Science, Part II: The Study of Science[†]

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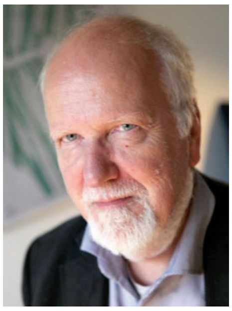

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Abstract: This second part of the trilogy[0] about science, focus on the various fields studying science studies ("science studies", "metasciences" or "sciences of science"). Section 4 focus on the major fields (philosophy of science, history of science and sociology of science) but it also includes the minor fields scientometrics, psychology of science, information science, terminology studies and genre studies. Section 5 is about the fields of scholarly communication and knowledge organization. The main idea is that all the presented fields are important allies to information science with knowledge organization, and that information science should understand itself as a kind of science studies.

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4.0 The study of science

There is a need for an overall term for all kinds of philosophical, theoretical, historical, and empirical studies of science. Daston (2015, 242) suggested:

> The phrase 'science and technology studies' bears witness to these criss-crossing ties to other disciplines, serving as an abbreviation for the conglomerate 'history, philosophy, sociology, and anthropology of science, medicine, and technology,' which, however cumbersome, accurately reflects the ecumenical perspective of many historians of science.

However, very often, the term "science studies" is understood as sociological and anthropological studies of science (and sometimes with a certain theoretical commitment towards constructivism) and excluding the philosophy of science. Just as we need "science" as a broad, inclusive term for all kinds of scientific and scholarly inquiries, we need a broad meaning of "science studies". Sometimes "metascience" or "science of s" are used (e.g., Radnitzky 1970, Bourdieu 2004, Goldsmith 1967), but these labels are also used both narrowly about empirical research or broadly about both empirical, historical, theoretical, and philosophical studies of science. "Science studies" seems to be the mostly used term today, and, in opposition to "science of science", it also avoids the connotation associated with "science" as being limited to natural scientific studies of science.[71] Here we prefer the term "science studies" as the broad overall concept, although in Section 4.3 the term is used in its narrow meaning.

Three main groups of science studies are: (1) Philosophy of science, (2) History of science (3) Sociology of science.

To this comes a range of other disciplines, including psychology and cognitive studies of science, scientometrics, information science, knowledge organization, genre and terminology studies (and much more, not to be further introduced, including economics of science, pedagogy of science, science management etc.).

All these disciplines are interdependent, although this is not acknowledged in all traditions. For example, it is the tradition following Kuhn (1962) that emphasizes the connection between the philosophy and the history of science, whereas the analytic philosophical tradition remains rather ahistorical. All studies of science require domain knowledge about the specific field studied but are different from domain knowledge by providing specific perspectives about the domains.[72]

4.1 The philosophy of science

The philosophy of science addresses problems such as scientific methodology[73] (as considered in Section 3), the objectivity and robustness of scientific claims, the unity[74] versus disunity[75] of science, the demarcation problem (how we distinguish science from non-science and from pseudoscience), scientific theories and laws, models, natural kinds (if any exist),[76] and much more (see, e.g., Newton-Smith 2000 and the voluminous *Handbook of the Philosophy of Science* (Gabbay, Thagard and Woods 2006ff.).[77] Only a few issues in the philosophy of science can be presented in this article.[78]

4.1.1 Metaphysical issues

Two important controversies became significant in the wake of Kuhn (1962): (1) the discussion between realists (or materialists) on the one side and antirealists (or idealists[79]) on the other side and (2) the discussion between relativists on the one side and absolutists on the other.

Realists claim that scientific objects, e.g., atoms, animals, cells and the Milky Way, exist independent of the conceptual frameworks in which they expressed; antirealists claim the opposite. There are many views about realism and Kuhn was unclear on this issue and has been used to argue for what is often understood as antirealist positions like forms of social constructivism and postmodernism, although he (Kuhn 2000, 110) distanced himself from such interpretations and rejected *the Strong Program* in the sociology of science as "deconstruction gone mad".[80] (See further on *the Strong Program* in Section 4.3.2.2 and 4.3.2.4).

Haack (2009, 336) described the situation as follows:

> [T]he last thirty years or so have seen a major shift: from the Old Deferentialist view, which took science to deserve a kind of epistemic authority in virtue of its peculiarly objective method of inquiry; to a New Cynicism, which sees science as a value-permeated social institution, stresses the importance of politics, prejudice and propaganda, rather than the weight of the evidence, in determining what theories are accepted, and sometimes goes so far as to suggest that reality is constructed by us, and 'truth' a word not to be used without the precaution of scare quotes.

Under the term New Cynicism, Haack (2004, 35) included "radical feminists, multiculturalists, sociologists and rhetoricians of science, and ... a good many philosophers as well". She finds that the Old Deferentialism focuses too exclusively on the logical, the New Cynicism too exclusively on the sociological factors that an adequate philosophy of science should combine and that truth lies in between these positions. The natural sciences have been the most successful of human cognitive endeavors, but they are fallible and imperfect—not entirely immune to partiality and politics, fad, and fashion. Haack works from a position inspired by Peirce's pragmatism, which may be termed "pragmatic realism".[81]

It seems that both realists and antirealists have important arguments and views to defend, which need to be considered by any well-developed position. In Section 4.3.2.4 we shall see how "the strong program" has provided new arguments in support of the view that human knowledge at the same time reflects a mind-independent reality and human interests (like pragmatism in Section 3.4).

"Relativism" is, according to McAllister (2000, 405), the claim that the sentence "entity E has property P" should rather be formulated "entity E has property P relative to S", in which S can be cultures, world views, conceptual schemes, practices, disciplines, paradigms, styles, standpoints or goals. Relativism about P therefore implies that P is a relation rather than a one-place predicate. Many kinds of relativism are entirely unobjectionable, for example, the property "utility". However, relativism about truth-value, about rationality or the evidential weight of empirical findings are debated and have been a central issue in the so-called "science wars" (cf., Section 4.3.2.1).

The founder of "the strong program" in the sociology of science, David Bloor (2015, 595), acknowledged that he considered himself a relativist:

> [I]f 'relativism' is simply the denial of 'absolutism,' and the rejection of absolutism is a necessary and sufficient condition for relativism, then the Strong Program [see Section 4.3.2.2] is relativist, and rightly so. [...]
> Critics thus use an eclectic definition [of relativism], but in so doing they conflate questions that should be kept separate. They run together different intellectual

> traditions and fail to draw obvious distinctions. The dichotomy between absolutism and relativism is not the same as the dichotomy between idealism and materialism or between rationalism and irrationalism.

Further information about metaphysical research may be found in encyclopedias such as Kim and Sosa (1995) and in handbooks such as Loux and Zimmerman (2005). It should also be said that metaphysics is closely related to the philosophical field of ontology (see Poli and Seibt 2010), which has gained importance in information and computer science for the construction of ontologies, as systems for organizing knowledge, and thereby has an applied dimension (see Poli, Healy and Kameas 2010).

4.1.2 The demarcation problem

We already considered the demarcation problem in Section 1, because one cannot discuss the term "science" without considering what it includes and what it excludes (e.g., whether the humanities qualify as sciences). In this place, a few issues will be added. First, different concepts must be distinguished. Barseghyan, Overgaard and Rupik (2018, Chapter 6, electronic source no page, italics in original) wrote:

> Historically, many philosophers have sought to demarcate science from non-science. However, often, their specific focus has been on the demarcation between science and *pseudoscience*. Now, what is pseudoscience and how is it different from non-science in general? Pseudoscience is a very specific subspecies of non-science which *masks* itself as science.

There are many terms related to non-science and pseudoscience,[82] including, but not limited to, "fringe science",[83] "junk science", "occult science",[84] "parascience",[85] "pathological science",[86] "pre-paradigmatic science",[87] "protoscience"[88] and "voodoo science". Although some of these terms are used with relatively stable meanings, there seems not to be a general agreement about the terminology of different kinds of non-science. It is, however, important to distinguish "non-science" from "pseudoscience": While one may, for example, consider the humanities part of non-science, it would be wrong to consider the humanities as pseudoscience.

One may ask: what is the discussion of the demarcation problem important for? Mahner (2007) and Hansson (2017) noted that demarcations were particularly important in practical applications such as healthcare, expert testimony, environmental policies, science education and journalism. Laudan (1983) however, found no benefits by philosophers' attempt to define a set of criteria that distinguishes science from nonscience, and his article intended to close the debate about this problem. He wrote (119; italics in original):

> No one can look at the history of debates between scientists and 'pseudo-scientists' without realizing that demarcation criteria are typically used as *machines de guerre* in a polemical battle between rival camps. Indeed, many of those most closely associated with the demarcation issue have evidently had hidden (and sometimes not so hidden) agendas of various sorts.

Laudan warns against the attempt to make demarcation criteria, which he considered a philosophical pseudo-problem (but he maintained the importance of the question: "What makes a belief well founded (or heuristically fertile)?" which he finds should not be confused with the question: "What makes a belief scientific?"). The present author considers that the debate about the demarcation problem may illuminate the question about the meaning of "science": Any attempt to describe and classify something presupposes a clarification of the concept, i.e., what it includes and excludes.

Concerning the history of attempts to solve the demarcation problem Laudan (1983) distinguished "the old demarcationist tradition" (from Aristotle to late nineteenth century) and "the new demarcationist tradition" (from logical positivists and Popper and forward).

Laudan (1983, 112; italics in original) wrote on the old tradition:

> In his highly influential *Posterior Analytics,* Aristotle described at length what was involved in having scientific knowledge of something. To be scientific, he said, one must deal with causes, one must use logical demonstrations, and one must identify the universals which 'inhere' in the particulars of sense. But above all, to have science one must have *apodictic certainty.* It is this last feature which, for Aristotle, most clearly distinguished the scientific way of knowing. What separates the sciences from other kinds of beliefs is the infallibility of their foundations and, thanks to that infallibility, the incorrigibility of their constituent theories. The first principles of nature are directly intuited from sense; everything else worthy of the name of science follows demonstrably from these first principles. What characterizes the whole enterprise is a degree of certainty which distinguishes it most crucially from mere opinion.

The new demarcationist tradition, dominated by the logical positivists in the 1920s and 1930s was not based in epistemology and methodology, but in a theory of meaning. They suggested that a statement was scientific in the case it had a determinate meaning, where meaningful statements were those which could be exhaustively verified. For the positivists verifiability, meaningfulness, and scientific character all coincide. As a would-be demarcation between the scientific and the non-scientific, Laudan (1983, 120) found that it was a disaster: "Not only are many statements in the sciences not open to exhaustive verification (e.g., all universal laws), but the vast majority of non-scientific and pseudo-scientific systems of belief have verifiable constituents".

We will end this section by considering the relation between the demarcation problem and conceptions of science. It seems rather obvious that non-science is the opposite of science and therefore that any conception of science implies what is respectively "science" and "non-science". For example, if science is understood from the empiricist-inductivist point of view, then non-science is by implication what does not live up to empiricist norms. If science is understood as in Popper's philosophy as the attempt to falsify theories, then by implication non-science are the theories which does not have clear criteria for how they can be falsified. If science is understood from a Kuhnian perspective, then the demarcation criterion is sustained support of a puzzle-solving tradition. This insight indicates that the demarcation problem is not an independent problem, but a by-product from insights achieved by the philosophy of science.[89]

Most attempts to provide demarcation criteria tend to consider different fields as monolithic. A more constructive approach could probably be to criticize problematic tendencies in different fields and indicate which kinds of scientific practices and behaviors should be discredited. For example, tendencies to disregard or distort arguments from opponents can be considered a sign of bad scholarship. Much of what today carries the attractive label "science" seems to be dominated by a flood of low-quality papers. The suggestions by Mahner (2013) to consider "a cluster demarcation" based on a comprehensive checklist of science/pseudo-science indicators and providing a profile of any given field based on a thorough analysis rather than a clear-cut assessment seems closer to this idea than most other contributions.[90] We seems to come back to Laudan's suggestion to change the question to: "What makes a belief well founded?"

4.1.3 The classification of the sciences

The classification of the sciences (not to be confused with scientific taxonomy/classification in the sciences[91]) seems today almost to have disappeared as a philosophical field of research. As library scientist Francis Miksa (1998, 34) wrote:

> During the nineteenth century, the classification of the sciences became an activity of enormous propositions among a wide number of participants. I sometimes speak of it as a time when anyone who was anybody in the realm of scholarship wrote a treatise on the subject.[92] ... [p. 48]: The movement to classify knowledge[93] and the sciences ended just after the beginning of the twentieth century, a fact treated by R.G.A. Dolby [1979, 167 and 187-88].

This field is mentioned here, because it is of great interest to the field of information science and knowledge organization, and there are a few scattered, but important philosophical contributions, including Sandoz (2018) and Midtgarden (2020), although most research today comes from bibliometric "science mapping" (see Petrovich 2020).

4.2 The history of science

There is an overwhelming number of descriptions and interpretations of the history of science, both general (e.g., *The Cambridge History of Science*, 1-8[94]) and about single periods (e.g., *Companion to the History of Modern Science*)[95] or the single fields of knowledge (such as medicine,[96] physics,[97] psychology[98] and the humanities[99]). Such histories are written from many different perspectives, for different audiences and may be highly qualified or of a problematic standard (not seldomly they reproduce myths based on problematic readings of the primarily literature).[100] There are also an overwhelming number of scientific biographies and works on single concepts (e.g., objectivity, experiment, theory, and progress) and much more. Although all works are necessarily written from some point of view (as there can be no "view from nowhere") only some works are explicit about their views (e.g., feminist, Marxist or constructivist views), but an informed reader may be able to characterize the view which dominates a certain work. Theory and principles about doing research in the history of science are labeled historiography of science (an example is Agassi 2008).

About different schools in the history of science, Agassi (2008, 31) wrote:

> Two philosophical schools of thought support the thesis that science is always right, and they gave rise to two schools of historians of science. The majority (Baconian) school is the inductivist or a posteriorist: science is always right as its ideas are firmly based upon experience. The minority (Duhemian) school is the conventionalist; scientific ideas are mathematical conventions. Although my sympathy, if forced to choose, is unquestionably with the minority against the majority, I belong to neither schools. Rather, I find much more congenial the view of Karl Popper of science not as a body of solid knowledge but as a succession of ideas and of the attempts to criticize them, with no end in sight.

The present article cannot go into a deep analysis of historiographic philosophies, but the working hypothesis is that empiricist, rationalist, historicist, and pragmatic philosophy will turn out to represent the deepest level also in the historiography of science.

Two further issues to be introduced are (1) a way of history writing called "whiggish" and (2) problems in periodization in the history of science.

Whiggish historiography has been characterized as the 'great man' view of science or as "a particular pathology of history writing" (Schuster 1995b, Chapter 3, 14) and (the same source) as a simplified writing of history consisting of "good guys" or "bad guys", where the good guys are those who made steps toward present day views in science. The writing of biographies as "good guys" has been termed "Whiggish hagiography" and the opposite "Whiggish demonology". Roos (2018, 195) expressed: "Accounts of Eureka moments are a favourite in these works; the compression of decades of work into a single inspirational moment is entirely characteristic of these parables."

Schuster (1995b, 17) concluded his short chapter "The Problem of 'Whig History' in the History of Science":

> We are going to see that Whiggish history of science depends upon and reinforces the three key myths about science: method, autonomy and progress. Hence, we shall see that all these beliefs stand or fall together. If they stand, we remain at the level of cultural myth and mystification in our understanding of Western Science; if they fall, the possibility of a demystified historical understanding of science emerges, and that is where we are headed over the next twenty-three chapters.

We cannot here go deeper into the debates of Whig historiography, but the reader should consider that the problems are more complex than the description given here (see, for example, Alvargonzález 2013).

4.2.1. Periodization

Shaw (2020) introduces the issues related to periodization in the IEKO encyclopedia. Gabovich and Kuznetsov (2019) presented some of the plentiful criteria for temporal classification of science, which have been proposed in the literature, and they defend a theory-grounded periodization of science. We shall not consider this view further, but emphasize an important issue:

- internalism in the historiography of science views the development of science distinct from social influences, but determined by the knowledge generated within a science itself (or more or less interdisciplinarily influenced);
- externalism in the historiography of science is the view that the history of science is due to its social context.

These two views on the development of science are still the subject of analysis. Lakatos (1978, 118–122 and 190) attempted to explain the distinction between internal-rational and external-empirical history of science, which has been criticized but recently supported (see, e.g., Dimitrakos 2020). Omodeo (2019, 2-3) found that this distinction was maintained by the Cold War.[101]

The writing of the history of science (in general or a specific science) can thus be guided by opposite assumptions. Abrahamsen (2003, 149-51) described how two Danish histories of music described the same field (the history of music) in vastly different ways regarding, for example, periodization. In one of them, based on a "style paradigm" (but relatively uninterested in theoretical explanations), the history of music is seen as fundamentally different from general history because of music's aesthetic character, and it expresses the implicit view that the musical work is relatively autonomous. Consequently, this work focuses more on the composer's and performer's role in the development of music. In the other work (based on a materialist philosophy, and more explicit about its epistemological commitment) the culture of music is viewed as a part of a historical process, where the music is included in an interaction with political, social, economic, and ideological elements, and the description of the music's function in this interaction is this book's main concern.

4.3 The sociology of science and "science studies"

Collins and Evans (2002) outlined three waves of science studies:

- The first wave (about 1950-1970) was characterized by the aim (239; italics in original) of "understanding, explaining and effectively reinforcing the success of the sciences, rather than questioning their basis [...] This wave of 'positivism' began to run into shallow academic waters in the late 1960s with Thomas Kuhn's book and all that followed. By the end of the 1970s, *as an academic movement*, it had crashed on to the shore".
- The second wave (from about 1970 until today) has shown that it is (239) "necessary to draw on 'extra-scientific factors' to bring about the closure of scientific and technical debates – scientific method, experiments, observations, and theories are not enough." In this phase "sociologists have become unable to distinguish between experts and non-experts".

- The third wave Collins and Evans (2002) labeled "studies of expertise and experience" (SEE) and is described as something that may already exist in embryonic form (in 2002) and for which their article is a further argument. The authors suggest (238) that it "should accept the Second Wave's solution to the Problem of Legitimacy [that the basis of technical decision-making can and should be widened beyond the core of certified experts], but still draw a boundary around the body of 'technically-qualified-by experience' contributors to technical decision-making."

Collins and Evans (2002) found that sociologists in the second wave have dissolved some dichotomies and classes and left a need to build new ones based on a "normative theory of expertise". In the present paper we consider three schools of science studies. The first (4.3.1 Merton) represent the first wave, while the other two (4.3.2.2 The strong program and 4.3.2.3 Bruno Latour and "actor-network theory") are classified as constructivist and mostly representing the second wave. We have here no specific coverage of anthropology, but Bruno Latour is also considered an anthropologist of science. An early, important contribution from anthropology is Elkana (1981).

4.3.1 Merton

Robert King Merton (1910–2003) has been called the "founder of the sociology of science" and "undoubtedly the most important sociologist of science" (Cole 2004, 843). In his dissertation (Merton 1938), he asks why it is that science emerged so strongly in the third quarter of the 17th century in England and why this particular institution does well in one society at one point in time. His answer was that science flourishes in societies in which scientific activity is highly regarded by the society at large. Among the specific concepts and theories developed by Merton the following can be mentioned:

- "CUDOS": an acronym with four (in some versions five) normative principles, comprise the ethos of scholarship, introduced by Merton (1942) and later modified:
  - Communalism (originally: "communism"): all scientists should have common ownership of scientific goods (intellectual property), to promote collective collaboration; secrecy is the opposite of this norm.
  - Universalism: Scientific validity is independent of the sociopolitical status/personal attributes of its participants.
  - Disinterestedness: scientific institutions act for the benefit of a common scientific enterprise, rather than for the personal gain of individuals within them.
  - Originality: the commitment to the pursuit of new knowledge. "Whereas objectivity is a value that works to safeguard the *truth* of science, originality is a value that works to safeguard it from stagnation" (Sztompka 1986, 52).
  - (Organized) Skepticism: scientific claims should be exposed to critical scrutiny both in methodology and institutional codes of conduct. Science must systematically examine claims, be anti-authoritarian and skeptical.

Merton considered these norms as ideals, not as descriptions of the actual behavior of researchers.

- "Foci of attention": What determines researchers' choice of topics/research problems? Merton (1938) demonstrated that scientists were strongly influenced by the practical concerns of the day, such as navigation (this can be interpreted as a support of the pragmatic epistemology presented in Section 3.4).
- "Matthew effect" or "the Matthew effect of accumulated advantage" is an expression introduced by Merton (1968) derived from the Bible (Matthew 25:29): "For to everyone who has will more be given, and he will have abundance; but from him who has not, even what he has will be taken away". It is sometimes summarized as "the rich get richer and the poor get poorer". In science studies it relates to the claim that scientists who have had an advantage tend to be overrated. In bibliometrics is has been used to claim that those who are known and have many citations will get more citations than they deserve.
- "Multiple discovery" are discoveries made independently by more than one researcher. Merton (1963, 307) and Merton (1996) found that more researchers independent of each other often make such discoveries. By implication, scientific development does not dependent on a few geniuses, but the geniuses just accelerate the process.
- "Obliteration by incorporation": Certain ideas become so universally accepted and commonly used that their contributors are no longer cited. Eventually, its source and creator are forgotten ("obliterated") as the concept enters common knowledge (is "incorporated"). Obliteration occurs when "the sources of an idea, finding or concept, become obliterated by incorporation in canonical knowledge, so that only a few are still aware of their parentage." (Merton 1968, 27-8)
- "Serendipity" (unplanned, fortunate discovery) (Merton and Barber 2004). (This concept has derived much research in information science. However, as pointed out by Carr (2015), serendipity in the stacks it can usefully be framed as a problem: "From a process-based standpoint, serendipity is problematic because it is an indica-

tor of a potential misalignment between user intention and process outcome".
- "Uncitedness". Merton (1977, 54-5): "For if one's work is not being noticed and used by others in the system of science, doubts of its value will arise."

Merton was, prior to the time of his death, the most famous living sociologist of science. Merton's sociology of science has, however, been criticized on different levels. Cole (2004) contains a rather serious criticism of the research done by Merton (and even of Merton as a person). He claims, for example, that the Matthew effect simply is wrong when tested empirically. The main issue in later generations of sociologists and researchers associated with social constructivism is however, that Merton's view is considered "weak" because it left the cognitive content of science out of the sociological explanation.[102] Merton explained the social conditions for scientific flourishing, but he did not ask which the social conditions made science construe the contents of scientific theories.

4.3.2 Social constructivism

4.3.2.1 Introduction

By contrast to Merton's sociology of science, most newer researchers in the sociology of science have been associated with the label "social constructivists" (but not all, Stephen Cole, for example, is an exception), and there are wide divergences in views within constructivism. The different meanings of the term have been clarified by Hacking (1999). Golinski (1998, ix), who characterized his own attitude as sympathetic but not uncritical towards constructivism, defined the term in this way:

> By 'constructivist' outlook, I mean that which regards scientific knowledge primarily as a human product, made with locally situated cultural and material resources, rather than as simply the revelation of a pregiven order of nature.

In this broad understanding, constructivism is very much in line with the pragmatic view described in Section 3.4 (where pragmatism was characterized by seeing the inquirer as influenced by socio-cultural factors). However, as already said, constructivism consists of many different views, of which just two are briefly outlined below.

Constructivist theories have been considered as, on the one hand, liberating, and on the other had as harmful ideas. They are considered liberating ideas because they imply that we do not have to accept scientific knowledge claims, but can engage in alternatives, which we find more satisfactory. As formulated by Hacking (1999, 6-7):

> Social construction work is critical of the status quo. Social constructionists about X tend to hold that:
> (1) X need not have existed, or need not be at all as it is. X, or X as it is at present, is not determined by the nature of things; it is not inevitable.
> Very often they go further, and urge that:
> (2) X is quite bad as it is.
> (3) We would be much better off if X were done away with, or at least radically transformed. [...]
> X was brought into existence or shaped by social events, forces, history, all of which could have been different.

From the other view, constructivism is considered harmful by tending to undermine science. There have even been "science wars" between scientists and philosophers on one side and constructivist sociologists on the other side. These "wars" have, according to Hacking (1999, x) "temporarily destroyed the possibility of friendly discussion and scholarly collaboration". Hacking (67) also writes that "many science-haters and know-nothings latch on to constructionism as vindicating their impotent hostility to the sciences" but he also suggests (68) that this is not the case for leading constructivists such as Pickering and Latour.

4.3.2.2 The strong program

The so-called "strong program" (or "the Edinburgh School") in the sociology of science was formulated by an interdisciplinary group of researchers in Edinburg in the 1970s (for introductions see Barnes, Bloor and Henry 1996 and Bloor 2015).

The principles formulated by Bloor (1976) elaborated four principles guiding the strong program:

- the first principle is that sociology, as science, has to offer causal explanations;
- the second principle is that sociology needs to investigate social beliefs without imposing the standards of the investigator upon the subject of investigation;
- the third principle, reflexivity, suggests that the theory of sociology should not be immune to its own argument; it must be possible to conduct a sociology of sociology;
- the final principle, symmetry, holds that both true and false, and rational and irrational ideas, in as far as they are collectively held, should all equally be the object of sociological curiosity, and should all be explained by reference to the same kinds of cause.

However, in practice, the studies made by this school has been more philosophical and historical than they have been empirical-sociological (cf., Hacking 1999, 37). This seems important because the suggested ideals for empirical studies seem to reflect the ideals of logical positivism (Collin 2011, 37ff.), whereas the philosophical principles derived from or applied to case-studies seem to provide a clear alternative to positivism and to be related to pragmatism.

The strong program defends a view of knowledge, "interest theory" (cf., Barnes 1977), in which all knowledge is considered social, and interests influence the scientific process. This program, as constructivism overall, opposes the view that sociological analysis can only explain knowledge that is in error, whereas true knowledge remains insulated from social forces.

Constructivism has provided much controversy and social explanations of scientific claims have been considered incompatible with realism: that scientific claims are true claims about the world. This question about whether the strong program represent a realist position or not, is much debated. Barnes, Bloor and Henry (1996, 81-88) explained their realist position carefully, but many others deny this and find constructivism to be "idealist" (e.g., Downes 1998).[103] Bloor (2015, 592-3) discussed and refuted what he called "The False Charge of Idealism".

The examples from science given by Bloor (1982), and the application of the network model, provide convincing arguments for the possibility for systems of knowledge to reflect society and be addressed to the natural world at the same time. They demonstrated how social interests were active in the development of scientific knowledge. Bloor provides little guidance, however, in determining how sociological research may help us determine contemporary scientific controversies (including classification problems in science such as the Periodical system in chemistry and physics and biological classifications, which are still subject to scientific debates).[104]

Slezak (1994) provided an extremely critical review essay of Bloor (1991) and wrote (338):

> Certainly, the contingent, causal connection claimed to hold between science and society, content and context, entails that we should be able to predict the substance of scientific theories given the details of the social, cultural milieu. Recall that the much-touted case studies of the Strong Programme are taken to have established precisely this kind of connection. However, Popper offers a formal, logical argument to the effect that no scientific method can possibly yield its own future results, and hence predicting the future course of human history is also impossible to the extent that this is influenced by the contents of scientific theories.

This criticism seems not to have been answered by Bloor, but Suchting (1997) provided a fruitful analysis with the overall conclusion "that each account [Slezak's and Bloor's] alludes to different and crucial aspects of the nature of knowledge without, severally or jointly, being able to theorise them adequately", and then suggested his own twelve epistemological theses for a more adequate theory of knowledge.

4.3.2.3 Bruno Latour and "actor-network theory"

Latour is a very influential figure in the sociology and anthropology of science. His research includes detailed empirical studies of scientists work in their laboratories, ontological theorizing, and political speculation. His position is complex and difficult to summarize.

As an example of Latour's empirical research is the book *Laboratory Life* (Latour and Woolgar 1979 and 1986[105]), which describes his empirical observations of the endocrinological researchers in the Salk laboratories in San Diego, who discovered the thyrotropin releasing hormone (TRH) and (together with others) were awarded the Nobel Prize in 1977. There is extraordinarily little TRH in the world. To study it, five hundred tons of big brains had to be shipped to the laboratory on ice to distill just a microgram of TRH. Hacking (1999, 175) wrote:

> And what was this TRH? It was a substance that passed certain assay tests. But there was no agreement on what the assays should be, and different labs had different assays. The winning labs 'determined' the assays and so determined the practical criteria of identity for TRH. Second, when a certain peptide had been synthesized, and declared to be TRH, that was the end of the matter. The drug company that had sponsored much of the research patented and started selling synthetic TRH. The question as to whether this really is TRH simply dropped out, with the skeptics turning their minds to other things. Synthetic TRH became a laboratory tool in its own right ...

According to traditional scientific norms, new research demanding hundreds of tons of brains should have continued until consensus has established what TRH is. But it did not. As Hacking (176) wrote: "Who will collect another 500 tons of big brain to distill a microgram of whatever it is?" Latour say that TRH is a scientific "fact" that was constructed by the leading lab, and TRH gave rise to a whole new research field, which therefore also is "constructed". Hacking (1999, 177) offers an alternative interpretation: "A realist need only say that among all the possible facts to be discovered in the endocrinology of the hypothalamus, this particular structure has been singled out and will determine the future possible structures to be discovered, shutting off others from the screen of possibilities."

Another of Latour's works, *The Pasteurization of France* (Latour 1988), analyzed how the French microbiologist, Louis Pasteur, was constructed as a great man, who is said to have revolutionized French agriculture by, among other things, discovering the cause of anthrax and creating a vaccine for the disease. Instead of just considering this the work of an individual genius, Latour (in the words of Law 2009, 145), "charts how a network of domesticated farms, technicians, laboratories, veterinarians, statistics, and bacilli was generated. He describes how they were shaped (in some cases created) in this network. And he shows how the result was generative. Farms were turned into laboratories, vaccines made from attenuated bacteria, cattle stopped dying of anthrax, and Pasteur became a great man".

Latour's downplaying of the role of the individual and emphasis on the collective of which he is a part, is well known from, for example, Marxist theory and from many (non-Whig) historical writings, which emphasize the background factors of discoveries. Latour's interpretation is, however, radical. We can recognize his "actor network theory" (ANT) in the example of Pasteur. Law (2009, 141) characterized ANT in the following way:

> Actor network theory is a disparate family of material-semiotic tools, sensibilities, and methods of analysis that treat everything in the social and natural worlds as a continuously generated effect of the webs of relations within which they are located. It assumes that nothing has reality or form outside the enactment of those relations. Its studies explore and characterize the webs and the practices that carry them. Like other material-semiotic approaches, the actor network approach thus describes the enactment of materially and discursively heterogeneous relations that produce and reshuffle all kinds of actors including objects, subjects, human beings, machines, animals, "nature," ideas, organizations, inequalities, scale and sizes, and geographical arrangements.[106]

The radicalism attributed to ANT has particularly focused on ANT's recognition of subjectivity or agency in other than human beings, and even in other than non-living things. Muniesa (2015, 82) wrote:

> ANT is often associated in popular views with an insistence on 'nonhuman agency', that is, on sources and agencies of action other than purely human, conscious, and intentional. In fact, ANT stands as a reaction to both the downplaying of human agency in accounts of events favored in the natural and formal sciences (an ellipsis of the action of the experimenter in a microbiology laboratory, for example, in reports of findings) and the downplaying of nonhuman agency in accounts of events favored in the social sciences and the humanities (an ellipsis of the actions of bacteria, the medium, and the laboratory instrument).

The strong program and ANT have expressed critical views towards each other. Bloor's (1999a) article even bears the title "Anti-Latour" (answered by Latour 1999 and re-answered by Bloor 1999b). However, when members of the strong program tried to be positive, they (Barnes, Bloor and Henry 1996, 115-6) wrote:

> It is important however that we take note of the virtues as well as the limitations of Latour's work, so let us praise it for its methodological significance. It encourages anyone disposed to consider science from the perspective of political economy to consider every single action[107] without exception in that light, and to refuse to exempt any aspect of science whatsoever from that kind of scrutiny.

Omodeo (2019, 18-19) and Mirowski (2017, 447) found, however, that ANTs depersonalizing of action and subjectivizing of nature[108] undermines the possibility of criticizing or improving science.[109] As we shall see below (Section 4.3.2.4) Latour seems to admit that his research may have supported unhealthy influences and needs a revision.

4.3.2.4 Conclusion on the sociology of science

Merton's research (and other sociologists outside constructivism) is a respected field which has contributed much to our understanding of science. The role of the many constructivist schools is much more controversial and difficult to evaluate. A key issue is here constructivism's status as either "realist" or "idealist" and the related problem of whether constructivism is supporting or damaging science.

Downes (1998) wrote: "These forms of constructivism [explicitly including the strong program], claiming that scientists have access to nothing other than representations, are reminiscent of the idealism of Berkeley".[110] Haack (1996, 263) argues that some social constructionist positions treat natural objects themselves as if they are socially constructed rather than having an independent existence. She does not, however, specify which positions she is referring to, and in relation to the strong program, Bloor (2007) seems effectively to have refuted her claim.[111] Barnes, Bloor and Henry (1996) provide a convincing theoretical frame for a sociology of science in which the scientific objects (nature) are included, but still science is understood as also influenced by social interests. In Section 3.4 we discussed the same issue and found that some pragmatists did not see an opposition between pragmatism and realism.

Concerning the issue about whether constructivism undermines scientific argumentation, Kemp (2005, 707-8) wrote:

> What Bloor wishes to demonstrate is that constructionists of the Strong Programme variety have been wrongly targeted in this respect. He argues that there is a clear distinction between the perspectives and purposes of scientific actors and social constructionist analysts. Scientific debate is undertaken in order to reinforce or undermine the credibility of scientific claims. Constructionists, on the other hand, take a step back to analyse the field of play from a non-judgemental perspective, examining the construction of scientific credibility without assessing scientists' claims for credibility. This being the case, constructionist analyses do not challenge or undermine scientific argumentation, leaving it untouched.

This argument seems problematic, however. Should epistemological, historical, and sociological research on science work from a non-judgmental perspective as suggested by Bloor? Feminist epistemology, for example, has critically analyzed "positivist" research and suggested better alternatives (see, for example, Hjørland 2020). Likewise, Kurt Danziger's research about the social construction of psychological knowledge seems extremely useful for understanding psychology as a science. For example, in Danziger (1997), it is shown how intelligence was constructed as a psychological concept, and how this construction was connected to social interests such as the military and the school system and thereby, at least implicitly, suggesting how psychology could be developed to better serve other interests, such as the needs of teachers and students. It seems rather obvious that the sociology of knowledge should examine which interests are served by given research, and which are relatively harmed or neglected, and thereby, at least sometimes, should try to undermine a given piece of claimed knowledge. However, it should certainly not undermine science as an institution. In this connection Latour (2004) contains a revealing point of view. He wonders if science studies enabled right-wing climate denialism, alternative medicine, and a politics of conspiratorial thinking. Latour says he made a career of asking critical questions of scientists, and now his political foes are using the same kinds of arguments to delegitimize science and breed excessive distrust (227). Therefore, he suggests that science studies change course, likening himself to a general who alters strategies as the battlefield conditions change, suggesting that deconstructivism should become a constructivism, adding to reality rather than merely subtracting from it (232).

Sociology of science after Merton often seems to provide fewer concrete findings, but to focus on historical and philosophical problems: Perhaps for this reason bibliometricians, for example, have often found the new sociology of science unhelpful.[112] The following points constitute an attempt to list what can be regarded as specific contributions from the new sociology of science:

- supporting a pluralist view of knowledge[113] and exposing the contingency of some of our social practices that we have wrongly come to regard as inevitable (e.g., Danziger 1990);
- supporting the pragmatic realist view according to which scientific knowledge is at the same time reflecting reality and reflecting human interests (e.g., Barnes, Bloor and Henry 1996);
- illuminating the role of gender in science (e.g., Fishman, Mamo and Grzanka 2017);
- contributing to describing and understanding the information infrastructures in science (e.g., Slota and Bowker 2017);
- describing degrees of consensus in different fields of sciences (e.g., Cole 1992);
- contributions to a critical understanding of technology (e.g., Feenberg 2017);
- contributions to understanding the relation between science and society and providing new conceptualizations of science (this aspect is further reported in Section 6 below).

4.4 Other fields studying science

4.4.1 Scientometrics

Scientometrics is a subfield of bibliometrics which studies quantitative issues in science, primarily based on scientific publications; it is an interdisciplinary field with strong basis in information science and the sociology of science, among other fields. A very productive journal in this field is *Scientometrics: An International Journal for all Quantitative Aspects of the Science of Science, Communication in Science and Science Policy*.

4.4.2 Cognitive science of science and psychology of science

The role of psychology has been controversial. Some, for example, George Boole, saw logic as part of psychology (cf. the title of his 1854 book *An Investigation of The Laws of Thought on Which Are Founded the Mathematical Theories of Logic and Probabilities*), whereas, for example, Edmund Husserl devoted the *Prolegomena* in his *Logical Investigations* (originally published 1900, here cited from the English translation, Husserl 2001b, 40) to a detailed refutation of psychologism, i.e. the thesis that logic is merely a branch of psychology such that logical laws can be reduced to psychological laws.

Willard Van Orman Quine coined the term "naturalized epistemology" by which he suggested that epistemology should be replaced by psychology. In *Epistemology Naturalized* (Quine 1969, 78) he wrote:

> If all we hope for is a reconstruction that links science to experience in explicit ways short of translation, then it would seem more sensible to settle for psychology. Better to discover how science is in fact developed and learned than to fabricate a fictitious structure to a similar effect.

From the point of view of the sociology of science, Bloor (2007, 216) found that all individualist and subjectivist accounts of concept application clearly are in trouble,[114] but nonetheless found a place for the psychological study of science.

We cannot in this article go deeper into the role of psychology and cognitive science for the study of science, but just mention one line of development that seems particular fruitful. This is an approach based on the historical study of conceptual revolutions in science (in the wake of Kuhn, 1962) which focus on the cognitive processes during paradigm shifts (see, e.g., Andersen, Barker and Chen 2006 and Thagard 1992 and 2012).

4.4.3 Information science

Information science studies how scientists and scholars use various information sources and contribute to the knowledge about and technologies for scientific infrastructures (e.g., information retrieval systems). The subfield of knowledge organization contributes knowledge about, for example, classification systems, indexing, social tagging, and ontologies (see further in Section 5.1). In bibliometrics, research is often closely related to that of sociologists. Ingwersen et al. (2020), for example, demonstrated how researchers with conflicting views tend to publish in different committed scientific journals. A recent paper basing information retrieval and knowledge organization on the philosophy of science is Hjørland (2021). (See further about this field in Section 5).

4.4.4 Terminology studies

Terminology studies was founded by Eugen Wüster (1898-1977), an engineer with a strong interest in information science and knowledge organization (he was associated with the journal *International Classification*, now *Knowledge Organization* for a period from 1976). His theoretical views are discussed by Cabré Castellví (2003), demonstrating how this field also developed away from positivist principles. An overview of the field is provided in the series *Handbook of Terminology* the first volume of which was edited by Kockaert and Steurs (2015).

4.4.5 Genre studies and composition studies

Genre studies and composition studies is the study of academic writing and communication and its different genres. Prominent contributions include Bazerman (1988), Hyland (2000), Swales (2004) and Thelwall (2019).

4.5 Conclusion

Science is study by many different fields and from many different perspectives. Each of these fields and perspectives contributes to the overall understanding of science. However, all these fields are influenced by different epistemologies and are depending on more overall conceptions of science.

5.0 Scholarly communication and knowledge organization

5.1 Overview

Scientific and scholarly communication is an interdisciplinary field of research and teaching. It includes parts of information science and knowledge organization because they are about providing infrastructures for science in the form of institutions (like research libraries, archives, and museums), systems (like bibliographical databases, classification systems and ontologies) and processes (such as indexing, retrieving, and synthetizing scholarly research).[115] Therefore, as mentioned in Section 4.4.3, information science and knowledge organization should also be considered part of science studies in the broad sense. The study of scholarly communication is institutionalized in, among other, library and information science (LIS) departments.

Scholarly communication may be considered from a broad, overall perspective on what has been termed "the information ecology", and from the perspective specific topics, concepts, systems, and processes in this ecology. The overall information ecology has, for example, been modelled by "the UNISIST model" (UNISIST 1971; Fjordback Søndergaard, Andersen and Hjørland 2003) as a system of actors, institutions, systems, and processes in two dimensions:

1. from knowledge producers to users with primary, secondary, and tertiary information services;
2. via different communication channels (informal, formal published, formal unpublished and tabular channels).

The broad perspective includes the study of concepts such as data, information, knowledge, science, documents, relevance, media etc. as well as the theories in which these concepts form part as related to science infrastructures.

The study of specific elements in the information ecology encompasses an exceedingly long range of concepts, systems, and processes. From the perspective of information science, two focal points can be emphaticized:

A. Information retrieval, searching and seeking.
- Information retrieval research (IR) is today mainly (but not exclusively) a part of computer science and is involved in the construction of Internet search engines. Its main paradigm is based on statistical relations between term frequencies in documents, requests, and collections of documents and links between documents (and it should rather be termed "document retrieval"). In a broader sense IR includes other perspectives, including what is below termed "information searching".
- Information searching (or document searching, including literature searching, picture searching, music searching, people searching, data searching etc.) is about bibliographies and bibliographical databases, terminology, and search strategies (e.g., the use of controlled vocabulary vs. free text or the use of citation searching vs. term searching). It is traditionally an important activity in libraries and traditionally a core competency of librarians and information professionals. Today, it plays an important role in evidence-based practices, but also more broadly in what has been termed "information literacy".
- Information seeking studies is mainly descriptive studies of how people search for information rather than prescriptive norms for how search should be done.

Today science studies play only a limited role in these three fields. It is, however, the opinion of the present author that closer connection with science studies in the broad sense may improve these fields considerably.[116]

B. Knowledge organization (i.e., the description, classification and indexing of documents and concepts, metadata assignment, and the development of classification systems, ontologies, and other kinds of knowledge organization systems). This field has a connection to the philosophical classification of the sciences, as well as to the philosophy of classification. However, like the fields mentioned in point A, knowledge organization needs to develop a closer relation to science studies.[117]

A long range of specific topics includes:[118]
- document types and genres[119] (e.g., books,[120] bibliographies,121 encyclopedias,[122] journals,[123] journal articles,[124] patents,[125] systematic reviews[126] and standards[127]);
- publishing,[128] editing,[129] peer-review,[130] open access,[131] predatory journals,[132] grey literature;[133]
- bibliometrics and citation analysis,[134] altmetrics,[135] research evaluation,[136] influence and ranking[137], h-index;[138]
- libraries,[139] archives,[140] museums,[141] institutional repositories,[142] and other "memory institutions";[143]
- research/knowledge synthesis;[144]
- evaluation of information sources, source criticism.[145]

Each of these many topics are often studied in a fragmented way and in the absence of overall perspectives. The field therefore seems to be without clear formulated research programs and strategies. Also, there have been strong tendencies to separate "user-oriented" approaches from "systems-oriented" approaches although it seems clear that the purpose must be to study socio-technical infrastructures to improve them, or to help the users better to navigate in them. What documents should be found is neither a psychological issue nor a technological issue, but an epistemological issue: what should be retrieved as answers to a query is what is best supported by scientific research (see further Hjørland 2021). That scientific research often is controversial does not, however, makes any answer as good as any other answer, but just introduces new questions about how to evaluate research. Before we return to this issue in Section 5.4, let us consider how IT-developments have provided new conceptions of science.

5.2 Information technology's (IT) influences on science

There have been claims of fundamental changes in science caused by the developments in IT, using terms such as "data-driven science", "big data science", "e-science",[146] "open science", "the fourth paradigm" and "the end of theory" (for a broad overview see Dutton and Jeffreys 2010). Such developments can only be very selectively and briefly presented here. No attempt will be given to define and distinguish these labels or to construe a taxonomy of them. In general, the terms represent literatures containing a mixture of (a) important breakthroughs in IT which influences the way science is carried out; (b) attempts to apply perspectives from science studies; (c) problematic philosophical assumptions, often based on a naïve form of empiricism, and (d) much hype.

The labels mentioned reveal important developments in the quantity of available data and of new tools, such as text mining, deep learning, visualization, network analysis and much more. We are now living in "the petabyte age", and the availability of these huge amounts of data for science, may often provide a much higher inclusiveness for scientific analyses, and new tools for data analyses. Big data may permit larger sample sizes, cheaper and more extensive testing, and continuous assessment of theories. Mazzocchi (2015, 1251-2) found:

> Many valuable insights have been gained by applying this approach. In bioinformatics, for example, it has triggered a change in modeling strategies to obtain biological insights from experiments. The process of model building is driven by the massive amount of data produced and less dependent on theoretical presuppositions or hypotheses [...]. The use of data analysis helps researchers to cope with the astonishing complexity of these systems, especially when large spatial and temporal scales are involved.

It seems important to distinguish between big data produced by scientists for specific purposes versus big data produced for other purposes, but also used for scientific purposes. In 2012 CERN announced that it had finally proven the existence of the Higgs boson. This was done by the LHC (Large Hadron Collider), the world's largest and most powerful particle collider, which generates up to 600 million collisions per second and produces 15 petabytes of data per year. Big data, distributed computing and sophisticated data analysis all played a crucial role in the discovery of the Higgs boson. In this case all the data were carefully constructed by the scientists for this specific task, and as Mazzocchi (2015, 1253) writes:

> ... the discovery of the Higgs boson was not data-driven. The collider experiments were mostly driven by theoretical predictions: it is because scientists were attemptig to confirm the Standard Model of elementary particles that the discovery of the Higgs boson—the only missing piece—could occur.

In other cases, however, data have not been produced for a specific task, but have been found. This raises the question of the quality of the data for the given purpose. As Geoffrey Bowker (2005, 184) said: "Raw data is both an oxymoron and a bad idea; to the contrary, data should be cooked with care." In contrast to science driven by theory and hypotheses (and by careful preparation of data) the concept "data driven science" has been suggested. It has even been suggested that science has now reached a stage of development which has been called "the end of theory" (Anderson 2008), where "the data deluge makes the scientific method obsolete". Anderson stated (electronic source, no pages):

> The scientific method is built around testable hypotheses. These models, for the most part, are systems visualized in the minds of scientists. The models are then tested, and experiments confirm or falsify theoretical models of how the world works. This is the way science has worked for hundreds of years.
> Scientists are trained to recognize that correlation is not causation, that no conclusions should be drawn simply on the basis of correlation between X and Y (it could just be a coincidence). Instead, you must understand the underlying mechanisms that connect the two. Once you have a model, you can connect the data sets with confidence. Data without a model is just noise.
> But faced with massive data, this approach to science — hypothesize, model, test — is becoming obsolete. [...]
> There is now a better way. Petabytes allow us to say: 'Correlation is enough.' We can stop looking for models. We can analyze the data without hypotheses about what it might show. We can throw the numbers into the biggest computing clusters the world has ever seen and let statistical algorithms find patterns where science cannot.

Anderson (2008) has been cited at the least 298 times according to *Web of Science* on August 12, 2020. However, according to Norvig (2008) it was a provocation stating untrue claims, which even the author himself considered false. What is a reality, however, is the widespread use of the term "data driven" with its tendency to a return to an inductivist epistemology, which has widely been criticized, for example, by Frické (2015, 651), who argued that theory is needed in every turn and wrote: "Data-driven science is a chimera".

Microsoft published an edited book (Hey, Tolle and Tansley 2009) which contains chapters about developments in scientific infrastructures and scholarly communication in different fields of science and about general issues. Each chapter takes as its point of departure the conception of "the fourth paradigm", which as suggested by late computer scientist James Nicholas Gray and in the book (xvii-xxxi) transcribed from a talk. Gray's central idea is that science has developed through four paradigms (Gray 2009, xviii; italics in original):

- first paradigm: thousand years ago, science was empirical and describing natural phenomena;
- second paradigm: last few hundred years, theoretical branch, using models and generalizations;
- third paradigm: last few decades, a computational branch, simulating complex phenomena;
- fourth paradigm: today, data exploration (eScience), unified theory, experiment, and simulation;
  - data captured by instruments or generated by simulator;
  - processed by software;
  - information/knowledge stored in computer;
  - scientists analyzes database/files using data management and statistics.

(Gahegan 2020, 1-2, interpreted these paradigms further, see endnote [147])

A closer look at these "four paradigms" poses, however, a range of questions:

- is it true that empirical science is thousand years old, and that theoretical science is only few hundred years? Would the opposite generalization not come closer to the truth? Would it not be better to say that it was the Scientific Revolution from about the middle of 1500s that made science experimental and empirical? (Although, of course, both empirical and theoretical elements have been influential throughout the history of science);
- is the electronic revolution really a scientific revolution comparable with empiricism and rationalism? Bell (2009, xi) compared the fourth paradigm to the revolution caused by the invention of printing. But the inventing of printing does not appear among Gray's four paradigms. Would that be a more adequate predecessor? Is it the media or the epistemologies, that are changing, or how is the interaction between epistemology and media?[148]
- the model uses concepts such as "data", "information" and "knowledge" without a proper examination of these concepts or an attempt to provide fruitful definitions. Because they are not explicated, it cannot be seen if, for example, data are understood from the problematic philosophical assumption that a datum is "a single, fixed truth, valid for everyone, everywhere, at all times" (Edwards 2010, 283). About the data concept see further in Frické (2019) and Hjørland (2018);
- it may be recalled that the view proposed in the present article is that rationalism, empiricism, historicism, and pragmatism always and in all disciplines are interacting and competing paradigms (although at a given time or domain one will tend to be the most dominant). The implication of this view is that today's eScience should also be viewed as a struggle between paradigms. Books such as Edwards (2010) about climate science and Leonelli (2016) about data-centric biology, provide insights on such different paradigms in eScience, that are unfortunately absent in "The Fourth Paradigm".

5.3 Do "data" displace academic documents?

A separate question is about the changing relations between academic papers on the one hand and "data" on the other hand. This question is connected to the problem whether science is driven by hypotheses (implying primacy of papers) or by data (implying primacy of data). Ginsparg (2009, 190) found that "we should neither overestimate the role of data nor underestimate that of text", while Goble and de Roure (2009, 144) opinioned: "datacentric science could be characterized as being about the primacy of data as opposed to the primacy of the academic paper or document [with reference to Erbach 2006]".

This view was, however, not what Erbach (2006, 221)[149] claimed, which was: "a view that makes data sets first class objects requires certain changes in publication and documentation practice, for example the records for projects and publications in e-science information systems should be extended with new fields 'used dataset' and 'generated dataset' and the record for datasets with a field 'depends on dataset'." Erbach thus did *not* suggest that academic papers are being supplanted by "data".

Schöpfel et al. (2020) argued that although data in themselves are not documents, data represented in information systems are always kinds of data documents (e.g., "data repositories", "data studies" or "data sets", all distinguished from academic papers like journal articles, including "data papers"). The authors also found that data documents are little cited compared to the citations received by journal articles. This may indicate either that even in data-driven science the journal article is considered more important than the data set[150] or it may indicate that in major data-driven fields like climate research, data may have a huge importance but live relatively independent of the scientific literature. This provides thoughts of Price's (1970, 8) statement that science is "papyrocentric", but that technology is "papyrophobic", that scientific articles quote a lot of literature, but that technological magazines does not. The reason for this, Price suggested, is "If you want to make capital out technological discovery, the last thing you want is that open publication that [sic] determines intellectual privacy property for the sciences". In the sciences, on the other hand, recognition and citations are the capital, because these are the indications that the research is known and found relevant. We show below in Section 6 that many recent conceptualizations of science describe an increasing commercialization. Could it be that many areas of science based on big data (e.g., meteorological research) are more like technologies and therefore relatively "papyrophobic"? Or at least that they consider publications a less important kind of capital?[151]

5.4 Conclusion: epistemology as the basis for studying scholarly communication

We have formerly quoted Frické (2015, 651), who argued that in science, theory is needed in every turn. We have further argued that all domains of knowledge can be understood as the competition between different epistemologies driven by empiricist, rationalist, historicist, and pragmatic assumptions. The same is therefore also the case in the study of scholarly communication.

In the case of scholarly communication this means that every algorithm and system, every choice of terminology and every means of evaluation and synthetization research must be considered hypotheses in need of research. Also, the criteria from which all systems and processes must be constructed and evaluated (summarized by the concept "relevance", cf. Hjørland 2010) are by their nature deeply rooted in epistemology: What researchers in one paradigm find important, may be considered unimportant by researchers in another paradigm (see Hjørland 2002). Therefore, the study of scholarly communication cannot avoid philosophical issues, but has to uncover in what way different positions influences the information ecology and how users can be helped to navigate given this condition.

Endnotes

0. The first part of this trilogy is Hjorland, Birger. 2021. "Science, Part I: Basic Conceptions of Science and the Scientific Method." *Knowledge Organization* 48(7/8): 473-98.

71. To consider the study of science as part of natural sciences seems to be a contradiction in terms because science is a human and social activity. However, it has been suggested that bibliometrics is "the scientific method applied to science itself" (this view is probably widespread; the source here was an informal communication with a deceased bibliometrician, Finn Hjortgaard Christensen; see also Azoulay 2012).

72. As Berghofer and Wiltsche (2020, 29) wrote: "To begin with, although Heidegger's stance towards naturalism can generally be seen as somewhat ambiguous (cf. Rouse, 2005), he agrees with the Husserlian sentiment that the natural sciences are in principle incapable of investigating themselves in a philosophically satisfactory manner: 'The moment we talk 'about' a science and reflect upon it, all the means and methods of this science in which we are well versed fail us' (Heidegger 1967, 177). This is equally true of biology, where we 'cannot put biology under the microscope' (Heidegger 1967, 177), and of physics, which 'itself is no [sic!] a possible object of a physical experiment.'"

73. Gutting (2000, 463) found: "There is no doubt that philosophical accounts of scientific methodology aimed at telling scientists how to proceed with their work are today otiose. Such accounts made sense in the seventeenth century".

74. About unity of science, see, for example, Bertalanffy (1951) and Oppenheim and Putnam (1958).

75. About the disunity of science, see, for example Dupré (1993), Fodor (1974) and Galison and Stump (1996).

76. About natural versus human kinds, see, e.g., Khalidi (2013).

77. *Handbook of the Philosophy of Science* (Gabbay, Thagard, and Woods 2006ff) contains the following volumes:
    1. General Philosophy of Science: Focal Issues. Series Volume Editor: Theo Kuipers. Published 18th July 2007.
    2. Philosophy of Physics. Series Volume Editors: Jeremy Butterfield John Earman. Published 20th October 2006.
    3. Philosophy of Biology. Series Volume Editors: Mohan Matthen Christopher Stephens. Published 5th February 2007.
    4. Philosophy of Mathematics. Series Volume Editor: Andrew Irvine. Published 11th June 2009.
    5. Philosophy of Logic. Series Volume Editor: Dale Jacquette. Published 19th October 2006.
    6. Philosophy of Chemistry. Series Volume Editors: Robin Hendry Paul Needham Andrea Woody. Published 1st November 2011 (WorldCat has 2012).
    7. Philosophy of Statistics. Series Volume Editors: Prasanta S. Bandyopadhyay Malcolm R. Forster Published 25th May 2011
    8. Philosophy of Information. Series Editors: Dov M. Gabbay Paul Thagard John Woods. Published 10th November 2008.
    9. Philosophy of Technology and Engineering Sciences. Series Volume Editor: Anthonie Meijers Published 20th August 2009.
    10. Philosophy of Complex Systems. Series Volume Editor: Cliff Hooker. Published 4th May 2011
    11. Philosophy of Ecology. Series Volume Editors: Bryson Brown Kevin de Laplante Kent Peacock Published 28th April 2011.
    12. Philosophy of Psychology and Cognitive Science. Series Volume Editor: Paul Thagard. Published 23rd October 2006.
    13. Philosophy of Economics. Series Volume Editor: Uskali Mäki. Published 23rd April 2012.
    14. Philosophy of Linguistics. Series Volume Editors: Ruth Kempson Tim Fernando Nicholas Asher. Published 14th January 2012.
    15. Anthropology and Sociology. Series Volume Editors: Stephen Turner Mark Risjord. Published 27th October 2006.
    16. Philosophy of Medicine. Edited by Fred Gifford. Published 21st July 2011.

78. Among the textbooks on the philosophy of science Chalmers (1999) should be mentioned.

79. Baur (2005, vol. 3: 1078; italics in original) wrote: "The term *idealism* in its broadest sense denotes the philosophical position that ideas (mental or spiritual entities) are primary and lie at the very foundation of reality, knowledge and morality, while non-ideal entities (such as physical or material things) are secondary and perhaps

even illusory. Strands of idealistic thought can be found in ancient and medieval philosophy, but modern idealism begins in the wake of René Descartes (1596-1650), whose method of doubt problematized the relation of the mind (or spirit or ideas) to the material world and thus raised questions about how ideas 'inside' the mind can be known to interact with or correspond to any material, extended thing 'outside' the mind." It is important to realize that both rationalism (as mentioned, e.g., by Descartes) and empiricism (in particular George Berkley (1685-175) has strong idealist tendencies. This is the opposite of the popular belief that empiricism and positivism are materialist or realist positions.

80. While Thomas Kuhn emphasized how our ontologies are implied by our theories and paradigms, he nevertheless emphasized that we cannot freely invent arbitrary structures: "nature cannot be forced into an arbitrary set of conceptual boxes. On the contrary . . . the history of developed science shows that nature will not indefinitely be confined in any set which scientists have constructed so far" (Kuhn 1970, 263). The world provides "resistance" to our conceptualizations in the form of anomalies, i.e., situations in which it becomes clear that something is wrong with the structures given to the world by our concepts. In this way Kuhn's view may be interpreted as (pragmatic) realism, although he is often interpreted as antirealist.
81. Pragmatism is related to perspectivism, cf., Giere (2006) and Chang (2019). Teller (2019) discusses the relation between perspectivism and realism.
82. About pseudoscience see, for example, Hansson (2017).
83. About fringe science see, for example, Dutch (1982).
84. About occult science see Hanegraaff (1996).
85. On parapsychology see, for example, Hyman (2001); (the 2015 edition of the same Encyclopedia had no entry on this topic).
86. On pathological science see, for example, Langmuir and Hall (1989).
87. "Pre-paradigmatic science" was a concept developed by Kuhn (1962).
88. On protoscience see, for example, Brakel (2000, 160-161).
89. Pigliucci (2013) suggested "a family resemblance" clustering of sciences, which included concepts such as:
    - "established science" (including particle physics, climate science evolutionary biology and molecular biology);
    - "soft science" (including economics, psychology and sociology);
    - "proto-/quasi-sciences" (including search for extraterrestrial intelligence (SETI), string physics, evolutionary psychology, and scientific history);
    - "pseudoscience"* (including Intelligent Design, astrology and HIV denialism**).

    * Laudan (1984) used the label "pseudo-science" about "the strong programme" in the sociology of knowledge. This example indicates that what is considered "pseudoscience" depends on theoretical assumptions. **HIV denialism is a contradictory set of claims without foundation in science that the human immunodeficiency virus does not exist, or that it exists but is harmless, and that acquired immunodeficiency syndrome (AIDS) does not exist.
90. Among the criteria suggested by Mahner (2013) is looking at the people involved and asking questions such as:
    - do they form a research community, or are they just a loose collection of individuals doing their own thing?
    - is there an extensive mutual exchange of information, or is there just an authority figure passing on his doctrines to his followers?
    - is the given group of people free to research and publish whatever they want, or are they censored by the reigning ideology of the society they live in (e.g., Aryan physics, Lysenkoism)?
    - does the domain of study consist of concrete objects, or does it contain fuzzy "energies" or "vibrations," if not ghosts or other spiritual entities?
    - what are the philosophical background assumptions of the given field?
    - does its ontology presuppose a natural, causal, and lawful world only, or does it also admit supernatural entities or events?
91. Scientific taxonomy or classification in the sciences, such as, for example, biological classification of the species, has, contrary to classification of the sciences, met a growing interest among philosophers. Examples of contemporary philosophical contributions to scientific classification include Cooper (2017), Dupré (2001), Ereshefsky (2000), Richards (2016) and Wilkins and Ebach (2014) to mention just a few.
92. Miksa (1998, 34-5) mentioned: "Those who participated included physicists and other scientists such as André-Marie Ampère, Neil Arnott, Wilhelm Wundt, and Karl Person, and a variety of philosophers of all kinds, such as C.-H. Saint Simon, Auguste Comte, Herbert Spenser, William Whewell, Thomas Masaryk, and Frederich Engels. The list could easily be extended. Footnote: Listings with discussions can be found in the work of E. C. Richardson (1930), Robert Flint (1904), and E. I. Shamurin (1955-59)[Miksa cited Russian edition and the spelling Shamurin, here the German edition, Samurin (1964) is cited]",
93. Often in older literature, "the classification of knowledge" and "the classification of the sciences" is used as

synonyms, but today scientific knowledge should be considered one among more kinds of knowledge, and the two terms therefore should no longer be considered synonyms.

94. *The Cambridge History of Science* (Lindberg and Numbers, 2002-2020) begins chronologically with Ancient Mesopotamia and classical Greece and Rome, through the Medieval period, early modern Europe, and on through modern science and that approach continues on up to Vol. 4, after which the volumes split off into modern histories of different branches of science:
    Volume 1 (2018): Ancient Science, edited by Alexander Jones and Liba Taub;
    Volume 2 (2013): Medieval Science edited by David C. Lindberg and Michael H. Shank;
    Volume 3 (2006): Early Modern Science, edited by Katharine Park and Lorraine Daston;
    Volume 4 (2003): Eighteenth-Century Science, edited by Roy Porter;
    Volume 5 (2002): The Modern Physical and Mathematical Sciences, edited by Mary Jo Nye;
    Volume 6 (2009): The Modern Biological and Earth Sciences, edited by Peter J. Bowler and John V. Pickstone;
    Volume 7 (2003): The Modern Social Sciences, edited by Theodore M. Porter and Dorothy Ross;
    Volume 8 (2020): Modern Science in National, Transnational, and Global Context, edited by Hugh Richard Slotten, Ronald L. Numbers and David N. Livingstone.
95. *Companion to the History of Modern Science* (Olby, Cantor, Christie and Hodge 1990)
96. E.g., *The Oxford Handbook of the History of Medicine* (Jackson 2011)
97. E.g., *The Oxford Handbook of the History of Physics* (Buchwald and Fox 2013).
98. E.g., Danziger (1990): *Constructing the Subject: Historical Origins of Psychological Research*.
99. An example of a recent history of the humanities is Bod (2013).
100. For example, Richards (2016, 38) wrote: "The essentialism story is misleading at best. The history of biological classification, beginning with Aristotle and continuing through to Darwin, is not a simple history of essentialist thinking about biological classification. It isn't clear, for instance, that Aristotle or Linnaeus, the two arch essentialists in the essentialism story, were essentialists about biological taxa in the assumed way at all. Nonetheless, this essentialism story has been widely accepted, even in the face of contrary evidence from primary sources and the skepticism of various historians and philosophers".
101. Omodeo (2019, 2-3): "The socio-economic approach [...] emphasized the collective character of science, the continuity between knowledge, production, and technology, as well as the concrete and practical dimensions of science. It explicitly opposed the *bourgeois* celebration of individual genius and the idealistic understanding of science as a purely intellectual endeavor, the progress of which purportedly depends on exceptional minds motivated by disinterested curiosity. This glaring opposition between the supporters of the *externalist* comprehension of science and the *internalists,* those who sought for the purity of scientific reason, can only be understood against the background of the cultural-political clashes of the Cold War. I will delve into this paradigmatic example at length in the book."
102. Zammito (2007, 802; notes omitted) wrote: "The first compelling formulation of a 'new' sociology of knowledge emerged in the early 1970s. The intellectual energies for this 'sociology of scientific knowledge' (SSK) gathered primarily in Britain among thinkers inspired by Thomas Kuhn and Ludwig Wittgenstein, not trained in mainstream sociology. They proved willing to propose a most aggressive form of the 'social construction of reality' and to challenge the positivist tradition in its most sacred space: the privilege of natural scientific knowledge. They challenged not only the stablished field of sociology of science as developed by Merton and his followers, but also the Received View of the philosophy of science."
103. Downes (1998) wrote: "[C]onstructivists are accused of believing that scientists literally 'make the world', in the way some make houses or cars. This is probably not the best way to understand constructivism. Rather, constructivism requires only the weaker thesis that scientific knowledge is 'produced' primarily by scientists and only to a lesser extent determined by fixed structures in the world. This interprets constructivism as a thesis about our access to the world via scientific representations. For example, constructivists claim that the way we represent the structure of DNA is a result of many interrelated scientific practices and is not dictated by some ultimate underlying structure of reality. Constructivist research provides important tools for epistemologists specializing in the study of scientific knowledge."
104. Concerning a contemporary debate on biological classification and nomenclature see, for example, Sluys, Martens and Schram (2004); concerning debates on (a part of) the Periodical system see, for example, Vernon 2020.
105. Latour and Woolgar (1986) is the second edition of Latour and Woolgar (1979). In this edition the word "social" was omitted from the title, which indicate a theoretical shift in Latour's philosophy (an emphasis on non-social agents contribute to the construction of scientific facts). Hacking (1999, 39-40) discussed the redundancy of the term "social" and found: "But one

need not agree with his [Latour's] agenda in urging that we drop the 'social', except for an occasional emphasis".

106. Law (2009, 141) continued stating "it is *possible* to describe actor network theory in the abstract. I've just done so, and this is often done in textbooks. But this misses the point because it is not abstract but is grounded in empirical case studies. We can only understand the approach if we have a sense of those case studies and how these work in practice."
107. Among the issues taken up by Latour is the way scientist use bibliographical references in their papers. Here two theories are often described (1) "the normative theory of citing", according to which citations are a way to acknowledge intellectual debts and, thus, are mostly influenced by the perceived worth, as well as the cognitive, methodological, or topical content of the cited articles and (2) the social constructivist's "persuasion hypothesis", according to which references are used to gain credibility by association (cf., Nicolaisen 2007). In general, social constructivism has been negatively considered in bibliometrics, and Nicolaisen's paper claimed that the "persuasion hypothesis" is empirically rejected. However, two issues should be distinguished (1) that scientists behave in an unethical way, which Nicolaisen cite Latour for claiming and (2) that science is fundamentally influenced by interests (as described, e.g., by Barnes, Bloor, and Henry 1996), which influences their perception of the value of different documents. The first meaning of "social constructivism" is problematic if understood as a norm (but may be true as a problematic practice by some authors in some cultures), while the second seems to be a fruitful hypothesis (cf., Hjørland 2002).
108. Latour's subjectivizing of nature is criticized by Omodeo (2019, 18) with reference to Latour (2014, 3) who describes the Earth as a full-fledged actor with emotions.
109. Mirowski (2017, 447) wrote: "Latour believes politics consists of struggle without any hope of a transcendent court of appeal, which is why he is so attracted to figures like Hobbes, Walter Lippmann and Carl Schmitt. He explicitly eschews any appeal to Truth to ground politics, growing out of a conviction that constructivism dictates that truth is the outcome of struggle, but exhibits no special epistemic regularities or ontological stability. Because the portrait is one of unceasing agonistic strife, there is no program of reform, no conception of superior political institutional structures, no exemplar of political virtue to be found in his work. Science may be roiled with dispute and dissention from time to time, but the public just has to learn to roll with it. Most of all, there is nothing but ill-concealed contempt for those who strive to undertake science critique. The upshot of this Latourist project is that what exists in the way of science organization and scientific research is just fine the way it is."
110. "In *A Treatise concerning the Principles of Human Knowledge* (1710), George Berkeley argues that there is no external, material world; that houses, trees and the like are simply collections of "ideas"; and that it is God who produces "ideas" or "sensations" in our minds. This position has later been termed "subjective idealism".
111. Bloor (2015, 592-3) provides an explanation of why "The False Charge of Idealism [against the strong program]" has been so widespread. A main argument is that the contribution of "nature" versus "society" in scientific theories has often been understood as a zero-sum game – the more of the one ingredient meaning the less of the other. But this is wrong. It makes no sense to ask how much nature contributes and how much society contributes, just as it makes no sense to ask how much our visual experience is influenced by the object seen and how much by the eye, but it makes good sense to ask how the object and the eye influences our experience and how nature and society influences science.
112. Small (2016, 49) wrote: "As someone trained in science and the history of science, the constructivist view did not ring true. Perhaps I was stuck in my story-book version of science. In any event, the bibliometrics community ignored the new sociology and remained largely empirical and atheoretical." However, as argued in Hjørland (2016 and 2020), there cannot be such a thing as atheoretical science or atheoretical empirical research. The bibliometrics community probably ignored the new sociology because they found it theoretically unfruitful, but instead of attempting to develop an alternative theory, they seem to have chosen to an atheoretical approach. If it is correct that an atheoretical approach is impossible, it follows that they have chosen an approach the implications of which is not examined. It should be added, that in spite of this criticism, Small's contributions have been very important.
113. The pluralist view of knowledge is the view that some phenomena require multiple accounts. Due to the complexity of the world and our representational limitations various models may be necessary, perhaps even incompatible models (cf., Kellers, Longino and Waters 2006). It is related to the idea of scientific perspectivism (see Giere 2006, Teller 2019 and Chang 2019).
114. Bloor (2007, 216) wrote: "[I]f a sociological model is on the right lines then all individualist and subjectivist accounts of concept application are clearly in trouble. Wittgenstein put his finger on the source of their weakness. Because, on these theories, there is no external standard outside the individual, then whatever seems right to the individual is right. But that, said Wittgen-

stein, means that one cannot talk about right in this case at all. There has got to be an external standard of right or wrong concept application and that standard is a social one".
115. Other parts of information science and knowledge organization is about broader cultural mediation, such as public libraries, but these parts are not unrelated to the scientific parts.
116. About the importance of epistemology for information retrieval and searching see for example Hjørland (2011a), Hjørland (2016) and Hjørland (2021).
117. An important book on philosophical issues related to metadata and data classification is Leonelli (2016), which is considering the field of (data-centric) biology. See also Ibekwe-SanJuan and Bowker (2017), Hjørland (2011b), Hjørland (2013b) and Hjørland (2021).
118. Many of these specific elements sometimes claim to form independent sciences; for example, the study of journals has been called "journalogy", cf., Lock (1989). The disciplinary boundaries are not well established in these fields.
119. About academic genres see, for example, Swales (2004).
120. About books, for example, Nunberg (1996).
121. About bibliographies see, for example, Krummel (2017).
122. About encyclopedias see, for example, Bergenholtz, Nielsen and Tarp (2009), Collison (1964), Fozooni (2012), König and Woolf (2013).
123. About academic journals see, for example, Cole (2000), Cope and Phillips (2014), Kronick (1962), Lindsey (1978), Lock (1989) and Morris et al. (2013).
124. Concerning the study of journal articles see also the references mentioned in Section 4.4.5 Genre studies and composition studies.
125. About patents see, for example, White (2017).
126. About systematic reviews see, for example, Hammersley (2006).
127. On standards, see, for example, Ransom et al. (2017).
128. On publishing see, for example Baensch (2010) and Bhaskar (2013).
129. On editing see, for example, Ginna (2017) and Butcher (2006).
130. On peer-review see, for example, Lee et al. (2013).
131. On open access see, for example, Albert (2006), Björk (2012).
132. On predatory journals see, for example, Yeates (2017).
133. About grey literature see, for example, Schöpfel and Farace (2010a+b).
134. About bibliometrics see, for example, Bellis (2009).
135. About altmetrics see, for example, Thelwall (2021).
136. About research evaluation see, for example, Moed (2005).
137. About ranking of scientists and scientific journals see, for example, Andersen (2000).
138. About the h-index see, for example, Bornmann, Mutz and Daniel (2008).
139. About research libraries see, for example, Kennedy (2018).
140. About archival science see, for example, Duranti and Franks (2015).
141. About museology see, for example, Vergo (1989).
142. About institutional repositories see, for example, Lynch (2003).
143. Concerning memory institutions see, for example, Hjørland (2000).
144. About research or knowledge syntheses see, for example, Cooper and Hedges (2009).
145. About evaluation of information sources see, for example, Hjørland (2012) and Bailin and Grafstein (2010).
146. On the developing concept of e-research see Jeffreys (2010).
147. Gahegan (2020, 1-2; italics in original): "As described by Hey *et al*. (2009), science has evolved three main paradigms thus far. The first is *Experimentation*, characterized by observation and measurement. A good example is determining the relationship between the length of string and periodicity of a pendulum. Experimentation became possible with the invention of reliable ways to measure physical quantities, such as time, weight and length. Accurate measurement allowed observations to be standardized and compared, and so generalizations could be sought. The second paradigm is *Analytical Theory*, which searches for the theory that might explain some system. Note that the system does not need to be measurable, or even real, it can be hypothesized, so does not necessarily require data. Einstein's famous equations describing special relativity fall into this category, and were motivated by thought experiments. The data to establish the validity of special relativity was not even available until after his death. The third paradigm is *Numerical Simulation*, characterized by the extensive application of computing power to model the physical world, often in great detail, and using forecasting techniques to predict future states. A good example is climate forecasting, combining simulation, historical data and prediction methods. Another is the search for nanomaterials with interesting properties, using computational chemistry. Many newer branches of science now are home for research communities who work entirely on such *in-silico* experiments. The Fourth Paradigm, as proposed by Hey et al. (2009), is *Data-Driven Science*, and it goes further by suggesting that data itself can drive the discovery of new knowledge. Of all the current hype surrounding Big Data, this is perhaps the most intriguing aspect, with some authors even heralding: '*The*

end of theory' (Anderson, 2008). The argument goes that, with the availability of very rich data that comprehensively describes a given situation, it becomes possible to discover theoretically grounded explanations that make sense of, or explain, our observations."

148. This view is supported by Wilbanks (2009, 210-211), who (in the same book) suggested: "In this view, data is not a 'fourth paradigm' but a 'fourth network layer' (atop Ethernet, TCP/IP, and the Web [reference omitted]) that interoperates, top to bottom, with the other layers. This view seems to capture the nature of the scientific method a little better than the concept of the paradigm shift, with its destructive nature. Data is the result of incremental advances in empiricism-serving technology. It informs theory, it drives and validates simulations, and it is served best by two-way, standard communication with those layers of the knowledge network. To state it baldly, the paradigm that needs destruction is the idea that we as scientists exist as un-networked individuals. Now, if this metaphor is acceptable, it holds two lessons for us as we contemplate network design for scholarly communication at the data-intensive layer."

149. Erbach (2006) differentiated three views about research information:
   - The document-centric view: "The traditional network analysis method in library science is bibliometrics, which studies authorship (author/publication) and citation (publication/publication) relationships to determine derived relations such as co-citation or impact factors. This approach will be referred to as the document-centric view of research information."
   - The person-centric view: "In the context of research information, network analysis can be applied to determine relationships among people, based on their joint projects, organisations they work for, or data sets they work with. This is a person-centric view."
   - The data-centric view: "Likewise, it [network analysis] can be used to analyse relationships among data sets based on the people who work with them, the publications that reference them, and the data analysis methods applied to them. This will be referred to as a data-centric view of research information"

   Consider, however, that the data-centric view is identical with the document-centric view if data sets are considered documents (as discussed by Schöpfel et al. 2020); to some degree this is also true for Erbach's person-centric view: Bibliometrics, for example, shows relations between authors.

150. This view seems supported by the conclusion reached by Lynch (2009, 183): "Further, I believe that in the practice of data-intensive science, one set of data will, over time, figure more prominently, persistently, and ubiquitously in scientific work: the scientific record itself".

151. Ravetz and Funtowicz (2015, 254) mentioned "disentangling the publication" as one of the immediate problems facing the scientific communities.

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