

ARTICLE

Sjoerd Hofstra's foray into the sociology of knowledge and science: Why a promising prewar programme ultimately failed

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Abstract

In 1937, the Dutch ethnologist and sociologist Sjoerd Hofstra sketched a programme for a new sociology of knowledge and science in which he combined insights from Mannheim's sociology of knowledge, the anthropological study of "primitive" thought, and the Marxist-inspired views of leftist British natural scientists on the social role of science. Hofstra's programme looked promising at first but ultimately failed to become established. In this essay I will try to account for this failure by comparing Hofstra's programme with Robert K. Merton's contributions to the sociology of knowledge and the sociology of science. The essay also offers an explanation for the long delay of almost 40 years before the sociology of knowledge systematically began to study the social genesis of scientific knowledge.

Keywords

Sjoerd Hofstra, Robert K. Merton, sociology of knowledge, sociology of science, social anthropology

INTRODUCTION

The sociology of knowledge is more than a century old. It can be dated back to the 1920s when Max Scheler and Karl Mannheim staked out the field of *Wissenssoziologie* in Germany, or even earlier when Emile Durkheim and his French pupils launched the *sociologie de la connaissance*. In subsequent decades the new discipline would usually take the nominal object of its analysis, "knowledge", in a very broad sense, so that religious representations, philosophical worldviews, political ideologies, and common-sense convictions of everyday life all fell within its scope. The general aim was to inquire into the "existential determination" or social genesis of such ideas and views. But there was one type of knowledge that was left untouched by this endeavour: scientific knowledge, that is, as here understood, knowledge obtained by the exact and natural sciences. For epistemological reasons it was thought that scientific knowledge was not amenable to social explanation and thus beyond the jurisdiction of the new sociological speciality. The irony, therefore, was that the sociology of knowledge was about almost everything except what might be considered knowledge in the most preeminent sense. Hence the complaint that the sociology of knowledge

“failed to live up to its name” (Barnes and Edge 1982: 65). The *sociology of science*, which was to be developed by Robert K. Merton (1910-2003) in the postwar decades, would by no means remedy this deficit. It confined itself to the study of science as a social sub-system in which practitioners were competing for credit and recognition but excluded the content of knowledge from its explanandum. It was only in the 1970s that any remaining epistemological scruples against an investigation of the social genesis of scientific knowledge were overcome and a vigorous Strong Programme was launched, which finally inaugurated a genuine sociology of scientific knowledge. In retrospect, the question has been raised why it took so long for the sociology of knowledge to realize, as it were, its true mission (Kuklick 1983). Why the decades-long apparent hesitation? Was it simply a matter of “lack of nerve”, as one of the founders of the Strong Programme, David Bloor, suggested? (Bloor 1991 [1976]: 11).

In light of this historical conundrum, a prewar contribution by the Dutch sociologist and ethnologist, Sjoerd Hofstra (1898-1983), deserves special interest. In 1937, he published an interesting booklet titled (in Dutch) *The social aspects of knowledge and science* (Hofstra 1937). In it he sketched the outlines of a research programme for a new sociological speciality that could be designated as the *sociology of knowledge and science*. By using this designation, I want to indicate that the speciality Hofstra had in mind differed from the existing sociology of knowledge (especially Mannheim's *Wissenssoziologie*), on the one hand, and from the postwar (Mertonian) sociology of science, on the other. Hofstra's aim was to bring the sociology of knowledge and the sociology of science (in the customary sense of the study of the social relations among the practitioners of science) together under a single theoretical umbrella. His programmatic proposal was balanced, well-considered and systematic in intent. Yet looking back in 1978, Hofstra had to admit with regret that his promising first foray into this new sociological speciality had ultimately come to nothing and that the entire field of the sociology of knowledge and science had hardly been cultivated in the Netherlands (Hofstra 1978). In this article I want to investigate why Hofstra's bold first step looked so promising and why this promise ultimately remained largely unfulfilled. I will put the vicissitudes of his remarkable initiative in a broader historical perspective by also paying attention, for comparative purposes, to the academic career of the American sociologist Robert K. Merton, who in the 1930s made his debut with a paper, *Science, Technology and Society in Seventeenth Century England* (Merton 1938), a study that was arguably undertaken from a sociology of knowledge perspective, and published in 1970 as a monograph. After the Second World War, he would however, abandon this approach and develop a new paradigm in the sociology of science, which became influential and remained the dominant approach well into the 1970s. This so-called Mertonian paradigm put the content of scientific knowledge beyond its purview.

Through a comparative analysis of Hofstra's and Merton's contributions, we can also shed light on the role of historically contingent factors that help explain the emergence (or non-emergence) of new specialities in social science. Thanks to the *benefit of hindsight*, we may be better placed than Hofstra himself in 1978 to more fully assess the new developments in the sociology of knowledge and science that could already be discerned during the 1970s.

THE EARLY DISPUTE ON THE SOCIOLOGY OF KNOWLEDGE

Under the rubric of *Wissenssoziologie*, the sociology of knowledge was definitively put on the map in the 1920s by Max Scheler and Karl Mannheim. The central theme of the new speciality was what Mannheim called the *Seinsverbundenheit des Denkens* or the “existential determination of thought” (Mannheim 1949 [1936]: 239), that is, the dependency of knowledge in its various guises on the social (“existential”) backgrounds of its bearers and protagonists. In 1859, Karl Marx had already

preluded the birth of the sociology of knowledge by famously declaring, "It is not the consciousness of men that determines their existence, but their social existence that determines their consciousness" (Marx 1904 [1859], 11).

While from 1929 on, a fierce dispute raged in Germany on the legitimacy and pretensions of the sociology of knowledge (Meja and Stehr 1990), the battle was fought less vehemently in the Netherlands. Nevertheless, during the years 1936-1938, a genuine polemic was conducted on the pages of the journal *Mens en Maatschappij* (Man and Society) between the legal philosopher Johan Jacob von Schmid (1895-1943) and the communist neurologist Gerrit Willem Kastein (1910-1943) about the significance and main thrust of the new speciality (Schmid 1936; Kastein 1938). This debate provides some clues about the place taken by, or assigned to, the new subdiscipline within the wider panorama of academic disciplines and socio-cultural currents. It should be noted in this connection that the parent discipline of sociology was not fully established at the time. In his contribution, Von Schmid introduced the "sociology of thought", roughly following Mannheim's views. Rather surprising for the modern reader, however, is that he conceived of this sociological speciality as a part of philosophy and characterized it more particularly as a form of life-philosophy:

The sociology of thought, then, is a philosophy, which views reason as a product of social life [...] It is a philosophy of life, which lays claim to a higher and more general validity than the rational philosophy (Schmid 1936: 259).

Von Schmid held that the old rationalist philosophy, which took the autonomy of reason and the timeless character of its insights for granted, was finished and had to make way for a new approach. In his critical response to Von Schmid, Kastein contested precisely his view of the sociology of knowledge as a form of life-philosophy (he himself advocated a Marxist oriented sociology of knowledge, although he referred with approval to Mannheim's well-known analysis of conservative thought). Kastein did allow that the "irrational" aspects of life can be an object of rational scientific inquiry but emphasized that affective factors should not themselves serve as an "organon" of scientific analysis. His appreciation of the philosophy of life was rather negative; he took it as a symptom of decay, pessimism, and a sense of decline: "The philosophy of life is therefore no sign of a flourishing life" (Kastein 1938: 94). To substantiate this thesis, he commented (by way of example) on Heidegger's thought, an indication that in the 1930s the latter's philosophy was often seen as a variety of the philosophy of life (Heidegger had not been mentioned by Von Schmid, however).

Across the North Sea, prominent left wing natural scientists like Hyman Levy, Lancelot Hogben, J.B.S. Haldane, John Desmond Bernal, Joseph Needham, Patrick Blackett, and Julian Huxley had meanwhile become deeply interested in the role of science in society and especially in the limitations and restrictions for scientific research that appeared to arise from capitalism. This leftist current would become known as the Social Relations of Science movement (Werskey 1988 [1978]). To support their critical reflection, the leftist scientists – especially the radicals among them (communists and left wing socialists) – did not seek guidance from Mannheim's sociology of knowledge (even though Mannheim himself had fled to Great Britain after the Nazi takeover of Germany in 1933), but harked back to Marxism, which paid more attention to the social function of the natural sciences. The Marxist view on the relation between the economic base and the ideological superstructure can indeed be seen as an embryonic form of the sociology of knowledge. An important source of inspiration for the leftist British scientists was the contribution by the Russian physicist and Marxist, Boris Hessen, to an international congress on the history of science and technology held in London in 1931, titled 'The social and economic roots of Newton's *Principia*' (Hessen 1971 [1931]). Hessen put Newton's scientific work in the socio-economic and political context of 17th-century

England. His theories, according to Hessen, had been informed by the economic and technical problems of the mercantile capitalism of that age and by the ideological and political struggles of the rising bourgeoisie. For most attendants at the congress, such an analysis virtually amounted to lese-majesty vis-à-vis Newton's genius, but to the leftist scientists it was an eye-opener (Needham 1971). In subsequent years there would be no dearth of critical studies in the UK about the role of science in society.

Hessen's analysis was also a source of inspiration for the young American sociologist Robert K. Merton in writing his thesis on *Science, Technology and Society in Seventeenth-Century England*, which, as mentioned above, was subsequently published as a very extensive article in the science history journal *Osiris* (Merton 1938). Yet another source of inspiration for Merton was Max Weber's well-known monograph *The Protestant Ethic and the Spirit of Capitalism* (Weber 2001 [1904-5]). Merton argued that the new, experimentally oriented science of the 17th century was religiously sanctioned by the prevalent Puritan ethos (a variant of the so-called Weber thesis which would become known as the Merton thesis) and directed its attention in no small measure to the solution of technical problems thrown up by economic life and the demands of warfare. In this way he subtly combined a "materialist" angle (stressing the role of the economic base) with an "idealist" angle (stressing the religious factor). While his dissertation does not lack occasional references to Scheler and Mannheim, Merton had not explicitly presented it as a specimen of the sociology of knowledge – though it surely can be classified as such. In 1937, however, at the request of his Harvard mentor George Sarton, he published a critical review article on the new sociological speciality in the science history journal *Isis* (Merton 1937). In this, he argued Mannheim had entangled himself in various contradictions and aroused sterile philosophical controversies precisely because he adamantly tried to draw epistemological conclusions from his findings in the sociology of knowledge. Only by resolutely shutting off this avenue and strictly maintaining the distinction between the social genesis and the validity of judgments, Merton held, was it possible to develop this speciality as an empirical research programme. In a later criticism, Merton asserted Mannheim had simply *assumed*, and wrongly so, that the natural sciences (in contrast to the social sciences and the humanities) are completely immune to social, "extra-theoretical", influences (Merton 1968 [1940]: 552). In this connection, he referred to his own dissertation and to Hessen's work to combat this view. Nonetheless, in Merton's conception, the influence of social factors was still relatively limited; they were held to affect the choice of problems but not the nature of the solutions found.

HOFSTRA'S SKETCH OF A SOCIOLOGY OF KNOWLEDGE AND SCIENCE

For Hofstra, the *Seinsverbundenheit des Denkens*, or the general relation between socio-cultural background factors and the content of knowledge, was an important theme for the sociology of knowledge and of science. Arguably, a sociology of knowledge à la Mannheim had to become a key component of the new speciality, but to make it fit for purpose the component had to be revised. Hofstra commented that Mannheim had insufficiently specified the nature of the relationship between social factors and knowledge (Hofstra 1937: 49-50) – a similar criticism as would later be brought forward by Merton, a sociologist who at the time was probably still unknown to Hofstra (Merton 1968 [1940]: 552). Hofstra furthermore stripped Mannheim's sociology of knowledge of its more controversial aspects by putting less emphasis on the criticism of ideology and studiously avoiding the epistemological reflections in which Mannheim had indulged. Hofstra also opined that the traditional distinction between the social genesis and the validity of judgments should be strictly maintained to prevent the new speciality from entering controversial channels. In this respect there was a remarkable similarity with Merton's criticism that was published in the same year (Merton

1937). By rigorously adhering to the distinction between genesis and validity, the competence of the sociology of knowledge could be more narrowly circumscribed (Hofstra 1937: 45-46).

The revised version of Mannheim's sociology of knowledge is just one component of the broader sociology of knowledge and science that Hofstra had in mind. The two other components are the insights into the thought of "primitive" peoples as developed by social anthropology, on the one hand, and views on the role of science in society and the social responsibility of scientists as developed by the British Social Relations of Science movement. Hofstra held that these three components, despite the apparent divergence, could be forged together into a meaningful synthesis: "After all, all three, each in its own way, occupy themselves with the social aspects of knowledge and science" (Hofstra 1937: 3).

It is remarkable that Hofstra shared his curiosity about the leftist British scientists with Merton, who also developed an active interest in the Social Relations of Science movement (Mendelsohn 1989; Turner 2007; Enebakk 2007; Somsen 2001). Hofstra tried to fit the materials derived from those scientists into his own sociology of knowledge and science, although he failed to mention the work of their Russian inspirator, Boris Hessen. Prominent among the sources cited by Hofstra was a survey on British science published by Julian Huxley in 1934 (Huxley 1934) and a volume of essays published in 1935 under the revealing title *The Frustration of Science* (Hall et al. 1935). All this publicist activity would culminate in 1939 – too late to be included in Hofstra's analysis – in the classic study of the physicist and avowed communist John Desmond Bernal, *The Social Function of Science* (Bernal 1964 [1939]). The progressive scientists were united in the conviction that as a group they had too little influence on political decision-making. They fiercely opposed the cuts on research expenditures that were carried out in the 1930s. They were also outraged by the suggestion that the preceding decades might have spent too much on science (as was characteristically expressed in Aldous Huxley's dystopian novel *Brave New World*). The scientists would not let their faith in progress be so readily taken away from them. When applications of scientific findings led to unemployment, it was not the science that was to blame, but rather an irrational political and economic system. At any rate more effective and more comprehensive planning was needed – and wasn't planning after all, as Julian Huxley said, "an attempt to apply the scientific method all round" (Huxley 1934: 149). So, the problem was not, the progressive scientists argued, that too much science was being done and applied, but rather too little (in this regard, the biologist Julian disagreed with his literary brother Aldous). Hence the emphasis on the numerous promising scientific developments and potential beneficial applications which could all be realized if only they were not blocked by impediments that the existing system put in their way (such as the profit motive, vested monopolistic interests, political resistance, misplaced austerity, patents, secrecy, etcetera). Thus, the progressive scientists were convinced that their cherished science was being thwarted. Hofstra did not fail to put his finger on the tacit assumption in this widely shared complaint about the "frustration" of science: "The authors are [...] inclined to implicitly equate the possibility of applying scientific results with the desirability of such application [...]" (Hofstra 1937: 64).

The third component in Hofstra's synthesis consisted of anthropological insights into the character of "primitive" thought. It is not surprising that he considered these to be relevant to a sociology of knowledge and science. After all, the French school around Emile Durkheim had developed their *sociologie de la connaissance* on the basis of all kinds of ethnographic materials. Hofstra could also fall back on his own ethnological expertise. He had defended an academic dissertation in African ethnology in 1933 and worked from 1934 to 1937 as a research assistant to the famous anthropologist Bronislaw Malinowski. In the first part of his brochure, Hofstra went out of his way to refute the then

well-known thesis of the French ethnologist Lucien Lévy-Bruhl on the so-called “pre-logical” mentality of indigenous peoples. He fiercely contested the idea that natives, in all their daily activities, would be captive to a mythical haze that would make them lose sight of simple natural regularities and causal relations. Following his mentor (Malinowski 1955 [1925]: 87), Hofstra argued that indigenous peoples dispose of a considerable amount of empirically adequate knowledge; if it were otherwise, they simply would not have survived (Hofstra 1937: 25-26). He appealed to the results of his own anthropological research among the Mende (or Mendi) in Sierra Leone, who as an agricultural people knew quite a lot about, for example, rice and oil palms (see the reports posthumously collected in Hofstra 2014). Hofstra thus emphasized the continuity between the “primitive” and the modern mentality.

HOW VIABLE WAS HOFSTRA'S PROGRAMME?

Hofstra's sketch for a new sociology of knowledge and science may have looked promising in the 1930s, but it was mainly a programmatic proposal. Was there any chance at the time for the programme to be fruitfully elaborated, or was it hardly viable from the outset?

An indication that the programme may have lacked sufficient coherence is the fact that the expected contribution from anthropology stands largely isolated from the rest. The argument yields no more than the conclusion that “positive knowledge” already exists in indigenous cultures (Hofstra 1937: 34). Hofstra's opposition against Lévy-Bruhl's ideas about a “pre-logical” mentality may have been fully justified, but he nonetheless failed to use the opportunity to apply his ethnological expertise for a sociological scrutiny of *modern* forms of thought. That such use is possible in principle is suggested by the intriguing thesis of Emile Durkheim and Marcel Mauss that the natural classification or the cosmology of an indigenous tribe reflects their social organization (Durkheim and Mauss 1963 [1903]). Classifying is an elementary and ubiquitous cognitive activity which bears a communal character and is therefore, arguably, subject to social influences. It calls, as it were, for an analysis from a sociology of knowledge angle. Where to draw the boundaries between various sorts of things and how to deal with recalcitrant cases (“monsters”) are extremely interesting questions on which the Durkheimian-inspired anthropologist Mary Douglas has shed an illuminating light in her book *Purity and Danger* (1966) and in her later works. Her group-grid theory on the relationship between group properties and typical methodological styles (monster assimilation, monster adaptation, monster embracement and monster expulsion) has been applied in modern science studies in various areas (Caneva 1981; Boon 1979; Bloor 1978). But we do not need to refer to this rather late development. In the 1930s, the Jewish-Polish serologist Ludwik Fleck felt inspired by Durkheimian anthropology to develop his own sociological view of science, using a case study on the Wassermann test derived from his own medical specialism: *Genesis and Development of a Scientific Fact* (Fleck 1979 [1935]). Even so, Fleck reproached Durkheim and his pupils to have failed to extrapolate their sociological approach to contemporary scientific knowledge (Fleck 1979 [1935]: 47). It is for this reason that their *sociologie de la connaissance* did not develop into a genuine sociology of scientific knowledge. But Fleck's pioneering work went largely unnoticed in the 1930s.

Hofstra had rightly criticized Mannheim for insufficiently specifying the relationship between social factors and the content of knowledge (Hofstra 1937: 49-50). What Hofstra's own programmatic proposal however, sorely lacked, were *exemplars* or suitable didactic illustrations (an essential part of a flourishing paradigm, according to Kuhn), showing in what ways socio-cultural factors can be related to (the content of) scientific knowledge. There was of course Mannheim's classic study on conservatism (Mannheim 1984 [1925]), but this example relates to social and political thought. Hofstra explicitly declined to limit the idea of “existential determination” to political ideas and

worldviews (Hofstra 1937: 47), but he did not mention any particular field of science that would be a suitable object for sociological scrutiny. Biology might have been a promising candidate. An obvious example was the often-alleged influence of the capitalist economy as a social background factor in the formulation of the theory of evolution, postulated among others by Karl Marx, Charles Saunders Peirce and John Maynard Smith (Maynard Smith 1958: 33).

For a sociology of knowledge and science, it thus seemed that an extensive domain of study lay open to investigate various possible relations between social and political positions and substantive biological views, as held, for example, in the nature-nurture controversy (Pastore 1949). Naturally, these investigations would also require critical attention to the question of how such relations can be plausibly established and how the direction of causality can be determined. It is regrettable that in his brochure Hofstra did not mention anything substantial about these issues. Nor were other Dutch sociologists at the time very eager to turn their critical gaze to developments in the natural sciences. Until the late 1970s, they shied away from applying a sociology of knowledge perspective to other disciplines like economics or psychology, let alone the natural sciences.

THE ROLE OF NATURAL SCIENTISTS

As a matter of fact, however, many natural scientists were interested in the questions of concern to a sociology of knowledge and science. We have seen that both Hofstra and Merton made deliberate use of the insights obtained by natural scientists allied with the Social Relations of Science movement. These scientists had their own reasons to take an interest in questions related to the role of science in society and they tackled these questions with improvised conceptual means of their own making. After all, they could hardly have waited until the sociologists would be ready to offer more thorough and empirically founded sociological theories about such issues. This also means that the development of a sociology of knowledge and science is not an internal affair for the parent discipline of sociology, the more so because the speciality occupies the periphery of the discipline. In other words, the task is too difficult and too large to be carried out only by sociologists. In an otherwise laudatory review of Bernal's *The Social Function of Science* for the American Journal of Sociology, Merton fully admitted this fact:

The book contributes a great body of substantive materials in a field which has long needed cultivation. It would be ungracious to suggest that the physicist-author has failed to interpret these materials sociologically or has done so in an excessively simplified fashion – *ce n'est pas son métier*. This task may rather be conceived as a challenge to the sociologist of science who has all too often divorced theoretical speculation from empirical investigation (Merton 1941: 623).

After the Second World War, in 1952, Merton wrote a preface to Bernard Barber's book *Science and the Social Order*. From the select bibliography it transpired that more than half of the cited literature had been written by natural scientists, a quarter by historians and philosophers of science and only a small part by sociologists. From all this Merton drew a clear conclusion about the track record of the sociological speciality:

[...] the sociology of science has been nudged into being, not so much by sociologists as by occasional physical and biological scientists who occupy their leisure hours by working on this subject. [...] [N]ot many persons cultivate the field altogether and those that do are for the most part physical and biological, rather than social, scientists (Merton 1973 [1952]: 212).

From the point of view of an effective career strategy, it would not have made sense for a young social scientist just before or just after the war to devote all his or her efforts to the further elaboration of a sociological speciality when the parent discipline itself could hardly lay claim to an established academic status. This is reflected in the careers of both Merton and Hofstra. Both had done pioneering work in the field before the war, but in order to gain a solid academic position they had to engage with a wide range of theoretical and empirical issues in general sociology. It was only later in his career that opportunities opened up for Merton to return to his old love. Thus the famous "Mr Sociology" became the prominent "sociologist of science". We can read this transformation in the two collections *Social Theory and Social Structure* (Merton 1968) and *The Sociology of Science* (Merton 1973). We should note, however, that Merton had not been able to save all of his youthful legacy. In fact, he had abandoned his old interest in the sociology of knowledge by excluding the content of scientific knowledge from the scope of his new "sociology of science". The new Mertonian paradigm focused instead on the normative ethos of science and the operation of its reward system (recognition and reputation). Hofstra had (or created) fewer opportunities to revisit his old interest. He was preoccupied with theoretical problems in general sociology (he published, among other things, on the notions of function and normality), his contributions to African ethnology, and his directorship of the Dutch Society for the Protection of Animals (Noordegraaf 2017).

Apart from career pressures influencing Merton's and Hofstra's choices of problems, the general Cold War atmosphere in the early postwar period may have also made it less attractive to further pursue the older lines of inquiry into the social genesis of scientific knowledge. Karl Popper's influential attack upon the "enemies of the Open Society" (mainly but not only targeting Marxists) had included a forceful criticism of the sociology of knowledge (Popper 1974 [1945], 212-223), although his biographer later deemed his reading of Mannheim's work "ungenerous" (Hacohen 2000, 514 note). The notorious "Lysenko affair" of 1948, when Soviet leader Stalin suppressed the entire field of Mendelian genetics as "bourgeois" science, graphically illustrated the dangers of a crude Marxist reduction of scientific thought to socio-economic factors (Huxley 1949). By taking appropriate methodological precautions, the sociology of knowledge could arguably distance itself from unwelcome associations with such extreme views, but it might still be perceived as dangerously close to this anti-scientific approach.

HOFSTRA'S RETROSPECT IN 1978

Looking back in 1978, Hofstra noted that, in contrast to the sociology of religion, "the sociology of science, since Hofstra's first beginnings in 1937, has hardly been systematically cultivated any further in our country (for other countries I remind the reader of Merton's studies)" (Hofstra 1978: 34-35). And he continued: "It is true that also in our country contributions to relevant parts of such a sociology have appeared, such as about the responsibility of science. A renewed interest in the problem of values can also be mentioned here (Köbben) and more recently in the sociology of knowledge (Zijderveld) as well" (ibid.: 35). The tenor of Hofstra's brief remarks is rather cryptic. In the first place, he now spoke about the sociology of science, while his own initiative in 1937 covered the broader area of the sociology of knowledge and science. One also wonders what his reference to Merton's studies was meant to imply. Did Hofstra want to suggest that thanks to the sociology of science developed by the latter it would no longer be necessary to develop this speciality any further in the Netherlands? But Merton's sociology of science, as we have seen, involved the abandonment of lines of inquiry into social influences on the content of knowledge, which had also been part of Hofstra's original programme. In this respect the old programme remained unrealized. The reference to Anton Zijderveld's so-called "sociology of knowledge" is also rather curious. Zijderveld

(1937-2022) conceived of the sociology of knowledge not as a special subdiscipline of sociology, but as a particular way of treating general sociology (Zijderveld 1974). In this regard he followed in the footsteps of Peter Berger and Thomas Luckmann, who in their well-known book *The Social Construction of Reality* (1966) advocated a redefinition of the object of the sociology of knowledge:

[...] the sociology of knowledge must first of all concern itself with what people 'know' as 'reality' in their everyday, non- or pre-theoretical lives. In other words, common-sense 'knowledge' rather than 'ideas' must be the central focus for the sociology of knowledge. It is precisely this 'knowledge' that constitutes the fabric of meanings without which no society could exist (Berger and Luckmann 1971 [1966]: 27).

The paradoxical effect of this broad redefinition is that it would leave the original problem of *Wissenssoziologie* completely aside. Even if this effect was not logically inevitable, in practice it did follow from widening the scope of the discipline. Berger and Luckmann, and Zijderveld in their footsteps, were so much absorbed by investigating the common-sense 'knowledge' of everyday social reality acquired and possessed by ordinary members of society, that in the end they lost all interest in the formation of knowledge within the specialized domain of science. In the prewar period Hofstra would have resisted such a dilution of the sociology of knowledge, as he explicitly confined the object of analysis to "knowledge, which has a more or less expressly rational character and can be reckoned to belong to the manifestations of intellectual life" (Hofstra 1937: 5).

NEW INITIATIVES FOR A SOCIOLOGY OF SCIENTIFIC KNOWLEDGE

It is striking that in his retrospect from 1978 Hofstra passed in silence over the radically new initiatives which had arisen during the 1970s in the wake of Thomas Kuhn's paradigm theory (Kuhn 1970 [1962]) and increasing criticism of the all too modest ambitions of postwar Mertonian sociology of science. After the long dominance of the latter school, according to Dutch sociologist Bé Cornelis van Houten in 1977, the outlines of a new sociology of science gradually became visible:

Within sociology a rapprochement is approaching between the sociology of science and the sociology of knowledge [...] The development of the sociology of science as a discipline was based on the – meanwhile problematic – presupposition that scientific knowledge is immune to societal influences (Houten 1976/1977: 234).

In Edinburgh, Barry Barnes and David Bloor had already laid the foundations for the so-called Strong Programme in the sociology of science, which amounted to the precept that this discipline had to be conducted as a sociology of knowledge. Barnes had advertised a book published in 1974 as "a study in the sociology of scientific knowledge" (Barnes 1974), while Bloor formulated in 1976 the radical tenets of this new approach, the famous principles of symmetry and impartiality, causality and reflexivity (Bloor 1991 [1976]). Somewhat later the acronym SSK for "sociology of scientific knowledge" came into vogue. Similar tendencies occurred in the history of science (Shapin 1982). Ethnologically inspired students of science like Bruno Latour gathered sufficient courage to step inside laboratories with the aim to examine the esoteric performances of lab researchers as if they were the mysterious rituals of some native tribe (Latour and Woolgar 1979). At the end of the 1970s, the half-forgotten pioneering work of Ludwik Fleck from 1935 on thought styles and thought collectives became available as an English translation (Fleck 1979 [1935]) and a German re-edition (Fleck 1980 [1935]). In the Netherlands an important turning point was marked by the appearance in 1979 of a special issue of the journal *Kennis en Methode* (Knowledge and Method), titled (in Dutch) "Society or method", which was devoted to the debate on internalism and externalism. Contributors to this issue included not only philosophers and sociologists, but also several natural

scientists who were affiliated with the so-called Science and Society movement, a kind of reincarnation of the old Social Relations of Science movement. Another sign of growing interest in the sociology of knowledge and science on the part of progressive scientists was the book *Wetenschap als mensenwerk* (Science as a human endeavour), written by the chemist-philosopher Arie Rip (1978). The involvement of natural scientists in the new developments is finally demonstrated by the fact that prominent representatives of the Strong Programme in Edinburgh like David Edge, Barry Barnes, and Steven Shapin originally trained in the natural sciences, while Bloor had been active as an experimental psychologist (Shapin 1995: 296).

What was new about the new initiatives in the 1970s was that the content of scientific knowledge no longer remained out of range but was also held susceptible to sociological analysis. Investigators went beyond programmatic declarations and put the new tenets (especially the defining principles of symmetry and impartiality) to work in numerous empirical case studies. Gone were the days that natural scientists were fodder for sociologists of knowledge only when they unambiguously went beyond the bounds of propriety and reason (as with the Piltdown skull forgery in Great Britain or the Lysenko affair in the Soviet Union), in accordance with the blunt adage of philosopher of science William Newton-Smith: "sociology is only for deviants" (Newton-Smith 1981: 238). The sociology of knowledge would finally live up to its name and no longer *de facto* pass its days as "sociology of error". At long last, it seemed, the "Copernican Revolution" was finally consummated – a revolution that, remarkably enough, had already been proclaimed by Merton in 1945:

The "Copernican revolution" in this area of inquiry consisted in the hypothesis that not only error or illusion or unauthenticated belief but also the discovery of truth was socially (historically) conditioned. As long as attention was focused on the social determinants of ideology, illusion, myth, and moral norms, the sociology of knowledge could not emerge. It was abundantly clear that in accounting for error or uncertified opinion, some extra-theoretic factors were involved, that some special explanation was needed, since the reality of the object could not account for error. In the case of confirmed or certified knowledge, however, it was long assumed that it could be adequately accounted for in terms of a direct object-interpreter relation. The sociology of knowledge came into being with the signal hypothesis that even truths were to be held socially accountable, were to be related to the historical society in which they emerged (Merton 1973 [1945]: 11).

Reading this passage, one would almost believe that Merton was already formulating here, more than 30 years before Bloor, the defining principles of symmetry and impartiality of the Strong Programme. We saw above, however, that the "social accounting for truths" in Merton's prewar monograph was confined to the selection of the problems which scientists would investigate. The new sociology of scientific knowledge went much further and did not hesitate to make the entire content of knowledge, including the nature of the answers given to the chosen problems, into a topic for sociological scrutiny. To make that extra step, one probably needed, as Bloor suggested, some courage and sangfroid (Bloor 1991 [1976]: 11). In this connection it will certainly have been helpful that several protagonists of the new approach had background training in the natural sciences, so that they could confront the phenomenon of science in a sober, matter-of-fact, almost "naturalistic" manner, as if it just concerned a manifestation of nature. But perhaps the general social situation in the 1970s had become ripe for this postponed Copernican Revolution. The aftermath of the Vietnam War, the emergence of the environmental movement, the controversy about nuclear energy, the incipient debate on recombinant-DNA, and many other developments, all entailed that science was removed from its former pedestal and no longer enjoyed an unassailable status.

AFTERTHOUGHT

One anonymous reviewer has suggested that the above account is not just a history of the sociology of knowledge but should rather be seen as a first step toward a sociology of knowledge of the sociology of knowledge. I find this an intriguing suggestion, but I must confess that I have reservations about “going reflexive” and taking up a higher, meta perspective. My scepticism relates to the cognitive gains to be expected from such a change in perspective. It is often held that by adopting a meta point of view we can somehow correct for the distortions and blind spots contained in the theories under investigation. I do not believe this to be possible. Simply engaging in a reflexive mode of discourse does not provide us with the stronger tools of reasoning that we need to warrant such a claim. If the cognitive resources of the sociology of knowledge are deficient to begin with, we do not make them better by moving them to the meta level. There is no miraculous Munchausen bootstrap operation. It takes time and critical effort to find out about our mistaken assumptions, prejudices and blind spots. That's why in writing history we must exploit the benefit of hindsight.

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