

# Journal of Classical Sociology

<http://jcs.sagepub.com>

---

## **A Reconstruction of the Ethos of Science**

Ragnvald Kalleberg


*Journal of Classical Sociology* 2007; 7; 137

DOI: 10.1177/1468795X07078033

The online version of this article can be found at:  
<http://jcs.sagepub.com/cgi/content/abstract/7/2/137>

---

Published by:

 SAGE Publications

<http://www.sagepublications.com>

**Additional services and information for *Journal of Classical Sociology* can be found at:**

**Email Alerts:** <http://jcs.sagepub.com/cgi/alerts>

**Subscriptions:** <http://jcs.sagepub.com/subscriptions>

**Reprints:** <http://www.sagepub.com/journalsReprints.nav>

**Permissions:** <http://www.sagepub.com/journalsPermissions.nav>

## A Reconstruction of the Ethos of Science

RAGNVALD KALLEBERG University of Oslo, Norway

**ABSTRACT** Merton's early analysis of the ethos of science has gained increased relevance because recent developments – such as commercialization of research – have undermined basic norms, like impartiality. It is desirable to uphold the institutional realism in Merton's sociology of science in order to grasp contemporary challenges. But central elements in his analytical approach have also to be revised or rejected, especially his nomological conception of explanations, non-cognitivism in normative issues, and failure adequately to conceptualize *Homo sociologicus* as a reasoner. Reasoners present and discuss cognitive claims, related both to descriptive and normative validity. Merton's analysis of an ethos of science should be evaluated as a reconstructive type of empirical analysis where the goal is insight and adequate identification of phenomena. Several critics denying the existence of Mertonian norms tacitly presuppose the validity of the norms, thereby entangling themselves in self-defeating contradictions. As an unintended consequence, these critics confirm the existence of a fundamental ethos of science.

**KEYWORDS** argumentative influence, norms of science, performative contradiction, rationality (descriptive and normative), reasoner, speech act, tacit presupposition

Robert Merton described and analyzed institutional norms of science in several contributions, starting in the mid-1930s with his doctoral dissertation and ending seven decades later with the afterword to the book on serendipity. He claimed that there is an ethos of science, 'a set of cultural values and mores governing the activities termed scientific' (1968b: 605). But was there, and is there, such an ethos of science actually influencing the behavior of scientists? Harriet Zuckerman notes that such 'seemingly innocent questions have been controversial for decades' (1988: 514). In an introduction to science studies, David Hess articulates some widely held criticisms within this interdisciplinary field, claiming that 'subsequent research failed to confirm the existence of Mertonian norms' (1997: 56–7).

According to him, Merton misidentified the phenomenon, confusing ‘the ideology and the practice’. Hess thinks that Merton’s analysis can be saved only if understood either as the moral ideals of individuals, or as an occupational ideology for self-protection and getting support from society at large. Sal Restivo notes more generally ‘a widespread sense in science studies that the Mertonian paradigm has been vanquished and relegated to the museum if not the attic’ of science studies (1995: 97).

Such criticism is widespread, but mistaken. Merton identified a real phenomenon. His work was not only historically important, but is also essential today. There are, however, also missing and untenable elements in his approach, distorting his analysis of scientific norms. Merton’s sociology of science lacks theoretical resources to meet three essential challenges, having to do with his positivism, theory of action and normative analysis. Merton can be criticized for a positivistic interpretation of science, assuming that scientific theories and explanations in the last instance should be of a deductive-nomological kind (see Kalleberg, 2000: 224–5). In the classic article on the ethos of science, a positivist interpretation can be discerned, inferred from the definition of ‘knowledge’ as ‘empirically confirmed and logically consistent predictions’ (Merton, 1968b: 606). That is not necessarily a problem if we focus only on disciplines like physics and chemistry, but it creates irrationalities in the evaluation of cultural and social sciences.

Mid-20th-century norm-sociology *à la* Aubert (1965) or Merton (1968b) contributed to conceptual progress, focusing on norms, sanctions, roles and (un)intended consequences. But it lacked concepts for the fundamental importance of one type of social action, speech acts. It was tacitly assumed that actors – scientists included – reason outside of language. Merton lacks a micro-sociological understanding of scientists as reasoners, moving in language and able to present, reject, modify or accept knowledge claims with reasons. He also mistakenly assumes that normative questions cannot be discussed and decided with convincing arguments. Here he is a non-cognitivist, pointing to emotions, conventions and sanctions instead of better arguments.

There are new insights and approaches in the broader field of sociology and social theory, as in the work of Jürgen Habermas, Raymond Boudon and contributors within ‘the new sociology of ideas’ (Camic and Gross, 2001), that can help us to overcome misconceptions and missing elements both in Merton’s sociology of science and in the prevailing criticism of Merton in mainstream science studies.

Merton’s best known and most influential contribution to the analysis of the ethos of science is an essay first published in 1942, titled ‘A Note on Science and Democracy’. That essay is the focus of the following reconstruction of the analysis of basic institutional norms in science. I quote the essay from the version published in the third and final edition of his *magnum opus Social Theory and Social Structure* (1968b), where it is titled ‘Science and Democratic Social Structure’ (1968c). Hereafter, when only reference to page is given, it is to this version from 1968.<sup>1</sup>

# The Ethos of Science: A Set of Technical and Moral Norms

Merton distinguishes between science as technical and as moral processes and products. Science refers to a set of technical methods for certifying knowledge, and ‘a set of cultural values and mores’ influencing scientific activity (p. 605). The ethos of science belongs to the moral processes and is identified like this: ‘The ethos of science is that affectively toned complex of values and norms which is held to be binding on the man of science’ (p. 605). Four ‘sets of institutional imperatives comprise the ethos of modern science’, namely ‘universalism, communism, disinterestedness, and organized skepticism’ (p. 607). In the literature this set of norms is often referred to with the acronym CUDOS (Communism, Universalism, Disinterestedness, Organized Skepticism).

Merton did not only relate to contemporary problems, criticizing developments in Nazi Germany, as several critics have assumed. According to Merton, the ethos of science has characterized ‘modern science’ since the scientific revolution. At the beginning of the article, he refers to the mid-17th century (p. 604), mentioning several of the norms described in his Ph.D. – his dissertation was completed in 1935 (see Merton, 1970 [1935, 1938]: 225, 227, 231, 83, 56). In *On the Shoulders of Giants* (1985) he goes further back, documenting the articulation of the norm of scientific humility in 12th-century France.

## What Is the Difference between Technical and Moral Norms?

Merton insists on the distinction between ‘technical’ and ‘moral’ norms. The institutional goal of science is ‘the extension of certified knowledge’ (p. 606). The strategies to achieve this Merton identifies as ‘technical norms’. He does not explicate these norms. And he does not explicate in which way such technical prescriptions are different from moral prescriptions (‘mores’), but seems to assume that the first are cognitive, the second non-cognitive. He only refers to adequate ‘empirical evidence’ and ‘logical consistency’ (p. 606). These technical norms, then, obviously have to do with the truth of claims (‘consonance with facts’, 1968a [1938]: 595), argumentative consistency and coherence in a body of knowledge.

Merton also connects technical and moral norms, claiming that moral norms are intrinsically linked to the goal and methods of science. He gives two arguments to support this point of view. First he claims that the mores are actually ‘derived’ from the goal and (technical) methods (p. 606). (He does not realize that it is impossible to derive a social or moral ‘ought’ from a cognitive or technical ‘is’.) Then he insists that moral norms also have a technical side: ‘They are moral as well as technical prescriptions’ (p. 607).

In the original 1942 article, Merton made a sharper distinction between the two types of norms. He did not alter the original sentences in the 1949 edition of

*Social Theory and Social Structure*, but did in the 1957 edition. (The changes were then kept in later versions of the article, in 1968, 1973 and 1996.) The modifications were as follows. In the 1942 article he stated that the mores 'are binding, not because [1957: not only because] they are procedurally efficient, but *because they are believed right and good. They are moral, not technical prescriptions* [1957: moral, as well as technical]' (1942: 118, my italics). In 1942 Merton strictly distinguished between the two types of norms, but gave technical norms and values primacy. That primacy was maintained, but later he did not differentiate so sharply between the norms, insisting that technical and moral norms are aspects of the same institutional imperatives.

Merton consistently had difficulties in explicating what the difference actually was between technical or cognitive norms, on the one hand, and moral or social norms, on the other. When looking back on his contributions in 1990, he used the terms 'cognitive' and 'social' as synonymous with 'technical' and 'moral' (1990: 337–41). This typology can be interpreted in such a way that there is nothing social in the cognitive, and vice versa, opening the way for a strict distinction between sociological and rational explanations, the first focused on non-rational behavior, the second on cognitive, technical behavior.

But such distinctions – between technical and moral, cognitive and social – are deeply problematic, also according to Merton's own thinking. To express the problem sharply: is there, according to the logic of Merton's own analysis, any meaningful difference at all? Let us have a closer look at the moral norm of universalism in order to clarify and answer the question.

Merton presents three meanings of the term 'universalism'. The first of these moral (or social) prescriptions is presented as follows: '[T]ruth claims, whatever their source, are to be subjected to pre-established impersonal criteria: consonant with observation and with previously confirmed knowledge' (p. 607). But what is the difference between this *moral* (social) norm and the *technical* (cognitive) norms having to do with truth and consistency of claims? By choosing different terms ('technical' and 'moral'), Merton signals that there is an essential difference in social reality and that he uses the terms to refer to different concepts to capture this. But the moral prescription of universalism, as we see, is presented in exactly the same way as the technical norms about truth and consistency. Merton actually presents the 'moral' norm of universalism as a 'technical' norm, related to the social handling of truth, consistency and coherence of claims. He uses two different terms, but actually only one concept. That is: he does not succeed in upholding the distinction as a conceptual one, only as a terminological one.

At this point, it is enlightening to compare Merton's analysis of the norms of science with Knut Erik Tranøy's (1977, 1986, 1988) analysis of the same social reality. Tranøy conceptualizes scientific methodologies as 'normative systems', focusing on 'cognitive acts', scientific 'norms of inquiry' and the 'truth-commitment of inquiry'. Tranøy is more detailed than Merton in the explication of 'prescriptions, proscriptions, preferences and permissions' (Merton, 1968b: 605) and explicates

several norms, such as the following prescriptions articulating the universalistic truth-requirement of science: 'You shall accept demonstrable true and reject demonstrable false claims'; or: 'You are not allowed to accept or present ungrounded claims' (Tranøy, 1986: 146–7). Tranøy's analysis of truth-requirements can easily be reformulated as an explication of essential aspects of norms of universalism, disinterestedness and skepticism, ensuring that only evidence and arguments influence opinion formation and the certification of claims in this institutional context.

Tranøy sees no need for a distinction between social and cognitive norms. He identifies scientific methodologies as normative systems, referring to norms actually influencing cognitive interaction between scientists. These are cognitive norms that also are social norms, or social norms that also are cognitive. We have seen that this is also – in practice – the case with Merton's socio-cognitive norm of universalism. It is not difficult to show that this is the case for the other Mertonian norms too. Merton claims as his ambition that he 'shall consider, not the methods of science, but the mores with which they are hedged about' (p. 605). He does not succeed in this. And how could he? Whatever it is to test 'truth claims' (p. 607), it must refer to social processes among scientists, processes that can and should be described and analyzed by sociologists and historians as other interaction processes.

## How Many Norms Are There? From CUDOS to CUDOSH?

In the field of science studies, it is sometimes claimed that Merton after 1942 expanded the number of basic norms in his conception of the ethos of science in order to solve problems and inconsistencies. (Persons who worked closely with Merton also suggested new norms.) Reference is often made to an influential article from 1957 on priority struggles in science, where Merton highlighted a norm of originality and a counter-balancing norm of scientific humility (1973a [1957]: 303–5). But it is not difficult to find the two 'new' norms of originality and humility also in the 1942 article. They are articulated, for instance, in the discussion of the norm of communism. One of the themes discussed there is Newton's remark: 'If I have seen farther, it is by standing on the shoulders of giants' (p. 612). This is one of the most common ways of articulating the norm of scientific humility, and the one Merton preferred (1973a [1957]: 303; 1985). The institutional imperative of scientific humility requires that scientists shall keep in mind both how little the single scientist knows in relation to the total community of inquirers, and a respect for the complexity of reality. Laplace articulated the second aspect of this norm like this: 'What we know is not much; what we do not know is immense' (cited in Merton, 1973a [1957]: 304).

In the 1942 article Merton also discusses priority controversies and recognition by peers; this was not something new in 1957. Such conflicts derive from 'the institutional accent of *originality*' (p. 610), he claims in the explication of the complexities of the norm of communism. Originality is clearly also presupposed in

the discussion of the imperative of universalism, as when Merton talks about acceptance of ‘final *increments*’ (p. 607) to knowledge or ‘*new* technical advance’ (p. 607) (my italics).

In my view the norms of scientific originality and humility are so important in Merton’s analysis of the ethos of science that it is more adequate to talk about the six CUDOSH norms, instead of the – more common – four CUDOS norms. OS in CUDOSH then refers not to organized skepticism, but to originality and skepticism, while the H refers to humility.

Merton never intended to present an exhaustive typology of norms. It is not a deficiency that some norms are not mentioned or explicated. Both Habermas and Tranøy, for instance, insist on a norm that is lacking in the CUDOSH norms, namely the requirement for individual honesty. One of the formulations that Habermas gives of this norm is: ‘Every speaker may assert only what he really believes’ (1990: 88), a norm to be followed in all kinds of rational discourse. Tranøy uses concepts like honesty, sincerity and veracity (1986: 153–4). But Merton is also aware of this norm. When, in 1938, he used the expression ‘ethos of science’ for the first time, he mentioned both ‘intellectual honesty’ and ‘integrity’ (1968a [1938]: 596). Honesty, however, is not on the list four years afterwards. That may be due to the (inter-)institutional focus of the 1942 article. Merton generally presupposes individual honesty as an element in the ethos of science, deriving from institutional imperatives and ‘internalized’ in the ‘scientific conscience’ (p. 605) of individual scientists.

It is generally not necessary, or practicable, to require exhaustive typologies of norms. A large set of norms are activated and presupposed in real contexts. When science is described as embedded in actual institutions, like specific universities, concepts of institutional bundles and role-sets have to be introduced, with their accompanying complexes of institutional norms and counter-norms, presuppositions and sociological ambivalences (Kalleberg, 2000; McCaughey, 2003; Merton, 1976).

## Argumentation as Social Interaction and ‘Power’

How and why are technical (cognitive) norms binding on scientists? What goes on when scientists are testing truth claims? Merton does not give convincing answers. The explanation for this is not that he is against discussing intellectual contents, as Restivo claims: ‘The central dogma of the Mertonian paradigm is that the autonomy of science somehow makes scientific knowledge independent of social influences’ (1995: 97). A better explanation is the fact that Merton does not have adequate concepts for speech acts, reasoners and scientific argumentation as social interaction.

Merton is good at analyzing interdependencies between science and other institutions, for instance how religious or military interests can influence problem choice in science. Over seven decades he analyzed inter-institutional influences on the choice of research questions, obviously of essential importance in understanding cognitive contents (Merton, 1990: 360–1; 2004; Zuckerman, 1989). In the

1942 article, it is documented how conflicts during World War I converted men of science into men of war, stimulating perverted arguments, such as Pierre Duhem's claim that 'the "geometric spirit" of German science stifled the "spirit of finesse"' (p. 608).

Merton often discusses contents in contributions from humanists and social scientists, as in analyses of history of science, where scientists entangle themselves in fallacies of the latest word (1984); or when modern social scientists are too anachronistic to recognize that the rational heroes of the scientific revolution were also Christians 'glorifying God' (1968b: 631). He opens the way for grand questions about the influence of theology on basic contents of natural science, as when he notes the similarity between Calvinists believing in predestination and scientists believing in 'immutable laws of nature' (1968b: 638). His 1945 article on a paradigm for sociology of knowledge also offers several examples. Here he approvingly states: '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' (1973b: 11). To sum up: according to Merton, primary cognitive content also has its history and sociology.

Merton's problem is not that he flees from cognitive content, but that he does not have concepts for describing and analyzing actors as reasoners involved in speech acts, presenting claims and discussing cognitive contents. Conceptual poverty predisposes for lack of detail and realism. In the analysis of self-fulfilling prophecies, the president of the Last National Bank knew that 'a rumor of insolvency, once believed by enough depositors, would result in the insolvency of the bank' (1968d [1948]: 476). Merton is here analyzing knowledgeable actors who relate to cognitive contents ('rumors'). Certain definitions of social reality are false, but nevertheless realized, because the actors sincerely – and mistakenly – believe that the rumors are true, and so the 'prophecy of collapse led to its own fulfillment' (1968d [1948]: 477).

How do we actually persuade others and ourselves? With Merton's norm-sociological resources, it is not possible to give a convincing answer. We need a theory of communication and claim-making to identify, document and explain such socio-cognitive processes of persuasion. Merton did not have such a theory at his disposal in 1948, writing about self-fulfilling prophecies. In 1942 he used terms like 'truth claims' and 'validity of claims', but he had no clear concept related to a theory of communication. Nor did he adapt his theory to integrate such a conceptual apparatus when it became possible, for instance after John Austin's groundbreaking book *How to Do Things with Words* was published in 1962 (Austin, 1975 [1962]); or after Habermas, in the spring of 1971, in the Gauss Lectures at Princeton, sought to give sociology a 'linguistic foundation' based on an understanding of speech acts (Habermas, 2001).

A 'linguistic turn' has taken place in the social and cultural sciences during recent decades. There are several approaches in this reorientation, indicated with catchwords like conversational analysis, discourse analysis (in such different versions



as Foucault's and Habermas's), ethnomethodology, rhetoric and speech-act theory (*à la* John Searle). One of the most convincing approaches – to a large degree developed by restructuring mainstream sociological traditions – has been presented in several contributions by Habermas (such as 1984, 1987, 1990, 1996; see also Lafont, 1999).

The conceptual apparatus developed by Habermas in his theory of communicative action opens the way for more empirically sensitive descriptions and analyses of symbolic interaction, scientific behavior included, than was possible in norm-sociology, which failed to recognize the essential importance of speech acts. The theory is based on an understanding of argumentation, testing of validity claims and social interaction based on speech acts. In a sense, Habermas tries to answer an elementary and fundamental question: What are we *doing* when we talk? The general answer is that we present, defend and modify claims (or reasons), reasons that can bind us because we become convinced of their validity. Actors all the time do and say things because they primarily think them true and right. This is valid in general, and especially visible in well-functioning scientific communities. Bloor's 'strong program' in science studies is based on a misconception, oriented to explain developments in science with naturalistic causes and not with 'teleological' reasons. But developments in mathematics and the explanation of them have to be understood through the mechanism of convincing reasons related to truth: 'It is simply *because* it is true that we believe this statement' (Boudon, 1994: 192, emphasis in original; see also Skirbekk, 2005).

No social coordination and interaction would be possible without this presentation and evaluation of reasons. In our daily life, most reasons are more or less implicit. If not, we could not function. In scientific fields, claims have to be explicated. In general, *Homo sociologicus* is a reasoner, be s/he located in an ordinary status in family life as a parent or in working life as a manager or a professor. In communication between speaker and listeners validity claims are presented. They can be accepted or rejected or participants may decide that they need more information or clarification before they take a stand on a claim. We raise many types of claims, two of them being truth and rightness claims. For instance we claim that something has happened, that it is so and not so. We may be corrected by others, accept it and change our claims and act accordingly. In a similar way, we claim that something is unjust, better than before or an example of scientific misconduct.

Normative speech acts cannot be 'true' in the same way as descriptive speech acts, but they can be 'right', and both can be accepted as 'valid' and therefore intersubjectively binding (Habermas, 1990: 50–62). Knowledge claims of a descriptive and prescriptive character can be defended, criticized and modified with reasons. That is, they can be discussed rationally (see also Boudon, 2001: 93–118). Reasonable participants in argumentative interaction are both rational and fallible. A rational person in this perspective is one who is able to present and defend claims, listen to criticism, and eventually keep, modify or reject descriptive and normative beliefs as a result of this. In order to reach valid descriptions and

valuations, no better way is known than open discussions, governed by the force of better arguments (Habermas, 1984: 8–42; 1998: 34–46).

## Should We Distinguish between Sociological and Rational Explanations?

Merton shares the positivistic assumption that scientific explanations, including those of sociology of science, should in the last instance be of a ‘causal’ character (1968b: 150–3), for instance as exemplified by Boyle–Mariotte’s law about the causal relationship between volume and pressure in gases (under constant temperatures). Bloor with his ‘strong program’ has perhaps been the most explicit in science studies in insisting on the same point, claiming that scientists have to be explained in terms of causes, not ‘teleologically’ with reasons (Bloor, 1991: 5–8).

Merton notes the lack of causal explanations in sociology (e.g. 1968b: 150), and attributes this to the immaturity of the social sciences (1968b: 45–50).<sup>2</sup> Bloor explains the lack of such explanations in science studies as a ‘lack of nerve and will’ (1991: 4) on the part of sociologists. These two explanations are not convincing. The social sciences are at least three centuries old (Engelstad et al., 2005: Ch. 3). Neither is there a lack of nerve. What is lacking is an adequate concept of ‘explanation’. According to much recent philosophy of science, the basic reason for the lack of such Mertonian or Bloorian ‘causal’ explanations is that there are none to be found (see Engelstad et al., 2005: 93–108; Kalleberg, 2000: 223–5). There are no objectivistic, general laws in social science, of a deterministic or of a probabilistic kind (Boudon, 1986: 75). Explanations in all the social and cultural sciences have to be formulated in terms of the reasons of actors who have the freedom to choose between alternative actions, regularly creating complex aggregations of effects and unintended consequences (Boudon, 1986: 207–11; 2001: 57–69; Habermas, 2005: 155–86).

There are no good reasons for despairing because of this absence of nomological theories, despite the fact that claims to the contrary still can elicit anger and anguish among some social scientists. It is paradoxical that many social scientists, indifferent to philosophy of science, nevertheless are so attached to outdated philosophies of science, binding their scholarly identity to the possibility of generating such explanations. Readings of good contributions in social science are the primary empirical basis for documenting and evaluating the state of these disciplines. It is not difficult to argue that the social sciences are functioning well as sciences. We have theories ‘as rigorously scientific as the natural scientists aim to make their own’ (Boudon, 1986: 192), although they are based on other conceptions of ‘causality’ than in chemistry, physics or medicine.

Adequate explanations in the social and cultural sciences generally have to be articulated in terms of the reasons of actors; they are ‘teleological’. In order to describe and explain stability and change in culture and society, we have, then, to understand cognitive contents, namely reasons. In analyses of interaction among

scientists as scientists, such as in scientific controversies or in scientific change, this is especially visible. The common distinction between rational ('teleological') and sociological ('causal') explanations is misplaced. Cultural and social beliefs (reasons), be they true or untrue, morally right or wrong, have generally to be described, analyzed and explained 'rationally', focusing on actors, reasons and discourses, in their (sociologically) ambiguous institutional and historical contexts.

## Argumentation as a Type of Influence

Strong reductionist arguments have been common in science studies, implying that all kinds of influence in the last instance are a form of 'power', even that success in science is the continuation of war by other means (see Amsterdamska, 1989). Such points of view are regularly based on a too narrow and undifferentiated conception of power. Concepts like 'power' and 'influence' refer to a multitude of forms of interaction. At a general level, it is useful to operate with a broad typology of influence (or power), with unilateral use of physical force at one end and symmetrical argumentation at the other (Dahl, 1991: Chs 3, 4). In the middle, for instance, we have obedience in hierarchies or advertisements for commercial products. In social reality different forms of influence are ordinarily bundled together and can only be distinguished analytically.

Not to recognize the importance and ubiquity of the force of better arguments in social interaction indicates lack of realism. Persuasion based on reasons that can be intersubjectively tested and accepted is an essential form of unforced influence between actors. To persuade another person with convincing reasons is a type of legitimate, symmetrical 'power', actions that can be described, analyzed and evaluated by a sociologist. Influence through convincing reasons is used all the time, in all social spheres, and is an essential element in the practices and visions of modern democracies (deliberative democracy). In some institutions it is specialized and intensified. Well-functioning scientific communities are argumentation collectives primarily regulated by the 'power' of better arguments (Engelstad et al., 2005: 115–19, 307–13). Such communities, where the Mertonian norms of science are practiced, are actually well-functioning Habermasian publics (*Öffentlichkeit*). In 'publics' the ambition is that only arguments shall count and that the discourse is open for all relevant themes and participants (Habermas, 1984: 22–42, 233–42; 1989: 36–7).

Merton's article on self-fulfilling prophecies (1968d [1948]) is deservedly a classic, one of the most insightful ever written by a social scientist. A theory of communication does not invalidate the type of analysis given in that article. On the contrary, it can make it more realistic, analyzing interaction as inter-related speech acts. The analysis of actors as reasoners includes the description and analysis of distorted (factual and normative) reasons. 'False definitions' of situations, 'false conceptions', 'scrambled logic' and 'moral alchemy' can make 'the originally false conception come true', perpetuating 'a reign of error' (Merton, 1968d [1948]: 477, emphasis in

original), such as discrimination because of race, class or sex. As can also be seen here, Merton is not able – or willing? – to follow his own norm of value neutrality all the time. Here – and not only here – he is explicitly also operating as a critical theorist.

## Reconstruction as Empirical Analysis

In the opening of the article I referred to Hess's criticism of Merton, claiming that empirical research has 'failed to confirm the existence of Mertonian norms'. (Cole, 1992, interprets the literature differently, but there is no need to pursue that issue here.) Such criticism is based on too narrow a conception of 'empirical' research in the social and cultural sciences. We should distinguish between three forms of empirical description and analysis: *documentation* of states of affairs; *integration* of existing knowledge; and *reconstruction* of basic experiences, conceptions or existing scientific studies (Kalleberg, 2000: 220–2). These conceptual distinctions make it possible to identify Merton's analysis of scientific mores as a specific kind of empirical research, much practiced but seldom explicated as such by social scientists.

Documentary studies are based on primary data, for example interviews, observation or documentary sources, such as diaries, letters and newspapers. One example is Merton's *Science, Technology and Society in Seventeenth-Century England* (1970 [1935, 1938]), based on many kinds of documents, scientific publications included. Another example is *Student und Politik* (Habermas et al., 1961), based on intensive interviews with students in Frankfurt, the best empirical study from the re-established Institut für Sozialforschung. In recent science studies direct observation has been much practiced (see Knorr Cetina, 1995).

Integrating (synthesizing) studies are based on existing studies and the material presented there. Three examples are Merton's article on priorities in scientific discovery (1973a [1957]), Habermas's *The Structural Transformation of the Public Sphere* (1989) and Huff's *The Rise of Early Modern Science* (1993). Such contributions demonstrate that it is possible to develop original insights and explanations not identified or explicated in the studies used as the basis for integration.

Reconstructive studies are often focused on common experiences and practices in different social institutions. They can also be focused on explication of existing scientific contributions. Self-reflexivity is characteristic of such studies, where a common strategy in the testing of claims is to find out if they can in principle be applied also to the author and reader. Good examples of reconstructive studies are several of Merton's essays (1968b, 1973b), for example 'The Self-Fulfilling Prophecy' (1968d [1948]). Two other modern masters using such an approach are Habermas (e.g. his theory of communication) and Boudon (e.g. his analysis of the logic of explanation in social science).

Reconstructive studies take many forms, one being explication of presuppositions that are taken for granted, for instance in ordinary communication. Another is clarification and reinterpretation of existing studies, discussing authors from earlier times as if they were contemporaries, what Habermas labels *Theoriegeschichte in*

*systematischer Absicht* ('history of theory with a systematic intent', 1984: 140).<sup>3</sup> Boudon characterizes his version as *explication de texte* (Borlandi, 1995: 561). If important keywords for documentary and synthesizing studies are *finding* and *overview*, keywords for reconstructive studies are *insight* and adequate *identification* of social phenomena.

Reconstructive inquiries about socio-cultural reality are better known in philosophy than in sociology. In Britain, in the tradition of Robin Collingwood, the approach is called presuppositional analysis. In Germany it is often spoken of as transcendental analysis (Karl-Otto Apel), referring to the clarification and identification of unavoidable conditions of a social practice. Habermas talks about 'rational reconstructions' and mentions contributions from Piaget and Chomsky as examples. It is not surprising that it is a sociologist who is also a leading philosopher, who has given the most convincing presentation of reconstructive approaches in the social and cultural sciences (Habermas, 1990: 14–20, 21–32, 82–98).

Documentation, integration and reconstruction are often combined. In this paper, for instance, I document modifications of central formulations in Merton's original 1942 essay. I also integrate knowledge and insights from contributors not relating to each other. Merton referred to Boudon, but never to Habermas; Habermas often refers to Parsons, seldom to Merton and not to Boudon. Boudon refers to Merton, but seldom to Habermas, and it happens that he misconstrues the Habermasian arguments. It is unusual to integrate elements from Merton, Habermas and Boudon in order to open the way for new insights not found in either of them. I also try to articulate new insights by reconstructing existing studies and giving new interpretations of everyday practices in scientific and other institutions.

## Habermasian Reconstructions and Mertonian Inferences

In reconstructive studies insights are often developed on the basis of ordinary experiences, for human beings in general or in a certain category. One example can be found in criticisms of objectivism in social science, an approach focused on people only as objects, totally programmed by nature, nurture, social structure or culture. However, each one of us, on the basis of ordinary, daily experiences, knows that objectivism is incompatible with the most basic experiences and intuitions of ourselves. We know that we can choose between alternatives and are responsible for our actions. Such intuitions based on everyday evidence can be explicated and theorized. Contributors like Boudon (1982: 153–5), Habermas (2005: 155–86), Searle (1984: 86–99) and Skjervheim (1996: 127–33) have used such strong everyday evidence, reconstructively arguing that (generalized) objectivistic positions lack empirical realism. We are not only objects, we are also subjects.

In several traditions in social science, there is a tendency to regard concepts as 'theoretical', in some strange location outside of the 'empirical' world. Construction of concepts takes place in some non-social, perhaps supernatural, reality. But constructs can also – somewhat mysteriously – be tested in the real, empirical

world afterwards.<sup>4</sup> Such a conception is understandable in relation to the natural world, which is not symbolically mediated. (Perception of reality is, however, symbolically mediated in all sciences, as in daily life.) But socio-cultural facts, such as institutional norms or academic role sets, are in themselves conceptually structured. Appropriate concepts, for instance about social action, the ethos of science or sociological explanation, are therefore internally related to those social realities. Misconceptions of human beings as only objects can be criticized both as descriptively inadequate, and as normatively inadequate, as an attack on human intentionality and freedom. Conceptual clarification and testing of the adequacy of concepts in social studies are internally related to the phenomena analyzed, and consequently a form of analysis of social reality, and in that sense empirical analysis. Concept-constructing social scientists are performing all their activities in our common (empirical) world, and none outside of it.

Contributors to the analysis of speech acts have argued that there are certain 'inescapable presuppositions' characterizing communication where actors sincerely are trying to reach an understanding. Three such presuppositions are the following ground rules: (a) no speaker should contradict himself or herself; (b) the speaker should only claim what s/he really believes; and (c) everyone is allowed to question any assertion whatever (Habermas, 1990: 87–9). Habermas has convincingly argued that certain basic rules of discourse are not mere conventions, they are 'inescapable presuppositions' (1990: 89) for social action. He insists that 'deliberation' and 'argumentation', at least as informal practices, are found 'in all cultures and societies . . . there is no functional equivalent alternative to this mode of problem solving' (1998: 43). The argument is not that 'people want to act communicatively, but that they have to' (Habermas, 1994: 111).

Merton in his research practice regularly theorizes in a reconstructive way, but without an explicated conception of what he then is doing. He reflects on the formation of concepts, the clarification and codification of existing perspectives, concepts, paradigms and approaches. In the analysis of self-fulfilling prophecies, he constructs concepts, typologies and insights about ordinary social phenomena. The challenge in such studies is not to get more data, or document something that we did not know before, nor is it primarily to integrate existing contributions. In this kind of empirical analysis, we clarify, modify and develop adequate concepts. Merton claims that we do such theoretical work in order to 'liberate us from the patterns of cognitive misbehavior' (1968b: 146). Habermas and Boudon have done much work of this kind, for instance to broaden too narrow concepts of 'rationality' in mainstream social science (e.g., Boudon, 2004: Ch. 5; Habermas, 1984: 10ff.).

The reconstructive essay on self-fulfilling prophecies is not an explorative, 'theoretical' essay having to be 'tested' on empirical material at a later stage. It is an empirical and theoretical contribution in its own right. Criticisms of objectivism in social science are not 'theoretical' work in splendid isolation from 'empirical reality'. Merton's classic analysis in 1942 of the ethos of science was not

characterized by new data and new findings. The facts were well known. Neither was it a synthesis of existing studies. It was not intended as a prescriptive proposal either, but as an analysis of institutionalized practices. It was primarily a reconstructive essay, an identification and explication of presuppositions and norms taken for granted in well-functioning scholarly communities (in real-life societies). Such studies are examples of empirical, grounded analyses of the reconstructive type. In such theorizing, social science is improved not with better data, but with a more adequate identification of a phenomenon, general insights and better concepts.

There are several elements in Merton's analysis that support this interpretation. He is well aware of all the elements that are taken for granted, so obvious that we ordinarily do not see them. 'The persistent development of science occurs only in societies of a certain order, subject to a peculiar complex of tacit presuppositions and institutional constraints' (1968a [1938]: 591). He notes that the ethos has not been codified but can be 'inferred' (p. 605) from practiced mores. Inferring here is a form of reconstruction of values, norms and generalized insights that practicing scientists presuppose.

In mainstream science studies there are many misconceptions on how Merton constructed his conception of norms of science. For instance, Fuller claims that Merton 'simply surveyed the methodological pronouncements of distinguished scientists and philosophers' (2006: 15). This is incorrect. According to Merton, the ethos of science can be inferred from two main sources: (1) 'from the moral consensus of scientists as expressed in use and wont, in countless writings on the scientific spirit' (pp. 605–6); and (2) from the analysis of 'moral indignation directed toward contraventions of the ethos' (p. 606). He insists that the breaking of norms both makes it easier to identify and explicate them, and that 'the very process of contemning their violation' reaffirms the mores (p. 608). Merton gives several examples. European men of science became men of war during World War I and leading German physicists rejected Einstein's theories as 'Jewish' (see Merton, 1968a [1938]).<sup>5</sup>

Like in all other forms of research, contributions of this reconstructive kind are fallible. Presuppositions may be unavoidable, but the analysis of them, as for instance presented by Habermas or Merton, is of course fallible and open to criticism, modification and rejection. 'There is always the possibility that they rest on a false choice of examples, that they are obscuring and distorting correct intuitions, or, even more frequently, that they are over-generalizing individual cases . . . [therefore] they require further corroboration' (Habermas, 1990: 32).

Reconstructive studies should be complemented with other types of contributions. In Merton's case, with his insistence on long historical trajectories and the Weberian understanding of the specificity of Western science and culture, historical-comparative analyses are appropriate. It is therefore surprising that there are so few follow-up studies, documenting and trying out the conception of an ethos of science in a broad historical-comparative perspective. Toby Huff's study of *The Rise of Early Modern Science* (1993) is an exception that confirms the rule.



Analyzing the institutional revolutions in Western Europe during the 12th century in a comparative perspective, Huff makes it easier to see the specificities of European development and the essential importance of an ethos of science (Huff, 1993: 22–5, 213–36). In contrast to the then leading Arabic and Chinese civilizations, the Europeans distinguished between state and church and created independent institutions, such as cities and universities. This created essential elements in the institutional and cultural infrastructure of the revolutions that heralded modernity in the north-western corner of Europe half a millennium later.

## Self-Referential Consistency and Performative Contradictions

An analysis of third persons should also in principle be applicable to both the writer and the reader of the analysis. If that is not the case, we may have an example of self-referential inconsistency. This basic insight is articulated by most major theorists, for instance in this way by Merton: ‘Were the sociology of science not self-exemplifying, then either the general ideas and findings would have to be thought unsound or the field itself is nothing like the scientific specialty it is commonly supposed to be’ (1977: 4).

This is in line with the general requirements of socio-cultural descriptions, explanations and evaluations. In order to describe a social process, we have to understand the reasons of actors, in their specific contexts. That generally requires that we as social scientists imagine ourselves to be in the same situations as the actors studied. Boudon credits Weber for the explication of this basic insight, ‘probably one of the most important discoveries in the modern social sciences’ (1986: 29). In Weber’s view, ‘observers understand the action of an observed subject as soon as they can conclude that in the same situation it is quite probable that they too would act in the same way’ (Boudon, 1986: 31). The bare description of reasons requires that we are able to evaluate them with regard to their validity. In his critical-constructive discussion of Skjervheim, Habermas sums up the position as follows: ‘The *description* of reasons demands *eo ipso* an *evaluation*’ (1984: 116, emphasis in original).

In explanations and evaluations of socio-cultural phenomena, this has generally been taken for granted, but not explicated. Without too much effort we understand most ‘ordinary’ phenomena, based on common experiences. But when reasons are embedded in unusual contexts – for instance, in esoteric natural sciences requiring years of training and socialization to become a member – social scientists have to go through similar processes of learning in order to describe and explain what actually goes on in the primary processes of knowledge production. It is a tribute to Merton’s good sense that he abstained from discussing cognitive contents in the natural sciences, where he had little or no knowledge.

Reconstructions are good for testing requirements of self-referential consistency. One way of identifying and clarifying such rationality challenges is to identify performative contradictions. ‘A performative contradiction occurs when



a . . . speech act . . . rests on non-contingent presuppositions whose propositional content contradicts the asserted proposition' (Habermas, 1990: 80). A simple example is a person claiming: 'I doubt that I exist.' But when claiming this, the person obviously has to presuppose that he exists. This non-contingent presupposition negates the proposition presented. An appropriate example in our context, would be a social scientist claiming: 'I am completely causally determined.' Such a scientist (teleologically) presents an argument denying that there are teleological actions and explanations in social reality.

Hess, referred to in the beginning of the article, entangles himself in such self-defeating contradictions. He integrates several studies as a basis for claiming the non-existence of an ethos of science. Hess clearly wants the reader to take his claims seriously, intending what he says about Merton's analysis and the character of scientific institutions to be true not only for himself, but also for the reader, the scientific community and the public at large. But in presenting such truth claims, the norm of universalism is at work, and in integrating the literature, norms of organized skepticism, communism and disinterestedness are at work. Hess both explicitly claims that the institutional norms of science do not exist and tacitly presupposes that his own work is based on the validity and functioning of the same norms. This is a self-destroying, performative contradiction, a sufficient basis for rejecting his claims. The fact that such criticism is typical in mainstream science studies can be read as an indirect confirmation of the existence of Mertonian norms.

## **Normative Rationality, Research Ethics and Deliberative Democracy**

In an adequate contemporary sociology of science, Mertonian insights should be maintained, revised and transformed, not relegated to the museum of science studies. Four such essential insights in this legacy have to do with (1) the structural understanding of science as a social institution, (2) the understanding of the interdependence of science and other institutions, (3) the analysis of science as a 'sort of micro-model for social reality as such' (Sztompka, 1986: 3),<sup>6</sup> and (4) the identification of science as a moral undertaking, focused on institutional ethics, not (only) the ethics of individuals. I end with some critical and constructive reflections on the second and fourth of these themes.

### **How and Why Are Norms Morally Binding?**

When explaining why norms are morally binding, Merton one-sidedly underlines the importance of emotions and non-rational values and traditions. Discussing the incompatibility between an ethos of science and a totalitarian ethos, he argues that social codes and ethoses in general are sustained by sentiments and curbed by 'disapproving emotional reactions, which are mobilized by the supporters of the ethos. . . . resentment, scorn, and other attitudes of antipathy operate almost automatically

to stabilize the existing structure' (1968a [1938]: 601). In the introduction to the chapters on sociology of science in *Social Theory and Social Structure*, he discusses the importance of religious support for science during the scientific revolution, concluding: 'Even so rational an activity as scientific research is grounded on non-rational values' (1968b: 589). He discusses the sacred area of all institutions, using Durkheim's classical analysis, and claims: 'Every institution involves, in this sense, a sacred area that is resistant to profane examination in terms of scientific observation and logic' (1968a [1938]: 601). Asking about the authority of norms, Merton gives a non-cognitive answer, referring to emotions and traditions 'which deserves uncritical respect' (1968a [1938]: 601). His general position seems to be this: we do not follow norms (mores) as inter- and intra-subjectively binding because of reasons that can be rationally criticized, but because of internalized attitudes, intersubjectively sanctioned. Also scientific institutions have an irrational value base: 'The institution of science itself involves emotional adherence to certain values' (1968a [1938]: 601).

Emotions and 'sacred' areas are important, but not the only elements to be analyzed. Contributors such as Habermas and Boudon have convincingly shown that it is possible to give good reasons for normative positions, both to back and maintain them and to criticize and change them. Social actors, scientists included, follow norms also because there are good reasons to do so. Value-conformity based on emotions is a real phenomenon. But so is value-rationality, opening up a field where the better arguments can drive the weaker out. Norms can be binding on others and ourselves also because we understand – with reasons – that they are right and proper.

Merton would not be the rich classic he is if we could not identify and reconstruct cognitivistic elements in his writings. The important norm of scientific humility is an instructive example. Merton claims: 'The humility of scientific genius is not simply culturally appropriate but results from the realization that scientific advance involves the collaboration of past and present generations' (p. 612). I find it reasonable to interpret the word 'realization' to mean that he, if only implicitly, presupposes that this norm is also based on an insight, which can be explicated and backed with reasons. It is also worth noting that he says that the antipathy operates 'almost automatically', which also presents an opening for reasons.

There is a general phenomenon here. Classics in social science are sometimes more interesting in their research practice than in their philosophy of science and interpretations of their own contributions. This has been demonstrated in several reconstructive studies. Habermas has documented that Marx and Freud gave positivistically distorted interpretations of their own work, and that Weber practiced a broader concept of rationality in his sociology of religion than the one he presented in his methodological articles and chapters (1978: Chs 2, 3, 10–12; 1984: 279–86). Boudon has argued that if Durkheim in his own research practice had consistently followed the guidelines from his own book on methods, the canon of only looking at social facts and not actors, we would not have treated

him as the deserved classic he is (Borlandi, 1995; Boudon, 2004: Chs 2, 3). Merton can also be better in his research practice than in some of his reflections on such practice.

## Descriptive and Normative Research Ethics and Critical Tasks of Sociology

We can distinguish between descriptive and normative contributions in the study of norms. Disciplines like sociology, political science and history are especially strong in describing and comparing norms and values, but do also contain vital traditions for normative contributions, as in democratic theory and theory of development (Dahl, 1989; Habermas, 1996; Sen, 2002). Normative contributions can be the foundation both for critique of the *status quo* and for recommendations for how to improve social reality, for instance stimulating processes making production more efficient, governance more democratic or public discourse more enlightened.

Merton's contributions are of a descriptive character. He did not work out a normative conception of research ethics, a codification to be used by scientists and others in the external and internal regulation of research institutions and individual scientists. This is consistent with his non-cognitivist vantage point. If the stronger reasons cannot drive out the weaker ones, a field of discourse should not be made part of an academic discipline. Merton did not think that normative questions could be discussed, developed and decided with the force of better arguments, not among scientists, not in society outside. Values and norms are 'resistant to profane examination', and therefore should be located outside of scholarly work.

When Merton discussed science in Nazi Germany, he basically observed what was happening. He did not explicitly criticize social reality. No attentive reader, however, is in doubt that Merton was critical of what he observed. But that was – so to say – not part of his scientific analysis. He occasionally thematized such issues, as when he claimed that scientists analyzing 'the sacred sphere of political convictions or religious faith or economic rights' come 'into psychological, not logical, conflict with other attitudes toward these same data' (1968a [1938]: 601). But this is not a convincing position. We can also have to deal with cognitively valid criticism, not only with emotive psychological tensions. Social criticism, normative argumentation, is an essential task for social science (Dahl, 1991: Ch. 10; Engelstad et al., 2005: 130–42, 340–62; Habermas, 1987: 374–403).

Merton's conception of the ethos of science has, nevertheless, regularly been used in prescriptive courses on research ethics. That is reasonable. Sociology and other social and cultural sciences are both descriptive and normative disciplines (Engelstad et al., 2005: Chs 1, 4–7, 14). Sociology of science and science studies are traditionally most developed as descriptive fields, but should also be developed as normative and prescriptive fields (Kalleberg, 2002; Skirbekk, 2005). I have shown that Merton's analysis of the ethos of science is compatible with an explicated, normative research ethics.

## Science in Deliberative Democracy and Market Society

Merton always insisted on the connections between science and society, science influencing society, society influencing science (e.g., 1968b: 585–90). He himself made the most thorough studies of such relationships in his contributions on the scientific revolution in early modern Europe. He also recommended systematic studies of the German experience during Nazism. It was an ‘extreme and therefore illuminating case’ of the dependency of science on ‘the larger civil and political society’. It should not just be defined away and forgotten as ‘exceptional and pathological’ (1968b: 586). This challenge has yet to be taken up by sociologists of science.

Merton insisted on the internal relationship between the ethos of science and the ‘ethos of democracy’ (p. 609). He never, however, developed a more precise conception of the ethos of democracy, or of its relationships with the ethos of science. If we follow up this essential theme with contemporary conceptions, we can underline the importance of ‘democratic deliberation’ (Habermas) and ‘enlightened understanding’ (Dahl). Academics as intellectuals do – and so they should – contribute with scientific knowledge to public discourse. The role of intellectual is part of the basic fivefold academic role-set (Kalleberg, 2000: 229–32; 2005, 2007). In contrast to Parsons, Merton does not include the role of intellectual in his conception of the scientific role-set (Merton, 1973b: 519–22). But such an extension is compatible with his reasoning and also his practice (McCaughey, 2003: 375ff.).<sup>7</sup>

One of the norms to institutionalize and practice in this arena of public discourse is the (institutional) norm of scientific humility, the insight into each scientist’s or discipline’s limited understanding of the complexity of reality. Several of today’s essential challenges for democratic states, such as those related to ecology or human rights, also require mutual popularization between specialists from different disciplines and broader public discourse. Appropriate scientific humility is essential for learning and cognitive improvements in such processes (Kalleberg, 2000: 247–50; 2002: 167–70).

The strength of Merton’s thinking consists in its institutional realism. He never narrows the discussion of norms such that they are seen as requirements that apply only to individuals, as several philosophers, theologians and also some social scientists (without the desirable degree of sociological imagination) tend to do. Instead he realizes that one has to focus not only on individuals, but also on institutions and traditions. Such realism characterizes Merton’s discussion of the relationships between sciences and markets. He never discarded the conception of tensions between the public character of science and the private property of the market, claiming that the ‘communism of the scientific ethos is incompatible with the definition of technology as “private property” in a capitalist economy’ (p. 612). Obviously Merton did not recommend the abolition of markets, or that science should not contribute to economic growth. But he insisted on the importance of finding appropriate and sustainable balances between such incompatible ethoses, so characteristic of modern societies.

Destructive tendencies, such as uncontrolled commercialization of scientific knowledge and too much state control of science (see, e.g., Bok, 2003; Nowotny et al., 2005) are reminders of the problems created by institutional imbalances. Bok argues that ‘clinical trials funded by drug companies are far less likely than independent funded trials to arrive at unfavorable conclusions’ for the companies, and refers to an editor of a prestigious medical journal who is afraid of tendencies transforming the journal into ‘an advertisement mechanism rather than a vehicle for the distribution of sound medical science’ (2003: 68, 74). It is essential to use and reconstruct insights from Merton’s sociology of science to develop both an adequate understanding of such problems and insights into possible (inter-)institutional remedies and alternatives.

## Notes

1. In the three editions of *Social Theory and Social Structure* (1949, 1957, 1968b), in *The Sociology of Science* (1973b) and in *On Social Structure and Science* (1996), Merton actually misquoted the 1942 title, claiming it was ‘Science and Technology in a Democratic Order’, and not ‘A Note on Science and Democracy’.
2. Merton is here committing an ethnocentric fallacy – a fallacy of hasty generalization or of ‘unwarranted extrapolation’ (see Merton, 1968b: 559) – mistakenly presupposing that what happened in the development of the natural sciences also will take place in the social sciences. On ethnocentric fallacies in general, see Engelstad et al. (2005: 57–61, 106–8) and Kalleberg (2002: 171–80).
3. Merton’s positivistic interpretation of social science stimulates a too rigid distinction between history and systematics of social theory. Such a separation is generally easy to defend in the fields of the natural sciences, but is often inadequate and unfruitful in social theory and social philosophy (see Alexander, 1987). Merton rightly referred to a ‘puzzle’ that he named ‘the Phoenix phenomenon: the continuing resiliency of theories, ... such as Weber’s on the role of ascetic Protestantism ... even though they have been periodically subjected to much and allegedly conclusive demolition’ (1984: 1092). Merton’s tendency, however, to draw a scientific distinction between history and systematics of social theory, made it difficult or impossible for him to adequately identify and explain this essential phenomenon in the cultural and social sciences.
4. Such assumptions can be explicated as a type of simple-minded Kantianism, where one is reserving for oneself a trans-mundane field of pure reason and freedom in a noumenal world, whereas those studied belong to an empirical world characterized by situated reason and determinism. Obviously, these assumptions should not be confused with the type of ‘Kantian pragmatism’ developed by Habermas (2003: 83–130) or Hilary Putnam (2002).
5. Such a Durkheimian approach, where condemnation presupposes the legitimacy of norms and therefore can facilitate the identification of the norms violated, characterizes Habermas’s life-long inquiry into the institutional preconditions for rational discourse and symmetrical dialogue (see Habermas, 2004; also in Habermas, 2005).
6. After the influential article by Barnes and Dolby (1969: 8–11), Merton has often, mistakenly, been read as assuming that the norms of science are operative only in scientific institutions.
7. In an article from 1945, Merton mentions the role of being an ‘unattached intellectual’, whose ‘clientele is a public’ (1968b: 266). His conception here of a ‘bureaucratic intellectual’, working in

a staff function for policy-makers, is, however, more adequately conceptualized as the role of being an expert (or professional) working for a client (user) (see Kalleberg, 2005: 388). McCaughey's information about the year of the doctoral dissertation and Merton's year of death (see McCaughey, 2003: 378) are not correct.

## References

- Alexander, J.C. (1987) 'The Centrality of the Classics', pp. 11–57 in A. Giddens and J.H. Turner (eds) *Social Theory Today*. Stanford: Stanford University Press.
- Amsterdamska, O. (1989) 'Surely, You Are Joking, Monsieur Latour', *Science, Technology & Human Values* 15: 495–504.
- Aubert, V. (1965) *The Hidden Society*. Totowa, NJ: Bedminster Press.
- Austin, J.L. (1975) *How to Do Things with Words: The William James Lectures Delivered at Harvard University in 1955*, 2nd edn. Cambridge, MA: Harvard University Press. (1st edn 1962.)
- Barnes, S.B. and R.G.A. Dolby (1969) 'The Scientific Ethos: A Deviant Viewpoint', *European Journal of Sociology* 11: 3–25.
- Bloor, D. (1991) *Knowledge and Social Imagery*. Chicago: University of Chicago Press.
- Bok, D. (2003) *Universities in the Marketplace: The Commercialization of Higher Education*. Princeton, NJ: Princeton University Press.
- Borlandi, M. (1995) 'Should One Still Read Durkheim's Rules After One Hundred Years?' (Interview with Boudon), *Schweizerische Zeitschrift für Soziologie* 21(3): 559–73.
- Boudon, R. (1982) *The Unintended Consequences of Social Action*. London: Macmillan.
- Boudon, R. (1986) *Theories of Social Change*. Cambridge: Polity.
- Boudon, R. (1994) *The Art of Self-Persuasion: The Social Explanation of False Beliefs*. Cambridge: Polity.
- Boudon, R. (2001) *The Origin of Values*. New Brunswick, NJ Transaction.
- Boudon, R. (2004) *The Poverty of Relativism*. Oxford: Bardwell.
- Camic, C. and N. Gross (2001) 'The New Sociology of Ideas', Ch. 17, pp. 236–49. in J. Blau (ed.) *The Blackwell Companion to Sociology*. Oxford: Blackwell.
- Cole, S. (1992) *Making Science: Between Nature and Society*. Cambridge, MA: Harvard University Press.
- Dahl, R.A. (1989) *Democracy and Its Critics*. New Haven, CT: Yale University Press.
- Dahl, R.A. (1991) *Modern Political Analysis*. Englewood Cliffs, NJ: Prentice-Hall.
- Engelstad, F., C. Grenness, R. Kalleberg and R. Malnes (2005) *Introduksjon til samfunnsfag: Vitenskapsteori, argumentasjon og faghistorie* [Introduction to the Social Sciences: Theory of Science, Argumentation and History of Science]. Oslo: Gyldendal Akademisk.
- Fuller, S. (2006) *The Philosophy of Science and Technology Studies*. Oxon, UK: Routledge.

- Habermas, J. (1978) *Knowledge and Human Interests*. London: Heinemann.
- Habermas, J. (1984) *The Theory of Communicative Action*, Vol. I. Boston: Beacon.
- Habermas, J. (1987) *The Theory of Communicative Action*, Vol. II. Boston: Beacon.
- Habermas, J. (1989) *The Structural Transformation of the Public Sphere*. Cambridge, MA: MIT Press.
- Habermas, J. (1990) *Moral Consciousness and Communicative Action*. Cambridge: Polity.
- Habermas, J. (1994) *The Past as Future*. Cambridge: Polity.
- Habermas, J. (1996) *Between Facts and Norms: Contributions to a Discourse Theory of Law and Democracy*. Cambridge, MA: MIT Press.
- Habermas, J. (1998) *The Inclusion of the Other*. Cambridge, MA: MIT Press.
- Habermas, J. (2001) *On the Pragmatics of Social Interaction*. Cambridge, MA: MIT Press.
- Habermas, J. (2003) *Truth and Justification*. Cambridge, MA: MIT Press.
- Habermas, J. (2004) Public Space and Political Public Sphere – the Biographical Roots of Two Motifs in My Thought. Lecture, Kyoto, November 11.
- Habermas, J. (2005) *Zwischen Naturalismus und Religion*. Frankfurt: Suhrkamp.
- Habermas, J., L. von Friedeburg, C. Oehler and F. Weltz (1961) *Student und Politik: Eine soziologische Untersuchung zum politischen Bewusstsein Frankfurter Studenten*. Neuwied: Luchterhand.
- Hess, D. (1997) *Science Studies: An Advanced Introduction*. New York: New York University Press.
- Huff, T. (1993) *The Rise of Early Modern Science: Islam, China and the West*. New York: Cambridge University Press.
- Kalleberg, R. (2000) ‘Universities: Complex Bundle Institutions and the Projects of Enlightenment’, *Comparative Social Research* 19: 219–55.
- Kalleberg, R. (2002) Om vitenskapelig ydmykhet [On Scientific Humility], pp. 51–185 in *Samisk forskning og forskningsetikk*. Oslo: Forskningsetiske komitéer.
- Kalleberg, R. (2005) ‘What is “Public Sociology”? Why and How Should It Be Made Stronger?’, *The British Journal of Sociology* 56: 387–93.
- Kalleberg, R. (2007) ‘Sociologists as Public Intellectuals in the Norwegian Project of Enlightenment’, in S. Eliaeson and R. Kalleberg (eds) *Academics as Public Intellectuals*. London: Cambridge Scholars Press.
- Knorr Cetina, K. (1995) ‘Laboratory Studies: The Cultural Approach to the Study of Science’, pp. 140–60 in S. Jasanoff, G. Markle, J. Petersen and T. Pinch (eds) *Handbook of Science and Technology Studies*. London: Sage.
- Lafont, C. (1999) *The Linguistic Turn in Hermeneutic Philosophy*. Cambridge, MA: MIT Press.
- McCaughey, R. (2003) *Stand Columbia: A History of Columbia University in the City of New York, 1754–2004*. New York: Columbia University Press.
- Merton, R.K. (1942) ‘A Note on Science and Democracy’, *Journal of Legal and Political Sociology* 1: 115–26.



- Merton, R.K. (1949) *Social Theory and Social Structure: Toward the Codification of Theory and Research*, 1st edn. New York: Free Press.
- Merton, R.K. (1957) *Social Theory and Social Structure*, 2nd rev. and enlarged edn. New York: Free Press.
- Merton, R.K. (1968a) 'Science and the Social Order', pp. 591–603 in *Social Theory and Social Structure*, 3rd enlarged edn. New York: Free Press. (Orig. pub. 1938.)
- Merton, R.K. (1968b) *Social Theory and Social Structure*, 3rd enlarged edn. New York: Free Press.
- Merton, R.K. (1986c) 'Science and Democratic Social Structure', pp. 604–15 in *Social Theory and Social Structure*, 3rd enlarged edn. New York: Free Press.
- Merton, R.K. (1968d) 'The Self-Fulfilling Prophecy', pp. 475–90 in *Social Theory and Social Structure*, 3rd enlarged edn. New York: Free Press. (Orig. pub. 1948.)
- Merton, R.K. (1970) *Science, Technology and Society in Seventeenth-Century England*. New York: Howard Fertig. (Ph.D. dissertation completed in 1935; orig. pub. 1938.)
- Merton, R.K. (1973a) 'Priorities in Scientific Discovery', pp. 286–324 in *The Sociology of Science: Theoretical and Empirical Investigations*. Chicago: University of Chicago Press. (Orig. pub. 1957.)
- Merton, R.K. (1973b) *The Sociology of Science: Theoretical and Empirical Investigations*. Chicago: University of Chicago Press.
- Merton, R.K. (1976) *Sociological Ambivalence and Other Essays*. New York: Free Press.
- Merton, R.K. (1977) 'The Sociology of Science: An Episodic Memoir', pp. 3–141 in R.K. Merton and J. Gaston, *The Sociology of Science in Europe*. Carbondale: Southern Illinois University Press.
- Merton, R.K. (1984) 'The Fallacy of the Latest Word: The Case of "Pietism in Science"', *American Journal of Sociology* 89: 1091–121.
- Merton, R.K. (1985) *On the Shoulders of Giants*. New York: Harcourt Brace Jovanovich.
- Merton, R.K. (1990) 'STS: Foreshadowings of an Evolving Research Program in the Sociology of Science', pp. 334–71 in I.B. Cohen (ed.) *Puritanism and the Rise of Modern Science: The Merton Thesis*. New Brunswick, NJ: Rutgers University Press.
- Merton, R.K. (1996) *On Social Structure and Science*. Chicago: University of Chicago Press.
- Merton, R.K. (2004) 'Afterword: Autobiographic Reflections on *The Travels and Adventures of Serendipity*', pp. 230–98 in R.K. Merton and E. Barber, *The Travels and Adventures of Serendipity: A Study in Sociological Semantics and the Sociology of Science*. Princeton, NJ: Princeton University Press.



- Nowotny, H., D. Pestre, E. Schmidt-Assmann, H. Schulze-Fielitz and H.H. Trute (2005) *The Public Nature of Science under Assault: Politics, Markets, Science and the Law*. Berlin: Springer.
- Putnam, H. (2002) *The Collapse of the Fact/Value Dichotomy and Other Essays*. Cambridge, MA: Harvard University Press.
- Restivo, S. (1995) 'The Theory Landscape in Science Studies: Sociological Traditions', pp. 95–110 in S. Jasanoff, G. Markle, J. Petersen and T. Pinch (eds) *Handbook of Science and Technology Studies*. London: Sage.
- Searle, J. (1984) *Minds, Brains and Science*. London: Penguin.
- Sen, A. (2002) *Rationality and Freedom*. Cambridge, MA: Belknap Press.
- Skirbekk, G. (2005) 'Science Studies and Moral Challenges: Making It Explicit: An Updating of Science Studies', *Sats: Nordic Journal of Philosophy* 6(2): 51–78.
- Skjervheim, H. (1996) *Selected Essays*. University of Bergen: Department of Philosophy.
- Sztompka, P. (1986) *Robert Merton: An Intellectual Profile*. New York: St Martin's Press.
- Tranøy, K.E. (1977) 'Norms of Inquiry: Methodologies as Normative Systems', pp. 1–13 in G. Ryle (ed.) *Contemporary Aspects of Philosophy*. London: Oriel Press.
- Tranøy, K.E. (1986) *Vitenskapen – samfunnsmakt og livsform* [Science – Societal Power and Form of Life]. Oslo: Universitetsforlaget.
- Tranøy, K.E. (1988) *The Moral Import of Science*. London: Sigma Forlag.
- Zuckerman, H. (1988) 'The Sociology of Science', pp. 511–74 in N. Smelser (ed.) *Handbook of Sociology*. Newbury Park, CA: Sage.
- Zuckerman, H. (1989) 'The Other Merton Thesis', *Science in Context* 3: 239–67.

-----

Ragnvald Kalleberg is a Professor of Sociology at the University of Oslo. He has published in sociology of science (history of science, academics as public intellectuals, research ethics, research policy), sociology of organizations (knowledge organizations, work environment improvements, workplace democracy) and general social theory (theories of modernity, philosophy of science, discourse ethics). He has recently chaired the Norwegian National Committee for Research Ethics in the Social and Cultural Sciences for two periods, and tried to translate sociological insights into realistic institutional guidelines for research ethics.

Address: Department of Sociology and Human Geography, University of Oslo, P.B. 1096, 0317 Oslo, Norway. [email: ragnvald.kalleberg@sosiologi.uio.no]