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ARTICLE

The "Normative Structure" of Social Science:

Merton's Ideas as a Story of Success and Side Effects

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Abstract

Based on available literature, this essay looks at trends in scholarly attitudes and academic practices, primarily within the sphere of social sciences, and asks whether they have been in line with Robert K. Merton's institutional principles of science as they were formulated in his famous essay "The Normative Structure of Science." This essay argues that these principles have not been fully implemented but have become increasingly recognised and widely accepted as normative points of reference also in large parts of the social sciences. However, there have been both marked deviations and significant side effects. Given the internal heterogeneity of a discipline like sociology, practices that selectively interpret the Merton principles may add to existing internal cleavages.

Keywords

Robert K. Merton, sociology of science, scientific ethos, Mertonian norms, scientific practices, research, scholarship

INTRODUCTION

It has been 80 years since Robert K. Merton originally published his famous essay "The Normative Structure of Science" (1973 [1942]), in which he outlines what he regards to be fundamental features of the scientific ethos. Merton wrote the paper in the face of World War II and the emerging Cold War; most prominently, he observed a totalitarian regime severely controlling and using science—Nazi Germany—and consequently made a case for the autonomy of science (Turner 2007). Initial ideas had already been presented during a lecture in 1937, and Merton also drew upon insights from his empirical dissertation on early experimental science in England (Merton 1938 [1936]).

While it is far from evident that scientific practices at the time were indeed fully in line with the principles formulated by Merton, it is indisputable that the academic world (as with the world as a

whole) has changed significantly since then. It therefore seems appropriate to take stock of how far academia has developed along the lines sketched by Merton. This paper aims to contribute to that goal, with particular interest in the context of the social sciences. In his essay, Merton (1973 [1942]) gave examples predominantly from the field of the natural sciences. However, the abstract and fundamental nature of the outlined principles seemingly lifts them above both disciplinary boundaries and possible intradisciplinary divisions and disputes. It is thus no surprise that Merton's contribution has frequently also served as a point of reference in the practices of social science—as well as being a focus of conceptual critique by sociologists of science.

Based on available literature, this essay examines recent developments and the present situation of scholarly attitudes and scientific practices, particularly within the sphere of the social sciences. The overarching question is to what degree they are consistent with the institutional principles formulated by Merton. The first section of this paper briefly summarises and interprets Merton's concept of the ethos of science and further specifies this paper's perspective. Then the main part discusses recent developments in the social sciences, structured by the four "institutional imperatives" proposed by Merton (1973 [1942]: 270). Building on that, the following section discusses implications for the internal heterogeneity of sociology. A short section concludes the paper with a summary of arguments focused on the unfinished quality and possible side effects of the implementation of the Merton principles.

MERTON'S CHARACTERISATION OF THE SCIENTIFIC ETHOS

The selection of fundamental scientific values and norms presented by Merton (1973 [1942]) consists of four principles: *universalism*, (academic) *communism*, *disinterestedness* and *organised scepticism*. Merton neither invented the corresponding rules from scratch nor reported a formally codified ethos. Rather, he gave an ideal-typical summary of implicit social rules that had developed in modern science over the preceding centuries, forming what he saw as a moral consensus among scientists. Later work (e.g. Ziman 2000) has used the term *communalism* rather than communism and has added *originality* to the list, leading to the now well-known acronym CUDOS. The following paragraphs give a brief sketch of Merton's original arguments.

Universalism refers to the general goal of scientific practice, the quest for truth, and the idea that personal characteristics of scientists are irrelevant for both the validity of their arguments and their access to positions in science (cf. Merton 1973 [1942]: 270–273). Truth claims "are to be subjected *to preestablished impersonal criteria*: consonant with observation and previously confirmed knowledge" (Merton 1973 [1942]: 270, original emphasis).

"*Communism"* (original quotes) means in this context that scientific knowledge is a common good that is to be shared (cf. Merton 1973 [1942]: 273–275): "The substantive findings of science are a product of social collaboration and are assigned to the community" (Merton 1973 [1942]: 273). This academic communism is not perceived as contradictory to the personal recognition of individual scientists and their contributions.

As with the other principles, *disinterestedness* is emphatically understood as an institutional feature rather than an individual trait of scientists. According to Merton (1973 [1942]: 276), the behaviour

of scientists is characterised by a "distinctive pattern of institutional control of a wide range of motives." The principle of disinterestedness means that in their search for truth, scientists do not receive any personal advantage other than gains in their reputation (cf. Merton 1973 [1942]: 275–277). Giving other scientists adequate credit for their work is good practice, and the principle rules out the use of unfair means (including fraud) by individuals; in fact, Merton (1973 [1942]: 276) speaks of "the virtual absence of fraud in the annals of science."

Finally, *organised scepticism* denotes the imperative that every scientific statement is, in principle, subject to critical examination, not least in the form of scientific competition. Contrary interests of, in particular, state authorities or religious communities, are to be considered irrelevant (cf. Merton 1973 [1942]: 277–278). Once again, the mission of science is defined as asking "questions of fact, including potentialities, concerning every aspect of nature and society" (Merton 1973 [1942]: 277).

These norms are not only acquired by scientists through socialisation. They are also enforced by institutionalised social control within the community of scientists. Merton's presentation is concise and general, and at least from today's point of view, many of these ideas may look self-evident. In fact, together with the decided message regarding autonomy, Merton outlines a specific and historically contextualised model of science. In the following, we give a stylised interpretation of Merton's concept. The aim is not to present a comprehensive analysis but to highlight respective points of reference for the arguments proposed in the subsequent section, where we concentrate on what has become relevant in scientific practice.

The first implicit but central characteristic is the focus on research and innovation. This means that (modern) science is seen primarily as being about discovery and progress; it is much less about the systematisation (as in Merton's own essay), preservation and transfer of traditional knowledge or about auxiliary practices such as translations. Insofar as the focus is on a particular segment when compared to practical academic life, the specified norms serve an instrumental role: "The institutional goal of science is the extension of certified knowledge. […] The institutional imperatives (mores) derive from the goal and the methods" (Merton 1973 [1942]: 270). This means, for example, that universalism is primarily not a matter of justice. Rather, a violation of universalistic principles is regarded as a problem because the successful evolvement of knowledge may be endangered when the system of science cannot generally make use of the best human resources available. Negative consequences will not apply in every individual case, but as a matter of principle, even minor violations of such general norms may be perceived as precarious.

Second, science is conceptualised as a socially organised endeavour that entails a highly developed division of labour. The road towards truth is seen as a cumulative project with many potential contributors. Individual curiosity, legitimate self-interest and competition together are supposed to bring about the desired collective good, and Merton's (1973 [1942]) focus is on the basic rules guiding this social process. The social structure of the process itself is conceptualised in what is ultimately a very simple form. Merton's model is concerned with relationships among scientists or between individual scientists and the scientific community as a whole; although his essay is essentially about the organisation of the process of scientific knowledge, there is little explicit reference to intermediary scientific organisations.

Finally, the evaluative focus of the ideal-typical model is on science as a system, not as a multitude of individual scientists. Hence, the model is not concerned with the benefits for specific organisations nor for individuals; it is about the quality and progress of science as a whole, and the model approaches it from a long-term perspective. For the collective, a small and very specific contribution

may be highly valued, while a well-educated scientist's broad and deep knowledge may be irrelevant if it does not transcend the well-known in any respect. In practice, however, very high levels of specialisation may restrict utility on the local level, as measured by the quality of available scientific advice, studying conditions within a specific department, and so on.

Merton's concept has become famous but has also received a long list of criticisms, and questions have already been raised in conceptual terms. For example, fundamental points of critique against Merton (but also his critics in the historical tradition of Kuhn [1962]) were raised by Stehr (1978) concerning the status of the model (ideal type vs. a reasonable description of practices). Most important is an inconsistency or a "theoretical break" as Merton's arguments can be related to either cognitive or social norms. Another line of criticism points to a too-narrow focus and a lack of consideration of internal heterogeneity and contingency of scientific discourses and practices (representing different "thought collectives" and "thought styles"; Fleck 1980 [1935]). The relevance of smaller scale epistemological contexts is particularly obvious for the humanities and social sciences, but it is not limited to these disciplines. According to Daston (1995), science as a whole depends on a broader moral economy. Unlike the norms formulated by Merton, moral economies are historically created, modified and destroyed; they are enforced by culture rather than nature and are therefore both mutable and violable—and they are integral to scientific ways of knowing.

In the following sections, the focus is not on a further conceptual or theoretical critique of Merton's ideas. Our discussion is concerned with existing social practices and supported normative standards in social science and the question of the degree to which we can regard them as consistent with the Merton principles. Of course, not every change in specific practices is a challenge to the validity of the generic rules. Also note that compliance with the principles does not necessarily imply any "objective" qualities (e.g. validity) of scientific results in particular cases. The cited sources come primarily from Europe and North America.

RELEVANT TRENDS IN (SOCIAL) SCIENCE

A. Universalism

Regarding the principle of universalism, we focus on chances for access to and visibility within the academic field. This problem has at least two aspects. In terms of the personal aspect, the central issue is researchers' equal access to academic positions and relevant resources; in terms of content, the issue is appropriate participation in the (potentially global) scientific discourse of the respective discipline.

Recruitment and career patterns provide meaningful indicators in the personal dimension. Over the last few decades, many activities have aimed at creating equal opportunities in science (e.g. codes of conduct, equality reports, equal opportunity officers and commissions, etc.). The mere extent of institutionalised effort, at least in Western academic institutions, indicates that equal opportunity has become a widely shared norm in line with universalistic values. Still, there have been considerable social inequalities at the expense of women and minorities, which contradicts the factual validity of the universalistic principle in contemporary (social) sciences (Long and Fox 1995); typically, the proportion of women declines from one step of the academic career to the next, a pattern often interpreted longitudinally as a "leaky pipeline" (U-Multirank 2021). Underlying causes are obviously

more complex. For example, Box-Steffensmeier et al. (2015) found no significant differences in faculty retention rates at academic institutions in the United States. Analysing appointment procedures, Jungbauer-Gans and Gross (2013) found that sociology departments in Germany even revealed a relative preference for women when performance indicators were controlled (and to give high levels of relevance to publications). Interpreting publications directly as meritocratic indicators of performance assumes that authorship itself has been the result of universalistic processes and not of organisational power structures, seniority or personal assertiveness. In this regard, there have been repeated complaints about the under-recognition of women's academic achievements ("Matilda Effect"; Rossiter 1993), and there are gender differences regarding publications and citations. Also, women in sociology are under-represented in both the prestigious first and last author positions (West et al. 2013).

While the formal goal of equal opportunity seems to be beyond question within the scientific community, corresponding practices for achieving it have been more controversial. For example, policies of affirmative action may themselves be perceived by individuals as a conscious violation of universalistic principles. Moreover, equal opportunity is not just about personal characteristics; unequal resources are often provided by organisational or institutional backgrounds (Long and McGinnis 1981). In addition, a focus on questions of individual access to given positions may be too narrow because scientists' career chances depend on the supply of relevant positions, which is affected by, for example, the organisational expansion or contraction of scientific disciplines and subdisciplines. As Bourdieu (1992) and others have emphasised, such developments are not necessarily exogeneous to a scientific community. Rather, they are subject to strategic mechanisms regarding both the definition of specifically denominated positions and the labelling of candidates as having "relevant" qualifications for a particular domain. Mechanisms of social closure can be expected especially in the social sciences, where consensus about scientific achievement is often hard to reach. In a situation of intradisciplinary conflicts, unilateral adherence to resource-intensive universalistic procedures may even constitute a competitive disadvantage. Beyond academic micro-politics, context conditions such as demographically determined opportunity structures (cf. Hillmert 2003; Zuckerman and Merton 1972) may also put restrictions on equal access to the world of science. Hence, equal opportunity cannot be reduced to the absence of direct discrimination against individuals and compared to substantiating a systematic violation of universalistic principles; it is more difficult to positively confirm that career patterns have indeed been fully in line with meritocratic principles.

In terms of content, procedures such as double-blind peer review (see also the later subsection on organised scepticism) aim to enforce universalistic principles in selection procedures. However, perceived problems with universalism in scientific discourse obviously run deeper, and recent years have seen an intensification of the debate. In particular, feminist and postcolonial perspectives have highlighted blank areas in seemingly "universal" academic discourses. Indicators are seen in their canonical but narrowed content, including the dominant role of Western classics and the grand narrative of global modernisation originating in Europe, which is regarded as a reflection of Eurocentrism and academic imperialism (Bhambra 2011; Gutiérrez Rodríguez et al. 2010). Compared to other academic disciplines, this debate started relatively late in sociology (McLennan 2003), and elaborated alternatives to the conventional mainstream are often not prominent (Alvares 2011). Also, in quantitative terms, the global academic discourse has been dominated by scientists from Western countries (UNESCO/ International Social Science Council [ISSC] 2010), which reflects both barriers to entry and inequality of resources in a highly competitive field. Even among Western countries, there are clear differences in publication activities so that an intended internationalisation in respective journals may have resembled an Americanisation of publishing (Vanderstraeten and Eykens

2018; in contrast, Kwiek 2021). In explicit opposition to universalism have been reports of "hate speech," along with claims for "political correctness" and exclusive authority, which have also found access to academia (Lea 2009; Revers and Traunmüller 2020). The social sciences seem particularly sensitive to these debates, given that the field has close links to the discourse in politics and society. It has also been noted that a scientific debate in the sense of organised scepticism may sometimes be tough and that the demand for universalistic "academic freedom" should not be used to immunise personal points of view against all criticism.

B. Communism

We now turn to the principle of academic "communism" and look at practices of scientific collaboration, discussion and publication. In principle, social science seems to offer a good basis for ideas of open science. The broad dissemination of their knowledge is typically a social scientist's dominant (if not sole) interest; unlike, for example, in various technological fields, there are few restrictions by patents, military secrets, etc. Describing features of academic communism, we can generally distinguish between, on the one hand, questions regarding the individual researchers' capacity and willingness to share scientific knowledge and, on the other hand, questions regarding the recipients' problem of finding relevant knowledge and accessing it. Willingness to widely share novel knowledge is a strong expectation in contemporary (social) science and is not least fostered by considerable pressure to "publish or perish." Recent decades have seen an unprecedented expansion of scientific information in general. Following the idea that growth in science is the "fundamental law of any analysis of science" (De Solla Price 1963: 5), it has been estimated that the overall number of scientific journals has grown exponentially, with a nearly constant annual growth rate of approximately 3.5% over the last three centuries (Mabe 2003; Tenopir and King 2014). Furthermore, social science has adapted practices traditionally prevalent in the natural sciences, including a high degree of specialisation and an increased division of labour. Work on a specific question nowadays often takes the form of project work. Empirical data and the corresponding, often large-scale, infrastructure have played an increasing role in research, and various initiatives have been launched to ensure broad and sustainable accessibility of information (cf. the FAIR data principles; Wilkinson et al. 2016). Intensified collaboration also has consequences for topics of research, given that these are designed and assessed with respect to their fit, or at least connectivity, with the (global) system of research.

In the natural sciences, journals became the dominant form of publication during the 19th century (Csiszar 2018), and a standard form of the scientific article had evolved by about a hundred years ago (Di Trocchio 2006: 252f.). Over the course of the 20th century, publications with multiple authors almost completely replaced the initially dominant single-author paper (De Solla Price 1963: 87ff.). Papers in scientific journals have become important means of information also in large sectors of the social sciences, and in recent decades, publications written by groups of authors have also gained prominence in these fields (West 2013). Globalised discourses in scientific disciplines already existed in the 19th century, but national communicative boundaries did not vanish in the face of advanced means of communication, and there have been repeated episodes of nationalisation in the discourses during the last two centuries (Thelen 2011). Many parts of the social sciences have just recently become truly international (cf. Heilbron et al. [2017] regarding European journals and Boncourt [2017] regarding professional associations).

The most important recent changes have been brought about by digitalisation, which has been particularly visible in the form of the Internet. Digitalisation has once again increased both the global

scope and the speed of the dissemination of scientific information. However, publications (particularly older ones) that are not electronically accessible are at risk of falling behind. Innovations such as "online first" journal publications and publications on preprint servers meet the demand through their attempts to speed up the publication process, even under conditions of mass production. There is also a conspicuous rise in *predatory journals* offering dubious publication for profit (Beall 2012). Conversely, increased competition in academia (see also the following section on disinterestedness) is likely to *decrease* willingness to share information after (or even before) publication. Even with regard to publications, the general imperative of dissemination does not mean that all knowledge can in fact be easily shared. On the contrary, the question of how to get published and become visible within the scientific community is at least as pressing as it was prior to the introduction of electronic media. Today, academic recognition of publications is increasingly conditional on meeting the criteria of quality control. In practice, this means that restrictions for the sharing of legitimate knowledge are primarily set by gaining access to reputable journals (and/or publishers) and peer review prior to potential publication (see also the later subsection on organised scepticism). Journal editors and reviewers act as gatekeepers in this process. Further barriers include printing costs (traditionally) and (increasingly) fees for digital publications in a pay-to-publish open-access model. Such barriers still pose problems for scientists who are individually or collectively in low-income situations.

On the side of reception, informational expansion makes it increasingly challenging for participants in scientific discourse to find their way through the multitude of contributions that are heterogeneous in terms of both thematic focus and quality. As one consequence, new genres of publications have been established or have gained relevance, such as summaries, overviews or, most recently, "structured literature research." Standardised rules for conducting and presenting the information search itself have been developed and promoted (cf. Page et al. 2021). In spite of policies such as open access, barriers to access have also remained for readers, such as conventional publications behind high-cost pay walls and data monopolies. Additional obstacles for specific researchers include, for example, rich empirical data which are often available for users only in "secure data access" mode, meaning under restrictions and on-site in special research data centres. While normally established for good reasons of data protection, this situation may require considerable effort and resources, depending on the researcher's personal situation. Once again, this situation favours scientists in more affluent organisational and national contexts.

C. Disinterestedness

Regarding the principle of disinterestedness, we look at potential changes in motivation due to changes in the social system of science. Merton's model is concerned with individual scientists, their scientific community and the mutual relationships between individuals and the community. In practice, (intermediate) organisations have always played a major role in the construction and processing of scientific knowledge—be it working groups, universities and research institutes or professional organisations. Today's academic world is characterised by a dense organisational network of growing complexity and competition, including large-scale collaborative centres of research and close links to translational and applied science. Such organisations have brought additional expectations for scientific practices that may conflict with traditional academic norms.

Recent organisational changes comprise forms of governance (*New Public Management*: Broucker and De Wit 2015) that have introduced various instruments of external control and market princi-

ples. Managerial instruments include the use of quantification, rankings, and more, and they typically work against the backdrop of restricted budgets. The standards of evaluation are not necessarily defined by science itself; for example, global university rankings have increasingly become the business of private companies (Chen and Chan 2021). Academia is consciously seen not as homogeneous but rather as particular organizations competing with each other. Digitalisation has made extensive comparisons much easier. Activities such as securing third-party funding, institutional profiling and marketing also play increasing roles in social science. Moreover, there has been an institutional concentration and a tendency towards larger research clusters, often accompanied by a loss of autonomy and visibility of scientific disciplines *as* disciplines following an increase in centralised organisational power, which was previously unknown to social science. Contemporary corporate values may be in line with Merton's model, as in the case of diversity and equal opportunities, which match with the principle of universalism, but they may also be in opposition to Mertonian ideas, as is the case with economic imperatives and demands for practical applicability that do not align with the idea of scientific curiosity and the pursuit of knowledge for its own ends.

According to Merton (1973 [1942]), individual reputation is not a primary scientific goal, but it is a legitimate incentive for scientists. In that sense, science has never strived for equality, and it is well known that productivity is highly unequally distributed among scientists in that a small minority of authors is responsible for a large share of publications (De Solla Price 1963; Lotka 1926). However, it is debatable whether a phenomenon like the cumulative Matthew effect in science (described by Merton 1988) is an expression of or contradictory to universalistic principles. Scientists' achievements arguably endow them with a fair share of attention but not a disproportionate reputation and access to resources. A disproportionate reverence for academic icons is likely to put obstacles in the way of newcomers and impede the refreshing of knowledge—not unlike traditional forms of social closure based on seniority and formal status. It may eclipse successful attempts at falsification and make contributions of following researchers hard to distinguish when they draw largely upon the very same canonical sources.

Various observers believe that changes in the organisation and governance of science have led to fundamental shifts in collective and individual motivation (e.g. Oancea 2014). Reputation and status are seen as becoming individual scientists' primary goals rather than mere derivatives from a successful quest for "truth" (cf. Schimank 2010). In practice, this has consequences, such as publishing in a prestigious journal becoming a goal unto itself rather than a means of communicating a relevant finding. Technical criteria, such as the number of peer-reviewed publications, serve as common criteria in hiring processes and are explicitly stated in job advertisements today as an expectation. This further increases the pressure to "publish or perish." Self-promotion and strategic behaviour (e.g. splitting publications into "smallest publishable units" or the repeated publication of similar content) are consistent consequences. These may increase individual success but do little to advance the respective scientific discipline or the system of science as a whole.

Increased competition also leads to various practical disturbances. Disputes about authorship and, given the symbolic value of name ordering (Zuckerman 1968), even the order of authorship have become typical controversies in academic practice. Recent reports of scientific malpractice and extensive plagiarism have sparked debates about the negative effects of intensified academic competition over resources. It is unclear to what degree the social sciences are subject to fraud produced on an industrial scale ("fake-paper factories" or "paper mills"), as observed in resource-intense disciplines such as medicine (Else and Van Noorden 2021). However, authors like Weingart (1998; 2008) see the root causes of academic misconduct as the commodification of science, the resulting partial

interests, strategic behaviour and competition over resources and the attention put on various levels of, for instance, organisations and (sub-)disciplines as well as on the level of individual scientists.

D. Organised Scepticism

The final focus of this section is on trends in scientific autonomy and quality. The paragraph on organised scepticism is the shortest of the respective subsections on the four principles in Merton (1973 [1942]), but it is closely associated with the other principles. Intrusions by state or religious authorities into the system of science as they were envisaged by Merton still take place in many parts of the world, and there has been a history of lobbyism and disinformation disguised as (alternative) science (Oreskes and Conway 2010). Most prominently, however, science has been internally concerned with discussing practices of quality assurance, a term that has also become familiar to social scientists. While a realistic self-assessment may indeed be beneficial for individual scientists, science as a whole does not rely on individual self-discipline but rather on a collective and cumulative process of mutual checks to ensure scientific progress. Merton (1973 [1942]) was not explicit about details, but the concept of institutionalised forms of scepticism has close affinities with ideas of potential falsification and replication in the sense of Popper (1989 [1934], 1962). The collective quality assessment of science has a long-term time horizon: In an evolutionary manner, time (or "history") is supposed to tell what the correct and important findings are.

Strict examinations of statements in the form of experiments have been rare in the social sciences. Instead, more indirect tools of quality assessment have been used. Consenting citations are very common, albeit very weak, forms of approval. Although often based on pure plausibility, they establish some consensus about accepted knowledge and the state-of-the-art in a particular (sub-)discipline. An essential element of organised scepticism is transparency, and there have been prominent pleas for actually reproducible science (Munafò et al. 2017). Following this idea, in recent years, advanced and formal practices of open science and transparency—pre-registration of studies, publication of protocol and replication materials, etc.—have been promoted and endorsed also in the social sciences (e.g. Christensen et al. 2019a; Freese and Peterson 2017). They have come against the background of a critical discussion about replication and a perceived replication crisis, meaning a soberingly low proportion of actually replicable published findings, potentially undermining the credibility of entire disciplines (Ioannidis 2012). The applicability of replication has remained controversial in the areas of the social sciences, relying more strongly on theory or qualitative methods, although this should not imply a general exemption from demands for replicative checks in every part of the respective research. Concerning other means of organised scepticism, there has been a boom in assessment practices, such as institutional evaluation and auditing and advisory boards, which have also become standard for institutions and projects in the social sciences.

By far, the most effort during the last decades has been invested in practices of *peer review*. Merton himself repeatedly emphasised the importance of the academic referee system, especially for those who are not themselves experts on a specific subject (e.g. Zuckerman and Merton 1971). However, peer review follows in practice a specific idea of organised scepticism. Unlike the prototypical model of a permanent, open and collective challenging of scientific statements, peer review is typically concerned with *ex ante* (pre-publication) decisions about suitability made by the persons in charge. As the term "peer" suggests, the decision-makers' authority stems primarily from a difference in roles, not necessarily superiority in knowledge per se, and the idea is that these roles regularly take turns

among peers. Quite naturally, decisions are often made on the grounds of plausibility and consistency with existing evidence in logical or formal aspects, but the presented empirical findings are typically not explicitly tested by replication in this mode. It is therefore no surprise that, in the review processes, there is often no direct reference to aspects of validity. A typical journal provides reviewers with the following (or a similar) list of criteria for the assessment of manuscripts: relevance, novelty, internal consistency, fit with the journal, plausibility of manuscript structure and clarity of presentation. Criteria like these make sense, particularly in light of a collaborative model of the scientific process. Comprehensibility (if possible, also for the quick reader) and connectivity to the existing body of work have become important assets because they enable the work to become part of the envisaged cumulative process of knowledge building.

Still, systematic shortcomings remain in the peer review process. Reviewers' ex ante assessment is necessarily based on currently available knowledge. Arguments such as "not consistent with the state of research" may be legitimate; however, there is a thin line towards a potential *reversion* of the fundamental falsification logic: If, according to Merton and others, the basic goal of science is to *question* established knowledge, then demanding *consistency* of individual contributions with established knowledge appears to be an inadequate criterion for definitively assessing the quality of scientific work.

Peer review should be rated on its goals and possibilities (Ware 2011). It is certainly a useful instrument for identifying technical flaws, inconsistencies, redundancies, etc. in presented research, particularly under conditions of "normal science" (Kuhn 1962). Historically, it has also served as a means of external societal and political legitimation of science (Csiszar 2016). However, there are already imponderables on the practical level, simple errors and biased decisions due to personal conflicts, for example, which may not be completely unavoidable. The more severe criticisms put forth include the notoriously low consistency among reviewers (Cole et al. 1981), openness to strategic behaviour of both authors and reviewers—anticipations of "what the reviewers might like to read," self-interested requests for revisions and citations, etc.—and explicit fraud (Wennerås and Wold 1997). Social science seems particularly sensitive to potential dysfunctions of peer review because the perceived relevance of results is even more subject to social consensus among peers than has been shown for paradigms in the natural sciences (cf. Kuhn 1962). In any case, systemic bias towards the present, represented by the respective state-of-the-art of common knowledge, is not fully in line with the idea of continuous scepticism in an evolutionary process of scientific progress. Quite often, scientific quality and, perhaps more so, relevance can be assessed only with some historical distance. There is a long list of (later) prominent papers, including those of Nobel laureates, that were initially rejected by journals but, once published, were immediately highly cited (Campanario 2009; Slavov 2014). By no means does every rejected paper suggest brilliance, but in principle, ex ante peer review as it is commonly performed has systemic blind spots. For a comprehensive implementation of organised scepticism, it is not a thorough alternative to repeated (post-publication) examination and testing by the scientific community, making for an evolutionary and competitive struggle for truth. Of course, potential replications require that research is originally presented in a replicable format.

An additional argument for peer review as an implementation of organised scepticism is the selection function in light of experienced information (publication) overload. As it turns out, however, rejected papers tend to be resubmitted to other journals, often many times (cf. anecdotally, Gans and Sheperd 1994). This means that the capacity problem is shifted or even enforced as the volume of reviews increases dramatically. Moreover, collective quality may be affected if final success after multiple

rounds of (re-)submission becomes a matter of personal stamina rather than scientific content, a development facilitated by the low consistency among reviewers' ratings. Alternatives to the dominant form of (nominally) anonymous and ex ante peer review—such as accompanying reviews or post-publication assessments—have been proposed but have remained largely marginal so that reviews often function as verdicts rather than supportive correctives. One likely reason is that these alternatives would require additional effort in a situation where many scientists already experience the frequent requests to act as reviewers as an onerous burden with no immediate pay-off (see the previous section on sources of individual motivation).

RESEARCH, SCHOLARSHIP AND THE INTERNAL STATE OF SOCIOLOGY

While science, as a whole, is affected by the trends summarised in the previous section, they do not apply to all scientific disciplines to the same degree. Moreover, divergent scientific practices may exist simultaneously within the same discipline. This is particularly obvious in fields as heterogeneous as sociology. In a historical case study, Jaworski (1998) reconstructed the contested interpretation of Georg Simmel's work at two neighbouring academic institutions in New York (Columbia University and New School for Social Research) in the 1950s. According to Jaworski, Robert K. Merton (at Columbia) succeeded in pushing his primarily structural interpretation of Simmel, using it as a point of departure for his own research programme. As a basis of his analysis, Jaworski (1998) sketched various ideal-types of academic work:

> A useful strategy for comparing intellectual and professional styles of work is to chart them along an intellectual continuum. At one end of the continuum lies "scholarship," that is, intellectual work in harmony with humanist intellectual traditions. Assuming the unity of knowledge, this approach links sociology to literary, philosophical, and historical projects. Texts are examined historically and systematically, employing *explication du texte*, and biography is accepted as relevant to understanding a thinker. At the other end of the continuum is "abstracted empiricism," to borrow C. Wright Mills's (1959) phrase, in the scientific intellectual tradition. Work in this vein is characterised less by its questions than by its techniques. Armed with a positivist philosophy of science, it measures all knowledge by a restricted yardstick of truth. […] In the middle of the continuum is a style of work that shares in abstracted empiricism's instrumentalism, its goal of utilizing a text for some purpose, and in scholarship's universalism, its desire to advance intellect and learning. This middle course can be called the "research program." (Jaworski 1998: 5, emphasis in the original)

Also, in our present situation, the major alternative to a dominant model of professional research programmes can be found in a networked "world of (individual) scholars." Despite an academic reality dominated by busy daily routines, this model, rooted in the tradition of the humanities, is still popular. Rhetorical tribute is also paid to scholarly ideals in highly institutionalised settings, not least in academic commemorative speeches. A further alternative, particularly for social scientists, is seeking attention from mass media and the general public, taking on roles such as "public intellectual." Due to the different logic of the media system, this demand-based alternative may, in principle, be a way to circumvent procedures of internal quality control as specified by Merton and others. Success in the public market requires a good mixture of both accessibility and timeliness.

In the following, we concentrate on the distinction between the first two, the academic models. The distinction should not obscure the fact that, on a general level, there is a considerable amount of

normative consensus. In particular, there has obviously been broad nominal acceptance of the Merton principles among social scientists (e.g. Christensen et al. 2019b), albeit to a markedly different degree (cf. Macfarlane and Cheng 2008), and corresponding formulations regarding, for example, issues of transparency can be found in codes of conduct for safeguarding good scientific practice (e.g. DFG 2019). Also, our major distinction between the two ideal-typical alternatives of being a "researcher" or a "scholar" is not primarily directed at controversies about "truth" in the sense that these models imply incompatible divergent propositions about the world. They do not even necessarily compete with respect to the specific methods they perceive as adequate for getting closer to truth. Rather, the distinction relates to the "para-methodology" of practices in organised science and a number of corresponding decisions. Such a distinction may be very fine and concern seemingly mundane aspects, but it may become important (and controversial) when explicit decisions need to be made and legitimised, such as in the context of recruitment. Such practices can often be related to specific attention to or interpretation of the Merton principles. The following paragraphs present examples in a stylised form, relating them to the two ideal-typical models.

Focus and systematisation: The dominant model is close to Merton's concept insofar as it has an explicit focus on a process of continuous research activities. It may aim for novel "discoveries," but following the idea of cumulation, there is a focus on research questions that connect to the existing state of research. Therefore, there is a preference for specific questions that can be easily processed within the scientific community and that attract a sufficiently large group of like-minded researchers. In practice, few thematic areas in sociology have shown a long history of cumulative and replicative research on an international scale; an example would be social mobility research. A necessary requirement for broad scientific collaboration is, obviously, a restricted set of precise, relatively simple research questions. Research following the dominant model has invested a lot in the sophisticated organisation of scepticism. In particular, it aims for highly organised procedures and adherence to controlled standards in analytical work. This includes a plea for an intensified use of controlled methods, such as lab experiments (e.g. Falk and Heckman 2009).

The alternative scholarly model is less systemic and less based on the division of labour. Publications are typically more comprehensive and complex. This poses restrictions on potential replicability as a central element of organised scepticism. Academic work in this mode is more synthetic; it is not exclusively focused on innovative research but is also concerned with the preservation of traditional knowledge, as well as the formation of disciplinary expertise. While the idea is often to represent an academic discipline rather than a specific topic, the academic profiles of individual visible scientists are of great interest. This alternative model promotes the ideas of openness, creativity and serendipity, including the possibility that important contributions may come not only from professional mainstream research but also from outsiders (see Di Trocchio 1998; on the conceptual history of "serendipity," see Merton and Barber 2004). However, apparent "discoveries by serendipity" are not without presuppositions; fortune in research typically favours hard work, perseverance and attention (see Coser and Fleck 2007: 171f.; Weber 2002 [1919]).

Social organisation: The dominant, research-oriented model is characterised by a high degree of division of labour and specialisation. Thematically focused working groups with a competitive spirit have also become prototypical in social science. Large-scale research projects and long-term data collection (and provision) require a differentiated and powerful infrastructure. Organised research is also prominently located outside of universities. Scientific competition is, in principle, global and highly appreciated. Peer-reviewed publications in which scientists share their findings and define and defend their positions are essential for academic credibility. In practice, academic prominence

is often also grounded in successfully securing funding for and managing large research projects. To a considerable degree, trust and reputation are attributed to the specific organisations and, hence, only indirectly to the individuals involved. Interconnectedness and openness are central themes. While there have been serious attempts to enforce universalistic principles and transparency, the explicitly desired competition sets severe limits on both academic communism and disinterestedness.

In the alternative scholarly model, and perhaps even more so in a media-oriented model, contributions are instead made by visible individual researchers and original "thinkers." The corresponding scholarly ideal in the social sciences often follows the idea of an interdisciplinary "polymath," and scholars have established their specific forms of social organisation (exemplified, for example, by institutes of advanced study). Reputation is at least as important as in the dominant model and is granted directly through appreciation from peers and directed more explicitly towards scientists as persons and to their lifetime achievements rather than specific accomplishments. There is a long tradition in scholarly cosmopolitanism, but in terms of content, there is often an affirmative reference to (nationally) specific traditions and boundaries around topics, methods and style. The world of scholars has strong professional elements with regard to academic communism and disinterestedness. Traditionally, there is the idea of nominal egalitarianism among scholars, and formally, the model also aligns with universalistic principles. However, it should be acknowledged that the processes of social closure and socio-cultural reproduction described by Bourdieu (1992) have been particularly prominent in this academic world, despite its cultivated appearance of egalitarianism.

Presentation and style: Finally, there are marked differences in the presentation and publication of academic work and the results, which are enforced by both formal guidelines and informal feedback. It is known that there are different publication cultures in different subfields of sociology, methodological traditions or departments following them (Clemens et al. 1995; Moksony et al. 2014; Wolfe 1990), and we can also link them to our ideal-typical distinction. The dominant, research-oriented model not only focuses on fact-based content but also aims for standardisation in presentation and in knowledge distribution. The prototypical form for this is a peer-reviewed article in a high-quality, preferably international, journal. The model promotes standardisation in form ("one paper—one question"), format ("structure of a scientific paper") and style ("on point"). In a scientific world characterised by collaboration and steadily expanding information, efficiency and speed in information search and processing are vital, and accessibility and connectivity become paramount criteria for publications. Sections on the existing state of research as well as the author's own specific contribution are central elements of scientific publications. To ensure that the content appears readily accessible, simplicity and clarity in style are appreciated, whereas an unwieldy presentation appears increasingly unfeasible. Manuals and guidelines for how to achieve accessibility abound. The following quote from a member of a journal's editorial board—even if not strictly from the field of sociology—seems to be prototypical:

> If someone asked you on the bus to quickly explain your paper, could you do so in clear, everyday language? This clear argument should appear in your abstract and in the very first paragraph (even the first line) of your paper. Don't make us hunt for your argument as for a needle in a haystack. If it is hidden on page seven that will just make us annoyed. Oh, and make sure your argument runs all the way through the different sections of the paper and ties together the theory and empirical material. (Guardian 2015)

The scholarly mode appreciates originality in contributions to scientific discourse. Prototypical is an important and well-written, in short, "brilliant," book publication that brings in new ideas or stimulates debate, and the model promotes an intellectual, learned and balanced style. Individual positions, sometimes derived in a lengthy way, turn out to be more ambivalent. Scepticism may already be an integral part of the argument—not primarily the principle of an organised social process, as in Merton's proposition. There is also explicit appreciation of high-quality language and literary value, and originality and linguistic independence are regarded as merits rather than drawbacks. This means that a lot of effort may go into the specific wording to achieve academic brilliance. Complex language is not necessarily considered bad style—although this seems to be truer in some national cultures than in others.

This account has been very stylised, and part of the reported distinctions between academic modes or models may, in fact, reflect differences between various birth cohorts of scientists. It might be interesting in further research to reveal more about the internal tensions and dynamics (including specific interests and job opportunities) within these models. In recent years, sociology in general has seen marked internal divisions. Moreover, strong and specialised links have been established to neighbouring academic disciplines that share corresponding topics or research methods with specific parts of sociology. This also means that traditionally, sociological knowledge in these fields may be transferred to other disciplines. Looking at these tendencies, it is not easy to imagine sociology continuing as a unified academic discipline of research and scholarship.

CONCLUSION

Contemporary (social) science has become increasingly specialised and globalised, and it is evident that when scientific practices are under economic pressure and public demands for usefulness and applicability, they are rather different from the idea of scientific work driven by pure curiosity—if science ever was like that. Still, it seems premature to expect the end of the ethos of science in Merton's sense and the coming of a new ethos (Weingart 1998). We would not even share the corresponding claim that academia in the post-war period was closer to the Merton ideas than it is today. Our concluding assessment is more ambivalent.

Abstracting from significant heterogeneity (e.g. differences among national academic contexts), we can conclude that a large part of contemporary social science is committed to universalistic principles, explicit methodological standards and various procedures of quality assurance. In this regard, the social sciences have participated in common trends that have characterised the world of science as a whole, and in aspects such as publication strategies, they have actually come closer to the natural sciences. Geographically, formerly local academic communities have found connection to the international discourse, unleashing considerable potential for synergies, although this has not been a universal development on the global scale.

A major part of these trends is certainly associated with the rise of empirical social research. Efforts towards ensuring the transparency and comparability of the chosen analytical approach have become a standard expectation of researchers. As a result, large parts of the discipline today appear internationally, collaboratively and thematically differentiated, and there is clearly demand from the public and the political sphere for assured scientific evidence. In this regard, developments that can be linked to Merton principles represent a story of success in the social sciences as well.

It is difficult to assess the specific contribution of the principles—and, even more so, the role of Merton's essay—to these developments. However, it seems evident that practices in line with the principles formulated by Merton (1973 [1942]) have sparked these powerful developments despite, or perhaps because of, being a frugal concept.

Merton's contribution has provided proponents of corresponding practices with, if not a specific agenda, then at least an attractive vocabulary. Merton is not necessarily cited when using this vocabulary. Not mentioning the author might be interpreted as an example of "obliteration by incorporation," as described by Merton himself (1968 [1949]: 33, 37), but it certainly has to do with the fact that Merton was a collector rather than an inventor of these principles.

In any case, the grand picture should not obscure either that the realisation of the principles has remained incomplete or that it has shown significant side effects. First, the Merton principles have been implemented selectively, and we have given examples: Full equality in access to scientific positions has not been achieved. Blind spots in seemingly universalistic academic discourses are just about to be reviewed. Despite unprecedented means of communication, there are still barriers to accessing high-quality information, experienced particularly by scientists in low-income contexts. In contrast, problems associated with information overload are observable. We also pointed to the ambivalent role of peer review—as it is currently practised—as a central means of quality assurance. While ex ante (pre-publication) checks are legitimate and necessary, particularly for securing minimum standards, there is a much broader spectrum of techniques available for implementing Merton's idea of organised scepticism, including a dedication to methodological transparency and a stronger focus on attempts of ex post (post-publication) assessment and replication. Still, many of the respective practices of open science have, until recently, tended to get sidelined. This raises an even more fundamental point. If the Merton principles are commonly accepted, then should not only the scientific community as a whole but also any scientific procedure follow these principles? When looking at peer review practices or other gatekeeping positions, for example, there are typically deficits regarding transparency—often legitimised with needs for confidentiality—so that doubts about disinterestedness within these processes remain, particularly in the light of manifest competition among scientists. Furthermore, there is a broad range of opportunities for accumulating relevant academic capital—keynote speeches, research cooperation, membership in advisory boards, etc.—which are still typically allocated only by invitation.

Second, there have been important parallel trends counteracting moves along and towards the Merton principles. Particularly significant for academic life have been the role of scientific organisations and new forms of governance with primarily political and economic rationales. These deny the conventional primacy of disinterestedness and scientific curiosity, and the corresponding inter-individual and inter-institutional competition also limits the scope of academic communism. Especially in later works, Merton himself stated the importance of *ambivalence* or the duality of norms and potentially conflicting counter-norms guiding the behaviour of scientists and helping them cope with contingencies (Merton 1976; Mitroff 1974). For example, the norm of universalism is complemented with the norm of particularism, which reflects the necessary personal commitment and accounts for the social nature of science. However, norms and counter-norms do not operate equally in every situation, but tend to dominate depending upon the definition of the specific problem.

Third, some of the trends along the Merton principles, even when successful, have inherent downsides and specific attendant risks that may conflict with the principle itself or with other principles. For example, a high degree of collaboration and specialisation enables large-scale organised scepticism but also entails the danger of excessive attention to detail and fragmentation of insights so that

substantive checks of central findings may actually be impeded. A narrow focus on the new may obscure the wealth of available knowledge resulting from a long history of reasoning, relevant especially for the human sciences, and a successful transfer of available knowledge. It can reasonably be assumed that Merton was aware of unanticipated consequences in this area. Compensatory measures have become apparent, too. For example, a boom of systematic reviews and repeated demands for expert reports often contain considerable redundancy. For individual scientists, it is often cumbersome to meet concurrent expectations—such as to simultaneously make specific contributions, have a deep understanding of complex problems and grand questions and communicate in both a comprehensible and interesting way. Note that Merton (1973 [1942]) primarily asked what is "good for science" in general, not for individual scientists or organisations in specific contexts and with partial interests. We can also read this as a caveat against immediately deriving individual-level or organisational-level indicators from the Merton principles, for example, with recruitment decisions or institutional assessments.

Finally, there have also been consequences regarding the impact of developments on the internal state of scientific disciplines. This is obvious in the case of sociology, as the discipline has not, as a whole, followed all the trends in scientific practices associated with the Merton principles. Even seemingly minor differences in the "para-methodology" of specific practices of research and publication, as illustrated in this paper, may enforce cleavages in the social sciences when social scientists take a stand either in favour of or markedly against them. This adds to existing cleavages based on fundamental goals, theory, and methodology.

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