The process of transformation, he says, is a very common phenomenon in philosophy. It is the re-elaboration of themes and concepts that are borrowed from other philosophies and then introduced into one's own philosophy. In a philosophical world where the possibility of dialogue is challenged by increasing skepticism, Klapwijk supports not only the possibility but also the need for communication. More than this: he encourages, especially in philosophy, an exchange of ideas between different schools.

Klapwijk's idea of transformation is related, first of all, to the *re-appropriation of christian ideas*. In a world that has been heavily influenced by christianity, many philosophical schools have adopted and transformed for themselves ideas that originally belonged to the christian tradition. For example:

"What we presently understand by 'modern secularization' is in fact nothing other than a process of transformation - I call it 'inverse transformation' - that originated in the Renaissance and Enlightenment and entails the wilful or unconscious categorical bending and systematic transformation of the Christian spiritual and intellectual heritage in the spirit of humanism. I would even defend the thesis that practically the whole of modern humanistic philosophy derives from a transformation of the Christian inheritance" (Klapwijk 1987, 105-106).

It is time to claim back these intellectual treasures. To support his project, Klapwijk introduces the famous biblical theme of the *Spoliatio Aegyptiorum*. Was not the gold taken from the Egyptians later utilised in the temple to the glory of God? Perhaps this is the right place to introduce a few critical remarks. The metaphor of spoliation gives the impression that when golden objects (i.e. ideas) are “borrowed” by someone and introduced into a new system they are no more in possession of the original owners. This is certainly the case when we deal with gold. But ideas can continue to be present in two different systems of thought at the same time. We mustn't forget that when we talk about “property” of ideas (terms like *possession*, *appropriation*, *exchange* are frequently used by Klapwijk) we are using just an image.

If this is true, do people really need to re-appropriate the ideas that they have lent to someone else? Let's take the christian community for an example. We must admit that in every epoch there may be ideas that are “lost” by christians, forgotten for a while. It may also happen that those ideas are borrowed, transformed and developed by other philosophical traditions. Such ideas often need to be revived and re-shaped by the christian community for every new historical context. But why is it necessary to recover these ideas from non-christian philosophies? They are always available in their original (christian) form. (The same is true, of course, for the non-christian philosophical traditions who “lend” ideas to others).

After all, in the transformation process (e.g. from the christian-medieval context) ideas are adapted to the system in which they have been introduced. This transformation implies a certain alteration. Why then, should it be necessary (e.g. for the christian community) to recover such ideas in their altered version, and work on them instead of going back to the original idea itself? I am not speaking, of course, of abandoning the interaction with the “transformed version” of the idea. I am asking why Klapwijk recommends to the christian academic community the recovery of the “altered” version of an idea rather than the recovery of the original version. The latter, after all, is an idea that has been shaped in accordance with a christian worldview, ground motive, confessional tradition, philosophy, or whatever was considered relevant in the process of elaborating the specific idea. Is this simply irrelevant?

#### *4.6.f Klapwijk: from re-appropriations to new acquisitions*

What Klapwijk proposes, of course, is not suggested only to the christian community. Transformation, in the end, is always reciprocal and is part of the normal course of events in philosophy (Klapwijk 1986, 149-50). But at this point Klapwijk presents a second and fundamental aspect of his project of transformation: *the acquisition of ideas which did not originally belong to one's own tradition* (1986, 149).

In a period when scholars are increasingly skeptical about the possibility of effective dialogue Klapwijk's proposal seems to open new avenues and to bring new optimism. It implies that the

different systems of thought are not closed in themselves but can always borrow ideas from other systems, re-shape and adapt them, insert these ideas into their own tradition. Behind such a proposal there seems to be a wish: enough of conflicts and antithesis, this is a time for cooperation.

But does Klapwijk take the lesson, according to which theoretical thinking is to a certain extent bound to “paradigms”, religious motives and so on, seriously? He states quite clearly that he doesn’t like Kuhn’s philosophy, in which our ideas are prisoners of a specific paradigm (Klapwijk 1986, 143, fn. 9). He wants ideas to be mobile, and available to be borrowed and exchanged. Worldviews used to be seen as the roots of our divergent views. But if these worldviews could now be made more open, by incorporating new ideas, this would change the whole situation. The attitude is generous, although (I believe) a bit naive.

First of all, if the appropriation of certain ideas was justified by the fact that they originally belonged to the christian tradition, what justifies the appropriation of ideas not belonging to this tradition? Secondly, Klapwijk does not indicate the criteria that should guide the selection of the ideas to be borrowed. The question in this case is: are all ideas equally suitable to be borrowed or re-appropriated? In addition, which criteria, agency or paradigm should control the subsequent process of transformation and re-shaping of ideas? Wolterstorff’s famous discussion of the necessity of “control beliefs” (e.g. 1989, 76) comes easily to mind in this connection, and one wonders why it has not even been mentioned by Klapwijk. Finally, from a reformational point of view Geertsema (1987, 161, fn. 33) asks: will the systems themselves survive such operation? Will they not be altered as well by the incorporation of altered ideas? Is it possible to “christianise” ideas that have originated in a pagan context? Will this transformation not simply consist in a synthesis between conflicting ideas? These are some of the crucial questions that Klapwijk’s proposal has prompted within the circles of reformational philosophy, without providing a fully persuasive answer.<sup>106</sup>

Klapwijk’s suggestions encourage a deep optimism concerning the possibility of scientific dialogue. Yet Klapwijk does not sufficiently take into account the difficulties of communication as explored by (e.g.) Kuhn and the historical school. Ideas, like physical organs, cannot always be transplanted into individuals belonging to different species. I am not trying to deny that “transformation” sometimes does happen in practice. A community may be stimulated to accept and modify ideas initially proposed by another community. But this shouldn’t be seen as the main or the most relevant “project” for an academic community.

Transformation should rather be regarded as *one of* the processes leading to the elaboration of important ideas in a scientific community. But I believe it is normally a minor process, and its occurrence *de facto* should not be confused with its validity *de jure*. To determine its validity

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<sup>106</sup> Cf. the critical reactions of Bos (1987) Groenewoud (1987) and Dengerink (1988).

and legitimacy the issues concerning the selection of “foreign” ideas, the agencies controlling the process, the compatibility with the context in which ideas are introduced, should be object of much more attention and clarification by the proponents of the transformational approach.

To add a final concern, I am afraid that Klapwijk’s emphasis on the transformation of ideas may not be properly balanced by an adequate emphasis on other aspects of the academic activity equally important for a community of thought. I am referring especially to *the devising of new ideas* or theories. In my opinion Klapwijk’s strategy is too much concerned with the reshaping of “pre-owned” ideas and not enough with the “devising” of new theories. For this, a new look at Wolterstorff’s initial contributions might be advisable.<sup>107</sup>

My critical remarks however, are not meant to deny that dialogue is a precious component of a credible science, and that Klapwijk’s project has generously tried to open new avenues from a reformational point of view. Only if there can be communication between scientists of different schools, different epochs and different sciences can there be scientific progress as well.

It is time to come to a conclusion. In this chapter I have explored the growing distrust concerning the possibility of true scientific communication and the theme of incommensurability. I have explored the allegations that it might not be possible to compare certain standpoints or to entertain a dialogue between certain schools holding to different presuppositions. In the diagnostic section of this chapter I have argued that the problem of the lack of communication or incommensurability is connected to the humanist ground motive of nature and freedom. The latter requires the existence of totally different frameworks of thought, languages, and finally of totally different worlds. This pluralism of points of view and languages is required by the motive of the free creativity of the knowing subject. The common ground of a creational order is rejected in favour of the recognition of a plurality of language games, frameworks and premises. The problem is that, in this perspective, the possibility of communication becomes increasingly more remote.

In the final part of the chapter (dedicated to the suggested alternatives) I have sketched Dooyeweerd’s philosophy of communication in order to provide an alternative to the dilemmas encountered in this chapter. I have argued that while it is necessary to recognise the significant differences between the pre-theoretical starting points shaping theoretical thinking, a radical antithesis is only possible at the deep religious level. When we consider the theoretical level,

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<sup>107</sup> In pages 59-103 of Wolterstorff’s *Reason within the Bounds of Religion* (the section containing a positive contribution towards an understanding of Christian scholarship) I have counted at least 32 instances (there could be more) in which the concept of “devising” (new theories) is mentioned. The term is almost constantly associated with “weighing” (evaluation) of theories. Unfortunately in Wolterstorff 1989 the word *devising* is not to be found anymore, and only the concept of *weighing* is discussed, namely the evaluation of existing theories to determine how compatible they are with a christian approach.

the antithesis can only be relative and therefore complete incommensurability must be excluded.

Furthermore I have argued that theories are only partially dependent on broader “paradigms”, presuppositions or worldviews. Likewise, concepts are only *partially* dependent on theories. Therefore, there always remains a possibility of comparing and criticising competing theories, arising from different presuppositions. While in the last section of this chapter I have issued a “call to prudence” with respect to a few themes proposed by Klapwijk’s transformational philosophy, I believe that on the issue of scientific dialogue reformational philosophy has delivered a good service to the broader philosophical community.

In the next chapter we will focus our attention on the theme of progress. By doing this, we will explore another fundamental theme for the assessment of the legitimacy and credibility of contemporary science.

## 5

# THE PROGRESS OF SCIENCE: EVOLUTION, REVOLUTION OR INVOLUTION?

“Progress lies simply in the eye of the beholder”

Thomas Kuhn  
(1970, 163)

### Introduction

Another much debated issue in the philosophy of science of the last quarter of the 20<sup>th</sup> century is the problem of progress. The latter is linked, among others, to the issue of scientific communication. In fact, we can speak of progress only if there is some form of continuity between the different generations of scientists and between the theories they propose. Such a continuity is broken if communication is not possible, if adopting different “paradigms” means to hold to incommensurable views. Adding another example, progress would not be possible if each revolution should cause the scientific community to cancel the previous achievements and to start from scratch. The issue of progress is also directly related to the main issue of this study, the issue of the legitimacy of science. The research that does not seem to make progress certainly does not present itself as credible and authoritative.

Once again, one can be quite amazed while considering how, in the humanistic tradition, the initial optimism towards progress was gradually transformed into the contemporary disillusionment. No doubt both Descartes and Bacon had in mind a very linear type of scientific progress. The right method would have provided an accumulation of data and observations granting the growth of scientific knowledge. The new theories would have explained new phenomena, while also explaining all phenomena already clarified by the older theories. It was a matter of accretion.

The Enlightenment confirmed this basic trust in science and progress and actually joined the two in indissoluble marriage. The positivist tradition continued to safeguard this marriage, and sharpened its methods to guarantee a continued accumulation of knowledge. Religion, in the opinion of many, was rapidly retreating in front of the triumphant march of science. It was clear

that it would have soon been chased away from the last shelters, where the light of science was not yet shining in its fullness.<sup>108</sup> It would be just a matter of time...

But as the 19<sup>th</sup> century reached its last decades it started to be clear that industrialisation had heavy social, moral and psychological costs. Science started to show a face of abuse and exploitation. Together with some advantages it brought new threats of destruction; new weapons, while new diseases appeared. The world had not become a safer place to live, on the contrary the 20<sup>th</sup> century knew sufferings of un-precedented scale and depth. In the two World Wars the most recent scientific achievements of the time were explored in view of a possible utilisation for military purposes. Could science perhaps even cause regress? If so, could it still be legitimised?

The main (although not always explicit) questions touched by the philosophers of science mentioned here, are whether scientific knowledge itself is growing, why and how it grows, what characterises its progress. A bit more specifically: are the new theories in a certain field always better than the older ones? If so, does it mean that our knowledge is increasing? In which way does science progress: by evolution, accumulation or revolution? What factors grant the possibility of progress to science: experiment, rationality, its methods?

In the present chapter I will examine some of these questions. In the first part of this chapter (from 5.1 to 5.4) I will illustrate the gradual loss of confidence in the idea of scientific progress in the contemporary philosophy of science. As in the previous chapters, I will then (section 5.5) look for the possible reasons for this pessimistic attitude. My diagnosis will focus on the relativist view of progress resulting from historicism. Furthermore, I will indicate the lack of a clear distinction between the notions of structure and direction as being responsible, in certain cases, for skepticism about scientific progress. In the final part of the chapter (5.6) I will also delineate some reformational notions that might counteract this drift towards pessimism. I will suggest that progress in science does not occur randomly but is linked to the modal structures of reality. Scientific progress consists in the "opening up" of a field of science according to specific methods and criteria and in agreement with the purpose of science.

In this chapter the discussion about scientific progress has been broadened to deal more in general with cultural and historical progress as well (e.g. 5.3.2). Without denying or downplaying the differences between the two domains (scientific and general progress), I

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<sup>108</sup> M.C. Smit, an eminent philosopher of history in the reformational tradition, elaborates this point in the following passage. "Christian thought often made the mistake of claiming the scientifically inexplicable part of the world for the domain of faith, with the fatal consequence that not only the area commanded by faith dwindled steadily before the triumphal progress of science, or at any rate came to depend on the scope of science, but also that people formed an altogether erroneous notion of the object of faith (...) Essentially, the mystery of faith vis-a'-vis science was identified with what science had not yet disclosed. Incalculable damage was thereby done to faith and science alike!" (Smit 1987, 80).

believe this approach is nevertheless permissible. Although the focus of this study remains on scientific progress, *science is part of culture* and in every philosophical system at least a certain coherence is expected between the view of scientific progress and the view of historical and cultural progress that are proposed. In this sense, a *broadening* of the perspective allows to compare the two views, to see the analogies (or eventual discrepancies) and to implement a more articulate discourse.

In addition, a *deepening* of the perspective becomes necessary when we want to look for the deep roots of the problems delineated above. To clarify the previous statement I would like to say that as we look for the roots of such problems, we often have to move from the philosophical level to the pre-scientific level of worldviews and ground motives. It is there that each philosophical theory of scientific progress puts down its roots in a deeper and more general (pre-scientific) idea of historical and cultural progress.<sup>109</sup> The latter underlies and shapes the views of a particular philosopher even when s/he theorises about scientific progress. This is why it will not be irrelevant, at least in some cases, to pay some attention to the basic conceptions of historical development proposed by some thinkers, and to trace their implications for scientific progress.

Before moving to the next section I would like to point out that there are a few distinctions and specifications that I miss in some of the discussions related to the progress of the scientific enterprise. There is first of all the issue of the aim, the purpose of science. We can only measure the eventual progress (or lack of progress) in relation to some specific goals that we think the scientific enterprise should accomplish appropriately. But this task, unfortunately, is not always indicated clearly, as Stafleu (1987, 152) and Feyerabend (1970, 202) lament.

There is a second distinction that I miss while listening to the various authors. Dealing with scientific progress one should distinguish between short-term and long-term progress. There is a progress leading to a single new theory or discovery. There is also progress in the long run, eventually revealed by comparing a series of theories and discoveries up to the present. Philosophers discuss both of them, of course, but very often without clarifying which type of

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<sup>109</sup> One might think for example of Popper's theories concerning scientific progress as having definite links with his political views. In the political area, Popper (e.g. 1962, 188) maintains e.g. that the tradition should be reformed rather than abolished (in science cf. his non-revolutionary views of progress). In the same passage quoted above, Popper also maintains that political progress results from open democratic debate (cf. mutual criticism in science) and so on. These analogies between scientific and political progress point towards a basic, pre-scientific idea of progress that is applicable to different areas.

progress they have in mind. Some easily take for granted that what is true in the “short-term” context must be true in the “long run” as well.<sup>110</sup>

Finally, it would be useful to distinguish the different “components” *within* the scientific enterprise. For example concrete research, the methods, theories and models, collection of data, the application of results in society and in academic circles (Duvenage 1985, 29-31). There might be several departments showing a certain progress, while others could stagnate and even regress. The expression *the progress of science* is often used in a rather simplistic and all-inclusive way.<sup>111</sup>

We will start our analysis with Karl Popper, a philosopher who still maintained a considerable dose of optimism about the progress of the scientific enterprise. This will provide a frame of reference against which the subsequent loss of confidence and the adoption of more pessimist and relativist views will become more apparent.

## 5.1 Popper: the evolution of scientific knowledge

### 5.1.a Trials, errors and new trials

Once again, we find Popper at the crossroads between the old and the new dispensations in philosophy of science. He is not part of positivism, he is not ready for irrationalism. Popper rejects, for example, the cumulative view of scientific growth which was typical of positivism and rationalism. He rejects the view that science is about collecting more and more observations (what he calls the “bucket theory”: 1979, 341ff.). To him science is rather a matter of selecting problems and providing new and better theories (the “searchlight” view).

Nevertheless, Popper maintains a rather linear view of scientific progress. He does not believe in any “law of progress” directing the scientific enterprise from within. Actually, in his view “it is much easier for us to regress than to progress” (Popper 1963, 365). There is no “law of progress”, no guarantee of progress and the attempt at founding progress on some inborn (psychical or social) tendency of the human nature is totally misplaced (Popper 1961, 152-159). However, Popper’s general view of science allows for the possibility of linear progress. The preface to his *Conjectures and Refutations* is opened by the statement: “we can learn from our mistakes” (1963, vii). This is the central theme of the book according to Popper. If we can learn from our mistakes knowledge can grow and science can progress.

We learn through criticism, according to Popper, by the “repeated overthrow of scientific theories” (Popper 1963, 215). The expression might make us think of a revolutionary process,

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<sup>110</sup> With respect to this problem it should be admitted that Kuhn is the one that distinguishes more clearly than others between long-term and short-term progress and discusses the two without confusing them. This is mainly due to his distinction between normal science and revolutionary science.

<sup>111</sup> The need to distinguish certain levels or “coordinates” within science will be discussed in 5.5.d and 5.6.a.

similar to the one described by Kuhn. But this is not what Popper means. In his view, both in science and politics, progress is brought about by reform, not by revolution (1963, 132). We should never try, therefore, to erase completely the tradition of science, trying to provide some imaginary *tabula rasa*. Traditions are always with us: we can never start from nothing, and we should not even try to reject our traditions completely. Those who try to do so, will only substitute the old tradition with a new one. In science the correct way is to criticise the tradition and to improve it, not to eradicate it.

According to Popper we can approximate the truth. We can substitute the old theories with new and better ones. We will never reach absolute perfection, the truth or any final point, but we can progress. Progress is possible and is linear, it is brought about by the application of the correct method: conjectures and refutations. The critical tradition, started by the Greeks (Popper 1963, 126), provides the perfect ground for the growth of scientific knowledge. Actually, science *must* grow, otherwise its “rational and empirical character” would vanish (1963, 240ff.). Growth is an essential characteristic of scientific knowledge.

What is the aim of scientific research? We already know that the aim of the “social” sciences is different from the aim of the natural sciences (Popper 1963, 124-125). Concerning the natural sciences, their task is “the search for truth (...) for true theories” (1963, 229). But Popper also specifies that we are not looking only for truth. “We want more than mere truth: what we look for is interesting truth (...) we want more truth and new truth” (Popper 1963, 229). We look, according to Popper, for answers to our *problems*. Only then research and conjectures become relevant for science.

What is truth then? Truth is “correspondence to the facts”, as Tarski understood it (Popper 1963, 224). According to Popper both this specific view of truth and the idea of testability “can shed much light on the idea of progress” (1963, 231). Can we approximate the truth more and more? Can we speak of better correspondence? Actually, according to Popper, we cannot do without this idea. And here the ideas of truth and testability of the content are combined into a third one: *the degree of correspondence to truth*, (in the sense of correspondence with the facts- cf. p. 231-233) or degree of verisimilitude (Popper 1963, 231-32). The latter is not to be confused with the idea of degree of probability (1963, 236). Verisimilitude means rather truthlikeness. It is possible to establish whether a theory T1 has less verisimilitude than theory T2. First of all their truth and falsity contents must be comparable. Then it can be affirmed that T1 has less verisimilitude than T2 if either the truth content (but not the falsity content) of T1 is smaller than T2, or the truth content of T1 is not greater than that of T2, but its falsity content is greater (Popper 1979, 52). In this way it is possible to establish which theory is closer to the truth.

With this, Popper has sketched his rather confident approach to the theme of progress. Yes, admits Popper (1963, 216), the history of science is a history of obstinacy and error. Yet in science (perhaps only there) errors can be criticised. This is why we have progress. In other fields of human endeavour there is often change, but seldom progress! On this point, Popper is rather ungenerous towards non-scientific activities and achievements. The dignity of the scientific activities, on the contrary, is openly declared.

In Popper's system it is not very important, it seems to me, whether the new theories are considered as complementary to the refuted ones (or at least compatible) or rather stand in opposition to the old theories. The continuity or linearity of progress is not dependent on this fact: the basic tradition, in any case, remains unaltered. The tradition of mutual criticism remains intact, and this is the deepest foundation Popper can think of. Therefore one can say that progress remains rather linear in Popper's philosophy. It is not like in Kuhn's scheme of things, where from time to time revolutions substitute old paradigms with new ones that will have a big impact on normal science. In Popper it would not be very important how refuted theories relate to the accepted ones. Actually we do not progress "from theory to theory" but rather "from problem to problem" (Popper 1963, 222). The continuity of the critical tradition and the continuity between problems remain the constant basis of scientific progress.

In any case, Popper tends to see the refuted theories as "stepping-stones", as preparations to the new ones. He does not really see them as "mistakes", as Kuhn (1970a, 11-13) suggested (on the basis that Popper repeatedly speaks of learning from our *mistakes*). The main point, for Popper, remains approximation to the truth. All theories approximate the truth to a certain degree. A refuted theory is not only an instrument for pragmatic purposes. It is also a stepping stone towards the truth (Popper 1963, 245). Refutation is not to be seen as a failure, according to Popper, not even for the scientist who produces the refuted theory. Refutation is not a failure, but progress! There is no progress without refutations (1963, 243).<sup>112</sup> Old theories are thus collocated in a linear path towards progress.

### 5.1.b Popper's view of progress

In conclusion, one could define Popper's view of scientific progress as "linear" (cf. Stafleu 1987, 152). However it is not linear in the same sense as the received view meant it. It contains an evolutionary element. The main direction, in the long run, is one of progress (the critical tradition constituting the constant basis). But there is also a continuous process of adjustment, small reformations implying the rejection of theories and the search for better ones. Scientific progress seems to reflect the biological evolution: the species are improved through the

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<sup>112</sup> Popper (1963, 240) shows his wit once again by observing that when old theories are refuted: "*de mortuis nil nisi bene*" (one can only speak positively of the dead)!

elimination of the weaker elements. Science does not just progress by itself, in a cumulative way. Nothing is automatic, on the contrary, progress is the result of a constant struggle.

Popper's view of progress also contains a pragmatic undertone. It does not promise that some ideal "truth" can be reached, but more modestly (and *usefully*), it allows practicing, conjecturing and falsifying. Experience does not give us the certainty of the final truth, yet only experience is the final arbiter of theories. The evolutionary line does not contain the possibility of reaching some final achievement or fulfilment point. Yet continued growth is granted, and we get nearer to the truth, a process which is useful enough.

But is not the growth of our knowledge, in practice, also causing a considerable growth of our ignorance? Surely we become more conscious of it? After every new answer that we can give, after every new discovery, we have a whole new range of questions that emerge. On this point Popper maintains a view that sounds judicious: it is worthwhile to learn more, even when it implies that we become more conscious of how little we know! (Popper 1963, 29). Popper can therefore maintain a considerable optimism. Science is legitimate and authoritative because it progresses, and its progress shows that it is credible. Certainly it does not progress in the way imagined by the positivists, but its credibility can be safeguarded to a large extent. Once again, however, the atmosphere created in philosophy of science was rapidly changing. And changes of attitude are sometimes underlined by silence. In Polanyi's writings, for example, we don't find long or specific sections dedicated to the theme of scientific progress.

## 5.2 Polanyi on progress

### 5.2.a *What causes scientific progress?*

Polanyi might appear as being not extremely interested in the issue of progress. The reason might be that a rather "conservative" current permeates his system of thought. He speaks of tradition more than progress, and of authority and foundations more than innovation. On certain occasions it seems as if he is not concerned about the theme of progress. Yet one should not forget Polanyi's interest in the theory of evolution. In his universe everything seems to be involved in a process of "emergent meaning and truth" (Polanyi 1946, 17). The theme of evolution is not explicitly linked to the theme of progress, and the relationship between the two is not elaborated in detail.

Most of the work of Polanyi on this issue is devoted to demonstrate that the progress of science (especially "short-term" progress as a consequence e.g. of a new theory) is *not* due in particular to rationality or impersonal observation. Perception selects in a way that is not always or fully controlled by the perceiver (1946, 11). Scientific progress, for Polanyi, is less rational than Popper believed. It requires a previous faith, a *fides quaerens intellectum* (Polanyi 1946, 15). If there are specific rules for invention or discovery they cannot be dictated by

rationality: they must be “rules of art” (Polanyi 1946, 13-14). They are learned by example, rather than prescribed rationally. To learn by example implies submitting to a certain tradition, to accept a certain authority (1946, 15).

The part played by observation and experiment, in the process of discovery, is usually over-estimated, according to Polanyi (1946, 28). Discovery does not always occur at the culmination of a mental effort, but often after a period of rest and relaxation. One should not rely too much on rationality: on the contrary too much rationality causes the regress of science. Why is scientific progress sometimes hindered? Because the rigid procedures of falsification are applied too hastily to theories that seem to be “contradicted” by some type of evidence. Theories can be abandoned too soon, with great damage for the scientific enterprise (Polanyi 1946, 92).

One prominent aspect of his view of progress is his critique of the doctrine of doubt. Doubt is just equivalent to belief, says Polanyi (1958, 272ff.), there is nothing magic about it. It can be reasonable but also unreasonable to doubt certain theories or results (1958, 274). A scientist “can be strictly agnostic only on subjects of which he knows little and cares nothing” (1958, 276). Doubt does not guarantee the progress of “normal” science, therefore, neither does it help the progress during “revolutionary phases”. Neither doubt nor belief can be recommended at any point on the path to discovery (Polanyi 1958, 277).

Having discussed what according to Polanyi *does not* necessarily cause progress, we must also remember what Polanyi considers the most crucial factor for progress. It is personal commitment (Polanyi 1958, 123) which allows to bridge the gap between a hidden reality and a committed knower (cf. 2.2.c). Once again the key-factor is not a procedure, a method or an impersonal function, but a committed person. The next question we should ask Polanyi, is whether in his view scientific progress is linear, evolutionary or revolutionary in character.

### 5.2.b *Linear or revolutionary progress?*

Polanyi’s philosophy seems to oscillate between a linear-evolutionary and a more revolutionary view of progress. On the one hand it is possible (speaking of the “long-term”) to build on the previous achievements of scientists. Science knows continued growth. The past achievements remain valid for the present in many ways (1946, 89). Much of the old science is rejected today, but much is still considered true. Here the doctrine of a common ground brings Polanyi to affirm that “the modern premisses of science include a great deal of the old premisses” (1946, 90). Progress is therefore linear to a considerable extent, safeguarded by a common tradition and a community of scientists. Here one can recognise his “conservative” line.

But on the other hand, as we know, there is another line in Polanyi’s philosophy, and it proposes progress as something more revolutionary. In his preface to *Science Faith and Society*

(written in 1970) he affirms that progress does not simply change the *interpretation* of facts, while the accepted facts would remain the same (1946, 11). Progress seems to change the facts themselves. Unfortunately the discourse is not fully developed. However, even in *Personal Knowledge* Polanyi (1958, 165) insists that if the “giants of the past” were to return today they would not feel totally out of place in modern science. Two lines, the conservative and the postmodern one, seem to intertwine again, but it is not easy to decide which one prevails.

In general Polanyi does not devote a lot of attention to the issue of scientific progress. He seems to reserve his discussions on progress especially for the context of politics, where he denounces the reactionary character of marxism. Of course the concern for scientific progress is not totally absent from such political discussions. The progress of science, according to Polanyi, requires a free society where science is treated according to its true nature, not enslaved to political purposes. It is to be observed however, that unfortunately he doesn't provide a clear-cut definition of a specific *task* of science.

What we gather from Polanyi, in conclusion, is that faith in progress seems not to be very helpful. The latter is weakened in his philosophical system, as the importance of rationality itself is reduced. What generations of scholars regarded as the purpose of science, namely progress, is more or less left in the background by Polanyi. What even Popper regards as one of the essential characteristics of science, is more or less discussed *en passant*. The authors that I will examine in the following sections criticise more openly the linear view of progress, showing at the same time a growing skepticism about progress.

### 5.3 Kuhn: revolutionary progress

#### 5.3.a Normal science and its progress in *The Structure of Scientific Revolutions*

If Polanyi “forgets” to tell us about the aim of science, Kuhn is reproached by Feyerabend (1970, 202) for doing the same. In fact, Kuhn seems not to offer a specific declaration on the main purpose of science. Yet it is perhaps possible to deduce his view from his works. Certainly we must start by distinguishing the normal and the revolutionary phases of science. For normal science the aim is “puzzle-solving”, while the aim of revolutions is something like “exploration” of new directions. There is an instance where Kuhn mentions the two together and even says that (although their relationship is one of tension) they both characterise science (Kuhn 1963, 368).

However, in the context of the demarcation (between scientific and pre-scientific) especially normal science was taken into account<sup>113</sup> and suggested the criterion (i.e. puzzle-solving). In the discussion about progress, on the contrary, it is revolutionary science that is especially taken into account to clarify the nature of scientific progress. This is why a question like: “how are

we to understand the way in which science does progress?" is placed right in the center of a discussion concerning revolutionary paradigm changes (Kuhn 1970a, 19). Kuhn regards scientific progress not as linear or cumulative (i.e. as in normal science) but as revolutionary.

But in order to reach the above conclusion, is Kuhn not forgetting the developments within *normal* science, which he often admits to be cumulative? (1970, 52; 96. Cf. 1970b, 250). According to some of his own statements, there is indeed progress in normal science and Kuhn defines it as "linear", "additive" or "cumulative" (Kuhn 1970, 52-53; 96). Progress in normal science "seems both obvious and assured" (1970, 163. Cf. also 1970b, 245). Why should this not be taken into account in his definition of progress?

At this point we are confronted with another rather puzzling question. Is Kuhn really sure that there is progress in normal science? In fact, in some instances Kuhn seems to question his own admissions on this point. For example when he asks: who decides whether there is progress? Usually a certain community of scientists, those who determine the "rules of the game" (Kuhn 1970, 168). What else could they see? They work together under the same paradigm! (1970, 162-163). Of course they see progress in their field. But an external observer, who does not share the premises of the specific group involved in that specific (normal) research, might not see any progress (Kuhn 1970, 162). If I summarise Kuhn's views correctly on this point, the progress of normal science should be regarded as *relative*. In fact, both the premises and the phenomenal world of the particular group involved in normal science are often accepted only by that specific group. Progress is not totally absent in normal science, but it does not enjoy universal consensus. However, the eventual lack of progress would not detract anything from the scientific character of a discipline. In fact, Kuhn does not accept Popper's view that progress is simply one of the most fundamental characteristics of science.

"we tend to consider science any field in which progress is marked. There remains the problem of understanding why progress should be so noteworthy a characteristic of [such] an enterprise..." (Kuhn 1970,162).

Nowadays we even have difficulties, says Kuhn, in distinguishing science from technology because they both show progress. On the other hand, we tend not to consider the humanities as full sciences because their progress is not so obvious (1970, 160-161). Kuhn's question is penetrating: "does a field make progress because it is a science or is it a science because it makes progress?" (Kuhn 1970, 162). Having reflected on the above questions and themes, we are obviously invited to conclude that progress does not necessarily or automatically characterise true science.

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<sup>113</sup> See my discussion in 3.3.a

Progress is a puzzling topic. Kuhn explains for example that the lack of progress in many areas is actually assessed according to disputable criteria. One of them is the ability to represent the real. Painting has been considered a “cumulative” exercise for centuries. Only when artists abandoned the goal of representation did the gulf between painting and science become more relevant (Kuhn 1970, 161). Concerning the humanities, what people mean when affirming that they show no progress, is that there are many schools within each discipline. From within one school it is impossible to acknowledge the progress in another (Kuhn 1970, 162-3). The “external” observer, on the other hand, does not imply that Aristotelian philosophy (for example) has never progressed. He means that there is no full consensus between competing schools, and this seems enough to prove that there is no progress at all. We should rather conclude that there is a relative type of progress.

It is true, says Kuhn, that in the natural sciences there is more consensus than in the humanities. But it is in many respects due to indoctrination, to submission to a single paradigm, to the lack of knowledge of other paradigms. The “insulation” of the scientist from “the demands of the laity and of everyday life” (Kuhn 1970, 164) also plays a considerable role. Students in the humanities are actually in a better position: they are confronted with different paradigms and can realise the revolutionary phases in the history of their disciplines (Kuhn 1970, 164). In the natural sciences, on the contrary, normally the textbooks lead students to see science as a cumulative enterprise. The scientists are presented as working on the same set of problems for generations. Revolutions, in this way, become “invisible” (1970, 138-139) and only linear progress remains visible. But according to Kuhn, this linear progress attributed to the natural sciences is much more relative than we used to suppose.

### *5.3.b Revolutionary science and its progress in *The Structure of Scientific Revolutions**

The growth of scientific knowledge is not cumulative but revolutionary. And yet Kuhn warns us that in the long run there is no real progress, or at least not towards any final goal or truth. Progress, like beauty, is only “in the eye of the beholder”: more or less an illusion (Kuhn 1970, 163). The changes of revolutionary science do not imply progress. Once again, it is those who have supported the revolution that recognise the progress (Kuhn 1970, 166, see also 169). What else can be expected from them? But in practice, revolutions are non-cumulative (Kuhn 1970, 92). The previous paradigm cannot be considered a mistake (1970, 115) and its abandonment and substitution by a new paradigm cannot constitute progress.

But on this point Kuhn’s position becomes very nuanced (some would say contradictory?) Revolutions, he admits, are linked to some kind of evolutionary development. Progress from revolution to revolution cannot be totally denied. Scientific progress is not what we used to think it was, but it accompanies science (as Popper believed) as long as science is alive (Kuhn

1970, 170). And yet, says Kuhn (now in disagreement with Popper), paradigm changes do not carry scientists *closer* to the truth. New theories are better than the previous ones, but not in the sense that they *represent* nature better. Only in the sense that they are better instruments for puzzle-solving. In fact Newton improved on Aristotle and Einstein on Newton. But only insofar as they produced better instruments for puzzle-solving.<sup>114</sup> For the rest, says Kuhn (1970, 206), there is “no coherent direction of ontological development”. In some cases Einstein is closer to Aristotle than to Newton.

We can still, in this context, maintain the idea of evolution, provided there is no goal (Kuhn 1970, 171). As Darwin eliminated the idea of a goal from natural selection, we should eliminate it from scientific development (Kuhn 1970, 172). Contrary to Popper, Kuhn does not admit any gradual approximation to the truth. We should learn, according to Kuhn, to consider evolution from its point of departure, rather than from its hypothetical goal. In science we have a selection of the fittest (paradigm) by revolution. Here Kuhn (1970, 172) introduces the interesting idea of a “revolutionary selection”. He had already associated *revolution and evolution* in previous passages (1970, 92-93). For Popper the two ideas remain much more distinct, and probably opposed. It would be interesting to ask to what extent, from a philosophical point of view, the two ideas are compatible.

### 5.3.c Further development of Kuhn's views

In his writings after *The Structure*, Kuhn did not introduce radically new elements in his view of progress. As we have seen in chapter 3, he “softened” the idea of radical fractures between revolutions by recognising more elements of continuity and comparability. But although a certain “moderation” of specific traits must be recognised, it can be argued that Kuhn also maintained and defended the fundamental ingredients of his initial view. Linear progress accompanies the phase of “articulation” of a paradigm in normal science (Kuhn 2000, 13-14) 18-19; 29) until consistent anomalies lead to a crisis. He maintains the existence of scientific revolutions and does not accept the attempt of some of his critics at reducing the scope and radicality of revolutionary moments by pushing “the case for continuity too far” (Kuhn 2000, 56). If we can speak of progress through revolutions, we can only point to better instruments for puzzle-solving (Kuhn 1979, 418). But no clear goal or itinerary should be envisaged and the idea of a gradual approximation to the truth should also be rejected (e.g. Kuhn 2000, 85-86).

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<sup>114</sup> On this issue, one might observe a pragmatic undertone in Kuhn, similar to the one observed in Popper (see 5.1.b)

#### 5.3.d Kuhn's view of progress: towards an evaluation

In some of his initial writings Kuhn insisted that his view was still an evolutionary one and he invited the reader to acknowledge the many links with Popper's conception of progress (Kuhn 1970a, 1-2). Yet in Kuhn's view the evolutionary line of progress is constantly interrupted by the fractures brought about by revolutionary changes (which are not to be found in Popper). One can say that Kuhn's line of progress resembles the one designed by Karl Marx: progress is granted by subsequent revolutionary phases. In Marxism, however, the goal and destination of progress is clearly indicated from the start.<sup>115</sup> More recently a similar position is held for example by Capra, who indicates the direction and the phases of world progress on the basis of ancient Chinese wisdom (Capra 1984, 1-35).

But with Kuhn we have neither final goal nor clear direction. His view of progress is more similar to the one elaborated by Toynbee in his *A Study of History* (Toynbee 1935-1961). Each civilisation emerges, reaches its apex, and goes towards its decline. In its final phase however, it contains in itself the seed of a new beginning. The new phase will be radically different, yet linked to the previous one in a seminal way. In some instances Kuhn seems to reject this continuity (between paradigms), but basically some form of continuity is admitted (cf. Kuhn 1970, 169). In addition, for both Toynbee and Kuhn no clear purpose or destination can be delineated. We cannot even speak of a cyclical view, as we might find in Capra.

Although it would be interesting to expand these historical notes and systematic comparisons to a much larger extent, the purpose of this section is not primarily historical. Therefore a few brush strokes should be sufficient, just to give a very basic indication of Kuhn's position.

In conclusion we can say that, in comparison to previous thinkers, the possibility of progress is further weakened in Kuhn. Basically, progress seems to be limited to an increased ability in puzzle-solving provided by each new paradigm. But it must be remembered that after each revolution there are losses as well, due to the abandonment of the previous paradigm. In general, there is no gradual approximation to the truth. Once again, an even more radical position will be offered by Feyerabend.

### 5.4 Feyerabend and beyond: involution?

#### 5.4.a A few illusions about science and progress

As I indicated before, Polanyi does not indicate clearly the purpose of science. Also Kuhn, according to Feyerabend (1970, 202) fails to indicate the goal of science. Now Feyerabend is in his turn reproached by Stafleu (1987, 152), for the same reason! As I said earlier, to know

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<sup>115</sup> Kant also adopts a similar view and divides the history of humankind into four phases, culminating in a peaceful future with a league of nations living under international law. Venter (1999, 14 ff.) notices the

the purpose of science is necessary in order to assess its eventual progress. In Feyerabend, an explicit declaration on this topic is missing. Yet one should not forget that in his view: “the happiness and the full development of an individual human being is now as ever the highest possible value” (Feyerabend 1970, 210).

He says this while speaking about science, and I believe he considered this to be the ultimate aim of the scientific enterprise. If this is true, it means that progress must be seen in a different light. The aim of science is no longer internal to science, but external. What is progressive or reactionary must be determined in relation to the happiness of the individual. Whether science shows an “internal” progress or not, in relation (e.g.) to its own interests, is rather indifferent.

However, Feyerabend does discuss this “internal” progress of science. First of all, he agrees with both Polanyi and Kuhn on the observation that progress (in this case “short-term”: e.g. a new discovery) is not generated especially by rationality or by rational methods. According to Feyerabend science is the result of passions, of views that one wants to defend. It progresses by adjustments, *ad hoc* hypotheses and even unjustified resistance against refutations. Actually rationalism and empiricism, by trying to make science more precise and rational, end up preventing progress. Science progresses via errors and chaos. Without chaos there is no progress (Feyerabend 1975, 179). Progress is made possible by anarchism and by those who “unwittingly used this philosophy” (1975, 190). We might advance science not by induction or deduction but proceeding counter-inductively: by developing hypotheses that are inconsistent with both established facts and theories (Feyerabend 1975, 29ff.). Only in this way did (e.g.) the heliocentric theory emerge!

This can be considered Feyerabend’s view of progress in the short term, in “normal science” so to speak. What about progress in the long run? Feyerabend does not forget to discuss the relationship between old and new theories. The “official” view of this relationship, he notices, prescribes a few beliefs. First of all, the new theory incorporates in itself the “successes of the old theory”, which become part of the truth content of the new theory. What are rejected are the failures of the old theory. But in the new theory we also have additional material: the new predictions which allow new research and development. All this, says Feyerabend (1975, 174), is rather implausible.

In fact, the relationship between the new and the old theory might be totally different. The new theory might simply incorporate the old one into itself, by enlarging at the same time its explanations and predictions (Feyerabend 1975, 177). In this case, it would be mainly a matter of the new theory being “broader” than the old one. It might also be that the new theory, while incorporating the old one and expanding its scope, continues to leave unexplained the same

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tension between the kantian notions of human freedom on the one hand, and laws deterministically directing historical development on the other.

area that was left unexplained by the old theory. All this is in principle possible, says Feyerabend (1975, 178).

But in fact the relationship between the two theories follows a different model. The latter can be represented by two overlapping circles, symbolising the two theories. The intersection area represents “the problems and facts of the old theory which are still remembered and which have been distorted so as to fit into the new framework” (1975, 178). For the rest, the two theories cover different areas and have nothing else in common.

It is an illusion, says Feyerabend (1975, 178) “which is responsible for (...) the demand for increased content”. Here Feyerabend gives an erudite explanation while discussing why “this illusion” affects Lakatos in his appreciation of “progressive changes” from Copernicus to Einstein. In Feyerabend’s view Lakatos commits the mistake because he concentrates only on kinematics and omits dynamical and optical problems (Feyerabend 1975, 178, fn. 6). The new theory does not increase, according to Feyerabend, the content of knowledge with respect to the previous one. We can still imagine a link between the two theories, but the content that the two theories have “in common” is distorted in order to fit in the new theory. The result is not progress, but proliferation.

According to Feyerabend this view of science is desirable because it eliminates from the picture the arrogant claims of reason.

“We have to conclude then, that even within science reason cannot and should not be allowed to be comprehensive, and that it must often be overruled, or eliminated, in favor of other agencies” (Feyerabend 1975, 179-180).

The role of reason is reduced, both as the cause of single achievements and of progress between successive achievements. But is there still progress? Feyerabend speaks often of progress, and by indicating “other agencies” as relevant, one might have the impression that he still supports the idea of a progress. Yet we also know that Feyerabend often speaks of rationality, or communication, in order to “undermine” such ideas. Does he use the same technique for progress as well?

#### *5.4.b Types of science and of progress*

For Feyerabend, the Western type of science is not the only possible one. Popper (and most historians of science with him) are convinced that “science did not start before Greece and that scientific results can only be obtained with the scientific method as it is practiced today” (Feyerabend 1975, 49, fn.7). This is due especially, says Feyerabend, to the ignorance of those studying ancient cultures. Yet more recently, due to more accurate research on this topic, we have to admit for example the existence of an “international paleolithic astronomy, which gave

rise to schools, observatories, scientific traditions” (Feyerabend 1975, 49-50, fn.7). These theories were expressed in sociological, not in mathematical terms, and the traces of their achievements were left in sagas and legends. But they should be considered as effective and legitimate as our scientific activities.

Feyerabend observes that progress has not been achieved only by western science and its methods. It should be observed that Western science did not achieve its results only by itself. It is heavily indebted to the efforts of herbalists, alchemists, those who observed the stars in antiquity and so on. Are they also scientists? I am not sure how Feyerabend would answer this question, but he would probably say that they achieved results. As we have seen, the distinction between scientific and non-scientific is blurred in his system. We meet this problem here again, because science cannot be distinguished from other activities *achieving results* and providing satisfaction to individuals...

In medicine, for example, Voodoo has a firm material basis, though not sufficiently understood at present. Chinese traditional medicine was re-instated in communist China after a period of dominance by Western medicine, and nowadays acupuncture, moxibustion and herbal remedies are practiced even in the West (Feyerabend 1975, 50). “Primitive tribes have more detailed classifications of animals and plants than contemporary scientific zoology and botany, they know remedies whose effectiveness astounds physicians (...). There was the domestication of animals, the invention of rotating agriculture, new types of plants were bred (...) we have chemical inventions, we have a most amazing art which can compare with the best achievements of the present” (Feyerabend 1975, 306-7). Feyerabend reminds us also of the building of pyramids and of Polynesian travels. This type of science:

“was factually adequate *as well as* emotionally satisfying, *it solved both physical and social problems* (one cannot say the same about modern astronomy) (...) True, there were no collective excursions to the moon, but single individuals (...) rose from sphere to sphere until they finally faced God himself in all his splendour” (Feyerabend 1975, 306-7).

Feyerabend’s views might seem extravagant, yet they form a coherent whole. His view of science fits perfectly with the rejection of the distinction between science and non-science, with his definition of science, with the aim of science (the satisfaction of human beings), with the desirability of a “humanitarian” attitude in science. It also fits with the conviction that beliefs and worldviews should have precedence over evidence.

#### 5.4.c Feyerabend and scientific progress: an evaluation

When Feyerabend’s (1975, 174-178) view of the relationship between the old and the new theory is considered carefully, it shows that progress, at least in the traditional sense, is not

implied in the picture. He cannot join Kuhn in saying that “later scientific theories are better than earlier ones” (Kuhn 1970, 206). Progress and regress live side by side and sometimes they are not clearly identifiable. We should also remember that discussing his view simply in terms of “the new and the previous theory” means to apply categories which are typical of Popper or Kuhn. In their systems, *one* new theory supersedes the old *one* and reigns for a period. With Feyerabend the older theory can always resist falsification and continue to live alongside the new one. In this way we have a *proliferation* of theories and convictions, not simply a succession. In this view, in principle, progress is substituted by proliferation.

The traditional idea of progress disappears from Feyerabend’s system, especially insofar as the internal progress of science is considered. We are left with an assessment of the impact of science on society. Science can be said to progress or to regress according to its ability to provide the “happiness of the individual” (Feyerabend 1970, 210). The question is not: “is science achieving new or better results?” Is it at least solving its own puzzles better? The question is rather: “does it contribute to my satisfaction”? Is it “humanitarian” or totalitarian? When considering the consequences of science on society we can imagine two types of science, and two types of progress. The one will be humanitarian science and progress (which means also legitimate), the other will be a science and a progress which contribute nothing to our happiness.

In this context, Feyerabend’s merit consists in having discovered what some reformational thinkers call the “direction” of science. He points out the importance of the quality of science, its consequences, its influence on man and society. Feyerabend has found the directional dimension, yet he cannot find the structural one. He cannot see that there can be progress even when there is no immediate happiness or development of the individual (Feyerabend 1970, 210), even when it poses new questions or problems in the ethical, economic or social spheres (cf. 5.5.d). In this limited sense, his view is not complex enough. Feyerabend measures the progress of science only according to a meter that is external to science, (i.e. in its relation to society). There is no longer an internal, scientific measure to assess scientific progress.

However, even when the external measure is used, modern science is found by Feyerabend to be regressive. In Feyerabend’s view modern science represents mostly an involution, even a regress<sup>116</sup> with respect to previous achievements. This is due to the fact that modern science is no more humanitarian, it does not take into account social or psychological needs. It is rationalist and sterile, depending on the myth of objective knowledge, which is enforced on everybody by the most undemocratic means. Although this does not mean that progress is not

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<sup>116</sup> Once again Feyerabend’s view is more radical than others, but it was Popper who started to discuss the possibility of the regress of science (1970, 57-58) and said that it is easier for science to regress than to progress (1963, 365). Kuhn (1970a, 20-21) also asked the question whether it is not possible that modern scientists know less than those living in the 18<sup>th</sup> century.

possible, it means contemporary science constitutes an involution. It is definitely not legitimate science. The late-modern (humanist) reflection upon science, starting from linear progress, has reached a point where it takes seriously the idea of involution.

#### *5.4.d The late-modern philosophy of scientific progress: concluding remarks*

Considering the recent developments in philosophy of science, I believe one is allowed to speak of a loss of confidence in the progress of the scientific enterprise. This loss of confidence implies a certain challenge to the credibility of science. The symptoms of the malaise can be stated simply: progress is gradually disappearing from the horizon of the late-modern view of science. In Popper we still have a certain trust in an evolutionary, linear type of progress. Scientific knowledge is likely to grow, and with it its results and applications. Popper's philosophy, after due modifications, left in place much of the positivist system. However, Popper started to divorce the idea of progress from the idea of truth or certainty and this seems to represent a crucial change of direction. With Popper (1963, 223), it becomes possible to speak of progress without reference to truth. Popper, however, conserved the plausibility of progress to a large extent by introducing the concept of approximation to truth or verisimilitude (see 4.1.b). In this way he could also safeguard the legitimacy of a progressive scientific enterprise.

With Polanyi, Kuhn and especially Feyerabend, a rather skeptical attitude towards progress emerges. Polanyi is not extremely interested in the issue, while Kuhn eliminates approximation to the truth from the trajectory of progress. In Popper the regulative idea of truth and verisimilitude are considered essential. But Kuhn abolishes the idea of approximation to truth and sketches a progress without a precise goal, developing from its initial point of departure but without a clear direction or destination. Feyerabend theorises about a plurality of sciences and progresses. His emphasis on the "happiness" of the individual (as the goal of science) brings him to the conclusion that Western science is an involution with respect to previous achievements.

A broad overview to see what the landscape looks like after Feyerabend, shows that the skeptic attitude towards progress has not changed much. On the contrary, it has often been expressed even in more radical terms. With Lyotard, for example, science is supposed to be involved in a continuous revolution, to deny again and again its achievements, to deal with undecidables, to break down consensus and rather to look for dissension. Postmodern science theorises its own progress as catastrophic, non-rectifiable and paradoxical (Lyotard 1984, 60).

The notion of progress seems to be disappearing from the horizon of late-modern philosophy of science. Taking into account the dooyeweerdian analysis, Lyotard's position represents one of the most dramatic phases of the dictatorship of the freedom ideal upon science. Will a return

to the pole of nature, obviously in a new version, be really impossible in future? Will rationality and progress regain the lost prestige? Contributions like the one by Newton-Smith (1981) seem to allow even for this possibility. Of course the reformational thinker cannot augur any revival of rationalism or positivism. It would still conflict with the basic tenets of the reformational approach. For the moment however, we have to deal with the present wide-spread loss of confidence in progress and attempt a diagnosis in the following section.

## 5.5 Why is progress doubted? Towards a diagnosis

### 5.5.a Dooyeweerd: *progress and the ideal of freedom*

Dooyeweerd has attempted to analyse the complex reflection of humanist philosophy on historical and cultural progress. In this section I will illustrate his basic approach to cultural progress in general while in the next one I will apply some of his insights to the more specific topic of scientific progress.

Basically, according to Dooyeweerd, the history of modern culture evidences a gradual loss of a sense of historical purpose and direction from the second half of the 19<sup>th</sup> century. The cultural movements taking as a starting-point the nature-pole of the humanistic ground motive usually adopted a more “progressivist” attitude, especially in politics. They also nurtured a robust faith in progress. Of course there are differences that should not be overlooked. Dooyeweerd (1984, II, 349-50) distinguishes for example between the 17<sup>th</sup> and the 18<sup>th</sup> centuries’ Enlightenment. But in general the idea of a steady progress under the guidance of reason remained characteristic of the Enlightenment. On the contrary, the cultural movements inclined to the freedom motive (e.g. the “Restoration” in European politics and Romanticism in philosophy) have been rather conservative, and even reactionary in their most radical versions. They have attenuated and even rejected the faith in progress, by depreciating the ideal of science and the trust in reason.

Within the reformational tradition, those who prefer to characterise the humanistic ground motive as implying a dialectical tension between *nature and culture* (see fn. 94) can reach very similar conclusions. For example Venter (1999, 14) argues that in the Enlightenment period “progress is the movement of history to ever higher levels of civilization; one could say it is the level of dominance of reason [i.e. culture] over nature”. Concerning the movements linked to the opposite pole Venter (1999, 23) argues: “the inversion of progress (the idea of a return to nature or the dominance of the natural) became stronger as irrationalism took hold. (...) in some cases this ended in a pessimistic atmosphere of decline (Spengler); in others in an activist attempt to create progress (Pragmatism)”.

Coming back to Dooyeweerd, the dialectics between nature and freedom is not that simple, of course. One must also consider the attempts at finding a synthesis between the two opposite

poles, which occurred for example during the Romantic period. Then progress was welcomed under the control of the freedom-pole and regarded as causing an organic development of the free personality and national communities. Progress could then be accepted because it was not considered as linked to the inexorable mechanisms and laws of nature. It was rather regarded as being organic, as following a natural necessity, a creative process. Free personality and necessity were thereby combined (Dooyeweerd 1980, 74ff.).

However, the most nihilistic tendencies of the freedom motive in the long run manifested themselves quite clearly. These radical tendencies do not appear yet in the philosophies of Comte or Marx. A goal of history was still maintained there, as well as the idea of progress. But in Nietzsche nihilism tends to become a real threat. Dooyeweerd (1980, 81 and 111) in this context also mentions the philosophies of Dilthey, Spengler and Schelling.

The dooyeweerdian point of view, therefore, is that the gradual loss of confidence in progress can be regarded as a consequence of the fact that the philosophies observed above increasingly relied on the freedom-pole of the humanistic ground motive. This relativistic attitude is regarded by Dooyeweerd as leading to historicism, i.e. the absolutisation of the historical aspect of our experience.<sup>117</sup> The absolutisation of the historical aspect causes a thinker to view all of reality as involved in a constant process of historical change and sight is lost of the constant and stable structures of created reality. The latter are regarded as the merely temporary products of a particular culture in a particular time and context. As a consequence, for the thinker caught in the historicist way of thinking there are no more constant norms or universal laws. The link between progress and created order is lost.

Historicism has not only been inclined to the “abolition” of universal norms, but has also often *substituted* those universal norms with something more individual or typical. For example, during the Romantic period, the unique tradition of a particular nation became the norm according to which the historical development of a certain culture was supposed to be shaped. We will see that it is plausible to envisage similar “substitutions” in contemporary philosophy of science as well.

The consequence of elevating tradition to a norm was a rather conservative attitude which is the opposite of the faith in progress that had nourished movements like the Enlightenment (i.e. linked to the opposite pole of the humanistic ground motive). Under the hegemony of the ideal of freedom, the historical development is not seen as proceeding according to universal laws or

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<sup>117</sup> Basically, we can say that Dooyeweerd uses the term historicism to indicate relativism, i.e. as the result of anchoring philosophy to the pole of the autonomous personality (i.e. subject). Admittedly, this is a rather unique use of the term. Historicism has a different meaning (e.g.) in Popper. What Popper means by “historicism” is the view that historical prediction is the aim of the social sciences and this aim “is attainable by discovering the “rhythms’ or the ‘patterns’, the ‘laws’ or the ‘trends’ that underlie the evolution of history” (Popper 1961, 3; see also 1962, 3-8).

according to a rationality hidden within history. The rationalistic tendencies of the Enlightenment are replaced in historicism by irrationalist and relativist tendencies.

#### 5.5.b *Historicism and progress in recent philosophy of science*

The previous analysis provides a framework to understand the late-modern approach to progress<sup>118</sup> in philosophy of science. As contemporary philosophy of science abandoned the positivist views, it gradually came under the influence of the ideal of freedom. This gradually required the abandonment of the optimistic views, so typical of the previous eras. Scientific progress had to be redesigned according to the new demands of the ideal of freedom.

Now scientific progress was gradually made independent from any universal order. For example, according to Kuhn (1970, 171-72) progress proceeds knowing its point of departure but deprived of any “ontological development” or simply any “goal”. Progress does not bring us closer to any truth, it only creates increasingly useful tools to solve our puzzles.<sup>119</sup> Having lost its link to the truth and to a universal order, the aim of this progress is to satisfy the needs (social, psychological etc.) of the individual (Feyerabend). The happiness of the individual in this case becomes the new subjective “norm”, substituting the universal norms that have been rejected. In other cases the community becomes the true *locus* of order for science and therefore for scientific progress as well. In fact, the historicist turn leads to an emphasis on the scientific community.

In the end it is this community, as Botha aptly states, that becomes the “initiator and sanctor of the legitimacy of scientific knowledge and language” (Botha 1994, 21).<sup>120</sup> Progress itself should be sanctioned by the scientific community. But, as the scientific community usually does not reach universal consensus on the recognition of the progress of a particular school or within a particular field, progress tends to disappear from the horizon of many contemporary philosophies of science. Progress becomes mainly an impression, a prejudice of the communities working under the same paradigm, or supporting the same revolutionary change (Kuhn). What remains is the postmodern carnival of a plurality of views, ancient and modern, “achievements” and pretensions, a proliferation of beliefs and views (Feyerabend) and language games (Lyotard).

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<sup>118</sup> Referring to my footnotes 13 and 14 on the links between modernity and postmodernity, it is interesting to notice that Dooyeweerd traces the roots of this skepticism towards progress within the modern era itself, not only in the so-called postmodern period, in which the optimistic views of modernity concerning progress would be rejected.

<sup>119</sup> A pragmatist view of progress has been supported especially by Laudan (1978, 121ff.), who exaggerates the importance of problem-solving in science. In his view problem-solving is virtually the only aim of science while he denies that science searches for truth. According to Stafleu (1987, 235) Laudan overlooks the widespread scientific belief that the solution of a problem is satisfactory only if it is considered a *true* solution.

Postmodern philosophy has taught us that reality is plunged into a historical flux of constant change: changing historical situations, the incessant flux of linguistic meaning and constantly altering social practices. Our culture is impregnated with an image of constant flow excluding anything enduring and persistent. How ironic that in this change there is no room for progress. To admit progress would mean to recognise something that is not simply washed away by the historical flux, something that persists and even grows in this flux. Therefore the idea of progress must be relativised. But the historicist (in the sense of relativist) forgets to relativise the reality of flux and change, and by so doing he achieves the opposite of what he wanted to obtain, namely the destruction of the same concepts of change and history. To use Strauss' words "if everything is history, there is nothing left which can have a history" (Strauss 2005, 225). When change and flux are the only realities it does not make sense to speak of change anymore.

In the sections 5.6.a and b we will see how Stafleu has tried to re-affirm the link between scientific progress and universal laws, but this discussion will have to wait until we reach the stage in which positive suggestions will be provided. For the moment, in the following section I will analyse the diagnostic suggestions proposed by Goudzwaard on the topic of cultural and scientific progress. Although his suggestions are similar to those proposed by Dooyeweerd, I will argue that they contain a problematic element and they constitute a less convincing diagnosis.

#### 5.5.c Goudzwaard on progress: a less convincing diagnosis?

Like Dooyeweerd, Goudzwaard relates all cultural activities (including science) to the religious ground motive supporting and shaping a certain culture. He has shown that the modern confidence in progress (including scientific progress) is the result of a misplaced faith in the ideals generated by the humanist religious ground motive. More precisely, Goudzwaard's diagnosis is that the modern faith in progress has its roots in the Faustic ambitions of the Renaissance toward a complete control of nature, i.e. the nature ideal of modern western thought and culture. It is not difficult to detect the dooyeweerdian influence on this aspect of Goudzwaard's thought.

What stands at the roots of the overdevelopment of the West, for Goudzwaard (1975, 3), is a faith in the redemptive power of progress. This faith has produced unemployment, pollution, the erosion of public confidence and other problems. It is time that a new faith might provide a re-direction, towards a new society and a new economic order. In *Capitalism and Progress* (1979) Goudzwaard explores the relationship between the idol of progress and Western

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<sup>120</sup> In this respect it is interesting to note that after the historicist turn philosophy of science followed a "sociological turn". (See e.g. the title of Brown 1984, *Scientific rationality: the sociological turn*).

capitalism. But faith in progress has not only produced capitalism. It has also produced utilitarianism in ethics, realism and impressionism in art and *positivism in science* (1979, 13-14).

Goudzwaard's view of the progress-motive is linked to Dooyeweerd's understanding of the motive of nature and its Faustian ambition to control. Yet one has the impression that Goudzwaard insists especially on one side of the humanist ground motive. Goudzwaard seems to be so eager to launch his attack on certain trends and certain sectors of the humanistic front, that he forgets to examine the opposite pole of the humanistic ground motive. This is the pole afflicted by nihilism and historicism, the one which has caused the present loss of faith in progress in philosophy of science. The ideal of the scientific control of nature has produced positivist tendencies in philosophy of science. But what about the most recent relativistic tendencies? Unfortunately Goudzwaard seems to attribute the symptoms of the postmodern malaise once again to the pole of nature taken to its extreme consequences (Goudzwaard 1979, 54).

Here it is necessary, to ask: in the late-modern era, do we still experience a strong and optimistic belief in scientific and technological growth? The idea of endless and cumulative development is undoubtedly the result of the Enlightenment. But now it is necessary to acknowledge that this dream has vanished, and it has been replaced by new dreams. The former naive belief in progress (including scientific progress) is not applicable anymore in the present intellectual and cultural climate. Is it not rather necessary to modify Goudzwaard's critical diagnosis to address the new situation?

Goudzwaard has more recently tried to modify such analysis, by admitting that the naive version of faith in progress has now disappeared. Nevertheless faith in progress continues, according to Goudzwaard, by focusing on the *institutions and structures* of Western society (Goudzwaard 1992, 74). Yet even his refined argument that faith in progress is now especially related to various social institutions does not seem to acknowledge sufficiently that the postmodern era has introduced a much more pessimistic attitude towards the possibility of progress. Would it not be better rather to place a stronger emphasis upon the postmodern sense of pessimistic powerlessness or chaotic lack of direction of modern scientific development? In brief, more attention should be paid to contemporary postmodern thought and culture by analysing and diagnosing its religious roots.<sup>121</sup>

Goudzwaard has provided valuable reflections in many areas. He has brought to light the implications, for science and society, of the biblical ideas of calling, stewardship, the responsive dimension of reality, the importance of normativity, the need for a simultaneous

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<sup>121</sup> For an alternative reformational appraisal of the postmodern view of progress see e.g. Griffioen 1987.

realisation of norms and so on. In this limited case, in my view, an unbalanced analysis has produced a diagnosis which looks one-sided.<sup>122</sup>

It would be interesting to compare Goudzwaard's analysis with a recent contribution by Venter (n.d.2). Though the focus of this contribution is human dignity, it explores themes that Goudzwaard has often discussed: science, economy, progress and so on. One remarkable aspect of Venter's analysis is that both poles of the humanist ground motive (which he defines as nature and culture) are held responsible for the distortions affecting science and social life. In this analysis both modern and postmodern philosophies, both rationalist and irrationalist trends are criticised. Venter does not limit his critique to "faith in progress". He criticises both "futuristic" and "pastistic" attitudes, those who trust the ideal of the gradual scientific control of nature and those who long for creative freedom. Venter (n.d.2, 35ff.) suggests an alternative as well. "We need to link the 'yes' to subjectivity with the guiding pathways of a differentiated love which expresses itself in norms and institutions". Interestingly, he finds support for his alternative approach in T.P. Van der Kooy, who can be considered as a predecessor and inspirator of Goudzwaard's work.

In the following section I will extend my diagnostic observations by considering another problematic aspect, a missing element in late-modern philosophy. This missing element is the distinction between structure and direction (see end of section 1.3.b), which is a simple but effective instrument for detecting unilateral and simplistic approaches to (scientific) progress.

#### 5.5.d Griffioen: distinguishing structure and direction

Dooyeweerd's philosophy of progress<sup>123</sup> has been both endorsed and critically reshaped by a new generation of thinkers.<sup>124</sup> In the present section I will borrow an idea from one of these thinkers.<sup>125</sup> Griffioen has utilised the distinction between *structure and direction* (see end of section 1.3.b) in his analysis of cultural socio-political progress. I will borrow this idea and I

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<sup>122</sup> In partial attenuation of my criticism, I must say that in "Idols of our time" (1984), Goudzwaard does criticise the view of progress linked to the freedom ideal of the humanistic ground motive. For example he criticises the "ideology of revolution" (29-38) and the "ideology of the nation" (39-48). Both ideologies are rooted in the ideal of freedom and have influenced postmodern philosophy.

<sup>123</sup> Dooyeweerd offered his positive contribution to a theory of progress by elaborating his idea of the "opening process" in culture (Dooyeweerd 1984, II, 181-192).

<sup>124</sup> I will refer especially to Griffioen and Stafleu, who seem to me most relevant in this context. Among those who have dealt with Dooyeweerd's idea of progress (both scientific and cultural in general) are: Stafleu (e.g 1980 and 1987) Griffioen (e.g. 1987), Goudzwaard (e.g 1975), Wolterstorff (1981) and Mc Intire (1985). A commentary-reply to Mc Intire (1985) has been provided by Van der Hoeven (1987, 194-202).

<sup>125</sup> Griffioen has both criticised Dooyeweerd's view of progress and accepted many of its basic tenets in the elaboration of his own view, as it appears clearly in Griffioen 1987. One of the main critiques that Griffioen provides concerns the problem of the euro-centric character of Dooyeweerd's theory of progress. Yet he appreciates Dooyeweerd's distinction between structure and direction and the emphasis on the positivisation of norms, which he still finds not fully satisfactory (Griffioen 1986, 105 ff.)

will utilise it in the context of philosophy of science. In this way I will attempt a response especially to Feyerabend.

Griffioen's contribution consists in acknowledging the necessity of distinguishing a duplex dimension of development. The first dimension points towards a structural kind of progress. The second one has to do with directional development. Griffioen's sketch, applied to philosophy of science, helps understanding that there are different directions and levels of scientific progress. His duplex perspective implies that not all scientific developments can be considered as "progress", whatever they imply.<sup>126</sup> At the same time, however, not all scientific developments presenting some problematic aspect (here I have in mind Feyerabend's concern for the "happiness of the individual") can always be rejected as anti-humanitarian involutions. There is a structural dimension which demands the recognition of discoveries, even when they do not exactly fit our expectations, or when there is a need to handle with care some of their possible effects on society or on the environment.

The reformational reflection reveals the fact that the relationship between the structural and the directional dimensions can be a critical one. Griffioen (1995, 156) for example, illustrates the point with an example taken from social life. If one considers the establishment of a Hindu temple in our own neighbourhood, we will be plunged into a kind of dilemma. On the one hand it is a matter of justice that public room be made for a plurality of religions. This requires an attitude of cooperation. At the same time by cooperating one supports to some extent a religion that (from a Christian point of view) might in the long run have negative effects on societal and political life.

It is not difficult to see that the same dilemma is present with regard to scientific developments. Many of the modern discoveries have created new and positive possibilities that have enriched our daily life. At the same time they have sometimes constituted a challenge and even a threat to human well-being and even to the survival of the human community. In this context one can understand why there is a polarisation, even in christian circles, between those who consider science (and especially its technological applications) as a source of redemption and those who regard it as a demon (Schuurman 1993, 182-183). The distinction between structure and direction can help us acquiring a more balanced position.

Feyerabend's evaluation of progress, for example, is mainly conducted from a *directional* point of view (i.e. positive-negative progress) and he is inclined to disregard the *structural* dimension to a large extent. In other words, in Feyerabend's view of progress one can discern a confusion between the two notions of structure and direction. Actually the structural dimension is forgotten, and all that remains is the directional dimension. For example, Feyerabend (1970,

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<sup>126</sup> At the same time, Griffioen's suggestion opposes the idea that the scientific community should leave to someone else the considerations concerning the ethical side of its researches.

210) affirms that contemporary research does not contribute to what he considers the right, “humanitarian” direction of science (namely the happiness of the individual and the satisfaction of his manifold needs). By following this approach, Feyerabend is likely to disregard some forms of progress, for example when their “contribution” to the main purpose of science (i.e. the happiness of the individual) does not appear clearly from the beginning.

Basically, Griffioen’s suggestions point towards the need of recognising the “coordinates” of progress, an issue which has been largely neglected by late-modern philosophy of society. In this short section I have borrowed a very basic idea which is applicable to all forms of cultural progress. In the next section we will explore a more elaborated and specific contribution (by Stafleu) still concerning the “coordinates” of scientific progress.

## 5.6 Neo-calvinism and progress: a few suggestions

### 5.6.a *Stafleu: recognising the coordinates*

Stafleu (1987, 152) gives us his opinion on the purpose of science: “we consider this aim to be the opening up of the law-side of nature, the discovery and development of law-conformity in reality”.<sup>127</sup> In this way Stafleu stresses the normative character of science, as well as the relatedness of science to the created order. In order to deal with the theme of progress Stafleu (1987, 152-55) uses a strategy which implies a rather complex elaboration. Firstly he rejects the linear view of progress supported by Popper and Lakatos as an inheritance of logical positivism. According to Stafleu (1987, 152) “the development of science must not be conceived as a linear process but as a process in several dimensions, in which every direction has its own heuristic”. Therefore he proposes his model based on three distinctions which have been introduced by Dooyeweerd.

They can be seen as the three axes of a three-dimensional coordinate system. The first distinction (the “upward” z- axis) is that “between laws and anything which is subject to laws” (Stafleu 153-4). On the one side of the axis we have the law side of nature, on the other we have the subject side. In Stafleu’s definition, “subject” indicates whatever is “subject-ed” to a certain law and it refers, therefore, to both the subject and the object of knowledge (see fn. 5).

This distinction gives rise to philosophical questions concerning the relation between laws and their subjects, about the status of theories, statements and concepts, their meaning and corroboration, about deduction and induction (Stafleu 1987, 153). The distinction between law

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<sup>127</sup> A similar definition of the aim of science is provided by Botha (1996, 333). “Science is an attempt to describe and explain, with the aid of theories, the regularities and scientific laws that typify phenomena in reality”. Both definitions insist on the structural order. Stafleu’s definition above is a general one summarising seven more specific aims of the scientific enterprise. Briefly listed, they are: 1) explications of laws, 2) reduction and deduction of laws, 3) abstraction or analysis, 4) reconstruction or synthesis of

and subject, according to Stafleu, also leads to the question concerning the origin of laws. The answer to this question, if explicitly given, shows one's position in faith. In a Christian view laws (contrary to law statements), are not given or proved by man but have to be discovered in a careful and respectful exploration of the creation (Stafleu 1987, 154).<sup>128</sup>

The second basic distinction is that between universal or general modes of being and structural (typical) ones. This is the y-axis (horizontal). On the one side of it we have the universal and on the opposite side the typical.

The third distinction is represented by the x-axis (giving the third dimension to the coordinate system). It displays the "series of irreducible modes of experience" (1987, 154), the modalities. Speaking about the whole model or scheme Stafleu says:

"This picture shows that (...) research is not a linear process, but a multidimensional unfolding one. Whereas the first distinction is of special interest to *philosophers* of science, the other two determine the opening process and hence are very important for the understanding of the history of science, or rather of science as history" (Stafleu 1987, 154).

In addition, the scheme allows to identify four directions of research, besides the processes of induction (directed to the law side) and deduction (directed to the subject side), both linked to the first axis (z) mentioned above. The four directions of research are linked to the other two axes mentioned above, namely y and x. The second axis (y) characterises the distinction between universal and typical laws. They both concern the search for *unity* by the method of analogy, and the search for *structure* by the method of successive approximation. The other two directions of research are related to the z-axis, and they are the search for *objectivity*, by means of mathematisation and the search for *application*, by means of instrumentation" (Stafleu 1987, 154-155).

Represented schematically: z-axis (processes of induction and deduction)  
y-axis (search for unity and structure)  
x-axis (search for objectivity and application)

Stafleu's analysis continues with a description of the four directions of research, but I don't think it is necessary to follow him in the details of his explanation. My schematic presentation of his model is probably already sufficient to appreciate the depth and differentiation of his

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typical laws 5) designation of modal aspects and exploration of retrocipations and anticipations 6) objectification 7) explanation of individual facts and phenomena (Stafleu 1980, 29).

<sup>128</sup> In Stafleu's view, there is a specific reformed view of natural laws. The reformed view introduced the idea that truth is conformity to the law (Stafleu 1987, 241). At the same time, this reformational view that the aim of science is "the opening up of the law-side of nature" according to Stafleu has been accepted well beyond reformational circles (Stafleu 1987, 151).

theory of scientific progress. In his view “the historical development of science is by no means irrational” (Stafleu 1987, 151). It is on this basis that we are going to explore his view of the “opening-process” in relation to scientific development.

#### 5.6.b *The “opening up” of a scientific field*

Stafleu’s view of the opening process is based on the notion of modal aspects as presented by Dooyeweerd (1984, II, 181-192) and it represents an elaboration of the basic idea of cultural and historical opening process. It is necessary at this point to recollect briefly (see fn. 11) and clarify the concept of analogical anticipations and retrocipations. The modal aspects are irreducible to each other, but not isolated from each other. As soon as we reflect on their mutual relationships it becomes clear that (e.g.) the term “historical movement” refers to the historical aspect of reality, but at the same time refers to the aspect of motion, to the kinematic aspect which is earlier in the modal order. This type of analogy is called a “retro-cipation” because in the modal scale the kinematic aspect is antecedent to the historical one. In this case the analogy refers back to a “lower” modality and is therefore called retrocipation. On the contrary, when an analogy refers to a successive (or higher) modal sphere/aspect, we have an *anticipation*. In this case the lower modality points towards a higher one. One example in this case is the phrase “artistic feeling”. Here we have a phenomenon in the psychic sphere that anticipates the aesthetic modality and points towards it.

According to Stafleu what happens during scientific “revolutions” is by no means the elusive phenomenon described by Kuhn and only vaguely linked to a structural order. What happens is that in the “pre-paradigm phase” scientists are not yet aware of the meaning of their concepts. The consensus gained in the phase that Kuhn calls “mature science” is therefore gained through a realisation of the specific modal meaning of the scientific concepts (Stafleu 1980, 26). The advent of the first paradigm is usually accompanied by the discovery of the retrocipatory analogies of the modal aspects. The subsequent paradigm changes are brought about by the discovery of either a retrocipatory analogy, or (more spectacularly) by the discovery of an anticipatory analogy. Such discoveries are made possible by the increasing degree of abstraction and, at the same time, by the opening up of new typical structures, both in a technical and scientific sense (Stafleu 1980, 26).

Stafleu offers several examples of his model from the history of mathematics, geometry and other sciences. The history of mathematics shows that initially the meaning of the concept of number had to be established. Only after that phase the negative and rational numbers were introduced by abstraction. Then the real numbers and vectors were found by anticipation. Examples like these abound in the history of sciences. In Stafleu’s words:

“the history of astronomy, of physics and of chemistry are full of examples of sudden increases in understanding due to developments in retrocipation, anticipation, abstraction and specification” (Stafleu 1980, 26).

Stafleu’s model is also complemented by studies in the history of the sciences. These studies provide concrete examples confirming the validity of different aspects of his approach. The identification and isolation of a field of science (leading to what Kuhn would define as “mature” science) by Stafleu (1979, 15-25) is applied to magnetism and electricity. The realisation of the specific meaning of concepts within the specific fields was anticipated by authoritative “summaries” (e.g. by Gilbert for magnetism and Du Fay for electricity). This brought about the “isolation” of the specific fields of study, i.e. the realisation of its specific borders and characteristics. Each “summary” consisted of a number of empirical generalisations which were acceptable for adherents of different theories. Such summaries therefore did not constitute the first paradigms gaining the consensus of the community and eliminating rival views. They rather served as starting points for different “schools”, in view of the further theoretical and experimental development of the field. For electricity the “opening up” occurred some fifty years after Du Fay’s work, with the mathematisation of electricity. For magnetism the opening up had to wait for about 150 years (Stafleu 1979, 28-29).<sup>129</sup>

Stafleu’s model is also in a position to account for both the revolutions and the continuities between revolutions. In fact, in the opening up of a modal aspect, the latter remains a fundamental and irreducible mode of explanation, though it is seen in a different light. Whether we use euclidean or non-euclidean geometries, the aim of geometry remains to account for spatial relations. According to Stafleu (1980, 27) therefore, the fact that Kuhn discovered revolutionary changes while Holton underlined the persistence of certain “themes” in the history of the various sciences, is perfectly understandable from a reformational point of view.

### 5.6.c *Meaning variance and invariance*

What has been said in the previous sections also has some implication for the possibility of mutual understanding and communication. Although the topic belongs to the previous chapter, it could not be easily introduced before dealing with the issue of the opening up of a modal aspect. However, we are now in a position to say that the possibility of communication can be

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<sup>129</sup> In this context, Stafleu also criticises several aspects of Kuhn’s theory of paradigms. He points out for example that what Kuhn considers the first paradigms for electricity and magnetism do not show the typical characteristics that Kuhn attributes to paradigms. In the case of electricity, the acceptance of Franklin’s “paradigm” never concluded the previous conflicts about fundamentals. In addition it never obtained the consensus of the rival theories. The same can be said for magnetism. In this case the conflicts about fundamentals started only after the “paradigmatic” work of Gilbert (Stafleu 1979, 26).

supported from another perspective, related to the problem of the meaning variability and invariability.

In the positivist tradition it was assumed that data was largely independent of theories (see 4.6.d). The accumulation of new data could not produce any change in the meaning of older theories. As a consequence, communication among different schools was always considered possible at least in principle. Meaning invariance was a central tenet in positivism. Of course in the new philosophy of science, Kuhn and Feyerabend among others criticised this assumption on historical grounds. In their view all changes of paradigms implied a change in meaning, also with respect to observational facts.

The reformational tradition did not support any kind of meaning invariance. In fact, in the opening process meaning is both deepened and relativised. With the opening up of a modal aspect, the latter is not abolished, it is simply linked through analogies to aspects that may appear before or after it in the modal order. On the one hand, that aspect continues to exist as a fundamental mode of explanation, though it may now be seen in a different light. But the aspect does not remain just the same either. The analogies express the meaning of the aspect, so that both continuity and change take place. This is why I said that the meaning is both relativised and deepened. A reformational approach goes beyond simple meaning variance or invariance, or the idea that facts are “theory-laden”. From this point of view, the fact that both revolutionary developments (Kuhn) and persistent themes (Holton)<sup>130</sup> have been discovered in the history of science, becomes more understandable.

In conclusion, in this chapter I argued that the concept of progress seems to be gradually disappearing from the vocabulary of late-modern philosophy of science. In some instances progress is understood only in terms of usefulness or puzzle-solving, without reference to truth and objectivity. In some cases it is even argued that contemporary science constitutes a regress with respect to previous forms of scientific or technical achievements.

In the diagnostic section I argued that this rather pessimistic attitude towards progress finds its roots in the adherence to the humanistic ideal of freedom, which causes a rather relativist way of thinking. Furthermore I explored another reason for the present skepticism concerning progress, namely the confusion between structure and direction (see fn. end of section 1.3.b). I argued that Griffioen’s distinction between structure and direction is a helpful tool for our reflection on progress and points towards the necessity of distinguishing the basic “coordinates” of scientific progress.

In the third and final part of the chapter I argued that reformational philosophy links progress to the opening up of the analogical moments of the modal structures of created reality. In order

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<sup>130</sup> See e.g. Holton 1973.

to clarify this idea I analyzed Stafleu's view of scientific progress and of the "opening up" of a field of science. This analysis has allowed me to introduce the theme of meaning variability and invariability, a theme that sheds light on the fact that reformational philosophy makes room for the recognition of both revolutionary changes and constant themes in the history of science. It is now time to move to a general conclusion concerning the main themes explored in this study.

It seems therefore plausible to affirm that a reformational approach has something to say on the issue of scientific progress. Such approach would avoid the rationalistic trust in linear progress stemming from the nature motive. It would also avoid the pessimistic and relativistic views stemming from the freedom-ideal. A reformational approach could distinguish between different levels and sectors of progress, providing a more mature assessment of scientific development. It could also be in a position to clarify the coordinates of scientific development and the main directions of scientific research. Starting from these resources and elaborating their implications, the reformational community could certainly have relevant ideas to offer to the broader community of scholars.

## CONCLUSION

The first theme I explored is that of scientific certitude, which is at the basis of the late-modern humanist tension between objectivism and relativism. This enquiry showed that late-modern humanist philosophy has increasingly adopted a nominalist and subjectivist position, thus implicitly questioning the existence of a universal order for reality. It became apparent that three problems arise as consequences of the strategy of stripping concrete reality of its universal trait. First, the object of scientific research becomes more elusive, inaccessible and dependent on the views of the scientific community. Second, the *locus* of order (which is formally said to be inaccessible or non-existing) is nevertheless placed in the subject of knowledge. Furthermore, an archimedean point is sought in various aspects of reality. The third problem is the incongruence between science and reality: nominalism recognises only the existence of the individual traits of created entities, but scientific knowledge is abstract knowledge referring to the universals. This creates an incongruence that nominalism is not in a position to overcome.

The exploration of this issue showed that the problems illustrated above find their roots in the cartesian division between the object and the subject of knowledge. Caught in this polarisation, the late-modern philosophy of science does not acknowledge the presence of a law-order which conditions the existence of both the subject and the object of knowledge. The humanist philosophy of science either ignored the presence of this order for reality or declared it inaccessible and as a result has placed the anchor of certainty either in the knowing subject or in the object of enquiry. As in late-modern philosophy the *locus ordinis* has increasingly been placed on the subject, the object of scientific investigation has gradually been experienced as elusive or inaccessible.

Furthermore, the cartesian dichotomy has led the late-modern philosophers to search an archimedean point for their enquiries in the different aspects of created reality. This search is however characterised by lack of insight in the modal structure of created reality and has resorted to absolutizations of various modal aspects. Finally, the same cartesian polarisation is responsible for the incongruence between science and its purported object of study, inasmuch as it prevents considering the structural order for reality as the proper object of study of scientific research.

The recognition of the law-order for reality, in principle, provides a solution to the problem of incongruence between science and reality, which is implied especially in the nominalist approaches. This is an important resource made available by the philosophy of the "cosmomic idea". Instead of trying to anchor scientific knowledge in the object or the

subject, it is necessary to acknowledge the universal law-order, which constitutes the *locus ordinis* for reality and the appropriate anchor of certitude. In addition I also argued that it is possible to know both universality and individuality, which are not entities but traits of everything that exists.

Knowledge of individuality is typical of the pre-scientific attitude of thought, which focuses on concrete realities. Scientific knowledge, on the other hand, attempts to give an account of the patterned and regulated universal nature of reality. I have indicated the link between scientific and pre-scientific knowledge as a resource to be explored in order to affirm the possibility of knowing both individuality and universality. Finally, I showed that the structural order for reality, which is the proper object of investigation of the different scientific disciplines, is accessible to scientific investigation. In order to support the above argument I explored the role of abstraction, theories and the metaphorical use of language as providing an epistemic access to the structural order. Although I have mentioned (see end of chapter 2) a few possible reasons for the rejection of the notion of universal order in late-modern philosophy, further research would be desirable on this theme, especially concerning the presence of deep commitments and prejudices that may prevent the recognition of such a fundamental notion.

In the exploration of the role of pre-scientific or pre-theoretical presuppositions in late-modern humanist philosophy it became clear that a growing emphasis is being placed on the role of frameworks, premises and paradigms in scientific theorising. As this emphasis is not balanced by a recognition of universal norms which are not the creation of the knowing subject (or community), I argued that scientific objectivity appears to be threatened. From a historical point of view, in the late-modern era scientific theories increasingly appeared to be regarded as dictated by beliefs and worldviews (which are sometimes shaped by ideological or non-scientific types of interests).

In the diagnostic phase I argued that this emphasis on presuppositions is part of a broader shift from positivism (which was rationalist and in most cases objectivist) to the new philosophy of science with its irrationalist and subjectivist traits. Dooyeweerd's idea of religious ground motives is a very useful tool to understand the swing (from positivism) towards the historicism and subjectivism of late-modern humanist philosophy of science. In Dooyeweerd's philosophy the "religious ground motives" are the basic pre-theoretical starting points of philosophy and culture in general. They control theoretical thought by means of a triad of transcendental ideas concerning a) the origin, b) the unity of meaning and c) the relation of coherence and diversity between the different aspects of created reality (Dooyeweerd 1984, I, 93-102).

According to Dooyeweerd the humanist ground motive “contains” two poles in dialectical tension between them: the pole of *nature* and the pole of *freedom*. Historically, the pole of nature is dominated by the ideal of natural science, implying the scientific control of nature. But control over nature also means control over the human being, who is part of nature and therefore conflicts with the second pole of the humanistic ground motive, namely the pole of *freedom*. Once rigid control is exercised over all nature, there is no more room for human freedom (Dooyeweerd 1979, 152-153).

One interesting avenue for further research would be the exploration of the links between the poles of nature and freedom and the subjectivism-objectivism polarisation. I have already argued (2.5.e) that in my opinion they are strictly related. Adherence to the ideal of freedom also results in a subjectivist position. In addition, this determines the choice of placing the archimedean point in certain modal aspects and not in others. More precisely, the choice will fall on modal aspects that qualify the subject/community of knowledge, like the social, historical or linguistic aspect. But this basic intuition needs to be substantiated by further historical evidence and systematic research.

Positivism was inspired by the theme of the control of nature via natural science. Great importance was attributed to nature itself: facts were supposed to speak for themselves while the intervention of the human personality was experienced as an intrusion. The role of the scientist was reduced to a minimum and had to be characterised by absolute objectivity, to the point of neutrality. However, when the ideal of the free human personality reclaimed the priority over the pole of nature, the presence and the role of various types of presuppositions had to be recognised as a fundamental component of scientific research.

Unfortunately, due to the influence of the freedom motive, this positive recognition could not be accompanied by a parallel acknowledgment of the universal order conditioning and constraining the knowledge of the subject. The recognition of the role of presuppositions, therefore, could not be understood within its proper limits and did not have only beneficial effects. In fact, it rather contributed to an excessive emphasis on the subject and his creative freedom, which was in direct conflict with the recognition of an objective reality, not dependent on the subject holding to certain presuppositions.

Reformational philosophy offers interesting insights towards a better comprehension of the nature and the role of presuppositions, their link to the religious sphere in the knowing subject and their influence on the scientific endeavour. However, it is a recognition of a created order that provides the needed external anchorage for scientific knowledge. The recognition of the order for reality, partially independent of the knower, counteracts relativism and offers a more balanced picture of the scientific activity. In addition, the requirement that the archimedean point must be chosen outside the horizon of created reality, positively constrains the theoretical

enterprise and helps avoiding both the absolutisation of particular modal aspects and the choice of relativism or objectivism. Following the dooyeweerdian tradition I have argued that it is necessary to recognise both the pre-scientific starting point of all theorising and the existence of states of affairs that are conditioned by structures for reality.

The notion of objectivity targeted by the majority of contemporary philosophers is, admittedly, one inspired by the positivistic tradition and has been criticised by reformational philosophy as well. I have however argued that in order to avoid the relativistic tendencies of postmodern philosophy of science it is important to retain a proper notion of scientific objectivity. In the reformational tradition objectivity is not to be sought in a correspondence between theory and facts, but rather in a respectful search for and submission to the universal law-order for reality.

With respect to the widespread distrust concerning scientific communication which has led to the debate on incommensurability I argued that the increasing emphasis on presuppositions has also created the impression that it is extremely difficult to communicate with scientists holding different views, paradigms or theories. The idea that it might not be possible to compare certain standpoints or to sustain a dialogue between certain schools has been increasingly popular. Initially this possibility was contemplated with a considerable concern, more recently it seems to be welcomed as paving the way to a new and liberating pluralism. Postmodern science is not supposed to look for communication and consensus, but possibly for dissensus (Lyotard 1984, 65).

I related the problem of the lack of communication and incommensurability to the humanist ground motive of nature and freedom. The ideal of the control of nature (in the positivist era) required a world which is the same for all and a language that is understandable by all. The motive of freedom, on the contrary, requires the existence of totally different worlds, frameworks of thought and languages as the result of the creative activity of the human personality. Under the hegemony of the freedom motive, however, incommensurability becomes a threat because the common ground of a creational order is rejected in favour of the recognition of a plurality of language games, frameworks and paradigms. As a consequence, when the language is not shared there is no real possibility of dialogue or communication.

While reformational thinking does recognise the reality of different approaches and views, the structures conditioning states of affairs constitute a constant reference point for scientific dialogue. I have illustrated Dooyeweerd's approach to scientific dialogue as an example of the reformational attitude of taking into account both the created structures and the different points of view. A radical antithesis is only possible at a deep pre-scientific level. When we consider the theoretical level, the antithesis can only be relative and therefore complete

incommensurability is not plausible. Finally, the statements and concepts are only partially dependent on theories and theories are only partially dependent on broader “paradigms” or presuppositions. Therefore I have defended the possibility of comparing and criticising competing theories, arising from different presuppositions.

On this point, reformational philosophy agrees with many recent contributions (see fn. 69) rejecting both radical incommensurability and naive unawareness of the difficulties of communication in science. Although this agreement must be valued, at this point I would like to take the opportunity for a more general remark concerning the reformational approach. I would like to avoid the possible impression that such approach consists mainly in proposing a judicious compromise, a synthesis between two extremes. The fact that humanist philosophy does not easily reach a good synthesis or balance is due to the basic polarisation inherent in the humanist ground motive. The reformational alternative does not consist in finding a golden middle between these two conflicting poles. Rather, it proposes to overcome that duality and to find a way out on the basis of a new ground motive which is not dualistic. In this context it is perhaps worthy mentioning the example of the realism-nominalism debate. Though the reformational position recognises moments of truth in both positions, it does not simply propose a synthesis between the two positions, but rather a third position which constitutes a valid alternative.

Ontologically speaking, this alternative position is characterised by five complex concepts (Hart 1984, 35). I will briefly mention them. First, both universality and individuality, though radically different from concrete entities, must be recognised as real. Second, they are always correlated. This means that there is no universality apart from individuality and vice-versa. Third, universality has to do with conditions, laws and orderliness. In other words, “universality is a nomic characteristic of limitation and determination” (Hart 1984, 35). Fourth, each concrete individual entity can only exist in the context of universality and (fifth) is knowable only in such a context.

With respect to the crisis of postmodern philosophy of scientific progress, my approach was one in which I departed from the point of view of the possibility of progress. The cumulative view of scientific progress, typical of the positivist period, has been replaced by late-modern philosophy with more “modest” views. On the one hand this has contributed to a more realistic assessment of science but on the other hand, to a sort of opposed extremism, in which the notion of progress has met much more skepticism. In some instances progress is only related to scientific societal usefulness and internal puzzle-solving, without reference to truth and objectivity (Kuhn 2000, 85-86). In other cases it is even argued that contemporary science

constitutes a regress with respect to previous forms of scientific or technical achievement (Feyerabend 1975, 306-7).

There is a connection between a relativist view of scientific progress and the adherence to the humanistic ideal of freedom. Historically, in fact, the acceptance of the ideal of freedom is associated with a rather conservative and even reactionary attitude towards cultural progress in general. Choosing an example from political history, philosophical romanticism (in which the freedom-ideal was dominant) has been characterised by a rather reactionary attitude in politics, as illustrated by the period of European history called "Restoration". As the adherence to the pole of nature stimulated the faith in cumulative scientific progress, the opposite pole of freedom increasingly determined a much more skeptical attitude.

According to Dooyeweerd, the relativist attitude implies an absolutization of the historical modality of experience. Historicism regards norms and structures from the point of view of the ever-changing historical developments and therefore no stable universal norms are recognised. The relativist approach severs the link between scientific progress and universal norms that might constrain the freedom of the scientific community. Scientific progress is then regarded as proceeding in a "catastrophic, non-rectifiable and paradoxical way" (Lyotard 1984, 60). As progress is no longer linked to universal norms but to the legitimation of the scientific community (the subject of knowledge) irrationalist and relativist views of progress inevitably emerge. In this section I have also indicated another (more secondary) reason for the present skepticism concerning progress, namely the confusion between structure and direction or the suppression of one of these two dimensions leading to a unilateral view of scientific developments.

Trying to suggest a few alternatives, I argued that a recognition of the different "coordinates" of scientific progress is a beneficial and necessary instrument for a more elaborate and appropriate assessment of scientific progress. It is necessary to link progress to the opening up of the analogical moments of the modal structures of created reality as proposed by Stafleu (e.g. 1980, 5-28). It is also necessary to move beyond the dilemma of meaning variance-invariance (a topic which is relevant for the discussion on communication as well). In fact it is necessary to acknowledge both the revolutionary changes and the constant themes characterising the development of science.

This research has dealt with some of the challenges that contemporary philosophy of science has to face. Those challenges amount to a crisis. This is not meant, however, to justify any hopeless attitude. The etymology of the word "crisis" (Greek: *krino*), indicates a choice, a decision to be made. Every crisis places us in front of decisions to take and in this sense opens up new possibilities. This study has tried to indicate a few alternative solutions and new directions, the available resources and the possibility of a different approach.

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