Chapter 2
Images of Science: A Reality Check

Abstract It will be argued that the dominant form of current academic science is based on ideas and concepts about science and research that date back to philosophy and sociology that was developed since the 1930s. It will be discussed how this philosophy and sociology of science has informed the ideas, myths and ideology about science held by the scientific community and still determines the popular view of science. It is even more amazing when we realize that these ideas are philosophically and sociologically untenable and since the 1970s were declared obsolete by major scholars in these same disciplines. To demonstrate this, I delve deep to discuss the distinct stages that scholars in philosophy, sociology and history of science since 1945 to 2000 have gone through to leave the analytical-positivistic philosophy behind. I will be focusing on developments of their thinking about major topics such as: how scientific knowledge is produced, the scientific method; the status of scientific knowledge and the development of our ideas about 'truth' and the relation of our claims to reality. It will appear that the positivistic ideas about science producing absolute truth, about 'the unique scientific method', its formal logical approach and its timeless foundation as a guarantee for our value-free, objective knowledge were not untenable. This is to show how thoroughly the myth has been demystified in philosophy and sociology of science. You think after these fifty pages I am kicking a dead horse? Not at all! This scientific demystification has unfortunately still not reached active scientists. In fact, the popular image of science and research is still largely based on a that Legend. This is not without consequence as will be shown in Chap. 3. These images of science have shaped and in fact distorted the organisational structures of academia and the interaction between its institutes and disciplines. It also affects the relationship of science with its stakeholders in society, its funders, the many publics private and public, and policy makers in government. In short, it determines to a large degree the growth of knowledge with major effects on society.

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In this chapter, but throughout the book, I will present a narrative in which I will take my own intellectual and scientific journey from 1971 as a chemistry student who did a minor in the philosophy of science in academic year 1975–1976. Since then, I followed the classical career path of a professional biochemist/immunologist, as PhD student, post-doc, group leader, department head, director of a small research institute, to finally become dean and board member of a large University Medical Centre. Going through this professional sequence, I kept a persistent and ever stronger interest in the science of science. It is from the perspective of a true understanding of the practice of science in its various aspects that I will use specific authors a lot, but others much less or even neglect work of many scholars that to specialists in the different fields are considered important but are of little or no relevance for the daily practice of active researchers and most other actors in the field.

2.1 Part 1. Images of Science, a Reality Check

'The empirical basis of objective science has thus nothing 'absolute' about it. Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down any natural or 'given' base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being'. (p109) (Popper, 1959)

Introduction

Unlike most natural scientists writing about science that are not philosophers or amateur philosophers like me, I am convinced that I need to discuss the origins of the philosophical ideas and concepts that are the basis of the dominant image of modern science that in 1981 still was *'the widespread popular conception of science'* (p2) according to Ian Hacking in his influential book *Representing and Intervening*. (Hacking, 1983) I experienced time and again during my professional career that it are these obsolete and incongruous ideas about science and research that even now determine and distort to a large extent our views, attitudes, policies and politics, discourse, professional and collegial interactions in academia. I fully realize that the analysis that follows, to readers with less than average knowledge of the history of the philosophy of science, may feel as a much too deep dive. Understandably, they will wonder whether they need to know all that. The story of analytical philosophy and logical positivism and how it has impregnated our image of science, is essential for my argument to understand the origins and persistence of the problems of science and academia. One can without a problem skip, the whole or Part 2 of this chapter and only take note of the conclusions of Part 1. For a more general quick read, I refer to Chap. 3 of my *Science 3.0* (Miedema, 2012) or the very nice paper by (Pinch, 2001) or Shapin's *Science and the Modern World*. (Shapin, 2007).

The Frontstage and Backstage Paradox

The popular image of science, mainly of the natural and biomedical sciences is sometimes called the Standard Model. It is the well-known narrative of 'the scientific method' and 'the vocational noble' scientists discovering nature and truths'. It is based on a blend of normative philosophy, mainly of epistemology designated the 'Legend' and normative sociology, both were developed in the first half of the twentieth century. This romanticised image is still widely used 'on stage' in the media, in public debates not only when science is besieged or if scientists feel besieged or fear budget cuts. Paradoxically, contrary to this 'frontstage' image, most scientists, 'backstage' in their training and daily professional life are somehow aware that there is no unique method, no formal logic which guides scientists to the truth. In contrast, when being introduced to the daily research practice, they are trained to use a set of instrumentalist principles and methodologies how to make reliable knowledge. Most of these are practical principles referring to techniques, producing and reading texts being journal articles or books, how to set up experiments or investigations, about interpreting and discussing experimental results, the requirement of reproducibility, and thus how to conclude what is to be believed or if you will, is 'true'. These are being passed on to new generations of researchers while they are doing their first rotations in laboratories and departments as master students or PhDs. Of course, there are courses on methods in the field of research -for instance in my case as a BSc chemistry and MSc biochemistry/immunology student since 1971 chemistry, biochemistry, immunology, bacteriology, virology, molecular biology-, and on methodologies like epidemiology, statistics, bioinformatics, spectroscopy, mass spectrometry, NMR, fMRI, genetics. Students are introduced to the state of the art of the discipline with its most novel technical developments and findings. In the natural and biomedical sciences introduction is done almost without reference to history, the pathways that led to that state of the art in the field.

We, as natural scientists do not worry too much about a formal timeless foundation on which we build our investigations, experiments, claims and conclusions. The most important thing you learn is that your claims must hold, that is, can be successfully used by others inside or outside the laboratory or department. Those exceptional scientists who started to think and write about science did not spent too many words on the philosophy and sociology of science. In the natural and life sciences one can become a tenured professor without ever having to read or having read Popper, Merton or Kuhn although most of them want us to believe they once did. There is slightly more interest in the history of the sciences, which mostly are romanticized narratives about the classical gems with an even more classical linear narratives explaining how we arrived were we are now, with a lot of attention for the top scientists, the geniuses in the field. These histories until the 1970s were almost all written from the perspective of the Standard Model. The most famous and widely read exception still is James Watson's *The Double Helix* published in 1968 which for that reason had a very critical reception that still is of great interest to our understanding of images of science and scientists for which I will return to below. (Watson, 1968)

The Standard Model and the Legend

Still the best-known image and narrative of science, of how inquiry and research is being done, I am afraid, is an idealized picture that has in the literature been designated the Standard Model sometimes also called the popular view. The Standard Model is an interesting composite. Its image is built on the one hand on the classical theory about scientific investigation, its unique method, the status of its knowledge claims and the belief system associated with it. This image coming from the philosophy of science has been designated 'the Legend'. Indeed, until this day, implicitly but also explicitly very much of the Standard Model echoes the ideas of what used to be the dominant philosophy and sociology of science until the 1960s.

These ideas about the theories and statements of science and the unique formal status of its knowledge claims, have been developed in the philosophy of science in the first half of the twentieth century. This originated from the seventeenth century Cartesian rationality of *Modernity* which takes its name after Descartes. There are some influences from early positivists like Comte but its form is mainly determined at the beginning of the twentieth century when it became admixed with elements of the logical positivistic tradition of the Vienna Circle, the analytical philosophy of science and the works of Popper. Descartes assumed a formal mathematical method that would be grounded on a set of timeless universal principles, an objective foundation and even unique 'God-given endowments to the human mind' were invoked (Descartes, 1968). This would be the general solution to the problem of the logical formal relation between the observed and the observer. The positivists and Popper, however rejected this timeless and objective *'God's eye perspective'* or 'Archimedean point' as metaphysics, non-empirical and thus per definition as unscientific. To deal with the problem of objectivity- how can we objectively know without our own cultural biases and hidden personal values – an independent analytical foundation for the logical relations between theoretical statements and statements about observed entities and facts was postulated. The prominent members of the Vienna Cycle (Wieners Kreis) in the years before the second World War sought refuge in the USA and there started departments of philosophy in different universities there. In these departments with their approach to philosophy of science, in the analytical, empirical or logical- positivistic tradition, they made school. As a consequence, this philosophy was dominant for a long time around the globe. For a highly readable and informative history of the Vienna Circle see David Edmonds, 'The murder of Professor Schlick'. Popper, was peripheral to the Vienna Circle, spent the years of the war in New Zealand and returned to London after the war. He had realized already that observational statements are theory-laden and eventually concluded that there is no *'given'* foundation, no formal set of principles to build on. He wrote, *'we are drilling piles in quicks and until they stand, and we can build on them for the time being at least'*. We believe and accept or reject theories after serious experimental testing and scientific debate about the evidence he said (Popper, 1959). This Popperian fallibility reminds of Charles Sanders Peirce' early works on how and why we believe, published in the last decades of the nineteenth century.

The Standard Model thus explicitly, via the Legend largely follows the hypothetico-deductive cycle of proposing hypotheses and its derived statements, experimental testing of these statements, with the result of falsification or support or partly support from the observed evidence. This results in acceptance ('belief') or requires improvement and a new cycle of testing. From lower-level observational statements and laws, higher level ever more general laws are deduced which ideally conjecture universal and timeless truth as the most prominent results of scientific inquiry. The reductionist method it proposes is empirical, formal, logical and thus importantly a guarantee for objectivity, because it separates values from positive facts, scientific from non-scientific statements (Nagel, 1961; Hacking, 1983) The strict Cartesian dualism between observer and observed, between fact and value and between analytic and synthetic makes science per definition reliable, because its products are objective, value free and thus trustworthy. It was for a long time self-evident that this 'scientific method', with its rigor and potential for prediction and control building on the ideal of Euclidian mathematics, was the cause of the overwhelming theoretical and practical technical successes of the natural sciences. It so happened that positivism and Popperian demarcation of falsification between scientific and non-scientific knowledge became dominant.

It moreover, was generally believed to be the critical difference between the natural 'hard' sciences and the 'soft' social sciences and humanities. This demarcation is about methods of investigation, but also about its products, its theories and laws which can be tested and in the hard sciences preferably were expressed formally thus mathematically and were held to be universally true. If investigation was performed in that tradition and thus modelled after the natural sciences, especially after physics, it would be recognized as science. Given its main philosophical sources, the type of research of the Standard Model aims for the ideal of timeless universality, wants to produce general laws, formal basic knowledge using reductionist methods to contribute to the body of knowledge. It is historically mostly confined to the classical academic disciplines and operates in an international global perspective. It aims for value-free research and neutrality, is in principle against interference from whatever powers outside academia or even from within academia outside the own discipline.

Based on its own criteria for what is considered to be science, research done in this way always was, and to a large degree still is the highest in rank within academia compared to the social sciences and the humanities (SSH). SSH until 1958 not in the least for this reason was not regarded serious science or research and for instance not a discipline in the National Science Foundation in the USA. As I will argue in later chapters in more detail, still in the third decade of the twenty-first century, within virtually every discipline and faculty, there is a visible gradient of research esteem according to the degree of the use of formal quantitative methods that employ or at least imitate the methods of the Legend and thus of the natural sciences.

The Mertonian Social Order

The Standard Model is a composite of the Legend of the scientific method described above, but in addition, explicitly builds on the classical sociological image of science which has originally been developed by the famous American sociologist Robert Merton and his students between 1930 and 1970. (Merton, 1973) In this image of science, it is a human activity different from all other human activities in that scientists are altruistically looking for the truth. This is, according to Mertonian sociology, done in a open community, chatacterized by sceptical debates about each other's work in order to get to the best knowledge. Knowledge is considered or at least aims to be universal and not bound or restricted to time and place. Importantly, the scientists are fair in discussing the works of their peers and are honest or at least strive for honesty. They are not in it for their own personal or intellectual interests. They publish their results for their peers to judge and to be used for further research. Their findings are thus expected to be made freely available and in all respects are considered common good. They can through the workings of the incentive and reward system, commissioned by the scientific community, get credit for their work, which is required to advance their careers and gain in reputation and standing in their respective field of research. Reputation is gained for instance by so-called 'priority', being the first to discover and report facts, theories and novel methods, and contributions that by peers are considered relevant and original. In this vision there is fierce competition and consequently to it stratification. There are elites in every discipline, which in the Mertonian social order is however not felt to be (too) problematic, but is considered instrumental for the functioning of the enterprise and thus reflects the natural order, a logical consequence of the type of activity the community is engaged in (Ben-David & Sullivan, 1975). Merton in 1968 did however already point out several unwanted effects of stratification inherent to the reward system (Merton, 1968). Although all researchers are in principle regarded equal, elitism is acknowledged but thought to be functional. Merton coined the term *Matthew Effect* for the famous, or more recently considered, infamous mechanism of accumulative advantage that elites in the system have. These advantages concern influence, authority and professional power which gets converted in material advantages like, research facilities, grant support and access to the most prominent academic functions and positions. If you read the paper more then 50 years later, you are struck by the normative and outright naïve and idealistic wordings by which Merton describes his expectations how the top scientists will deal with or even counteract any perverse effects of the *Matthew Effect* if it would ever become 'an idol of authority'. He has amazing faith in top scientists because of their unusual characters and high standards of integrity (Merton, 1968). In adhering to the norms, and so producing results and publications, scientists are recognized as good citizens by their peers and members of the community and accepted and respected as members of the scientific enterprise. Moreover, by keeping up this academic social culture, science, it is believed is trusted and earns respect from the public and government as a reliable institute in society. In the Mertonian view, science is a closed social system within society that decides itself who is excellent and who is not, who gets the credits, the jobs and the grant money. This implicates that the growth of knowledge in this view is an internal affair. Science is a value-free, neutral, activity where autonomous individuals disinterestedly pursue their inquiries in the context of a social system governed by its own unique internal scientific criteria and norms.

Dispatches from the Trenches

I realized the problems the popular image of science, held by the science community and the public and started to study it, in the early 1980s during the start of my scientific career as a researcher on the pathogenesis of aids and HIV infection. That was in a truly unique setting in which my group, or as we say in our field 'my lab', worked on HIV/aids in Amsterdam in a cohort study of men who have sex with men (MSM) and IV drug users. In these Amsterdam Cohort Studies it had been clear from the start in 1985 that to understand the problem of aids and HIV infection, a truly multidisciplinary approach was needed. My colleagues came from the social and behavioural sciences, medical anthropology, epidemiology of infectious diseases, bioinformatics, internal medicine, pathology, pre-clinical and medical virology. Next to this array of scientific disciplines we interacted proactively with the participants of the cohorts, mainly homosexual men. Listening to their concerns, their problems and immediate needs but also to keep them informed about the work we did using their blood samples and the epidemiological and behavioural information they provided in the questionnaires. The work was done the Municipal Health Centre, AMC and my group was working on viro-immunology in the Central Laboratory of the Blood Transfusion Service (CLB, now Sanquin). At my institute with respect to aids, research was done in the wider context of the safety of blood supply which was at that time of the highest daily concern. This bloodbank context involved cellular and protein chemistry, virology and technical issues of manufacturing of biologicals, but also sociology, economics and ethics of blood donation and screening of donors.

I read Latour's *Science in Action* in 1987, as a young principal investigator working on HIV/AIDS already getting deep into international science (Latour, 1987). The researchers that Latour followed in the lab and outside the lab talking to the different stakeholders, on their travels abroad were pretty busy. All of it was familiar to me. Only years later I discovered a major early source of Latour, Bourdieu who applied his theory of the 'field' to academia with its concepts of habitus, socialization, the power struggle, stratification and elitism (Bourdieu, 1975, 2004). Few biochemists or natural scientists in their scarce time do read such scholarly studies about themselves, despite the insightful analysis of the familiar academic microcosm which we virtually on a daily basis were deeply involved. It made me aware of quality and credibility, the standing of the different sciences and institutions, about competition and power games, reputation, getting credit, about the moral values and the

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personal motivations involved in science, that implicitly and explicitly could be observed in daily verbal and non-verbal interactions.

After spending 35 years in that multidisciplinary environment in a highly competitive national and international world of science it was obvious that scientists from different fields and disciplines see the world differently and speak different languages. These are, however, minor issues compared to the much more serious and also widely held misconceptions and prejudices about research and inquiry, about the different academic disciplines and what the true aims of science are. These appeared to be mostly based on obsolete ideas derived from the classical philosophy and sociology of science.

This would not be a problem,

if it would not have adverse effects at the national or institutional level, for instance on agenda setting and the growth of knowledge
if this would not cause major science waste and production of much poorly performed and useless research
if this would not be the cause of major obstacles for translation of research to societal impact for those in the real world who need solutions and relief badly.

Unfortunately, daily experiences in the community of science already over a very long time show differently. It did and until this day does cause various serious problems that affect science and inquiry at many levels and affects its potential to impact society. It is because of this that I will in more depth discuss the popular images of science, their origins and problems and how they affect the practice of science. After that I will in this Chapter discuss the philosophy and sociology that forms the foundation for these popular images and discuss how these ideological and normative concepts, with their respective famous dualisms have in the past 40 years been shown by philosophers, sociologist and historians of science to be scientifically untenable.

The Mythical Image of Science

The Standard Model thus is an image of science that is a composite of two narratives, based on a philosophical and a sociological theory established in the first half of the twentieth century. First there is a powerful ideal, derived from philosophies based on the natural sciences with an implicit positive image of scientist's intentions and social interactions, in which the unique relation between theories and its knowledge claims with reality stand out. Next there is the sociological image of a community of vocational altruistic investigators who in daily practice go through daily struggles and hard labour to discover the secrets of nature and come to a set of unifying ideas about the world. The Standard Model does not present a consistent idea of science because these two components synergize but fail to merge into an overall *theory* of science that explains **how science really works and how that relates to its reliability, success and credibility.** It is exactly because of this hybrid, with these two complementary faces, the Standard Model as an image and a general narrative about science has worked well for science in its interaction with the outside world in the past.

Obviously, it has had its value and advantages, but it is I will argue, also since long the root cause of the most urgent problems in the relation between science, government and society, and at the lower level in academia, between scientists and between scientists and their publics. Both aspects of the popular image or science described above do not resonate much with active researchers. The way we have made and make knowledge that works and leads to successful follow-up investigations and subsequent growth of knowledge as well as successful interventions in the real world, the practice of science in the natural sciences including physics, is fundamentally different from what the Legend holds on philosophical grounds to be the unique scientific method to arrive at true, believes, statements and insights. Active researchers in the different fields and disciplines do not pay too much attention to the rules of engagement of the Legend as far it concerns the celebrated scientific method. They don't need to. In addition, with respect to the Mertonian norms, there are written codes of conduct and written and tacit mores, that researchers intuitively and indirectly are aware of it. As soon a sociologists started to actually take a look at the practice of science, they couldn't help themselves seeing major and general aspects of behaviour and mores of active researchers not in agreement with the Mertonian ideal. This was observed at the individual level, but also at the institutional level. This has in the past 10 years increasingly drawn attention within the scientific community and lately this was discussed in the media and public debates as well (Chap. 3).

The Standard Model: A Reality Check

I will discuss the criticisms that have started to develop mainly since 1960 regarding the philosophical theory as well as on the sociological theory that formed the main pillars of the Standard Model. These criticisms are based on research in philosophy, sociology but also history of science. We will see that both components of the model have been shown to be normative in nature, not reflecting nor impacting much the practice of the sciences.

Possessed by the Normative, Demeaning the Descriptive

> *Philosophers have long made a mummy of science. When they finally unwrapped the cadaver and saw the remnants of an historical process of becoming and discovering, they created for themselves a crisis of rationality. That happened around 1960. It was a crisis because it upset our old tradition of thinking that scientific knowledge is the crowning achievement of human reason. Sceptics have always challenged …but now they took ammunition from the details of history* (p1) (Hacking, 1983)

As described in the previous section, until 1960 the dominant philosophy of science was based on concepts and ideas developed in the empiricist and logical positivistic tradition very much inspired and lead by the way of thinking of analytical philosophy. It is totally devoid of historical perspective and did not at all take into account the diverse research practices, the way research was being done and thus how in the laboratory we actually produce knowledge and decide what to belief. Even in recent times, members of the scientific community, when being asked, still belief in the ideals and norms of the Standard Model. Although deep inside they know that at the organizational and at the personal level science has never functioned according to these rules and norms, as sociological and historical researchers have demonstrated in the past 40 years. (Hanson, 1958; Toulmin, 1972; Kuhn, 1962; Ravetz, 1971; Ziman, 1968, 1978; Latour, 1987; Latour & Woolgar, 1979; Mitroff, 1974; Shapin, 1982). Furthermore, although the foundations and the logic of the scientific method were questioned already since the 1930s, in several disciplines, −biology, medicine, economics, including the social sciences- subdisciplines and research fields emerged that copied the formal quantitative methods and style of research of the ‘hard’ sciences. They have a craving for the type of science that never was which is also called ‘physics envy’. Toulmin for the field of economics describes this development in a chapter under the title ‘Economics and the Physics that never was’. (Toulmin, 2001).

As we already saw, which in this light is truly remarkable, the ideas, or as some say images, of science in these philosophies were by most scientists not only taken for granted but also somehow believed to be descriptive. One wonders why the science community and the public did (does) go along so well with the Legend. Was it despite the fact, or is it because it is normative and ideal, and not in any sense related to how science was done in practice? Do we all still very much want to believe and hope that science is really different from all other human activity and do we like to deem scientists as virtuous and pious as the high priests and cardinals that never where. Even when confronted with flagrant deviations, when the Legend is in doubt *‘there is often a significant shift in perspective. The image is no longer seen as descriptive but normative. Despite this shift, a connection with description usually remains. The problematic work is a deviation from the proper course of scientific activity, a course taken to be exemplified in the overwhelming majority of scientific investigation.’ (*Barker & Kitcher, 2013*).*

In his ‘*Human Understanding’* published in 1972, but also in his illuminating earlier and later work, Toulmin was one of the first to see this separation of the practice of knowledge from its theory as the major problem in our theories about science and research and thus of human understanding. Early in his career in Oxford he says: '*This was seen as being quite separate and independent and so a concern of different intellectual professions. At these times, natural scientists kept their eyes outwards, so as to avoid becoming entangled in philosophical word-splitting'p1.* But he continues *'There are in fact good reasons, both historical and substantial, for our establishing links between the scientific extension of our knowledge and its reflective analysis and reconsidering our picture of ourselves as knowers in the light of recent extensions to the actual content of our knowledge.' (p2).* On that same second page he already anticipated anxiety, uncertainty and scepticism, but he reassured the reader that *'a realistic appraisal of human understanding has often been an instrument for its systematic improvement'*. (Toulmin, 1972).

Toulmin could have known better, his early work in the 1950s took a different position on rationality and reasoning from the then mainstream philosophy. His ideas about the philosophy of science were inspired and in effect went through a reality check when he was being exposed in the war to real physics research and the actions of researchers in the lab. After the war he returned to study with Ludwig Wittgenstein who in those days had reconsidered the formal approach in analytical philosophy. Toulmin took up the historical approach to studying science in a natural way blended with philosophy and sociology. In this 'historical turn' he was a front runner and was therefore side-lined and largely neglected for three decades by mainstream philosophy (Toulmin, 2001), which as Shapin wrote, still did hurt after 40 years (Shapin, 2002). Interestingly, in line with my own experience as a student from 1975 on, those who in those days started to study the philosophy of science, somewhere in their career of an experimental natural scientist, gradually realized that the philosophy and sociology did not relate to practice of the natural science.

Introduction into Philosophy of Science

After obtaining a bachelor's degree in chemistry from the University of Groningen, I spent the academic year 1975–1976 studying philosophy of science. In my master study it was a minor with a major in biochemistry. This was inspired by my older brother who studied in the same period history and philosophy of education and philosophy in Groningen. Had my older brother chosen to study theoretical physics instead of pedagogy and philosophy, the course of my intellectual and personal life would most likely have been very different. Because I was completely ignorant, I had to study in the spring of 1975 as introduction the first 300 pages of Ernest Nagel's *The Structure of Science: Problems in the Logic of Scientific Explanation* (Nagel, 1961) in combination with Toulmin's more idiosyncratic *Philosophy of Science* (Toulmin, 1953). This was meant to be a high-speed introduction to be able to study Kuhn's *'Stucture'* and Poppers *'Logic'* followed by an intensive winter-course on the seminal book '*Criticism and the Growth of Knowledge'*, edited by Lakatos and Musgrave (1970). I found the image and discussion of science in Toulmin's book logical and his metaphor of maps for theories plausible. I

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recognized a lot of common sense in the description of instrumentalism by Nagel (1961, p129–140). Instrumentalism was down-played very much compared to the overwhelming emphasis on the natural sciences, mathematics, geometry and physics and its empiricism and logical axiomatic systems of positivism. For me, despite my chemistry bachelors with introductions in math, chemistry, biophysics but even some quantum physics, it was simply too much. Until very recently I labelled Nagel as a diehard logical positivist. I however should have paid more attention to the introduction of his classical book. Nagel clearly shows his preference for pragmatism in the Peircean style which is a plain critique of the empiricist-positivist philosophy of the 'Legend'. I also could have paidd attention to his references to C.S. Peirce, Frank Ramsey and John Dewey's '*The Quest for Certainty*', although then I had no clue who these writers were and how their position was in the field. I think I should, at that time, have been made to study Nagel's very interesting and illuminating chapters on the methodological problems of the social sciences and humanities that are, he clearly explains much less different from those in physics then generally believed. The reviewer in *The Times Literary Supplement* thought these chapters were *'the most interesting in the book'* as Nagel '*is concerned to establish that the social sciences are capable of producing useful general laws and explanations though their methods are necessarily not completely identical with those of the physical sciences…For the defense of the social sciences he considers among other, the objectives of non-repeatability and subjectivity in the selection of materials*.' Unfortunately, as said these chapters were exempted from my examination and only very recently when preparing for this writing I returned to Nagel and read them 45 years too late. Only very recently I realized that professor J.J.A. Mooij, a scholar of mathematics, physics, ethics, literature and analytical philosophy, who was the examiner, like Nagel, probably must have had affinity with American pragmatism, especially Peirce and must have also known Toulmin's *The Uses of Argument* from 1958. Apparently, I was well primed by this preparative reading, as I received Polanyi's *Knowing and Being*, as a gift from close friends in February 1976 on the occasion of my BSc graduation. In Polanyi's book the piece on *The Republic of Science* and comment on C.P. Snow's *The Two Cultures* are still quite amazing (Polanyi & Grene, 1969). I then bought Polanyi's *Personal Knowledge* in July 1976. Despite my disagreement with Polanyi's ideas about the interaction between science and society, for me his work really was an eye opener presenting intuitive and pragmatic support for the new post-empiricist philosophy (Polanyi, 1962). On my shelves I still have also one of the books of C.A. van Peursen, *Wetenschappen en Werkelijkheid* published in Dutch in 1969, which I read and marked up in the fall of 1975 preparing for the course. Van Peursen, who was a leading philosopher in the Netherlands in his time, already concluded that the best philosophy of science was a mix of Popper's and Dewey's philosophy, also

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referring to the later work of Wittgenstein, Quine, Polanyi, Winch, Gadamer and Habermas. At the end of this book he critiques the idea of value-free inquiry and with Dewey and the pragmatists firmly states that scientists, here used as including scholars in SSH, don't need to complement their work with 'diepzinnige' theories about 'reality'. 'Diepzinnig' may be translated with 'profound', but also with 'abstruse' or even 'esoteric', and it is the latter word that Dewey used to criticize philosophy which in his opinion had lost touch with science and the real world. Science and knowledge, he states was not the goal, but that science and research are integral to the life we live and want to live and are an important means to the end of our responsibility to create instruments for the right policies and their actions. In August 1976 I bought *Technik und Wissenshaft als Ideology* by Jürgen Habermas which made a huge and lasting impression (Habermas, 1968). Habermas argues for an ethically and politically proper interaction between science and social life and offers a model for it that is explicitly based on Dewey's pragmatism. My recent revisiting of this early work of Habermas made me realize that the discussions in those days about Science and Society took place in a very different public context than the current discussions about Open Science. Yet, the message to opening up science and engage and communicate with the publics is the same. Finishing this book in the early summer of 2020, I must hopefully add that the COVID-19 pandemic has made a lot of people in science, society and government aware of the power of the practices of Open Science. As the corona crisis was not only a global public health catastrophe but also caused a deep global social and economic crisis, the idea that we can do science differently may even linger a bit longer than it did after both world wars.

As has been noted by many, the very first lines of Kuhn's book immediately disclosed the exact same problem, I here in this book still feel must be addressed, although in 2020 for slightly different reasons: *'History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed'*. At few lines down he states: '*This essay attempts to show that we have been misled by them in fundamental ways. Its aim is a sketch of the quite different concept of science that has emerged from the historical record of research activity itself'….however this new concept of science will not be forthcoming if historical data continue to be sought and scrutinized mainly to answer questions posed by the unhistorical stereotype drawn from science texts. …a concept of science with profound implications about its nature and development (p1)* (Kuhn, 1962).

It was immediately very clear that Kuhn dramatically changed the discourse of the philosophy of science and its research agenda by taking the 'historical turn'. Ian Hacking has in his typical and eloquent but straight forward manner described the conceptual differences between Kuhn and the major concepts commonly held in the standard image of science (p6–16). (Hacking, 1983) These differences do concern issues of how science is being done in the real, but also affect the philosophical assumptions and prescriptions of the *'unhistorical stereotype'*. Differences do regard the classical image of individual inquiry compared to communities of inquiry bound by research traditions and paradigms and the idea of distinct phases of normal versus revolutionary science. The community aspect was not disputed, but a lot of subsequent modern historical work showed that the very distinct scientific revolutions in time, as described by Kuhn in physics and chemistry, are not common and that most of the time in science different schools and paradigms do operate simultaneously until one of them is favoured. Kuhn's work did not provide support for the use a general method which unifies science, an important aspect of the standard image of science for the positivists but also for Popper until then. But there was more. A paradigm in Kuhn's view is a composite of classical internal formal scientific rules, techniques and experimental methods and values, but also conveys values of external social, cultural, ethical and practical origin. These are involved in daily question on which grounds new results and claims are judged by peers and when major claims and theories are questioned, and their novel competitors have to be considered. Paradigms give guidance in deciding what to belief. Here we advance to the second level of criticism of the Legend. Kuhn based on his historical work deviates from the positivistic norm of what scientific statements are, the analytic-synthetic dualism and the criterium of objectivism, a major pillar of the Legend and the Standard Model, as we have seen above. He was, a bit to his own surprise, caught in serious long-lasting discussions about relativism, subjectivity and objectivity. These discussions about the internal logic and consistency of the major theories and assumptions of the standard image of science were in 1962 already for quite some years ongoing between highly esteemed members of the discipline of analytic philosophy, as we shall discuss below. Hacking wrote a very concise and comprehensible explanation of the immense importance Kuhn's book has had and still has (Hacking, 1983) Kuhn did not only question the Standard Model and Legend regarding the ideas about the scientific method versus its mismatch with the daily practice, but he also questioned the logical-positivistic ideas of rationality. He did not engage in their highly esoteric and technical discussion but showed based on his historical work that scientists simply did not comply with some of the major prescriptions, and that anyhow even if they would have tried, they fail because these could not be followed in the practice of inquiry. He receded to some degree in this in response to his critics saying that he believed that internal empirical scientific data and findings ultimately were the most important criteria for believing or rejecting a claim, statement or theory. It is of interest to note that after Kuhn's book appeared 'fresh interactions between philosophers and historians of science' came about. There may then have been several reasons for the separation of these now closely related disciplines, but Toulmin very critically points to '*George Sarton from Harvard (who) ruled over academic History of Science in the United States'* and had declared collaboration taboo (p6). (Toulmin, 2001) Toulmin makes it clear that the study of the history of science stood in a lower rank than philosophy and that the history of science field had its own ideas about what good history scholarship was. With Kuhn, he concluded that historians held their distance from inquiry that involved study of external, social and cultural, economic and political factors. Bernal's seminal work "*The social function of Science* published in 1939 also for that reason was neglected for a long time (Toulmin, 1977).

The Empirical Turn in the Sociology of Science

The Other Mertonian Thesis. It is not only fair to say, but highly relevant for the logic of my book, that I until now presented the dominant and legendary interpretation of Merton's sociology. This was the image of an autonomous social system which was governed in an ideal fashion by scientists who were not troubled by the moral and social defects of all other human beings in modern societies. But there was another side of Merton's sociology which is in agreement with the sociology of science that became mainstream in the 1970s but is of of totally different kind as the Mertonian legend. Steven Shapin, and later Harriet Zuckerman, the latter who at Columbia was a collaborator of Merton and much later in life his married wife, demonstrated that Merton clearly recognised external influences on science and not only of the religious, but also of the utilitarian and military kind (Shapin, 1988; Zuckerman, 1989). Merton has become widely known, and criticized, for his thesis, following Max Weber's well know theory, that Puritanism, Calvinism and Pietism are important external factors that may explain why the rise of modern science occurred in Western Europe (Cohen, 1994). Shapin quotes many lines and phrases from Merton's early book on the history of science that was published in 1938 (Merton, 1938), to show that Merton has not been properly read in this matter: '*Merton then proceeded to point to "further orders of factors," some cultural, some social, that might be thought relevant to explaining the historical materials with which he was concerned. These included interesting speculations about population density, the rates and modes of social interaction characteristic of different societies, and other features of the cultural context not included in religious construct. Merton carefully noted that Puritanism only "constitute[d] one important element in the enhanced cultivation of science." In other settings "a host of other factors - economic, political, and above all the self-fertilizing movement of science itself'- worked "to swell the rising scientific current." Since science burgeoned in Catholic sixteenth-century Italy, Merton freely acknowledged that "these associated factors" might come to "outweigh the religious component'*. p595–596. (See Shapin for references to these citations of Merton.) Merton describes the mutual interdependency of science with other social institutions and their vested interests which has directly or indirectly influenced the direction of science and research through problem choice. This obviously is a problem in view of disinterestedness and objectivity of the Legend, which Shapin addressed upfront: *'at the very core of his enterprise, historians nervous about the black beast of "externalism" should be reassured. Neither in his 1938 text nor in subsequent writings was Merton ever concerned to* *adduce social factors to explain the form or content of scientific knowledge or scientific method*'. p594 (Shapin, 1988). Merton discusses the external socioeconomic effects on the dynamics of problem choice and subsequently that of scientific (sub) disciplines. Issues of the different personal motivation's scientist may have and which they often openly state which may relate to the potential practical and technological application of their research but also looking to the social status of research for their upwards social mobility. These studies about social interdependencies seem to have been collectively and selectively overlooked by historians and sociologists, verging according to Zuckerman on counterfactual history. For Merton, as Zuckerman points out, during his whole career the Puritanism Thesis was minor, compared to *'military, economy, geography and society'* as is reflected in the number of chapters devoted to them in Merton's book of 1938, reprinted in 1970) and subsequent writings. She refers to I.B.Cohen's review of the book (after it was reprinted), who thought that this minimal interest in influence of socio-economic and military factors on science was in the 1930s not new because it was already a major theme in Marxist sociology of science, whereas the proposition of a connection between religion and science was novel. I argued above discussing the work of Bernal, in agreement with Shapin, that indeed these ideas were dominant in Marxist sociology and theory of science, but not acceptable outside these circles and surely not mainstream in the late 1930s. With McCarthyism in the late 1950s and after Sputnik, during the years of the Cold War these chapters on external factors were, to put it mildly, tainted with Marxism and Socialism and not 'in sync' with the ideologies and images of science of the Legend.

In the 1970s and 1980s a new sociology and history of science was developed, called Sociology of Scientific Knowledge (SSK), from the perspective that in a '*sociological approach to knowledge-making, people produce knowledge against the background of culture's inherited knowledge, their collectively situated purposes, and the information they receive from natural reality.*' (Shapin, 1982). This research in sociology and history thus goes further than the classical dominant forms of history of science and further that Kuhn by bringing in external social values in the equation. It not only, as discussed above, shows how the practice of science really is, but is also shows how theory choice is done and how beliefs and scientific statements become accepted, and in that respect provide empirical sociological evidence against the Legend. The quote above is from an early seminal paper by Steven Shapin, a historian who became in his own words a sociologist and was one of the pioneers leading the way in this new interdisciplinary field between history, sociology and to some extend philosophy of science. Shapin very explicitly contrasts the two main approaches to the study the sociology of scientific knowledge. I will stay away from too much technical language but summarize the main points most relevant for the context of our present discussion of the demise of the Legend. Shapin builds a strong case, with a well-developed critique of the mainstream history of science, complemented by an overwhelming series of examples of more recent historical research with an empirical sociologically inclination. The latter research by among others Collins, Pickering, Geison, Wynne, Harvey, MacKenzie and Barnes, and Latour and Woolgar produced evidence obtained from cases widely distributed in time, place, and discipline for influences of 'non-cognitive' external cultural and religious values, political principles, beliefs and ideas on the process and the ultimate outcome of scientific inquiry. In effect, supporting the theoretical hypothesis as formulated in the quote on top of this paragraph.

We have seen above that the dominant history of science before 1960 or so, was confirmatory to the myth of the Legend and positivism under heavy direction of George Sarton. In a striking analogy, also in the history of sociology such a thing has been dominant for a large while. I will cite Shapin on the characterization of this sociology which he calls *'the coercive model'*. I will start with his conclusions: *'…. more significant problem arises from a largely informal model of sociology of knowledge which seems to be prevalent among a number of philosophers and historians of science…..Its main characteristics can be briefly described: (i) it maintains that sociological explanation consists in claims pf the sort "all (or most) individuals in a specified social situation will believe in a specified intellectual position"; (ii) it treats the social as if one could derive it by aggregating individuals; (iii) it regards the connection between social situation an belief to be one of 'determination' although little is explicitly said about the nature of determinism; (iv) it equates the social and 'irrational'; (vi) it sets sociological explanation against the contention that scientific knowledge is empirically grounded in sensory input from naturally reality.'* This has informed the classical sociology of science with respect to the role of individuals in the community '*generally regarded as troublesome'* and '*the connection of the social and the cognitive would generally be sought through the use of individual orientation particularly through motivation…factors internal to the scientific community would be viewed as non-social. Finally one would say as little as possible about the fact that scientists conduct experiments, look through microscopes, go on field expeditions, and the like , for wherever 'reality enters in, the sociological explanation is obliged to stop …the coercive model has* ***two splendid advantages****. First…no successful instance of its practice will ever be encountered. Second, it portrays the role of the social and of sociological explanation in an unpalatable normative light: as if it were said that "no rational person would ever allow himself to be socially determined! Nevertheless, there is one major problem…; namely that it is not an accurate picture of sociological practice'. P195 In a sociological approach to knowledge-making, people produce knowledge against the background of their culture's inherited knowledge, their collectively situated purposes, and the information they receive form natural reality. Perhaps the most puzzling charge sometimes laid against relativist sociology of knowledge is that it neglects the role played by sensory input. On the contrary, the empirical literature employing this perspective shows scientists making knowledge with their eyes wide open to the world' p196.*

Shapin explicitly elaborates on inquiry and its purposes and goal-directedness not set by *'contemplative'* individuals, but by a community where *by doing things with knowledge that its meaning is produced*'*. The purpose for which knowledge is produced and according to which it is evaluated may vary widely: they may include legitimation or criticism of tendencies in the wider society, or they may encompass goals generated exclusively within the technical culture of science.'*

Shapin argues that the ideal type of the modern scientist should take these sorts of considerations, of this broader spectrum of social and cognitive scientific interests, into account. In this view of science, which is not compatible with 'rationality' of the normative Legend, according to Shapin, *'the role of the social is to pre-structure choice and not to preclude choice'* p198.

There clearly is in 1982 still a huge tension here with the Legend and its positivism: *'While it may be banal to say that statements of scientific fact may be theory-laden. It is not, apparently banal to* <u>*demonstrate*</u> *this empirically and to pin down the specific networks of expectations and goals affecting the production and evaluation of statements of facts…Historians act as if, after all, observed facts count as 'hard case'; making a fact into a historical product (an artefact) is an exercise which historians of science approach with great caution (even though scientist do it routinely)'* (p159). The latter remark is of interest and sounds familiar in the present context because it refers to the way how active scientists 'pragmatically' deal with these philosophical ideas. Shapin states that the classical historians of science assumed that with the professionalization of science the scientific community obtained autonomy towards social factors and their influences. Here social factors are regarded as limited to obviously external social and political values. "*To many writers an 'influence from Malthus (or from Paley)* [on Darwin] *has not been something to describe and explain, but something to be* <u>*explained*</u> *away, since from the present perspectives it would be regarded as an illegitimate inclusion in properly objective scientific thought.'* It is because of this influence, according to Gillispie, '*that it is inconceivable that* the *Origin of Species could have been written by any Frenchman or German or by an Englishman of any other generation.'p179.*

Shapin draws attention to professional vested interests that are internal to science and research, but not strictly cognitive. Active scientists know these very well as they determine the ongoing discussions, at the moving front of research, with reviewers 'from other schools' at journals, grant review committees, scientific committees selecting conference contributions (selection of main speakers and of oral abstract presentations), academic promotions committees and decisions who writes or contributes to textbooks. All of these judgements determine what 'we' hold to be 'good' research or 'the best' research at some point in time, which over my 40-year career developed and changed rather quickly (Miedema, 2012). An outstanding analysis of the diversity of private, professional, cultural, social and economic factors that influence the practice of inquiry and knowledge making is Gerald Geison's study '*The Private Science of Louis Pasteur'*. (Geison, 1995) This book was by many especially French scientist considered to be debunking Pasteur. It was published, at the same time as more hagiographic biographies at the centenary of his death in 1995, but by experts highly praised because it provides deep and detailed insights how knowledge, in basic but also in applied biomedical research with enormous societal and economic impact, was and is produced. In a critical, humiliating review of Geison's book, Max Perutz (1914–2002), who was a famous biophysicist, defended Pasteur, against Geison's demonstrations and judgements of Pasteur's obvious foul play (Perutz, 1995). The bottom-line of the defence was that in the end Pasteur had been proven right, only the facts count in Perutz' opinion. The real issue at stake, that clearly surfaced in the exchange that followed in the NYRB, was that an outsider, not a man or woman from the lab, apparently not with 'pious reverence' and excessive respect, was messing with men of science and its methods (Miedema, 2012). Other writers of recent history of science, such as Crewdson have been overly critical, for instance regarding the role of Robert Gallo, in a study of the discovery of HIV, the aids virus in 1983 (Crewdson, 2002). Crewdson on the other hand has undue sympathy for the 'underdog' in this dispute that involved massive professional reputation including a 2008 Nobel prize, national politics and economic interests (Miedema, 2002).

Shapin has since 1982, written a number of classical highly influential journal articles and books about the practice of science and the production of knowledge in the seventeenth century and in our times by doing in depth research using historical and sociological methods in which all of the above topics, theories and problems are addressed (Shapin, 1994, 1996, 2008; Shapin et al., 1985) In the last pages Shapin provides a balanced discussion of how to view the influence of external and non-cognitive factors on knowledge production. Some researchers simply regard it as wrong based on the ideal of objectivity and value free science and studies in sociology or history that reveal these influences are considered damaging and 'aspersions'. Some regard these influences as realistic, it happens and is difficult to avoid, but they are <u>per definition corrupt</u> because science is, and its institutions are in that way being hijacked by all kinds of powerful politically and socially organized groups and their interests. Shapin regards these views as 'a misunderstanding' as external values and concepts have had and may have beneficial effects on the growth of knowledge. Opening up science to less powerful publics has these risks and as discussed in depth in Chap. 5, it will require continuous debate to resist the capture of science by the economic powers in society.

The Myths of Science: Frontstage and Backstage

Humans and scientist alike need certainty, a logical method, an algorithm, with timeless and thus objective foundations. But the Quest for Certainty has failed. We have in reaction to this in the 1990s seen academic debates and worries about loss of certainty and foundation of scientific truth. This mainly was a reaction against certain forms of excessive post-modernism, relativism and subjectivity. Several authors have discussed these worries to demonstrate that science is unique as a knowledge producing system, that produces robust, reliable and significant scientific knowledge even if we acknowledge that there is no metaphysical, given formal method or rules and foundations to guide us at truth. I will return to that discussion in Chap. 4 when discussing the default of pragmatism for the philosophy of science after the era of the Legend.

For now I want to discuss the reasons for the anxiety and worries academics experience whenever it is publicly discussed that the legendary image of science does not match with the practice behind the doors of the sociology, psychology, philosophy and history departments, but don't make a mistake, behind the doors of laboratories of the natural, biomedical and geosciences as well. This anxiety almost every time pops up also at less public debates about the Legend and how to arrive at a more inclusive way of thinking about science and the design and organisation of our academic institutions. I use the vocabulary frontstage – backstage from a framework developed by Goffman (1959). Thinking about the Legend, our popular image of science, the myth of which has been shattered by its novel criticisers but also by its erstwhile major proponents, Goffman's dramaturgical model for social interactions can be of use. Not only humans in their interactions knowingly assume different behaviour and roles regarding the relationship, interaction and social context they operate in, but likewise public organizations and institutions show different behaviours in different situations, meant for different publics. In many instances in public theatres, formal meetings or media appearances presentations by representatives from financial institutes, banks, government or institutes affiliated with government, the church, the hospital administration and private companies follow the frontstage narrative or storyline. This, of course, presents the perspective of a reassuring, sophisticated, empathic, politically and socially correct reflective organisation. Of course, for different organizations, different items may be considered for an idyllic frontstage story and attitude. It is precisely this function that the Legend has had, and to a still lesser degree still has for science and academia. Most of the writers I cited thus far and will be cited further on, in the introduction and epilogue, but often throughout their analyses in many different wordings relate to the worry they or the scientific community may have when they debunk the myth of the Legend. The myth of the Legend, as demonstrated above, has been debunked by a few in the 60s, but openly many times since the 1970s by prominent thinkers which has reached a relatively wide audience, outside and inside science. Relatively, since in most cases even during the so-called Science Wars of the 90s when a larger audience got interested in a short while, it is a fairly limited readership. As pointed out above, active natural science researchers or even humanities scholars, in normal times take the Legend for granted, they intuitively know how to produce knowledge and now the mores of their field, but get nervous when the spell is broken, the myth of the Legend destroyed. All of a sudden one has to realize what the real backstage situation and the correct corresponding narrative is for that. That is very, very hard, since we are coming from the Era of the Legend, where scientific inquiry as we have seen is held to be unique, timeless, to provide for knowledge with absolute truths and because of its methods, rules and bedrock logical foundations has proven to be successful and to be successfully applied in the modern world. It may have been a problem for the philosophers who gradually saw the Quest for Certainty and their dreams and wonderful philosophies of timeless foundations, unified science, formal analytic methods, realism and positivity come to an end. For scientist and those working in science and academia the problem is less esoteric and practical but felt to be tricky. The fact that we can no longer use the Legend as a frontstage ideal narrative of science, that has carried much weight since the 1940s, is indeed difficult. It has been rather effectively used to claim authority for science in public debates, about safety of vaccines, the cause of climate change and what should be done about that. It has been used to discuss many public health and prevention and political issues relating to inequality, fair economics, the regulatory role of government in neoliberal times, but also on an annual basis by some about the absolute prominence of basic natural science.

So, one wonders if we admit that science in the real is done as we do it -producing the claims and insights we believe by a uniquely robust and open, continuous purging, process of testing, of experiments, repeating of experiments, a lot of criticism and debate in a cycle of improving and rejecting- will that convince the public as well as we did convince them with the story of the Legend? Most of the writers, including myself, say yes, that shall do. Be honest, show how knowledge is and has been produced, how robust the process is also when we know that social interests of cultural and personal source are at play. Be frank about the fact that every claim, theory, method, action based on this process is fallible and may eventually be improved, corrected and rejected because it is replaced by a better alternative.

I will here not discuss the Science Wars of the 1990s. '*The One Culture'* by Labinger and Collins (2001) presents a highly readable series of short papers of heavily involved authors with different perspectives on that. The Science Wars was a reaction of the natural science defenders of the Legend to claims in academia that-because postmodernist relativism had shown that there is no scientific method as held by empiricism and positivism- scientific theories and accepted beliefs are in essence not different from the beliefs derived outside science from superstition and all kinds of popular, religious and personal opinion. This image of science, which derives to be honest in some respects from Rorty's bold interpretation of Willem James' pragmatism, was at the far end of the spectrum opposite of the theory of inquiry of Peirce and Dewey, later extended especially by Putnam (see Chap. 4). The defenders or 'bulldogs' of science went all out with an appeal on the Legend which was not constructive. Fortunately, many philosophers have offered realistic and pragmatic views of science and its practice, without taking refuge to metaphysical and foundational myths of the Legend. I refer to Ian Hacking again, and especially his *The Social Construction of What?* where he in Chaps. 1, 2, 3 and 7 makes a very clear case for the realistic and naturalistic middle ground (Hacking, 1999) and to a very insightful and opiniated review by Shapin (1982). These studies show that there are clearly social and cultural factors at play, but that there are constraints to our claims and ideas in the confrontation with and observation of natural and social reality and these together in a continuous critical debate guide the process of how beliefs get accepted and hypothetical claims become facts. (p33) Our realistic understanding of the practice of scientific research, where collective reason, experiment and action ground our beliefs which is constrained by conditions in the real world being the natural or the social world.

The good news in my mind is that we can pragmatically make a very good point for the reliability of science as follows. Since modern times we have this new robust collective way of doing science by hypothesizing and experimentation, its ever-improving methods, techniques, technologies and the ever-growing collective experience with judgements of claims and experimental results using ever improving sophisticated methods and methods of reasoning. This has resulted indeed in impressive success, changing our lives by changing the unfriendly environment, improving our health and life expectancy, allow quick, convenient and mass transportation, modern communication, increasing personal and global wealth, dealing with issues of energy, and so on. This all has been achieved despite the fact that even in the natural sciences we never had a unified formal objective value-free scientific method, an no timeless foundation for our knowledge to build on. Social and political values have always at several levels been involved in our evaluations and criticism of what to study and what to belief in scientific inquiry. This inclusive deliberation has steered science in society also in modern history to the good but sometimes to the bad. Our common-sense collective methods of inquiry have brought us time and again wonderful results that changed our life's in the past 200 years.

'OMG…….There Is No Foundation!'

The epistemic core in the philosophy of science and the Legend is empty, was the conclusion of Nowotny et al in their *Re-Thinking Science* that I will discuss in detail in Chap. 5. But I use it here for its analogy with the evolution of the thinking that many of us have had regarding religious beliefs. The story about the Legend of science feels, I image, to many who were raised on the Legend since elementary school, high school and university as loss of certainty and loss of a familiar story that provides for calm and rest of mind. For me it compares to my growing up in a Calvinist family in the North of the Netherlands during the 1960s, where despite the non-academic background of our parents for them and us, reading and studying was part of life. Gradually, I came to realize at the age of 6, I think, that Santa Claus did not exist, but that was alright with me. Much more complicated was in the years between my 14th and 20th year how to think about the origins, foundations and the revelations of our Christian beliefs, ethics and ideal practices. Specially my father was convinced and believed the factual truth of the New Testament, from cover to cover, and this and the ethics and prescribed practices were regularly discussed at home. As a bachelor student I started reading modern theology amongst others Rudolph Bultmann, which made a lasting impression on me. In particular, his demythologization of the biblical texts and his rejection of the supranatural as world views belonging to another cultural context in the past, not appropriate for our modern time were strong images for me. He posed the idea that the biblical stories are not facts but language and texts describing acts of God. There is a core in the text, a message that in every time and culture can have its own narrative form. I had concluded that I did not believe in any of the supernatural, which until now has not caused me more than average anxiety. I was, however, for ever a Calvinist engaged by the ethics and social-democratic politics that came with my upbringing and later reading the modern ethical and political interpretations of the Biblical texts by

(continued)

Bonhoeffer, Sölle, Bloch, Moltmann, and Pannenberg. These writers influenced Kuitert a Dutch theologian who's public intellectual and emotional struggle I with many others followed since 1971 until his dead in 2017. In a series of books, he goes through a sequence of phases in which he gradually peeled of the layers of classical Calvinist theology and its dogma's. Eventually and it seemed inevitably, he had to admit in the 1980s that there was no foundation, all our speaking and theology about the divine and the supranatural was the product of humans. He was also clear to point out that these revelations thus were not Divine revelations and not God given. Here again the same question as for science comes up, do we have a good enough narrative about Christianity and religion in general if we demythologize its foundation and reduce it to ethics and action in contribution to human flourishing and the good life. Harry Kuitert argued that these 'inspired' ethics and this social-political awareness based on diverse cultural and personal values may shape socialist, conservative or liberal worldviews and policies alike.

2.2 Part 2. The Crisis in Analytical Philosophy

> *The spirit of Cartesianism is evidenced not only by rationalists but by all those who subscribe to strong transcendental arguments that presumably show us what is required for scientific knowledge, as well as those empiricists who have sought for a touchstone of what to count as genuine empirical knowledge.….the first attack was made by Peirce. Nevertheless it has taken more than hundred years for us to become fully aware of how the Cartesian view distorted the way in which science is actually practiced.'p71* (Bernstein, 1983).

The crisis in analytical philosophy started around 1960 in the philosophical discipline that created the problem in the first place. Crisis became apparent in open debates when philosophers officially declared the dead of positivism and empiricism. Philosophers had admitted much earlier that there were already cracks in the idea of a foundation and other aspects of the Legend. C. S. Peirce was on one of the first *'to attack the Cartesian framework especially in regard to characterizing scientific knowledge' (p71)* (Bernstein, 1983). His work, in the last decades of the nineteenth century that was followed up by the American Pragmatists James and Dewey until 1940, did not belong to main-stream analytical (logical-empiricist) philosophy and did not get much attention there, apart from Frank Ramsey who's engagement with pragmatism was cut-short and almost forgotten by his untimely dead in 1930 and Nagel, which will be discussed below. Eventually the debate developed with the work of W. V. O. Quine in the 1950s; Popper and Michael Polanyi 1958, 1959; Kuhn in 1962; followed by Toulmin, Feyerabend, Apel and Habermas, Hesse, Hacking, Putnam, and Rorty in the 1970s early 1980s.

This critique on logical positivism and empiricism in the 1970s reached a much larger audience also outside the departments of the philosophy of science. Gradually, it was picked up some active natural scientists or SSH scholars who had an interest in philosophy and sociology of science. However, it appeared -and even in 2021 appears- to be hard for several reasons to go beyond the truly mythical Legend, letting go of the ideas of a timeless foundation and the dreamed formal methods of a science, even when it was realized that it was a method of 'a science that never was'.

Regarding what was at stake, I will again quote Bernstein who has discussed in great detail and transparency these debates and in strong statements the image of science that emerges in the post-empiricist philosophy of science in contrast to image of the logical-empiricists for which Ziman and Kitcher coined the name 'Legend': '*We can interpret this movement of thought as contributing to the demise of Cartesianism that has dominated and infected so much modern thought. The Cartesian dream of hope was that with sufficient ingenuity we could discover, and state clearly and distinctly, what is the quintessence of the scientific method and that we could specify once and for all what is the meta-framework or are the permanent criteria for evaluating, justifying, or criticizing scientific hypotheses and theories. The spirit of Cartesianism is evidenced not only by rationalists but by all those who subscribe to strong transcendental arguments that presumably show us what is required for scientific knowledge, as well as those empiricists who have sought for a touchstone of what to count as genuine empirical knowledge.....the first attack was made by Peirce. Nevertheless it has taken more than hundred years for us to become fully aware of how the Cartesian view distorted the way in which science is actually practiced.'p71* (Bernstein, 1983).

A Detailed History of the Philosophical Demise of the Legend

As I pointed out at the introduction of this chapter, it is the analysis of the origins and effects of exactly this distortion that is the topic of this book. I will in these remainder of this Chapter discuss the philosophical arguments that convincingly show why the analytical and positivist philosophy failed. I will not go in great detail about the technical discussions. I chose to offer a diverse chronological selection of thoughts and conclusions of the most prominent scholars. I provide the most illuminating citations taken from their work. Readers may wonder why I sometimes cite longer paragraphs. It is because in my opinion they are essential and because I want to give the reader the opportunity to directly read this primary 'material' with no need to have to rely on and trust my paraphrase's and interpretations.

Below I discuss in historical order the work of the major scholars since 1945 the problems of positivism, the analytical philosophy and empiricism, which demonstrates the collective developments in the field and in some cases the personal development and struggle to break free from foundationalism. For the readers who do not know the authors which I am going to name in the remainder of this chapter, without exception they all were, or are when still alive, the absolute top scholars in their field. It makes you wonder that only the true elite, the leading scholars in exactly the field of interest were in a position to challenge the main theoretical ideas and concepts of logical positivism and empiricism, largely the legacy of the Vienna Circle that had been build up over the past 50 years. Most of them had actually trained with that previous generation of top philosophers who had all contributed and shaped exactly these philosophies. They were mostly students or second-generation students of Wittgenstein in the UK, and Carnap, Reichenbach, Hempel, Quine in the USA. It apparently is quite difficult, and it requires a reputation and a position of intellectual power to change the thinking in a field, which is a case in point of Kuhnian paradigmatic revolution and of the power struggle in a 'field' as described by Bourdieu. (Bourdieu, 1975) Ludwick Fleck, who anticipated Thomas Kuhn's major work by at least 30 years, writing in 1935 about criticism in science said that writers who trained as sociologist or in classics, 'no matter how productive their ideas, commit a characteristic error (Fleck, 1979). They exhibit an *'excessive respect, bordering on* ***pious reverence*** *for scientific facts'*, cited by Ian Hacking, but Hacking adds: '*The era of excessive respect has passed'* (p60) (Hacking, 1999). I believe that this excessive respect was not so much for 'scientific facts', but for the mythical power of the scientific method of positivism that claimed the status of these facts and the status it provided to the scientists.

I start with C. S. Peirce who wrote long before any of them and was part of his own "Metaphysical Club' some 30 years before the Vienna Circle had started (Menand, 2001; Misak, 2013a). Peirce, as said, was later recognised as the *'first to attack the Cartesian framework'* and influenced many if not all of major modern philosophers, before 1940 and after some lag time again directly or indirectly since the 1970s (Bernstein, 2010).

C.S. Peirce who did his most influential writing at the end of the nineteenth century, was one of the first to attack the Cartesian framework (Bernstein p71). The framework of the idea of a transcendent foundation and the empiricist formal method. He was trained as a chemist in the natural sciences and is now considered to be exceptional regarding his many original ground-breaking contributions to natural science and in particular in the philosophy of science which are studied with renewed interest until this day. Many influential philosophers have payed tribute to Peirce.

Ernest Nagel in 1939:

> *'Peirce's distinctive contributions to logic as the general theory of signs, centre around his pragmatism, his critical commonsensism, and his fallibilism. By far the best known is his pragmatic maxim, proposed as a method for clarifying ideas, eliminating specious problems, and unmasking mystification and obscurantism hiding under the cloak of apparent profundity. In one form or another his proposal was adopted by a number of distinguished thinkers, for example, in this country by William James and John Dewey, so that to-day it is almost a common- place. Peirce's own formulation of the pragmatic maxim leaves much to be desired in the way of explicitness and clarity; and more recent formulations, such as those by Professor Carnap and others, have the same general intent but superior precision. I nevertheless venture two general remarks on the Peircean version of pragmatism which, though obvious, merit attention. The pragmatic maxim was intended as a guiding principle of analysis. It was offered to philosophers in order to bring to an end disputes which no observation of facts could settle because they involved terms with no definite meaning. It was directed at the Cartesian doctrine of clear and distinct ideas, which found the terminus of analysis in vague abstractions claimed to be grasped intuitively, as well as at the common tendency to convert types of behaviours into unknowable agencies controlling the flux*

of events. Above all, it pointed to the fact that the "meanings" of terms and statements relevant in inquiry consist in their being used in determinate and overt ways. ***Pragmatism, to employ Peircean language, was thus a proposal to understand general terms in terms of their concrete application, rather than vice-versa****. '(bold case by FM)*

At the risk of treading upon ground on which angels fear to step, I should also like to mention the elementary point that in terms of Peirce's emphasis neither terms nor statements can be regarded as designating, independently of the habits involved in their use. Consequently, "the meaning" of expressions is not to be sought in self-subsisting "facts", "essences", or other "designata", but must be construed in terms of the procedures associated with them in specific contexts.

'Peirce claimed no infallibility for the beliefs of every-day experience, and indeed one of the cardinal tenets of his thought was a universal fallibilism. Peirce's fallibilism is a consequence of his regarding the method of science as the most successful yet devised for achieving stable beliefs and reliable conclusions; it has nothing to do with the malicious scepticism which rejects science on the ground that its conclusions are after all not established as being beyond the possibility of error, only to invoke a special set of imperatives as indubitable objects of human endeavour. Peirce noted that the conclusion of no scientific inquiry is exempt from revision and correction, that scientists feel surer of their general logic of procedure than of any particular conclusions reached by it, and that the method of science is self-corrective, both as to its own specific features and the specific conclusions gained with it.' Read at the Fifth International Congress for the Unity of Science, Harvard University, September 3–9, 1939. (Nagel, 1940)

Habermas in his *Erkenntnis und Interesse* in 1968 translated in English in 1971 (ref) devoted two chapters to Peirce: *'What separates Peirce from both early and modern positivism is his understanding that the task of methodology is not to clarify the logical structures of our scientific theories but the logic of the procedure with whose aid we obtain scientific theories. We term information scientific if and only if an uncompelled and permanent consensus can be obtained with regard to its validity. This consensus does not have to be definitive but has to have definitive agreement as it goal….modern science distinguishes itself by a method of arriving at an uncompelled consensus about our views.'* p91 '*For Peirce there was one method of inquiry, based on deduction, induction and to a small degree inference to the best explanation* (designated abduction by Peirce). *Truth was roughly, whatever hypothesizing, induction and testing settled down on.*' (p118) Peirce named it the 'scientific method', the logic of or method of inquiry, but he did not mean to suggest that it is a logical formal system that allows us to get to the truth. Habermas: '*For Peirce this concept of truth is not derivable merely from the logical rules of the process of inquiry, but rather only from the objective life context in which process of inquiry specifiable functions: the settlements of opinions, the elimination of uncertainties, and the acquisition of unproblematic beliefs-in short the fixation of belief.' p119* Peirce resolutely rejected the Cartesian foundations, transcendental necessity and conditions, the so-called 'spectator theory of knowledge' that assumes the fact-value dualism. *'For Peirce it is the method'*, says Habermas, '*that takes over the role of an unshakable foundation, the a priori judgements that per definition cannot be doubted because they are a 'given'*. p97 This thinking of Peirce was many years later followed up by great men like Sellars and Quine. Peirce assumed a constant state not of scepticism but fallibilism, with continuous doubt about our claims in which he anticipated much of Popper's falsificationism published in 1935.

John Dewey already concluded in the beginning of the twentieth century in many of his writings that philosophy appeared to be an internal debate for philosophers, esoteric and of little value to understanding and guiding the practice of scientific investigation and its relation to reality, society and human life. Dewey in *The Quest for Certainty* (1933) and elsewhere wrote extensively about what Bernstein called 'the 'Cartesian Anxiety', *the belief of Descartes that the philosopher's quest is to search for an Archimedean point on which we ground our knowledge'*. (1983, p16) I will cite the crisp and concise remarks of Hacking about Dewey's criticism of the philosophy of science of the empiricist and positivistic tradition. Hacking later confessed (Misak, 2007) that he himself found it hard to read Dewey, '*it goes on and on'* and that feeling is familiar to me. Hacking: *'Truth is whatever answers to our present needs, or at least those needs that lie at hand. Dewey gave us the idea that truth is warranted acceptability. The world and our representation of it seems to become at the hands of Dewey very much a social construct. Dewey despised all dualism- mind/matter, theory/practice, thought/action, fact/value. He made fun of the 'spectator theory of knowledge'. He said it resulted from the existence of a leisure class, who thought and wrote philosophy, as opposed to a class of entrepreneurs and workers who had not the time for just looking.*

Hacking, says about Dewey: *'My own view*, *that realism is more a matter of intervention in the world, than of representing it in words and thought, surely owes much to Dewey.'* (p62) (Hacking, 1983). Pragmatism, from Peirce, Dewey, James, Nagel, Quine to Habermas and Hacking, is beyond Cartesian empiricist philosophy and holds that it is this relation to practice, intervention, and actions based on our accepted beliefs that gives value to our beliefs, and not timeless transcendent formal principles that cannot be tested.

Karl Popper (1902–1994), was a most influential philosopher of science who in his later years also wrote extensively about the open society, freedom and democracy. He was in time and space close to the empiricist positivist philosophers of the Vienna Circle but did however not agree with most of their philosophy. In his "*Logic der Forschung: zur erkenntnisstheorie der modernen naturwissenschaft*" published in 1935, a translation of which appeared in 1959 under the title "*The Logic of Scientific Discovery*", he criticised the positivist and empiricist philosophy on their major ideas. It has been said that this critique, after the members of the Vienna Circle having tried to incorporate some of it, eventually in the 1950s caused the declaration of the death of logical positivism. Popper wrote in his autobiography, *Unended Quest* (chapter 17), that he rather thought the Vienna Circle came to end because they did not address the real problems, but got immersed in debates about minor problem, puzzles and in particular the meaning of words. Although this echoes the critique on philosophy of Peirce, James and Dewey, they are not mentioned by Popper in this discussion of philosophy of science. Toulmin, but not even Kuhn is mentioned which is remarkable, given the impact of Kuhn's work on the legacy of logical positivists, that was already tangible at the time of Popper's writing (Popper, 1976).

In the 1958 introduction to the English translation of *The Logic of Scientific Discovery,* Popper states that he is a pluralist and he commends the philosophers *'who do not pledge themselves in advance to any philosophical method, and who make use of epistemology, of the analyses of scientific problems, theories, and procedures, and, most important, of scientific discussions. …Its most important representatives… were Kant, Whewell, Mill, Peirce, Duhem, Poincaré, Meyerson, Russel and later in some of his phases Whitehead. Most of those …would agree that scientific knowledge is the result of the growth of common-sense knowledge. But all of them discovered that scientific knowledge can be more easily studied. It's very problems are enlargements of the problems of common-sense knowledge. For example it replaces the Humean problem of 'reasonable belief' by the problem of the reasons for accepting or rejecting scientific theories.'* p22 (Popper, 1959).

Hacking compared Popper's philosophy with that of Carnap's logical positivist philosophy, saying *'They disagreed about much, only because they agreed on basics. It would be nice to have a criterion to distinguish such good science from bad nonsense or ill-formed speculation.*' (p3) Hacking, who wrote that he has been most influenced in his early days in England by Popper, concludes that despite these differences the positivists and Popper contributed a lot of the timeless image of science The Legend, that ruled before Kuhn, before 1960: *'They thought that the natural sciences are terrific and that physics is the best. It exemplies rationality and from that they believed in the unity of science.*' p5 (Hacking, 1983).

As I have discussed above, the positivists started with observations from the bottom, building it up into a system of verified statements about the world. Popper did reject this idea on philosophical logical arguments. In his view it starts top down with hypotheses, that are based on previously obtained knowledge, discussions with peers or simply wild ideas. These conjectures and their contexts determine how we subsequently observe and how we interpret the observations about the world. In Popper's view the claims derived from these observations may after severe experimental testing and discussion between scientists become accepted, held to be 'true'. However, per definition they are not verified. On the contrary, theories and their statements are to be regarded falsifiable, open to refutation, at any time by further testing and criticism. Poppers 'method' of conjectures and refutations, and his falsificationism reminds of the 'scientific method' described by Peirce 50 years before. Like Peirce, Popper completely rejected the idea of the independent, 'given' foundation and the dichotomy between facts and values. Observation, ideas and theory were always entangled. In his thinking, like Peirce, Popper emphasized the power of the method of rigorous and endless testing and of criticism in the <u>community</u> of peers. *"Basic statements are accepted as the result of a decision or agreement, and to that extent they are a convention. The decisions are reached in accordance with a procedure governed by rules'….. 'Thus the real situation is quite different from the naive empiricist. Or the believer in inductive logic.'….'Theory dominates the experimental work from its initial planning up tot the finishing touches in the laboratory'. (p106)* In a most fascinating metaphor of the 'swamp' he resolutely deals with the issue of the foundation and the 'given'. It is the visualization of this powerful metaphor that I literally never got out of my mind after reading it in August 1975: *'The* *empirical basis of objective science has thus nothing 'absolute' about it. Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down any natural or 'given' base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being'*. (p109).

In his 1972 Addendum he added*:'1. My term 'basis' has ironical overtones; it is a basis but is not firm. 2. I assume a realist and objectivist point of view: I try to replace perception as 'basis' by critical testing'. Our observational experiences are never beyond testing; they are impregnated with theories. 4. 'Basic statements …are like all language, impregnated with theories'.(p109)* In a later paper, *The Rationality of Scientific Revolutions,* which takes in to account the community of inquiry and some of the sociological and psychological aspects of the research process, he describes the problems that may arise from this phase of debate and criticism due to the human factor (Popper, 1981) which were discussed in his *Conjectures and Refutations* at length (Popper, 1972).

Willard Van Orman Quine (1908–2000) is everywhere, when you read about the demise of the Legend and about his role, or not, in the resurrection of pragmatism (Misak, 2013b) Quine was familiar to the members of the Vienna Circle but worked whole his life in the USA. In most cases his contribution is very briefly told with short citations. He did not write much, but he made an immense mark through his famous dogma's on empiricism especially by forever rejecting, on analytic logical grounds, thus by using their own weaponry, the analytic–synthetic distinction. This was a blow to the very important yardstick of logical-empiricism and the philosophy of the Vienna Circle (Quine, From a logical point of view, 1956). He demonstrated, or in fact built the argument that the principles of inference that we use to link theory with experience [observations done via our senses] are as Putnam (Putnam, 1981) says [not analytical, nor given or timeless foundations but] '*are just as much subject to revision as any other aspect of our corporate body of knowledge*.' (p30). These rules are thus not 'given', or a priori assumptions but result from our collective thinking, experience and discussion and are such that as Misak phrases: *'everyone would assent to them'* (p200) (Misak, 2013a).

Michael Polanyi wrote in 1959 a short fascinating comment on C.P. Snow's *Two Cultures* that originally appeared in *Encounter*, a monthly Anglo-American journal of politics and culture that did fit Polanyi's political ideas discussed above. This piece is written in the characteristic polemic style of Polanyi who also here puts the issue in a larger political neo-conservative frame, criticizing the hard-boiled scientific ideals and naturalistic scientism of Bentham and Marx that in his view disrespects truth. '*Our task is not to suppress specialisation of knowledge but to achieve harmony and truth over the whole range of knowledge. This is where I see the trouble. Where a deep-seated disturbance was inherently originally in the liberating impact of modern science on medieval thought and has only later turned pathological'.. 'Science rebelled against authority. It rejected deduction from first causes in favour of empirical generalisations. Its ultimate ideal was a mechanistic theory of* *the universe, though in respect man it aimed only at naturalistic explanation of his moral and social responsibilities'. '..scientific rationalism has been the chief guide towards all the intellectual, moral, and social progress on which the nineteenth century prided itself- and to the great progress achieved since then as well. …Yet it would be easy to show that the principles of scientific rationalism are strictly speaking nonsensical. No human mind can function without accepting authority, custom and tradition: it must rely on them for the mere use of a language. Empirical induction, strictly applied can yield no knowledge at all and the mechanistic explanation of the universe is a meaningless ideal….because the prediction of all atomic positions in the universe would not answer any question of interest to anybody'. 'Scientific obscurantism has pervaded our culture and now distorts even science by imposing on it false ideals of exactitude'. (p41)* (Polanyi & Grene, 1969).

Ernest Nagel has been an influential philosopher, not only through his famous textbook *The Structure of Science: Problems in the Logic of Scientific Explanation*. (Nagel, 1961) In that pre-Kuhnian seminal work he covered the whole of the philosophy of science of those days, but mostly limited to mainstream analytical philosophy, logical-positivism and empiricism and Popper's philosophy. There is a very short discussion of 'instrumentalism' which refers to American Pragmatism. He was sympathetic to pragmatism as I will discuss later and in his introductory chapter he makes a few remarkable statements which are a critique of the empiricist-positivist philosophy of the 'Legend' that he discusses in the next 300 pages of his book. *'The practice of the scientific method is the persistent critique of arguments in the light of tried canons for judging the reliability of the procedures by which evidential data are obtained and for assessing the probative force of the evidence on which conclusions are based'. ….'the difference between the cognitive claims of science and common sense which stems from the fact that the former are the products of scientific method, does not connote that the former are invariably true.'…… 'If the conclusions of science are the products of inquiries conducted in accordance with a definite policy for obtaining and assessing evidence, the rationale for confidence in those conclusions as warranted must be based on the merits of that policy. It must be admitted that the canons for assessing evidence which define the policy have, at best, been explicitly codified only in part, and operate in the main only as intellectual habits manifested by competent investigators in the conduct of their inquiries. But despite this fact the historical record of what has been achieved by this policy ….leaves little room for serious doubt concerning the superiority of the policy….'* (p18)

> *'For in point of fact, we do not know whether the unrestrictedly universal (positivist-empiricist premises) assumed in the explanation of the empirical sciences are indeed true… .'were this Aristotelian requirement adopted few if any of the explanations given by modern science could be accepted'… 'In practice it would lead to the introduction …that explanations are being judged to have merit by the scientific* (p43)

Polanyi, who as we saw criticized positivism, concludes two different things from Nagel's account of science: *'Nagel implies that we must save our belief in the truth of scientific explanations by refraining from asking what they are based upon.*

*Scientific truth is defined, as that which scientists affirm and believe to be true. Yet this lack of philosophical justification has not damaged the public authority of science, but rather increased it'(*Polanyi, 1967*)*

Marxism? Critical Theory?
Before discussing Kuhn's work and immense impact from the 2020 perspective, I want here from the 1977 perspective refer to another writer who has until this day influenced my thinking about science, research and society. In September 1976, after a year of philosophy, I had returned to the lab bench to study for my Masters in immunology at the Academic Hospital of the University of Groningen. I continued reading about science and in the Spring 1977 I read Jerom Ravetz's book *'Scientific Knowledge and its Social Problems'* (Ravetz, 1971). Ravetz (1929-) is a mathematician who became a philosopher of science. After his graduation in the US, he came in the late 1950s to the UK at a time when his even moderate Marxist sympathies were problematic with McCarthyism in the US. In Europe Marxist sympathies in the 1960s and 1970s were not at all a problem in academia and Critical Theory was very much under the influence of neo-marxist political and social thinking. At university in the early 1970s, there were hard-liners, but one was mostly exposed to Marxism-Light as I would call it. With this I mean, the analyses of socio-economic powers and dynamics, taken out of the Marxist view of inevitable collapse of capitalism and then post-capitalist utopia of the salvation state which had already then not proved realistic in rapidly changing and adapting capitalist economies. However, when re-reading the two collections edited by Rose and Rose from 1976, which I read in 1977, that provide a series of articles on science and society, from an downright Marxist perspective, the Marxist jargon, the mentioning of the blessings of Maoism and the illusion of the end of capitalism and the bourgeoisie is quite weird. Indeed, Stalinism and Leninism and then the Cold War as discussed had blocked these analyses of science and society in the US. Ravetz was most of his professional life affiliated with the Centre for Philosophy and History of Science in Leeds where he worked for a short period of time with Toulmin. Ravetz in his book presents a comprehensive analysis of science and research, starting with problems that he expected would become more prominent. He discusses in depth the consequences of what he called 'the industrialization' of science which goes against the Mertonion norms with its protection of property and top-down management. He argued that because of enormous increase in scale, loss of social and ethical control, the system would increasingly face poor quality 'shoddy' research because of the lack of shared value of individual researchers with the scientific community. On the other hand, he is deeply concerned about the external influences on the research agenda by powerful private parties, multi-nationals, but also the military and governments. We

(continued)

know noe that Ravetz writing that book at that time was quite visionary. During his whole career he studied issues of uncertainty, risks and unwanted effects attached to the use of novel scientific knowledge and technology in society (Funtowicz & Ravetz, 1990; Ravetz, 2011). He wrote about the ethics of science and scientists and criticizes the claim 'of neutrality' that was used by researchers to evade their social responsibility. At that early stage preparing for my professional life, reading this book for me was truly a transformative experience and Jerry Ravetz was an inspiration and it was special that he participated when in the late fall of 2012 through 2013 we prepared for the start of *Science in Transition* described in Chap. 3.

The huge impact of Thomas Kuhn's *Structure of Scientific Revolutions,* published in 1962, has already been mentioned many times. It has opened up the debates in the history and sociology of science, but at the same time affected the domain of the philosophers showing through historical and sociological research the problems of logical positivism. Kuhn presented a descriptive account of what scientists do, which sociologically, but also (methodo)logically deviates from the normative positivist scientific method. He did however not provide judgement about the way science was actually done from the philosophical perspective (positivism) and did not propose an alternative correct formal method. This, in the eyes of his critics, was not logic or if it was logical, they did not agree. They asked the question whether Kuhn's description wasn't in fact normative. They make it, Kuhn writes in discussion with his critics, clear that they don't like his normative prescriptions using terms as *'corrupt our understanding and diminish our pleasure'* and *'a plea for hedonism'*. (Lakatos & Musgrave, 1970) They accuse Kuhn not using logic while they themselves use normative non-cognitive arguments and language. (p237). *'History and social-psychology are not, my critics claim, a proper basis for philosophical conclusions'* (p235). This is an important issue as it points to the gap between the philosophy and the practice of science. *Criticism and the Growth of Knowledge, eds. Lakatos and Musgrave* (Lakatos & Musgrave, 1970) is based on the contributions to a symposium held 13 July 1965 in London. In the final chapter, *Reflection on my Critics,* Kuhn declares his epistemological viewpoints that are beyond positivism, foundationalism and Popper's theory of falsification, but not sceptic nor relativistic. In fact, Kuhn states that his descriptive account of the process of inquiry at the same time indeed is normative. Because, if you want your inquiry to succeed you should use that process, that scientific method, which of course involves logic, mathematics, statistics and other accepted methods at a given moment in time in a research community. Indeed, science as Kuhn concluded, is a process of the community and not of an individual. A lot of the discussion in *Criticism* in my reading then indeed was about the differences between the descriptive historical and in some respect sociological mode of Kuhn's approach versus the normative mode of especially Popper and to some degree Lakatos. Popper admits that normal science exists, but finds it degrading and compares it to applied science and warned for the dangers normal science could pose to science. This is very reminiscent of the elitist scientific attitudes Snow and Medawar were criticising. Popper even suggested Kuhn did not seem to dislike normal science, whereby he exhibited his normative way, not only of theorizing about science, but also of judging scientists (p52, 53).

I cite some of the most interesting lines of Kuhn:

'*I am no less concerned with rational reconstruction, with the discovery of essentials, than are philosophers of science. My objective, too, is an understanding of science, of the reasons for its special efficacy, of the cognitive status of its theories. But, unlike most philosophers of science, I began as an historian of science, examining closely the facts of scientific life'*

Kuhn *'discovered that much scientific behaviour, including that of the very greatest scientists, persistently violated accepted methodological canons,…' p236*

In the current context of course the question is: who exactly had accepted these canons? Philosophers, but apparently not researchers! In response to Lakatos, Kuhn describes succinctly his conceptual frame:

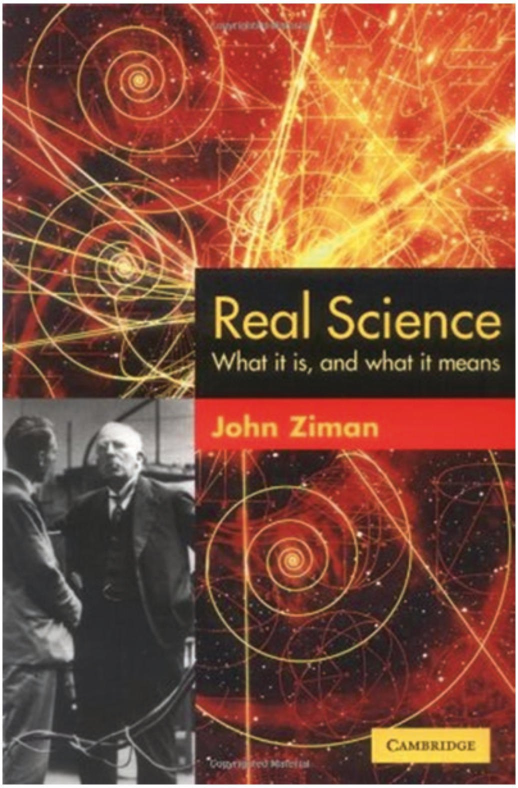
> *'some of the principles deployed in my explanation of science are irreducibly sociological, at least at this time. In particular, confronted with the problem of theory-choice, the structure of my response runs roughly as follows: take a group of the best available people with the most appropriate motivation; train them in some science and in the specialties relevant to the choice at hand; imbue them with the value system, the ideology, current in their discipline (and to a great extent in other scientific fields as well); and, finally, let them make the choice. If that technique does not account for scientific development as we know it, then no other will. There can be no set of rules of choice to dictate desired individual behaviour in the concrete cases that scientists will meet in the course of their careers. Whatever scientific progress may be, we must account for it by examining the nature of the scientific group, discovering what is values, what it tolerates, and what it disdains. That position is intrinsically sociological, and, as such, a major retreat from the canons of explanation licensed by traditions which Lakatos labels justificationism and falsificationism, both dogmatic, and naïve'. p237, 238.*

It is important to take note that Lakatos, in his one-hundred-page long contribution to this book, wrote that this debate *'did not start with Kuhn. An earlier wave of 'psychologism' followed the breakdown of justificationism. For many, justificationism represented the only possible form of rationality: the end of justificationism meant the end of rationality …………After the collapse of Newtonian physics, Popper elaborated new, non-justificationist critical standards. Finding them untenable, they identify the collapse of Popper's naïve falsificationism with the end of rationality self.*' P178. Lakatos, a true Popperian and believer in the 'scientific method' at that time, started to work on his concept of Research Programmes, a mix of Popperian and Kuhnian thought.

In the remainder of the chapter, Kuhn responds to the critique that his description of science, without a rejection of the methods used, opens the doors to relativity and nihilism. It was argued that personal opinion, mob psychology and elites with power and vested professional interests could determine the outcome of discussions regarding theory choice. He cites the non-cognitive, but important criteria and values that are being used and accepted in communities of inquirers and have been implied by Popper in his normative description of theory choice, including '*accuracy, scope, simplicity, fruitfulness*'. (P261, 262) Kuhn emphasizes that these are not rules that can be applied in a straightforward manner and his historical research has shown that they may evolve and change over time in the community.

When Kuhn prepared his book, in the late 1950s, logical positivism despite the prominent works by Quine and Sellars, still ruled in the philosophy of science and pragmatism was not considered to be a sound and fruitful alternative. Many still believed that the problems of positivism could be solved by analytical philosophy. But Kuhn's analyses and conclusions as expressed above are, although not cited by him, reminiscent of American pragmatism and the critiques of Peirce and Dewey on the dominant philosophy of science of their times.

John Ziman (1925–2005) was a physicist who between 1960 and 2000 was one of the first to write systematically, in depth and broadly about science. In 1968 he published *Public Knowledge* (Ziman, 1968) his first of nine books on science and as Jerome Ravetz, Ziman's contemporary colleague and science writer, in his obituary wrote: '*In this he bypassed the debates among the philosophers who saw science as a collection of "theories" requiring some sort of logical proof; for him the essential feature of scientific knowledge is its social character.*' (Jerry Ravetz, Guardian, February 2005).



It's Anthropology, Stupid!

In most of their books, Toulmin, Hanson, Ravetz and Ziman, but also Polanyi, take all aspects of the scientific enterprise into account in their analyses of how consensus regarding reliable knowledge is produced and thus what distinguishes science as a social activity. In their opinion it is exactly the complex of the methods, personal psychology, the community and the sociology of the researchers in organizations that determines what science is. Their writings went against the widely held believes about science and as a consequence were virtually neglected by main-stream philosophy, history and sociology. Because of its multidisciplinarity, in addition their work did not belong to one of these classical academic disciplines. Similarly, even Bruno Latour in his *We were never Modern* complained about the slow recognition of Latour and Woolgar's *Laboratory Life* by philosophers and sociologist of science. (Latour, 1993; Latour & Woolgar, 1979) This was duly confirmed by Hacking in his very late 1988 (!) review of '*Laboratory Life*'. With regard to this seminal book of 1979 and his own 1983 book that I here cite a lot and wherein he argues to take a look at the practice of science, he declares that *'it was shameful not to examine the one outstanding piece of work then available that took laboratory science seriously and argued the strong anti-realist doctrine in existence'*.(p278) (Hacking, 1988) Latour pointed out that we do accept anthropology crossing all these academic territories, but apparently we do not allow this for an anthropology of the tribe of humanity that is involved in science.

This may, Ravetz believed, be the reason that this type of work has had relatively little impact (Ravetz, personal communication 2013). That may well be the case, but as argued above, meta-science research drew in general very little attention from those active in research in the academic disciplines or in the 'corridors of power' of academia (Miedema, 2012). Lack of impact has also been blamed on the fact that the work of these authors lacked a novel theory, theoretical frame or a specific novel concept. Exceptions to this are Polanyi's concept of tacit knowledge and Toulmin's metaphor of maps for theories (1953) and his evolutionary concept of progress in science (1972). I disagree with this critique, as I my opinion the main hypothesis for which they provided evidence and which is the basis for this book, is that in the history of science, the dominant image of science which proved philosophically wrong around 1960, was strongly politically and culturally determined and has until now distorted and hurt the practice of science in many different ways. It is on the basis of these insights, that many scholars have since then began to study the practice of science. These studies in the recent past have resulted in renewed movements to improve the practice of science and make it more suitable to contribute to solve the grand challenges of the twenty-first century. John Ziman already in his early books *Public Knowledge* and *Reliable Knowledge* has provided insights from the trenches of science about the

(continued)

problem of the myth of the 'scientific method', which at that time still few others understood and for which Ziman later coined the term 'the Legend' (Ziman, 2000). He wondered in 1968 (!) (Ziman, 1968) how this *'logico-inductive' metaphysics of Science.. can be correct, when few scientists are interested in (it) or understand it, and no one ever uses it explicitly in his work? But if Science is not distinguished from other intellectual disciplines neither by a particular style or argument nor by a definable subject matter, what is it?* (p8). He then sketches the social process of inquiry, hypothesis, testing and criticism and states that '*it is not a subsidiary consequence of the 'Scientific Method'; it is the scientific method itself.*' '*The defect of the conventional philosophical approach to Science is that it considers only two terms in the equation. The scientist is seen as an individual, pursuing a somewhat one-sided dialogue with taciturn Nature. But it is not like that all. The scientific enterprise is corporate. It is never one individual that goes through all steps of the logico-inductive chain; it is a group of individuals, dividing their labour but continuously and jealously checking each other's contributions'*. (p9)

John Ziman could in those days, find virtually no literature on consensus building by the community and the social process and *'that makes the Philosophy of Science nowadays so arid and repulsive. To read the latest volume on this topic is to be reminded of the Talmud…' It is fiercely professional and technical and almost meaningless to the ordinary working scientist. This is unfortunate ..I shall try to heal the breach by talking semi-philosophically about the intellectual procedures of scientific investigation.'* (p31).

In *Reliable Knowledge*: *an exploration of the grounds for belief in science (J. M.* Ziman, 1978*)* an important book in this context, Ziman did the same bypass as in *Public Knowledge* as in all his books regarding the philosophical basis of the Legend. In the introductory paragraph 1.4 he firmly states that from data, diagrams, models or pictures, '*meaning cannot be deduced by formal mathematical or logical manipulation. For this reason scientific knowledge is not so much 'objective' as 'intersubjective' and can only be validated and translated into action by intervention of human minds' (*p7). Ziman is very realistic and knows the daily practice of physics and does not conceal weaknesses known to investigators but disguised by the believers of the Legend: *'The achievements of intersubjective agreement is seldom logically rigorous; there is a natural psychological tendency for each individual to go along with the crowd, and to cling to a preciously successful paradigm in the face of contrary evidence. Scientific knowledge thus contains many fallacies, mistaken beliefs that are held and maintained collectively and which can only be dislodged by strong persuasive events*.' (p8.) He describes how scientist are 'brainwashed' during their training in the concept, accepted beliefs and methods in the current paradigms of their field. He explains in great detail and nuance how in the 'social model of science', the scientific community produces the knowledge we designate as scientific knowledge and what makes it unique and reliable. Ziman builds further on the work of those who criticised positivism and the Legend - Polanyi, Hanson, Toulmin and Kuhn- published in the decade before. Ziman points to fact that there is not one scientific method, there are many dimensions to scientific knowledge and *'that explains the strange sense of unreality that scientist feel when they read books about the philosophy of science' (p84)*. From this point of view from the natural sciences, he concludes that the social sciences and humanities of course can produce reliable scientific knowledge and he states in an unexpected humanistic lyric paragraph *that 'the challenge to the behavioural sciences is not coming from physics but from the humanities'*. (p185).

Jerome Ravetz in his, in the STS fields well-known, *Scientific Knowledge and its Social Problems* presented a unique philosophical-sociological analysis. (Ravetz, 1971) It provides an integrated very rich view of science, its theoretical assumptions, its ideologies, power games, issues of ethics and social responsibilities and the sociology and politics of the system and the interaction with society. Ravetz cites a broad body of the most relevant scholars at that time. He refers frequently to the work of his temporaries Toulmin, Ziman, Rose and Rose and especially Polanyi's '*Personal Knowledge'* (Polanyi, 1958, 1962). He really 'took a look' at the practice of science and especially emphasizes science as craftmanship and subsequently discusses the philosophical assumptions about the special status of theories and how knowledge is produced. He, on the basis of his understanding how science and research is being done, rejects the positivist and foundationalist ideas With respect to 'the scientific method' and positivism he clearly states that in research the underlying '*principles and precepts that are social in their origin and transmission, without which no scientific work can be done.. guide and control the work of scientific inquiry.*' (p146) More explicitly: *'The individual scientist; and the criteria of adequacy are set by his scientific community, not by Nature itself.*' (p149) With respect to *'the maturity of a field an important part lies in the strengthening of the criteria of adequacy. This is not all of course; the development of new tools, and the creation of an appropriate social environment are equally important. Nor can the strengthening of criteria of adequacy be done in an abstract, automatic fashion, as by attempted imitation of a succesful field* (p157). About the relation between philosophy and the practice of science he says: '*Philosophers of science have attempted, with some success to provide a rationale for the different basic patterns of argument, showing why it is reasonable for an intelligent person to place reliance on them….But as these philosophical arguments become more refined and sophisticated, they drift further and further from the practice of science.*' Finally, for the present discussion it is of interest to close with the following citation on the dichotomy of values and facts. Ravetz, unlike Polanyi, but like Bernal whom he also personally knew, sees research primary as a social activity that needs conscious strategies to be able to make proper judgements regarding problem choice. He explicitly mentioned values other than strict cognitive arguments that have to enter into these evaluations (p161). '*The criteria of value, and judgements based upon them, form an interesting contrast to those of adequacy. …we shall find ourselves involved in problems of the social activity of science. …The exclusion of problems* *of value from the traditional philosophy of science has its roots in the ideology of modern natural science as it was formed through many generations of struggles….. the considerations of social value by which all other human activities are assessed were declared irrelevant'* p160.

Mary Hesse (1924–2016) studied mathematics, physics and philosophy and taught mathematics and philosophy at several universities in England. She has written extensively on the philosophy of science. Mary Hesse wrote in 1972: *'During the last half-century much of professional Anglo-American philosophy of science has been devoted to detailed development of internal logic of natural science based on empiricist criteria, and also on attempts to show how this logic applies also in the social sciences and in the study of history. Suggestions….to the effect that there are other modes of knowledge than the empiricist were sometimes actively resisted but more usually totally disregarded'. 'It was held that adoption of at least a modification this empiricist method is required for human sciences 'to attain knowledge status at all' which in her view is 'imperialism claimed for natural science'* (p27) (Hesse, 1972).

'These distinctions that I believe are made largely untenable by recent more accurate analyses of natural science.

1. *In natural science experience is taken to be objective, testable, and independent of theoretical explanation. In human science data are not detachable from theory, for what count as data are determined in the light of some theoretical interpretation, and facts themselves have to be reconstructed in the light of interpretation.*
2. *In natural science theories are artificial constructions or models, yielding explanation in the sense of logic of hypothetic-deduction: if external nature were of such a kind, then data and experience would be as we find them. In human science theories are mimetic reconstructions of the facts themselves, and the criterion of a good theory is understanding of meanings and intentions rather than deductive explanation.*
3. *In natural science the law-like relations asserted of experience and external, both to the objects connected and to the investigator, since they are merely correlational. In human science relations asserted are internal, both because the objects studied are essentially constituted by their interrelations with one another, and also because the relations are mental, in the sense of being created by human categories of understanding recognized (or imposed? By the investigator.*
4. *The language of natural science is exact, formalizable, and literal; therefore, meanings are univocal, and a problem of meaning arises only in the application of universal categories to particulars. The language in human sciences is irreducibly equivocal and continually adapts itself to particulars.*
5. *Meanings in natural science are separate from facts. Meanings in human science are what constitute facts, for data consist of documents, inscriptions, intentional behaviour, social rules, human artefacts, and like, and these are inseparable from their meanings for agents.*

> *'Let us however concentrate for a moment on the natural science half of the dichotomy what is immediately striking about it to readers versed in recent literature in philosophy of science is that almost every point made about the human sciences has recently been made about the natural sciences. And that the five points made about the natural sciences presuppose a traditional empiricist view of the natural science that is almost universally discredited'* (p277) (Hesse, 1972)

Richard Rorty's *Philosophy and the Mirror of Nature* published in 1979 had enormous and immediate impact and for most scholars of pragmatism was the start of the pragmatic turn (Rorty, 1979). Rorty, in chapters III and IV, starts by discussing in depth the serious critiques of Quine and Sellars on the classical dichotomies of logical positivism. In addition, he took the pragmatic turn in chapter VII discussing at length Kuhn's work and putting it firmly in the larger context of the pragmatism of John Dewey. He concludes that 'analytic' epistemology (i.e. "philosophy of science") became increasingly historicist and decreasingly "logical" (as in Hanson, Kuhn, Harré and Hesse) (p168). He discusses the 'behavioristic' critiques of Quine and Sellars, following Wittgenstein's *Philosophical Investigation*s published at the same time in 1953, on *'the two distinctions the "given" and "that what is added by the mind" and that between the "contingent" (because influenced by what is given) and the "necessary" because entirely "within" the mind and under its control)…he presents them as forms of holism. As long as knowledge is conceived of as accurate representing- as Mirror of Nature- Quine's and Sellar's holistic doctrines sound pointlessly paradoxical, because such accuracy requires a theory of privileged representations, ones which are automatically and intrinsically accurate. …I shall be arguing that their holism is a product of their commitment to the thesis that justification is not a matter of a special relation between ideas (or words) but of conversation, of social practice. …we understand knowledge when we understand the social justification of belief, and thus have no need to view it as accuracy of representation.'(p170)…*this is, Rorty says, '*the essence of what I shall call epistemological behaviorism, an attitude common to Dewey and Wittgenstein'. (p174) 'Epistemological behaviorism (which might be called "pragmatism" were this term not a bit overladen)…is the claim that philosophy will have no more to offer* than *common sense (supplemented by biology, history, etc) about knowledge and truth. (p176)*. The term 'behavioristic' may seem peculiar, but refers to the social process by which a community of inquirers come to produce and accept knowledge and beliefs.

In the pages that follow Rorty dispenses with foundationalism and even with philosophy at large, the latter goes much too far for philosophers like Kitcher, who see enough problems to philosophize about. Indeed, since the demise of the Legend, there is no systematic 'grand unified theory' in the philosophy of knowledge. As I will argue in Chap. 4, pragmatism has a lot to offer with regard to our understanding and philosophizing about knowledge and knowledge production. As Rorty discussed (p367), it may not provide a systematic alternative, but it does provide a <u>hermeneutical method</u> and viewpoint about science and inquiry (see also Kuhn The essential tension p xiii and xv). This, to many a philosopher of the analytic tradition may have been disappointing and the main reason to not take pragmatism serious as philosophy, but must be understood in that pragmatism is a reaction by 'peripheral' philosophers (James, Dewey, Wittgenstein, Heidegger) to a 'systematic' philosophy which Rorty designates a mainstream analytic 'superstition'. These 'peripheral' philosophers are according to Rorty the 'edifying' philosophers. They do not provide a system with a set of rules but offer moral and intellectual instructions and enlightenment.

As Flyvberg (2001) argues, hermeneutics is not only relevant for the social sciences but also for the natural sciences '*as it is now argued that natural sciences are historically conditioned and require hermeneutic interpretation. Natural scientist, too, must determine what constitutes relevant facts, methods, and theories; for example, what would count as "nature"*. (p28).

Nancy Cartwright, a mathematician and philosopher who has studied the practice of physic in relation to the myths of analytical philosophy. She wrote *The Dappled World* (Cartwright, 1999) in follow up of *How the Laws of Physics Lie* (Cartwright, 1983), in which she discusses the classical ideas of the unity of science and the myth of the universality of physics and she takes for comparison economics, the discipline that is famous for imitating (or since the financial crisis having imitated?) physics. The physics that never was, as Cartwright shows. *The Dappled World* is a very technical book, but its conclusions (p9 and 10) are clear theories and claims have been stablished in very artificial settings in the laboratory or as in economics by keeping everything else the same (*ceteris paribus*) both which in the real world are rare to occur: *'I conclude that even our best theories are severely limited in their scope. For, to all appearances, not many of the situations that occur naturally in our world fall under the concepts of these theories…..' 'The logic of the realist's claim is two-edged: if it is the impressive empirical successes of our premier scientific theories that are supposed to argue for their 'truth'…then it is the theories as used to generate these empirical successes that we are justified in endorsing. How do we use theory to understand and manipulate concrete things- to model particular physical or socio-economic systems? The core idea is … the belief in one great scientific system, a system of a small set of well-co-ordinated first principles admitting a simple and elegant formulation, from which everything that occurs, or everything of a certain type or in a certain category that occurs, can be derived. But treatments of real systems are not deductive, ….(not) even if we tailor our systems as much as possible to fit our theories, which is what we do when we want to get the best predictions possible.'*

This is the reason, and that is well known, why many drugs shown to have beneficial effects in a highly selected patient population and well-controlled clinical trials, don't do as well in clinical practice. Cartwright got a lot of criticism to the kind of criticism she articulated in *How the Laws of Physics Lie* but her response is clear, and relates to the myth of the Legend: *'I agree that my illustrations ….are 'a far cry' from showing that the system must be a great scientific lie. But I think we must approach natural science with at least as much of the scientific attitude as natural religion demands'*.

Her examples are from physics, economics, medicine and genetics. Her conclusions reminds on the one hand of the arguments of Nagel discussed above, and on the other hand of the persuasive work of Richard Lewontin, which in a less analytic and technical way, criticizing the ideologies of biology, genetics, molecular biology and the dream of the human genome project and thus of the positivist molecular-biologists and clinicians-researchers who believed would reductionist science solve the problem of our diseases- cancer, cardiovascular, and mental illnesses alike. (Lewontin, 2000; Lewontin et al., 1984).

Hillary Putnam (1926–2016) was a mathematician and philosopher who has had a broad and deep impact on mathematics, ethics and the philosophy of science. He is famous and admired for his critical thinking about the work of others, and interestingly, as well as about his own work and has as consequence changed his philosophical ideas and positions several times in his long career. He started as a student with Hans Reichenbach, a major figure in pre-war analytical philosophy. Via positions amongst others at Princeton and MIT he worked at Harvard until 2000. In his later years he wrote widely about American pragmatism (Putnam, 1995; Putnam & Conant, 1994) and in particular how it could overcome the problems of the analytical philosophical tradition including foundationalism, and the various dualisms such as the analytic-synthetic, the objective-subjective and the fact-value dichotomies. His *Reason, Truth and History* (Putnam, 1981) is illuminating with respect to the flaws of the positivist philosophy of the Legend. In particular Chap. 3, but also more broadly the thinking presented in Chap. 8 are insightful. In 2004 he published *The collapse of the Fact/Value dichotomy* (Putnam, 2002) where he discusses how most 'analytical philosophy of language and much metaphysics and epistemology has been openly hostile to talk of human flourishing, regarding such talk as hopelessly "subjective"- often relegating all of ethics, in fact, to that waste baker category' (p viii), and he argues for the economics approach of Amartya Sen. He delves deep, as always, and I will leave that to the more experienced reader but here I cite the very last paragraph which is in plain English but boldly worded which makes his position after a lifetime hard work on exactly these matters very clear:

> *'I have argued that even when the judgments of reasonableness are left tacit, such judgments are presupposed by scientific inquiry (indeed, judgments of coherence are essential even at the observational level: we have to decide which observations to trust, which scientists to trust-sometimes even which of our memories to trust.) I have argued that judgments of reasonableness can be objective, and I have argued that they have all of the typical properties of value judgments. In short, I have argued that my pragmatist teachers were right: "knowledge of facts presupposes knowledge of values." But the history of the philosophy of science in the last half century has largely been a history of attempts - some of which would be amusing, if the suspicion of the very idea of justifying a value judgment that underlies them were not so serious in its implications- to evade this issue. Apparently any fantasy -the fantasy of doing science using only deductive logic (Popper), the fantasy of vindicating induction deductively (Reichenbach), the fantasy of reducing science to a simple sampling algorithm (Carnap), the fantasy of selecting theories given a mysteriously available set of "true observation conditionals," or, alternatively "settling for psychology" (both Quine)- is regarded as preferable to rethinking the whole dogma (the last dogma of empiricism?) that facts are objective and values are subjective and "never the twain shall meet." That rethinking is what pragmatists have been calling for for over a century When will we stop evading the issue* ("knowledge of facts presupposes knowledge of values.") (insert FM) *and give the pragmatist challenge the serious attention it deserves?* (p145)

I have in this philosophical time-travelling now arrived in the twenty-first century. I want to discuss Philip Kitcher's work, which for several reasons is of interest in this context. Starting like Putnam from the analytical science tradition, he has described his intellectual history since the 1980s, in the beginning criticizing some and defending other parts of the Legend but gradually losing faith. Kitcher has been reflecting on the philosophical transition he went through, from empirical positivism, natural empirism to a form of neopragmatism. Even in times when the more general pragmatic turn was already going on in the field (Bernstein, 2010; Putnam & Conant, 1990), he experienced how different this philosophical approach was, not in the least in the eyes of his mainstream analytically thinking peers (Kitcher, 2012). Kitcher in 1999 was appointed as John Dewey Professor of philosophy at Columbia. From his website: '*Following Dewey, I believe in the need for a reconstruction of philosophy (so that it will not be a "sentimental indulgence for the few"), and I worry about the increasing narrowness and professionalization of academic philosophy. In working with graduate students, I hope to instil a capacity for clarity and rigor without sacrificing the sense of why philosophy matters.*'

In his *The Advancement of Science* (Kitcher, 1993), which carries the strong subtitle "*Science without a Legend, Objectivity without Illusion',* this struggle is throughout the book most visible, but Kitcher is to be recommended for being very explicit about it upfront and in the epilogue: '*Once, in those dear dead days, almost, but not quite, beyond recall, there was a view of science that commanded wide spread popular and academic assent'…. 'Legend celebrates scientists as well as science'. …..scientists have achieved so much through the use of the SCIENTIFIC METHOD.'..'there are objective canons of evaluation of scientific claims; by and large, scientists (at least since the seventeenth century) have been tacitly aware of these canons and have applied them in assessing novel or controversial ideas….' (*p3).

'*So much for the dear dead days. Since the late 1950s the mists have begun to fall. Legend's lustre is dimmed. While it may continue to figure in textbooks and journalistic expositions, numerous intelligent critics now view Legend as a smug, uninformed, unhistorical, and analytically shallow. Some of the critiques, science bashers, regard the failure of science to live up to Legend's advertising as reason enough to question the hegemony of science in contemporary society. I shall not be concerned with them, but with the critiques of the Legend bashers, those who believe that Legend offered an unreal image of a worthy enterprise.'*(p5) Kitcher acknowledges that although he believes that the classical philosophy *'belongs amongst the greatest accomplishments of philosophy of our century'*, it has been shown to have its problems. He only once in a footnote (!) (p7) cites the devasting critique of Popper discussed above and admits that '*despite efforts of a few philosophers, little headway has been made in finding a successor for Legend. If anything, recent work in the history of science and the sociology of science has offered eve more sweeping versions of the original critiques'……, I am not* ***yet*** *ready to abandon the search for generality'* (bold applied by FM) p8.

Kitcher is much concerned with the objectivity of theory choice where indeed (social) criteria are at play which according to Legend are non-epistemic because external. He also wrestles many pages with the classical problem of representation of reality by theory and of realism of the objects of science and in these discussions uses, as per Legend, the success of natural science as kind of foundation, a warranty for objectivity and realism. This feels like causality reversed. Kitcher at that time believed that Legend could philosophically and sociologically be rescued, in his way or another. He believed that *'the Legend was broadly right about the characteristics of science. Flawed people, working in complex social environments, moved by all kind of interests, have collectively achieved a vision of parts of nature that is broadly progressive and rests on arguments meeting standards that have been refined and improved over centuries. Legend does not require burial but metamorphosis.' p390* This defence of Legend is remarkable since writing this in 1993, he is aware and discusses the seminal work of the scholars who convincingly showed, as I discussed above, that the myth of 'the scientific method' and its normative canons, never did relate much to daily practice of inquiry and the idea of foundationalism did not hold. Kitcher (p10) admits that the Legend was a normative construction, but incorrectly seems to suggest it came from studying science and can be rescued by studying the practice of science again. Kitcher was at that time critized by Shapin (cited by Kitcher p303) that he still worked from the Legend's 'individualism' of the scientist instead taking the work of many scholars to heart that shows the social process and the community of inquiry in practice. Very interestingly, in the final pages he suggests that philosophy should be normative and could suggest ethics and values for how the enterprise of science could (and should) be organized to optimally contribute to human flourishing: '*Yet even if the metamorphosis of Legend attempted here clears away those errors, it does not address the issue of the value of science. To claim as I have done that that the sciences achieve certain epistemic goals that we rightly prize is not enough- for the practice of science might be disadvantageous to human well-being in more direct was, practical ways. A convincing account of practical progress will depend ultimately on articulating an ideal of human flourishing against which we can appraise various strategies for doing science. Given an ideal of human flourishing, how should we pursue our collective investigation of nature……..how should we modify the institution as to enhance human well-being?…. The philosophers have (no the Legend has .., FM) ignored the social context of science. The point however is to change it."* (p391) I will return to the later work of Kitcher, which shows his sharp pragmatic turn, when this topic is discussed further in Chap. 4.

Helen Longino (born 1944) has focussed throughout her career as philosopher on the social character of scientific inquiry. She is motivated in this work by Women's Studies, the role of social values and criteria, equality, gender and inclusiveness. She has studied it from different theoretical and practical viewpoints. She understands the Legend and the struggle of the classical philosophers, including Kitcher, to break free from the classical view of the scientific method, the Legend. She is avoiding the extreme, that there is no objectivity in scientific inquiry at all, argued by those who claim that it is determined by values and interests only and unconstrained by empirical observations. In her widely appreciated *'Science and Social Knowledge'*(Longino, 1990) her tour the force on this is described for the first time in an analysis contrasting the logical positivist philosophy of Hempel with the 'Wholism', as she calles it, of Hanson, Kuhn and Feyerabend. She goes basically through the same intellectual moves as the writers cited above and, in the end, tries to present a contextual empiricist 'scientific method' that is truly social in which the community of inquirers also takes social values pertinent to the context of the work into account. '*My concern is that with a scientific practice perceived as having true or representative accounts of its subject matter as a primary goal or good. When we are troubled about the role of contextual values or value-laden assumptions in science, it is because we are thinking of scientific inquiry as an activity whose intended outcome is the accurate understanding of whatever structures and processes are being investigated. If that understanding is itself conditioned by ours or others' values, it cannot serve as a neutral and independent guide.*' Against this she argues: *'The dichotomy of these approaches should not be seen so much as a contraction to be resolved in favour of one or the other position, so much as reflective of a tension within science itself between its knowledge-extending mission* (application in contexts*) and its critical mission* (better theories)' p34.

> *'In assessing particular research programmes, it is important to keep in mind that knowledge extension* (testing the effects of claims in experimental and real-world settings) *and truth* (as accepted beliefs, Longino must mean to say) *can guide scientific inquiry and serves as fundamental, but not necessarily compatible, values determining its assessment.' Thus, while a demonstration of the contextual value ladeness of a particular research program may serve to disqualify it as a source of unvarnished truth about its subject matter, such demonstration may have little bearing on one's assessment of it as an example of scientific inquiry.'(p36) (*non-italic inserts are mine).

There is in Longino's method, her epistemology, no timeless foundation, but there are background assumptions, ethical, political, social and other, and there is a practice of reasoning about them. They are under scrutiny, with full criticism and eventual acceptance by the community of inquirers thus correcting for subjective individual preferences (p216). These assumptions, like the classical scientific methods, are not insensitive to cultural and political changes brought about over periods of time by changes in the world views of citizens wherever they live their life. The myth or the Legend, Longino correctly observed, has served as a timeless and stabile disguise providing an account that can *'render invisible the background assumptions. The methodologies associated with logical positivism did render them invisible, which is, I suspect, one reason they remain persuasive among scientists even after being abandoned by philosophers……The myth of value neutrality, that is the consequence of the more general view that scientific inquiry is independent of its social context, is thus a functional myth.' (p225).*

This is an important insight. In fact, by employing this myth of neutrality, scientific inquiry and science as a knowledge system in society is in first instance mainly conservative, resisting critique regarding its accepted theoretical core, and its reflection on its own societal activity. It prohibits, or at least discourages on methodological (epistemic) grounds, also the critique through scientific inquiry of the institutions and social developments and conceals the interaction of science with public and private power structures in society. This is as an example reflected in the negative response of Polanyi and Russell, key opinion leaders in UK physics on BBC radio broadcasted the beginning of 1945, to a caller's question if something of practical use could be expected to be done with quantum physics. Much later in 1962 Polanyi *'actually," admits, "the technical application of relativity…was to be revealed within a few months by the explosion of the first atomic bomb."* 'Polanyi *argued that because science is unpredictable, then its subsequent technical and social outcomes are even more so. He weaves an intricate analogy between the conduct of science and the play of the economic market, both of which exemplify how individuals can maximize socially beneficial outcomes by pursuing their own interests and adjusting, mutually but independently, to the interests of others. The same "invisible hand" that guides the market guides science. While he allows that "Russell and I should have done better in foreseeing these applications of relativity in January 1945," he extends their own incapacity back a half century by also arguing that "Einstein could not possibly take these future consequences into account when he started on the problem which led to the discovery of relativity" because "another dozen or more discoveries had yet to be made before relativity could be combined with them to yield the technical progress which opened the atomic age" (*Cited in (Guston, 2012) Guston 2012 Minerva). A bit dubious this evasion of one of the major ethical and political issues of twentieth century science, since Einstein and Szilard having fled the Nazi's to the US, in 1939 urged Roosevelt to get an atomic bomb build before Hitler did. Its deployment against Japan had not been the idea of a pacifist Einstein and many involved scientists, they instead had seen it as a major means of deterrent. Einstein was until his dead active in the Federation of Atomic Scientists and the Pughwash Conferences against proliferation of nuclear arms.

Longino concludes that this myth of neutrality is detrimental to major aspects of the practice of modern science in chapters on research on sex differences, and the genetics and biology of behaviour where 'hard' data is interpreted based on uncontested hidden social assumptions. Inquiry explicitly investigation and criticizing these cultural assumptions is per Legend declared non-scientific though, because of contextual assumptions that are made explicit.

Ten years after, in *'The Fate of Knowledge'* (Longino, 2002) she has gone further down the road, further away from the timeless certainty of the Legend. She writes: *'My aim in this book is the development of an account of scientific knowledge that is responsive to the normative uses of the word "knowledge" and to the social conditions in which scientific knowledge is produced. Recent work in history, philosophy, and social and cultural studies of science has emphasized one or the other. As a consequence, accounts intended to explicate the normative dimensions of our concept- that is elaborating the relation of knowledge to concepts such as truth and falsity, opinion, reason, and justification- have failed to get a purchase on actual science, whereas accounts detailing actual episodes of scientific inquiry have suggested that our ordinary normative concepts have no relevance to science or that science fails the test of good epistemic practice. That can't be right. The chapters that follow offer a diagnosis of this stalemate and an alternative account. I argue that the stalemate is produced by an acceptance by both parties of a dichotomous understanding of the rational and the social.*' (p1).

This is one of the main problems in science and academia, nearly 20 years later because we still see this stalemate and in our debate about science its characteristic discourse. Longino addresses the underlying assumption of this classical dualism of the Legend and rejects them, which opens up the possibility of a concept of science where internal and external criteria of value both can be used to make choices in science. She in 2002 immediately (on p3) goes to the work of Mill, Peirce and Popper who early on realised that science and the method used to come to accepted beliefs is not an individual but a truly social process, which as we have discussed goes against the Legend. Regarding Popper she points out correctly that Popper, as cited above, praised philosophers who involve in their analyses '*theories, and procedures, and, most important, (of) scientific discussions', 'contingent factors operating in the world of human affairs are beyond his epistemology*'. '*Unlike discussions by Mill and Peirce, Popper's theory of knowledge deliberately bypasses the connection to science and inquiry as practiced and remains the ideal'* (p7). I cite her own resume of the book which is mainly dealing with the problem of what she calls the Rational-Social Dichotomy which as we saw is a main pillar of the Legend: *'The work in social and cultural studies has stimulated a range of responses from philosophers. Some simply rejected the relevance of this work to philosophical concerns, or ….have seen it as empirically and conceptually misguided. Some like Philip Kitcher…have tried to take the sting out of it, by sifting through the claims of the sociologists and sociologically oriented historians attempting refutation of those they deem extremist, and then incorporating a sensitivity to history or sociological analysis into their constructivist accounts of inquiry. …, I argue that these efforts, too, are vitiated* by *a commitment to the dichotomy of rational and the social. I offer an account of scientific knowledge that not only avoids the dichotomy but integrates the conceptual and normative concerns of philosophers with the descriptive work of the sociologists and historians.*

Longino aims to integrate in the understanding of scientific inquiry the fact that '*cognitive capacities are exercised socially, that is interactively'* and argues that more '*more complete epistemology for science must include norms that apply to practices of communities in addition to norms conceived as applying to practices of individuals. Following through on the consequences of the analyses breaking with conventional views of scientific knowledge as permanent, as ideally complete, and as unified and unifiable….means accepting provisionality, partiality, and plurality of scientific knowledge. ...I insist on an epistemology for living science, produced by real empirical subjects. This is an epistemology that accepts that scientific knowledge cannot be fully understood apart from its deployments in particular material, intellectual and social contexts*.' She makes it clear that there need to be pluralism in these epistemologies.

Longino wants to take advantage in her epistemologies of both the Rational and the Social and takes us through some technical chapters, in which she makes it clear that we have a lot to figure out if we (want to properly) use a mix of rational normative criteria from the philosophers and the social criteria and norms the sociologists have revealed. This is especially interesting knowing that scientists do use in their field validated standard, methods and accepted ways of reasoning, but do not take the normative canons of the Legend to seriously in their daily practice, whereas they use consciously and unconsciously the social norms and values derived from their cultural upbringing in all its aspects of a society. In the last ten pages she concludes that the Rational-Social classes of criteria and norms are thus not used in separation, *'sociality does not come into play at the limit of or instead of the cognitive. Instead, these social processes are cognitive. ….and the social epidemiologist must have resources for the correction of ..epistemically undermining possibilities.'* This is required since opening up to the social, the stakeholders in society, opens up to power games which may be to the disadvantage of those problems which are vulnerable to 'inappropriate exercise of authority and biases. This is as we discussed (in Chap. 1) a problem of all times, past and yet to come, because scientific inquiry is not autonomous, value-free and not neutral and is not guided by the invisible hand of the Legend who tells us how best to allocate our public and private funds. Longino offers at the very end of the book a set of questions that demonstrate that she sees a lot of problems here for philosophers to work on for instance how goal-oriented inquiry and *'different kinds of goal might affect philosophy and knowledge and practices*. She goes one step further and involves in these questions '*the institutional organizations and how they affect the content of knowledge'* and asks '*How can a society use science to address problems when scientific goals and community structures are not mutually aligned? These questions bring out the political dimensions of science and broaden our conception of what philosophy of science can be about.'*

Finally, she asks *'What kinds of institutional changes are necessary to sustain the credibility, and hence value, of scientific inquiry while maintaining democratic decision making regarding the cognitive and practical choices the sciences make possible and necessary*? *The fate of knowledge rest in our answers'*.

With these questions, that almost all philosophers of science like Popper consider '*beyond their epistemology* or *theory of knowledge and deliberately bypass',* we return to the main problems addressed here: how does the Legend still determine the ideas and politics of scientific inquirers which distorts the collective of scientific inquiry, causes the current problems of science. Legend and its legacy has detrimental effects on our interaction with society and their publics and thus the knowledge we produce, this is 'the fate of knowledge' Longino is concerned with. Longino, after her own struggle with the dualism of the Legend, boldly has been going where sociologists, physicists, chemists, historians, even anthropologists, but few philosophers have gone before. Still, the reviewers of the book who praised her for that, criticize her for not presenting a detailed epistemology. Longino knew how the work of Dewey and James had been received by the 'real analytical philosophers' of their times, that must have offered some consolation.

2.3 Conclusion

Towards a Realistic Pragmatist View of Science, Natural Science and Social Science and Humanities

From the late 1960s philosophers, sociologist and historians of science gradually, but definitely showed the Legend of the 'Scientific Method' to be untenable:

- **There is no one formal scientific method that leads us to the truth**
- **There is no God-given or timeless, universal foundation for such a method to build on**
- **Knowledge is arrived at, not by individuals in isolation 'talking to nature'**
- **There are many ways (methodologies) to do good research**
- **In sharing ideas and experimental results and methods, for debate and scrutiny in a rigorous and communitive process by the community of inquirers**
- **Inquiry is a social process producing reliable knowledge that produced objective (intersubjective) knowledge**
- **Research is guided by our common cognitive and cultural values, when tested in experiments and discussions with peers constrained by natural and social reality**
- **Knowledge is tested in interventions and (social) actions in practice**
- **It is then either rejected, improved or it is accepted for the time being**
- **Knowledge claims are fallible, absolute and always up to scrutiny and tests**
- **It is this communitive open, independent and transparent process that is unique to science which has produced knowledge which has been proven to be reliable over the past centuries.**

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