Serendipities
Journal for the Sociology and History of the Social Sciences

Volume 6, No 2 (2021)

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ARTICLE

Relating Democratic and Scientific Ethos in Academic Self-Governance

Governing Science Through Peer Review and the Democratizing Potential of Lotteries

Cornelia Schendzielorz & Martin Reinhart

schendzc@hu-berlin.de

Abstract

Robert K. Merton envisions science as embedded in a social order and explicitly links the ethos of science and the ethos of democracy. This contribution argues that the Mertonian norms are best seen as a set of procedural norms. Thus, the normative integration of science and society is to be conceived by means of the procedures that form the "in-between" of academia and democratically governed societies. We elaborate how peer review can be understood as a central mechanism of self-government in science. We analyze to what extent the governance of science through peer review aligns with the Mertonian democratic ethos. We investigate to what extent lotteries as a procedural element may hold the potential for new linkages between science and (democratic) social order. In conclusion, we summarize the benefits of conceiving of Merton's norms as procedural norms with regard to the ethos as well as the autonomy of science considering the integration of scientific and social order.

Keywords

INTRODUCTION

Mertonian Sociology of Science may seem like a set of more or less separate concepts, mechanisms, or theorems that aim for the "middle range." The Matthew effect, obliteration by incorporation, or the reward system in science are delimited phenomena for which Merton provided concise descriptions and explanations, as well as a memorable name. As such they have traveled well, because they can be lifted from their original context and applied elsewhere. We think, however, that the original context still matters and that this is especially true for arguably the most famous of these middle range concepts: "the ethos of science." The four norms—universalism, communalism, disinterestedness, and organized skepticism—can easily be applied to a plethora of phenomena in and around

science, while much of their descriptive and explanatory power rest, however, on two further Mertonian arguments that provide the general context: the Merton thesis and the idea of a social order that embeds science in society.

While Merton was prompted by the historical context of rising fascism and totalitarianism to think about the normative relation between science and democracy, we take this special issue as an occasion to think about this relation in the current historical context. Despite the long time span in between and the many obvious and not so obvious societal changes, a current narrative, again, emphasizes conjoint threats to science and democracy. Diagnoses of a "postfactual age" (Jasanoff/Simmet 2017; Sismondo 2017) or a "post-democratic era" (Crouch 2004) rely on an intimate link between the epistemic and the deliberative capacities of current societies. All the while science has produced reform movements from within that call for more open and democratic forms of governance (Open Science). We argue that a strictly Mertonian approach, conceptualizing the link between science and democracy as primarily normative, remains too restrictive. We, therefore, advocate conceptualizing the link between science and democracy as primarily procedural through modes of self-governance. Since peer review plays a pivotal role in this context, we focus on how peer review is practiced to link epistemic and deliberative ideals. One of the current challenges or extensions of peer review—experiments with lotteries to distribute funding for research—will serve as a case to discuss the normative implications of different self-governance practices.

THE CONCEPTUAL CONTEXT OF THE ETHOS OF SCIENCE

Merton envisions science as embedded in a social order, which is the precondition for the ethos of science to be realized. This is not to say that this relation must be a harmonious one, as social order and scientific norms are prone to come in conflict with each other (Merton 1973: 271). There is a dynamic associated with this conflict which forms the core of what has become known as the Merton Thesis (Shapin 1988). Building on Max Weber's claim that Protestant ethics played a pivotal role in the history of capitalism (Weber 2016), Merton argues that Puritanism played a similarly pivotal role in the history of science. Puritanism provided "the cultural soil of seventeenth century England [that] was peculiarly fertile for the growth and spread of science" (Merton 1938a: 597). Once science is established as an institution with its own ethos, it may, paradoxically, turn against this cultural soil from which it came: as capitalism subverts religious values and beliefs in Weber's account, so does science in Merton's. This dynamic then extends to one more paradoxical turn: "As a result of scientific advance, therefore, the population at large has become ripe for new mysticisms clothed in apparently scientific jargon" (Merton 1938b: 333). As with scientific advance fostering societal backlash in the context of a dictatorial social order, Merton emphasizes the self-defeating potential of normative orientations.

While pointing out that scientific knowledge production is not necessarily tied to a democratic societal order (Merton 1973: 269), he explicitly links the ethos of science and the ethos of democracy. He claims that "the imperative of universalism is rooted deep in the impersonal character of science" (ibid.: 270) and that "however inadequately it may be put into practice, the ethos of democracy includes universalism as a dominant guiding principle" (ibid.: 273). Merton's reflections on the normative structure of science are thus inseparable from his reflections on social order and the role of science in society:

> In a liberal society, integration derives primarily from the body of cultural norms toward which human activity is oriented. In a dictatorial structure, integration is effected primarily by formal organization and centralization of social control (Merton 1938a: 335).

This prompts us to think about the relationship between the normative orders of academia on the one hand and of (democratically) governed communities on the other. Since that relationship is not a static one, we argue that the ethos of science is best thought of as more than the (static) content of these four norms and primarily as a set of procedural norms. As such, the normative integration of science and society is less about the fit of the norms themselves and more about the procedures that form the "in-between" of science and (democratic) societies.

In consequence, while Merton emphasized how science and society were integrated (or disintegrated) normatively, we ask how science and society are currently integrated through procedures functioning as mechanisms of government to make up this "in-between". Merton's norms will serve as tools to analyze such procedures, e.g., peer review, that integrate science and the social order. Along the way we will consider to what extent Merton's norms may serve as a heuristic for discerning the government of academia through peer review. We will proceed as follows: As the first step, we give a brief overview of the peer reviewing processes in academia and elaborate how peer review can be understood as a central mechanism of self-government in science. Second, we ask to what extent the governance of science in peer review aligns with the Mertonian democratic ethos or at least is compatible with democratic social structures in which it is often embedded. We therefore explore and compare central mechanisms of government at work in academia and in democratic societies. Third, we focus on the debate over the integration of lotteries into peer review processes, in order to analyze the potential of an integration of lotteries into peer review for self-governance in science that takes democratic demands into account. To conclude, we take stock of the opportunities, challenges, and risks of an integration of lottery-elements into peer reviewing procedures for the ethos as well as the autonomy of science. Finally, we summarize the benefits to conceive Merton's norms as procedural norms and conclude with considerations regarding the integration of scientific and social order through peer review as a central mechanism of the government of science in society.

THE GOVERNMENT OF ACADEMIA THROUGH PEER REVIEW

Peer review processes in science can be understood as procedures in which a qualitative assessment takes place, and a specific value is attributed to the object to be reviewed. This value is set in relation to other evaluations and weighed up (Krüger and Reinhart, 2016). These processes produce a relational valuation, which is consolidated in the course of decision making. It is decided whether the manuscript is worthy of publication, whether the research project is worthy of funding, or the job candidate is worthy of tenure. These judgments on the worth of scientific objects are multi-dimensional, as they address the quality, integrity, and legitimacy of past and future academic work. Hence, we conceive of peer review as a procedure that governs science by legitimizing expert judgment and is, thereby, formative for the social structure in science.

Peer review comes in many formats and varieties that can differ substantially between them. Instead of using a typology of different peer review procedures (e.g., along organizational types: publishing, funding, and recruitment) we focus on the central activities and how they are combined to constitute a peer review procedure. Eight activities can be distinguished: First, the postulating activities claiming the publishability of a manuscript, the eligibility of a research proposal, or the employability of a

candidate for a position. Second, the consulting activities achieve qualified expert judgments to ensure a competent assessment and evaluation of the postulates. Third, the decision-making activities decide on the acceptance or rejection of the postulates on the basis of collected information and its weighted evaluation. Fourth, the administrative activities coordinate the submission, any preselection, and the evaluation and decision processes under the respective organizational frameworks. These four activities are the core elements of peer review, as they constitute a minimum procedure. Additionally, further activities occur in different combinations depending on the format, field of application, framework conditions, and subject area. Fifth, debating activities comprehending all written or verbal exchanges, comments, and discussions contribute to the assessment. Sixth, presenting activities consist of the oral presentation and explanation of the postulating authors or applicants. Seventh, observing activities encompass the monitoring of assessment procedures as well as the control of procedural compliance and its documentation regarding role-specific rights and duties and the overall purposes. Eighth, moderating activities that are needed, when procedures are complex and multileveled, involve explaining the procedure, accompanying and guiding participants through the process, and chairing discussions to uphold administrative and pragmatic rules, timeline, chronology of procedural steps, etc. (Schendzielorz and Reinhart 2020).

The diversity of peer review formats remains relevant, especially for comparative purposes (Reinhart 2012: 188-189). Not only depending on the goal of the respective procedure—worthy of publication, worthy of funding, worthy of hiring—but also within one of these categories, the procedures show a remarkably high variance. For example, with regard to questions of transparency, we find almost all conceivable variants within journal peer review, from open procedures to semi-open, so-called "transparent peer reviews", to single, double, and triple-blind procedures. Sometimes they are reviewed in parallel, sometimes remotely in different rounds, or both may be combined (Hesselmann et al. 2021). In peer review of grant proposals, multileveled procedures predominate, and their design varies depending on the size of the project, the scope of the funding, and the format (career-funding, project funding, funding of research centers, or large collaborations and consortia). Some are based primarily on individual reviews; in others, panel reviews play a decisive role; still others combine both, and sometimes a personal presentation is included. The amount of review work tends to increase with the amount and scope of funding, and accordingly these complex procedures are designed in several stages (Reinhart and Schendzielorz 2021b). Similar considerations apply to the peer review processes to select suitable applicants for scientific positions. The more long-term and influential the position to be filled, the more multi-stage the review processes are. In the course of this, all the more comprehensive information can be gathered on the basis of the postulates, various reviews, panel discussions, and personal presentations and interviews, which are then negotiated in a review process that is often as controversial as it is thorough and at times lengthy (Forsberg et al. 2022; Schendzielorz and Reinhart 2020).

As we can see, further functions of peer review consist of opening up a space for discussion and animating organized skepticism. Peer review thereby plays a crucial part in the continuous self-comprehension and self-assertion of the scientific community through mutual feedback, thematic curation, accreditation, and suitability assessment among peers. Against this background, we argue that the ethos of science manifests itself in peer review as the norms of communalism, disinterestedness, and especially organized skepticism, which are put to work in varying procedural designs of peer review (Reinhart: 131-145). The Mertonian norms are inscribed in the procedures demanding an examination of the research proposal in terms of methodology, its position in the common knowledge base, and a critical review by consulting peers from the scientific community. The way Merton's norms are operationalized in peer review thereby animate an investigation of fundamental

demands on academia in terms of quality, legitimacy, and integrity. Hence, these norms are not used as individual criteria in peer review but as regulative ideals orienting the procedure as a whole, implying that deliberation and decision-making are needed to satisfy these norms.

This general assessment that Merton's norms at least partly manifest themselves in peer review procedures and are related to the function and role of peer review in scientific knowledge production is not uncommon (see prominently a. o. Weingart 2015: 11). Furthermore, recent empirical studies support the thesis that the "ethos of science", as historically and normatively infused it may be, still persists as the Mertonian norms remain present as guiding principles widely shared among scientists (Philipps 2021: 106-109). Mulkay's criticism (Mulkay 1976), that these norms are often broken and not followed by scientists in their behavior, does not preclude the effectiveness of these norms. Merton's conception of norms already considers this deviation:

> These imperatives, transmitted by precept and example and reenforced by sanctions are in varying degrees internalized by the scientist, thus fashioning his scientific conscience or, if one prefers the latter-day phrase, his super-ego (Merton 1973: 269).

Indeed, the very characteristic of a norm is that its validity is not directly measured by whether and how precisely it is consistently followed in practice. Rather, it can be argued that the validity of norms proves itself precisely in the reactions to their disregard.[1] The latter should be kept in mind in view of the abundant literature on academic peer review, focusing on the deficits of this instrument through which the scientific community performs its self-regulation.

Intermediate results

Summing up, what does it mean to conceive of peer review as an instrument of self-governance? It signifies that peer review is not only a procedure to assess and evaluate scientific work in forms of manuscripts, project proposals, and job applications. It implies that peer review is a mechanism enabling the assurance of scientific quality as well as the legitimacy of the decisions made and the integrity of the whole endeavor. Given this, peer review is an instrument that allows to orchestrate different claims to the quality, legitimacy, and integrity of scientific work and scientific knowledge production in one procedure and to relate them to each other in a specific way. This also concerns various ontological-epistemological, normative, and not least organizational-pragmatic claims (e.g., in order to enable collective action) already contained in Merton's norms. Thus, an essential function of peer review is to operationalize claims and norms that are constitutive of science's self-understanding and particular nature in variable procedures. As such, peer review is the paradigmatic governing mechanism to understand the mechanisms shaping the "in-between" of science and (democratic) societies. As explained, the negotiated claims invoke several normative dimensions at once: those of quality, integrity, and legitimacy, which are separable only heuristically but are intertwined in practice. It is precisely because of this comprehensive negotiation, self-understanding, and governing function of peer review that we conceive of peer review as a mode of government in academia.

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[1] See also the argumentation of Weingart 2015: 12-13.

SCIENTIFIC AND DEMOCRATIC ETHOS IN PEER REVIEW

We can now take Merton's entanglement of democratic and scientific ethos further by examining the forms of government at work in science and society. In order to investigate *whether the governance of science through peer review is compatible with democratic government*, we will briefly juxtapose the social structure of democratic society and the scientific community. We therefore draw a parallel between the population of science in diverse disciplines and the population of nation-states, namely citizens in societies describing themselves as democratic. This seems appropriate considering that scientific and democratic modes of government show fundamental analogies in how the execution of power is legitimized: First, just as power should emanate from the people or at least from citizens entitled to vote (Cheneval 2015: 21-23), scientific decisions in academia should come from scientists. Second, both populations are deemed to be involved in the distribution of power and to participate in its execution. Similar to Cheneval's basic determination, Buchstein highlights the equality postulate (Buchstein 2016: 28) as fundamental and defines it as a minimum condition and basic principle of all the different and plural democratic forms at the normative level. Likewise, these fundamental democratic principles converge with Robert Dahl's minimum "criteria of procedural democracy", consisting of "voting equality" and "effective participation" as "standards against which proposed procedures are to be evaluated" (Dahl 1997: 61ff.). He considers these two criteria as sufficient "to say that any association satisfying these two criteria is, at least to that extent, procedurally democratic [...] in a *narrow sense*" (Dahl 1997: 63, emphasis in original).[2] Continuing the comparison, just as in democracy the people are supposed to be "the source of competence in law-making and law-application" (Cheneval 2015: 16)[3], in science scientists with knowledge of the scientific field, its structures, and the subject concerned are supposed to help shape decision-making procedures and exercise power in them (e.g., elected review board members in the DFG and appointed reviewers elsewhere, as well as elected staffing committees with representatives from different status groups). According to this claim, citizens in a democratic state, as well as scientists who commit themselves as part of the scientific community to the ideal of 'freedom of science' "should not be subject to just any political institutions, but to those they recognize as their own" (Cheneval 2015: 17). The differences are primarily in their goals. While democracy aims to organize the coexistence of citizens according to maxims of equality, freedom, and justice, science aims to produce knowledge and generate findings that are as valid and robust as possible, and at the same time amenable to revision in the search for truth. Given this, the essential equality postulate (Buchstein 2016; Cheneval 2015) in science could translate into the claim of scientific and epistemological equality of all scientists. In light of the variety of peer review procedures, such equality is conditional on the different levels of participation resulting from separation of roles and division of labor, especially regarding the speech act,[4] which needs to be considered. Robert Dahl emphasizes that any evaluation requires "additional judgments about the facts of the particular situation" (Dahl 1997: 63). Regarding this, the differentiation between minimum and maximum procedures of peer review mentioned above is particularly pertinent. Furthermore, scientific and democratic modes of government are equally characterized by the fact that an extremely heterogeneous variety of manifestations, formats, and procedures are subsumed under one label. The diversity of peer review in its procedures and practices as a governing

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2 To further accomplish and delineate its understanding of a fully procedural democracy Dahl argues for additive criteria: Enlightened understanding, final control of the agenda; inclusiveness. (Dahl 1989).

3 This and all subsequent translations of quotations from German-language publications into English were made by the authors, unless otherwise indicated.

4 For more detail see Goffman elaboration in "Forms of talk" (Goffman 2005). For the relevance of diverging participation status in peer review procedures see also Schendzielorz and Reinhart 2020: 106, 111–112.

instrument corresponds to the diversity of "forms, shapes and constellations of democracies" (Buchstein 2016: 20). This counts on the part of the theories of democracy, which, especially in the empirical theory of democracy, is thought of as a "pluraletantum" as well as for the actually existing democratic forms of government.

Let's take the analogy of scientific and democratic ethos one step further: The democratic ethos finds its expression not only in free elections of representatives of a population which form a democratic government. A democratic government is also committed to the interplay of separate powers—legislative, executive, and judiciary. What about the separation of powers in peer review? Relating the eight peer review activities distinguished above to the separation of powers in legislative, judiciary, and executive authorities, we find the following picture: The legislative power is concentrated in the hands of the administrative actors and thus in the organizations that initiate and design peer review processes. The executive power consists of consultative activities and, depending on the type of procedure, may also include debating, moderating, and observative activities. In addition to the reviewers, the process managers, supervisors, and any rapporteurs can play a crucial role. The judicative function is realized in the judgment, i.e., in the peer review in decision making activities, which are usually carried out by actors (editors, committee/panel members, commission members, etc.) who belong to decision making bodies. It is more difficult to classify the postulating and presenting activities as one of the three powers: Postulating and presenting roles are subject to the "constitution" of the respective peer review process and thus do not have any governmental power in these activities. However, from one peer review process to the next, the same individuals may find themselves in other roles as reviewers and/or part of decision-making bodies or representatives of a funding agency, and as such may then assume executive, judicative, or even legislative governmental functions in science. Since the scientists alternate between the roles of governors and governed in the context of peer review, the postulating and presenting activities are located in their internal procedural role on the side of the governed. But as the governed often take on the role of the governors elsewhere, it can be assumed that these activities are in practice considered to be part of the executive power, which can always correspond to past or future parts of the judiciary and, depending on the career path, also of the legislative power.

Drawing these parallels with regard to the legitimate execution of power and its challenges in dealing with varying matters of concern shows how the scientific ethos and its implementation in peer review is intertwined with democratic principles along demands that are equally formative for science and democracy: namely, requirements for collective action, for equal opportunities of participation for those recognized as status equals, for the right to stand for election, for the ability to realize decisions made collectively, and for the periodic auditability of decisions (Cheneval 2015: 10-41).

So *how do Merton's norms relate in detail to democratic principles?* Because the Mertonian norms relate to science, they have most often been thought of as epistemic and thus as connected to the epistemic value of objectivity (Daston & Galison 2007). This seems particularly obvious for the norm of universalism as a regulative ideal. The debates in the philosophy of science surrounding Popper's falsificationism and Kuhn's incommensurability thesis are historical examples of how universalism could relate to objectivity in a procedural way. More recent debates, e.g., around actor-network theory, understand universalism as equally procedural; however, they are less concerned with a strictly epistemological perspective. When looking for meaningful social linkages between science and social order, universalism pushes for connectability and comparability by critically examining the conditions of the possibility of generalization.

However, normative democratic theory also makes a universal claim, that "it derives its considerations from basic values for which it tries to justify that they can be considered worthy of general recognition" (Buchstein 2016: 28). Complementing this formulation as follows, "that they can [under the currently given circumstances] be considered worthy of general recognition" (ibid.), in science we may also approve the effort to strive for such generalization, at least as a part of analytical sharpening. Beyond that, the norm of universalism also alludes to a generalization of the accessibility of science and scientifically produced knowledge. Merton's call "to preserve and extend equality of opportunity" (Merton 1973: 273) in changing contexts points in a similar direction. This current drive for universalism is manifested in the recent but increasingly important and promoted paradigms of Open Access and Open Science.

The questions around the openness of science also connects to Merton's norm of communalism as it qualifies "the status of scientific knowledge as common property" (Merton 1973: 274). It not only claims equitable access to bodies of knowledge but also justifies this claim by acknowledging that the "substantive findings of science are a product of social collaboration" (Merton 1973: 273). The latter emphasizes the importance of partaking, sharing, and engagement of other scientists and the consequences of general participation rights for members of the scientific community. Trying to parallel it with democratic principles, Merton's understanding of communalism has a lot in common with Rosanvallon's concept of democratic openness through legibility. Legibility terms an active relationship, in which interpretative capacities are deployed during the reception of information. It thus means the comprehensive understanding and familiarity with procedures and mechanisms of governing. According to Rosanvallon, along with responsibility and responsiveness, legibility is one of the three principles that characterize the relationship between the governed and the governing in democratic societies (Rosanvallon 2018: 202-206). Asking how to deal with the claims of universal openness, such communalistic legibility, and everything that comes along, the norm of disinterestedness comes into play.[5]

Again, Merton's explanations on this norm come close to Rosanvallon's characterization of principles for a democratic legitimacy of government, namely impartiality. Defining impartiality as "distancing from party positions and particular interests" (Rosanvallon 2018: 30), it also applies to the requirement of governing science by means of impersonal criteria while restricting biases due to specific interests or certain positionings of different schools of thought.

Finally, the norm of organized skepticism "is both a methodological and an institutional mandate" (Merton 1973: 277). Hence, it connects to the other components of the scientific ethos in multiple ways, as its execution acknowledges that scientific knowledge is open to revision (Bogusz 2018), inquiry, review, amendments, corrections, and further developments as well as to trials of validation and verification, especially from peers. Juxtaposing it with Rosanvallon's principles of democratic legitimacy, organized skepticism parallels reflexivity. The latter involves the "consideration of plural expressions of the common good" (Rosanvallon 2018: 30), and thus with regard to science entails considering diverse regimes of knowledge. In this respect, organized skepticism can also allude to the equitable recognition of all particularities and conflicting approaches and paradigms by questioning claimed connections, systematization, and configurations, including relations of supremacy and subsidiarity.

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[5] This framing of communalistic challenges also alludes to the "Tragedy of the Commons" (Hardin 1968). However, to discuss the relationship between science and democracy against this paradox is beyond the scope of the paper.

Intermediate results

The scientific and the democratic ethos require strong normative references for the foundation of their respective orders as a social collective, be it the scientific community or democratic society. These normative benchmarks are invoked in all attempted definitions and possible codifications, despite the difficulties of empirical measurability:

> Although the ethos of science has not been codified it can be inferred from the moral consensus of scientists as expressed in use and wont, in countless writings on the scientific spirit and in moral indignation directed toward contraventions of the ethos (Merton 1973: 269).

Accordingly, Buchstein classifies attempts as self-contradictory when trying to identify their approaches in democratic theory as free from these groundings, since the claim of normative abstinence itself falls into the realm of normative statements. In addition, according to Buchstein, such attempts in democratic theory:

> are inappropriate to one's own research practice, because empirical and historical as well as formal theories of democracy must always establish certain references to normative democratic theory (Buchstein 2016: 34).

Otherwise, they would not be able to contour their respective research areas. In this way, the ethos of science and the ethos of democracy show a structural similarity with regard to the persistence of their normative dimension, even when at times some tend to suppress these grounds. To those who may still think the normative formulation of an ethos of science is overly emphatic, one more reasoning paralleling science and democracy should be considered: Democratic governance requires normative goals which, unlike laws, cannot be implemented directly but are fixed in preambles and constitutions. Why? Because without "legally implemented moral principles and rights of members and non-members, it would be indistinguishable from a terrorist group that ensures the equal participation of all members recognized as having equal status in its decision-making procedures" (Cheneval 2015: 15).

Meanwhile, we can note that, depending on the author's slightly varying emphasis (Cheneval 2015: 15ff.; Buchstein 2016: 28-29; Dahl 1997; Rosanvallon 2018; Bogusz 2018; Merton 1973), certain normative characteristics for the governance of communities emerge along the claims of participation rights, social equality, openness, impartiality, justice, reflexivity, and freedom, which, although not congruent, can in relevant parts be aligned with the self-understanding and self-description of science. The overlap seems large enough to contend that the government of democracies and the government of science face similar procedural constraints, in that their regulative ideals point in compatible directions.

CAN LOTTERIES DEMOCRATIZE PEER REVIEW?

After discerning these parallels, the further discussion of peer review as a mode of government in academia focuses on the debate over the integration of lotteries into peer review processes. This is for two reasons: First, the question to what extent the government realizes a scientific ethos through peer review that meets democratic norms can only be examined incrementally and via systematic comparative analysis of peer reviewing procedures. We have already pursued this path by conducting an empirical comparative analysis of peer review procedures in the German and Swiss research

systems, developing an analysis heuristic that results in the activity modules of peer review described above, structured along minimum and maximum procedures (see Schendzielorz and Reinhart 2020). Second, we choose to debate the procedural innovation of introducing lottery elements in peer review instead of other currently debated procedural innovations in peer review—formalizations along remote assessments, rapporteur, or synthesizing reviewer functions etc.—because lotteries are brought up as governing mechanisms to bring to bear the overall range of deliberation in selection procedures in both science and democracy. We thus suggest seeing lotteries as a procedural element in democratic government that may hold the potential for new linkages between science and (democratic) social order.

Hence, we ask: *what would an integration of lotteries into peer review mean for the democratic potential of self-governance in science?* At present, a small number of funders are experimenting with lotteries as part of their grant peer review procedures and justifying these experiments by claiming that there is widespread dissatisfaction with existing peer review procedures. The typical criticism of peer review is first and foremost directed at the outcome of the selection by peer review. It bemoans conservatism in the selection of eligible projects, that peer review is prone to cronyism and biases. From this derives the commonly invoked claim that peer review selection is not sufficiently fair and results in the diagnosis that the peer review system in its current form is deficient. Unfortunately, it is rarely addressed to what extent the very heterogeneous peer review formats generate legitimacy through their procedures (Luhmann 1983) and thus vary in terms of their fairness, issues of equal treatment of all submissions, and equity of subject matter (Reinhart and Schendzielorz 2021a, Schendzielorz and Reinhart 2020). Against the background of this narrowly output-centered criticism, the integration of lottery methods in peer reviewing procedures is discussed as an innovation in peer review with the potential to improve fair decision making (Brezis 2007; Gillies 2014; Avin 2015; Fang & Casadevall 2016; Roumbanis 2019: Liu et al. 2020). It is also increasingly put to the test in practice by different stakeholders, such as the Health Research Council of New Zealand since 2015 with the Explorer Grant, the Volkswagen Foundation since 2017 with the Experiment Funding, the Swiss National Science Foundation since 2018 with its postdoctoral mobility grant, and since 2021 with lottery elements as a tiebreaker (Adam 2019). The recent discussion considers the introduction of a lottery in peer review for the allocation of research funds foremost as a complementary part in the selection procedure—that is, to be implemented after traditional peer reviewing has been carried out. Given the widespread complaints about the output of existing peer review, the pressing question is what and how potential improvements can remedy the aforementioned shortcomings. How can peer review increase fairness and equity in selection processes through its procedures? We want to expand the current debate on lotteries in peer review by elaborating *how peer review procedures can become more adequate and equitable by making this instrument of government in academia more democratic.* Sharing the widely held assumption in the literature that "democracy is to be regarded as the system of government that realizes the best possible realization of freedom in collective self-determination" (Cheneval 2015: 25), the insights of political theory on the use of lottery procedures are worth noting. Democratic theory offers elaborate considerations that, as we argue, can also be adapted for improvements of peer review procedures (cf. Röcke 2005; Syntomer 2007; Buchstein 2009; Cheneval 2015: 66-70). Buchstein synthesizes five possible functions of lottery. First, lottery represents a "neutral, unerring, and procedurally autonomous random mechanism" (Buchstein 2009: 331) that may be suitable for making decisions in deadlock situations. Second, unweighted lottery procedures are an instrument that can ensure an equal distribution of opportunities in access to goods and offices through more egalitarian chances of success (ibid.). Third, lottery procedures can be used to relieve "decision-makers and decision-subjects" (e.g., in the allocation of scarce vital organs or high-risk missions of soldiers in war zones,

etc.) (ibid.). Fourth, lottery procedures function as a generator of uncertainty, which "can be brought to bear as an 'anti-corruptivum'" (ibid., quotation marks in original). Fifth, lottery procedures reliably generate "ever new chances" of success in the lottery procedure, which can have system-stabilizing effects, for example, in the allocation of official posts (ibid.). Various elements of these functions echo eclectically in the debate over the use of lottery in peer review. However, a lack of systematic reception of insights from political theory tends to result in a curtailed and decontextualized transmission, entailing inconsistencies and distortions.

Hinting at the *first function*, a prominent argument in favor of integrating lottery elements into peer review consists of promoting the draw as a neutral decision-making instrument, which allows for fairer decision making. Alluding to the *second function*, this ability to assure an egalitarian distribution of chances is especially pointed to in cases where proposals are considered to be of equal quality and equally good reasons exist for each of the possible choices (Avin 2018, Bedessem 2020). This transfer implies two problematic shortcuts: first, the equal distribution of opportunities counts for unweighted lotteries. But the preceding peer reviewing is not a neutral-decision-making instrument, and it purposely produces a weighted situation. Thus, considering the whole procedure, the discussed lottery complements in peer review do not provide equal opportunities in the strict sense nor can they generate decisions that can be deemed per se fairer due to its neutrality. Second, the same grades after the peer reviewing process does not generate substantial equality and, as we know well from politics, applying formal equality in the case of factual and material inequality can lead to injustice. Furthermore, assuming strict equality subverts the requested differentiation and ranking achieved through work-intensive deliberation in reviewing. Rather, we may find ourselves in a situation of undecidability. Nevertheless, the additive status of lottery elements is particularly emphasized by proponents of integrating lottery methods, for example to counter fears that decisions reached in this way could entail legitimacy deficits.

Hinting at the *third function,* a reason frequently brought up in favor of lotteries is the promise that they would reduce the burden of making a final and binding decision for which the competence to judge is lacking or a valid basis for judgment of a comparative selection is in doubt. The scenario of delegating these burdensome decisions to a lottery often comes with the assumption that a random selection would increase the diversity of the selected projects (Adam 2019, Avin 2018:13, Osterloh & Frey 2019). This implies that these types of decisions tend to be conservative and favor research projects that rely on the tried and trusted.

Hinting at the *fourth function*, an advantage of the lottery over the decision that is deliberately made can be seen in the fact that it can act as an anti-corruption instrument. Namely, since it is unpredictable which lot will be drawn, targeted bribery is not very attractive. As plausible as this argument is, e.g., with regard to the allocation of offices in politics, again it cannot be transferred one-to-one to the procedures discussed for scientific peer review. Since traditional peer review remains in place to pre-select projects, it would still be appealing to exert a targeted influence on this first phase in order to favor some of the proposals and prevent others on the way to the final draw.

The *fifth function* shines through when the terminology is changed quite emphatically from naming it a lottery, which is considered a gambling method, to labeling it as randomization, which avoids a positively or negatively biased selection, for example in randomized control trials. The idea behind this is that random selection follows the laws of stochasticity and thus provides the same chances to all who have a lot. But once again, the task of determining who gets into the lottery round is still attributed to "classical" peer review in the debate by proponents of lottery procedures. However, the stochastic distribution of chances would only unfold in the long run when the dice is rolled several

times, though, in the scenarios discussed, the dice is rolled only once in each case. Also, the possibility to participate in the next draw with the same proposal in the case of bad luck is merely debated. Considering this, the luck of the draw appears rather as a delegation of the decision to the fate of the dice. This aspect is also problematized in decision theory regarding the distinction between first and second order decisions. Considering issues "about cognitive burdens and also about responsibility, equality, and fairness" (Sunstein and Ullman-Margalit 1999: 7), Sunstein and Ullman-Margalit come to a similar finding. Asking "why might an institution or agent pick rather than choose?" (ibid.: 24), they determine picking instead of choosing as a form of delegation (ibid.: 10, 20). They further state:

> Picking can even be said to operate as a kind of delegation, where the object of the delegation is 'fate,' and the agent loses the sense of responsibility that might accompany an all-things-considered judgment (ibid.: 24).

Thus, their decision-theoretic reasoning concludes that delegation to fate could impair decision makers' sense of responsibility. It thus remains questionable to what extent the apathy of fate towards all proposals in the final round has a recognizable gain for the quality, integrity, or legitimacy of the decision.

One prominent argument frequently raised to promote lotteries does not allude to any of the mentioned functions: It holds out the prospect that integrating lotteries would reduce the expenditures of peer reviewing procedures, saving time for all involved and thereby money, at least for the agencies and institutes organizing the selection processes (Gillies 2014; Avin 2015; Fang & Casadevall 2016; Roumbanis 2019; Liu et al. 2020). Taking a closer look at the debate, this is a doubtable promise, considering that the integration of lottery elements presently is only thought of as a second step after the traditional peer review has been thoroughly conducted or as a kind of equipoise to other funding schemes (see Volkswagen Foundation). Without reducing the assessment and exchange on how to weigh which aspect in the evaluation amongst peers, the certainly high costs of peer review (Avin 2018: 6ff.) cannot decrease considerably by a draw following the peer review. Although this argument makes an arguable promise, it nevertheless remains present in the discourse (Philipps 2021). As this argument deals with questions of efficiency and does not claim to improve the fairness nor ameliorate the quality, legitimacy, or integrity of the selection procedure, it appears to remain, albeit convincingly, as an elusive selling point for funding agencies. Therefore, the question needs to be asked why funding agencies—more than exclusively scientifically staffed committees—want to delegate informed and reasoned selections to the luck of the draw? Why do the same players who argue for stronger incentives to ensure robust scientific research show such interest in and appetite for risk when it comes to experiments with lottery procedures? As pointed out, there is scant indication that they can achieve the effects associated with them. In light of this, it needs to be considered that lotteries, in case they are introduced to shorten proposals and reviewing procedures, would reduce the number of scientists involved in funding decisions. In turn, the weight of organizing practices from administrative personnel in decision-making bodies would increase simultaneously, namely by designing and managing the lottery, which includes deciding on the amount and the mode of allocation of lots. Regardless of who handles these tasks, they still remain prone to interest-driven biases in impartiality, legitimacy, and equality of treatment (Reinhart and Schendzielorz 2020). Hence, increasing efficiency in grant allocation through lotteries would also strengthen governmental power external to science. Therefore, the debate on lotteries has to consider whether lotteries in grant allocation along the way could lead to a loss in the relative

autonomy of science and whether, in view of this peril and given the uncertain achievement of the goal, it is worth the current experiments.

The integration of lottery procedures into peer review thus hardly fulfills any of the five positive functions that lottery procedures could develop. Even if the third function, the unburdening of decision-makers, is achieved, the hope attached to it of more diverse project funding can hardly be reliably reproduced as its outcome. This modest result is merely surprising. After all, the mentioned literature concludes that historically, from ancient Greece to the present day, lotteries are generally not applied to resolve matters of fact, but to the allocation of offices and personnel (Cheneval 2015: 68; Buchstein 2013: 386-389). Also, in citizen assemblies, as they are used today, the members are drawn by lot, but the decisions made there are worked out in deliberative discourse.

The arguments and reservations against the integration of lottery procedures in peer review focus on the overall quality and validity of the decision. First of all, it points out that the quality of a decision in science is usually determined by the expertise with which it is made. Thus, a decision in which the experts' competence in judgment is set aside, after a certain point, in favor of a lottery decision accepts a loss in its qualitative foundation and, according to Sunstein and Ullman-Margalit, risks the decline of an encompassing sense of responsibility for the judgment. In addition, as noted above, there is the disadvantage of reducing the legitimacy not only of the decision but also of the distinctive appreciation of the achievement, which for those selected comes with the approval of funding, as they were not selected on the basis of collective consent after intensive deliberation, but in the end by chance. This stands in contrast to the advantages of more or less agreeable outcomes from deliberative and aggregative procedures (Cheneval 2015: 68). This leads to the central insight, that a lack of justification can easily lead to a deficit in qualitative legitimacy (Cheneval 2015: 70). At this point, lottery procedures conflict with the justification requirement of democratic procedures, which also apply in peer review, as elaborated above. We therefore plead *to explore what democratic characteristics in peer review could be strengthened through the functions of lottery procedures.*

This requires refocusing the context in which the five functions have a positive effect: the context of joint discussion and deliberation for the purpose of a differentiated opinion formation or judgment in the collective. Here, the lot is not used for making a decision on the subject matter by drawing lots between different proposals, but rather for the staffing of the panels. Transferred to the peer review system, this would mean that the reviewers and other evaluators of the panels would have to be drawn by lot. Of course, this raises the question of appropriate representation. It seems obvious that this would first require classification into subgroups: on the one hand, according to expertise, in order to determine which area they can represent and to prevent biases through one-sided dominance of certain subfields; on the other hand, according to sociodemographic factors for which biases are to be feared (status, origin, age, gender, etc.). Nevertheless, the debate about whether the so-called mirror representation, understood as an exact statistical representation of the heterogeneity of the population, is actually fairer reveals the dilemma strikingly (Buchstein 2009: 344). Since every mirror representation can only take place on the basis of previously determined criteria, the following applies despite all efforts: "representation under any system is biased" (De Grazia 1951: 184, quoted after Buchstein 2009). But this does not render obsolete the attempt for an approximate representation of the social and, in view of science, possibly also the epistemological, ontological, and methodological heterogeneity of the population concerned. Research from democracy theory as well as from social psychology suggests that homogeneous groups tend to end up in circular self-affirmation and that members of heterogeneous groups are more receptive to reasonings of other group members. Furthermore, previous experience with social groups formed by

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drawing lots shows that it is above all the diversity of the group and not necessarily the precision of the representation that is crucial (Buchstein 2009: 345):

> In the end, it is diversity that appears to matter most in these procedures. When a group of deliberators is heterogeneous, it is less likely that they will enter into enclave deliberation and reinforce their own positions (Hendriks 2004: 97).

In line with this, studies and experiments with deliberative opinion polls show that members of the discussion in groups formed heterogeneously by drawing lots have a better understanding of the positions of others, tend to be more willing to deviate from their existing beliefs, and thus unusually often change their pre-discussion opinions (Buchstein 2009: 335-340). Hence, there seems to be a chance to improve peer reviewing by preventing prevailing prejudices from coming to bear. With regard to the question of what democratic potentials could profit from an integration of lottery elements in peer review, we arrive at the following conclusion: Lotteries can strengthen widely spread participation in peer review on both sides, but it can only develop deliberative effects if they are used as a representatively quota-based search engine for the relatively heterogeneous and balanced appointment of power positions that intends to determine the composition of decision makers, namely the reviewers.

Intermediate results

The scientific debate on lotteries in peer review is currently clearly dominated by its proponents. Bedessem, broadening the debate, stresses the importance of a proper evaluation of broad participation and the relevance of political and ethical normatively charged requirements (Bedessem 2020: 154-155). Seeking to readjust the focus of the debate, we can now answer our question of what an integration of lotteries into peer review means for the democratic potential of self-governance in science. The use of lotteries has a democratizing potential when it comes to the composition of committees, panels, and the allocation of positions therein, and less so when it comes to factual issues whose assessment requires pronounced expertise. Against this background, it is quite consistent that in all the discussed scenarios, the development of the expert judgment in peer review is not abandoned. Another crucial factor determining an effective use of lottery procedures is that the results and decisions reached in these committees are binding and enforced, because otherwise a sufficient motivation to engage seriously in these deliberative processes is at risk (Buchstein 2009: 341ff.). In contrast to many experiments with opinion polls in the field of political citizen participation, this relevance for action of the decisions reached is already given in all the scenarios debated and trialed for use in peer review. Another challenge for productive deliberative group work of panels assembled by lot is usually the definition of the randomly selected population, i.e., who gets a lot. This question has so far been discussed only with respect to the proposals and not with respect to the reviewers to be drawn. For the latter, depending on the purpose and subject of the peer review decision, various definitions are available along existing indicators (academic degree, e.g., B.A, M.A; Ph.D., Habilitation), which in different variants already define thresholds for entry into the scientific community. In order to achieve a promising diversification of the staffing, as well as to ensure a variety of perspectives on the proposals, there is an urgent need to expand the reviewer pool. Thus, there are good reasons not to additionally link these entry thresholds to concrete employment relationships at certain institutions (researcher at a university) and to occupied status positions (chair holder, professor). The larger issue at stake here hence is the integration of scientific self-governance through lotteries into the wider democratic social order.

The insights on the democratic potential of lotteries from democracy theory leads us to suggest that the best way to counteract the impairment of a just and proper decision, be it by the one-sided dominance of powerful reviewers, by pronounced peer pressure or group think, or by other biases due to homophily, is to make the composition of the committee as heterogeneous as possible and therefore to enlarge the reviewer pool. In this way, the integration of lottery elements into peer review could be put at the service of improving the deliberative quality of the decision reached. The improvement in governance through peer review would then include the following: First, it would be more democratic because the way power is exercised would meet the standards of democratic legitimacy and integrity to a higher degree. Second, there would be a realistic chance that diversifying the composition of reviewer juries by drawing lots could do much more to increase the diversity of the projects selected, especially if applied regularly, than drawing lots for the projects receiving funding, thus counteracting conservatism in peer review. The fact that, despite existing indications, this democratizing potential for science through the drawing of its mandate holders receives so little attention in the current debate raises questions about the possible interests of the debating actors, as well as about the demands on the culture of a decidedly scientific debate that takes into account the diversity of perspectives, acts reflexively, and is open to criticism. Overall, lotteries in which the reviewers are drawn have the potential to increase the participatory, representative, and deliberative quality of peer review decisions that are part of the government of science. Such procedural innovation would also be consistent with the parallels between science and democracy in its procedural constraints and regulative ideas, by supporting the normative dimensions of the general accessibility of evaluation panels, participation rights in collaborative knowledge production, and systematically organized skepticism in heterogeneous groups.

CONCLUSION: DEMOCRATIC ETHOS AND THE AUTONOMY OF SCIENCE

The Mertonian sociology of science prompted us to ask where the current relevance of an ethos of science lies for science itself and for the relationship with the social order in which science is embedded. We claimed that the Mertonian norms are best seen as procedural, as it is not the (abstract) norms but rather procedures such as peer review that integrate science and society on a continuous basis. Drawing from political theory, we were able to identify the procedural commonalities in the government of science and democracy. We used this analysis to think through the current attempts at using lotteries in the self-governance of science, finding that drawing lots for appointing members to decision-making bodies could improve effective and broad participation and adequate representation, thereby enhancing democratic deliberation in the distribution of research funding and, by extension, in all of peer review.

Recalling the 80th anniversary of Merton's norms, it becomes apparent that the recent debate, relevance, and effectiveness of the norms as procedural qualities are underdeveloped. Yet this discussion of procedural norms helps us to understand how they are also undermined in practice. Regarding the democratic potential of drawing lots to fill decision-making positions, it should be noted that the deliberative democratic models stand out among various models of democratic governance as particularly suitable for non-national governmental entities because they can deal more easily with the problem of boundaries (Buchstein 2009; Habermas 2005; Joerges 2002; Dryzek 2000). Therefore, it seems to fit most as a sparring partner and vehicle for questions of democratization of the government of science in both external governance and self-governance. The findings in this branch of democracy theory show clearly that a pluralism of people in power is an important stability factor

(Buchstein 2016: 17), which emphasizes once more Dahl's minimum criteria of procedural democracy, voting equality, and effective participation. But does a pluralism of power promote stability in science as well? At what level would a pluralization of power positions be required and necessary? And is such stability desirable here? Criticism of the conservatism of peer review suggests that other goals are currently considered more important. For example, that the system remains sufficiently dynamic to be permeable for original and unexpected innovations.

Merton noted that the normative integration of science in a democratic order holds the potential for self-defeating or paradoxical effects. As the democratic and the scientific ethos only partially align, this prompts us to ask whether lotteries might be a case of normative integration that result in a threat to the scientific ethos and maybe also to the autonomy of science. Are there democratic principles that might threaten quality, legitimacy, and integrity in science—for example, if participation is expanded in such a way that it is decoupled from expertise and judgment? Could it then foster populism in science, popularization of science, or even disinformation in a scientific guise? These questions indicate the tension between the democratic demand for participation and the burden of comprehensive participation that broad inclusion and participation necessarily entail.[6] However, this dilemma exists independently from the issue of integrating lottery procedures. It applies to procedures with lottery elements as well as to election, rotation, auction, or co-optation procedures. Considering this, the question arises whether, just as in deliberative democracy where political freedom of decision is limited by the constitution (Buchstein 2016: 5), a functional equivalence to the constitution is also required in peer review and other modes of government in academia. Insofar as a constitution secures inalienable fundamental rights and also protects against majorities that reject them, it can be argued that it preserves the democratic ethos. In turn, what are the inalienable fundamental values of science that must be protected against intrusions and attacks by deliberatively achieved majority decisions? This invokes keywords such as impartiality, openness, curiosity, systematic doubt and testing, formal logic, reflexivity, intersubjective comprehensibility, openness to revision, honesty, integrity, etc. Many of these aspects are part of or derive from Merton's norms and can be found in different formulations in guidelines and codes of good scientific practice or criteria of scientific soundness. Although these do not have constitutional status, as minimum qualitative requirements they are most likely to provide orientation for basic values of scientificity.

The question of social order also adverts to other criteria of procedural democracy: "enlightened understanding", which alludes to the condition of deliberative opinion formation in the discourse (Dahl 1989: 64), and "final control of the agenda by the demos" (Dahl 1989: 66). The latter consists of determining what is or is not a matter of concern, which is decided upon using procedural democracy and usually requires equivalent qualification of the "demos" or correspondingly the members of the scientific community (Dahl 1989: 66ff.). Whether or not the "enlightened understanding" is achieved may depend on the specific peer review constellation. It is apparent that the demand of procedural democracy to control the agenda is not met in peer review, as reviewers and other scientists rarely decide on what is set or not set on the agenda or which decisions are to be made through peer review. These guiding decisions usually fall within the realm of science policy actors. Taking this further in accordance with Dahl's emphasis on this point (Dahl 1989: 90f.), externalizing control over agenda setting carries a high risk of relinquishing control over various other aspects that are tied to it. Considering this, caution is warranted when innovations in peer review entail a shift in governing power regarding the design of procedures, criteria, and implementation areas away from

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[6] Regarding the "problem of inclusion", see also Dahl 1989: 68ff.

scientists and toward administrative and science policy actors. As Dahl predicts, arguing for procedural democracy leads into a conflict with meritocracy, conceived as a minoritarian government by an elite with outstanding qualifications (Dahl 1989: 90). As meritocratic government of science through peer review appears to be equally or even more prevalent in the current so-called democratic government of states, we dare to close this reflection with the open question: whether a meritocratic-like composition of decision-making bodies in science play a considerable role in the subversion of how Merton's norms can come to bear and thus how the scientific ethos can unfold.

To conclude, up until today Merton stimulates investigation into the relation of science and social order and enables us to think of democratic principles as possible "normative guidance" for the social order of science. Considerations of democracy theory have led to an outlook on the future democratic horizons of peer review. Given the fact that existing peer review as a mode of government clearly does not meet the requirements of democratic governments—as generally the states do neither—it may be worthwhile to envision peer review at least as a kind of a polyarchy in the sense of Dahl. Keeping up the "idealistic" references points as "norms" and "ethos" may help to improve the participatory, representative, and deliberative qualities in the government of science. The indissoluble tension between upholding the norm and its imperfect realization could also be the reason why peer review is so often described as deficient, because it is measured against the democratic ethos as an ideal. Precisely this could also be seen as a sign of quality, that peer review still raises these claims in the first place. At the same time, these claims are in danger of being abandoned if innovation in peer review risks overseeing demands of legitimacy and deliberative qualities, which are equally constituent for democratic as well as the scientific ethos.

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Author biographies

Cornelia Schendzielorz is post-doctoral researcher at the Humboldt-Universität zu Berlin and at the German Center for Higher Education Research and Science Studies (DZHW) in Berlin. Her research areas are in science studies, political sociology, sociology of valuation as well as qualitative methods. Recent projects focus on peer review, authorship in academia, and governance of science.

Martin Reinhart is professor of Science Studies at Humboldt-Universität zu Berlin and director of the Robert K. Merton Center for Science Studies. His work focuses on peer review and evaluation in science, on scientific misconduct, and on translational biomedical research.

ARTICLE

Beyond Scientificity:

Extensions and Diffractions in Post-Normal Science's Ethos

Karen Kastenhofer

kkast@oeaw.ac.at

Abstract

Eighty years have passed since Merton's famous publication of "a note on science and democracy," outlining the scientific ethos via four sets of norms, namely communism, universalism, disinterestedness, and organized skepticism (CUDOS). Merton's rationale was that the implementation of this ethos was instrumental in realizing science's institutional goal: "the extension of certified knowledge." Throughout the ensuing decades, Merton's conception has been at the center of heated debates in the emerging field of science and technology studies. It has also been addressed by empirical studies with a view to determine the scale at which CUDOS was supported by scientists themselves in explicit terms and/or conformed to in their actual practice. Some of these studies also make room for the possibility that CUDOS might have evolved throughout the past decades, incrementally adapting the norm sets. This article contributes to such empirical endeavors. Building on ethnographic work at a technology assessment (TA) institute, I find that a distinct shared ethos is tangible in TA's post-normal science practices—in collaborations with non-scientists as well as with "pure academics." A reconstruction of TA's distinct ethos from my empirical material results in the delineation of a post-normal scientific ethos, comprising "extended communism," "diffracted universalism," "diffracted disinterestedness," "extended organized skepticism," "diffracted originality," and "extended relevance." These "extensions" and "diffractions" have ramifications for the organization of post-normal science and its interaction with academia, publics, and polities.

Keywords

INTRODUCTION

When Robert K. Merton set out to map the ethos of modern science in 1942, "that affectively toned complex of values and norms which is held to be binding on the man of science," he defined four sets of institutional norms, namely communism, universalism, disinterestedness, and organized skepticism, abbreviated as CUDOS. He noted that these imperatives had not been codified but could be

"inferred from the moral consensus of scientists as expressed in use and wont, in countless writings on 'the scientific spirit' and in moral indignation directed toward contraventions of the ethos." They were "transmitted by precept and example," "reinforced by sanctions," and internalized by the scientists as their "scientific conscience" (Merton 1942: 116-7). Some later scholars have expanded the CUDOS set by discussing further components (e.g., "originality" as mentioned by Merton himself in later texts, cp. Stehr 1978), while a vast amount of ensuing science studies painted a more mundane picture of science as practice and culture, characterized less by distinct institutionalized imperatives than by individual strategic action—one need only recall Latour's 1984 "Portrait of a Biologist as a Wild Capitalist," Ziman's 2000 depiction of post-academic science in "Real Science," or Shapin's 2008 historical treatise on "The Scientific Life" as a "Moral History of a Late-Modern Vocation." Other scholars have highlighted the diversity of epistemic cultures (Knorr-Cetina 1999), thus drawing the unity of science into question. Commentators have summarized this turn towards a "New Sociology of Science" as a move to depict science as "just another P game," competing for power, prestige, and prosperity (Hooker 2003: 72), and to "deny that scientists have an ethos of their own and that they engage in a distinctive cultural activity" (Bunge 1991: 525). In more recent studies, interest in an internal scientific ethos seems to have been replaced by analyses of individual pragmatic coping strategies, disciplinary differentiation, and externalist explanations.

At the level of science policy, the situation has developed in equally ambivalent terms: on the one hand, a series of university reforms culminated in so-called full autonomy—providing for plenty of opportunities for self-regulation along an inherent ethos. On the other hand, these reforms went hand in hand with new modes of university governance, introducing managerial and entrepreneurial approaches in academia via target agreements and evaluation schemes, as well as strategic missions for all research via new funding schemes. Economic utility has become an important point of reference, complemented by missions to tackle society's grand challenges. As a result, the distinction between basic and applied research, as well as between private and public research, has blurred, and the contract between science and society has been put up for re-discussion (exemplarily: Hessels et al. 2009). But even if speaking of a scientific ethos nowadays comes with an antiquated pathos, there are good reasons to pay renewed attention: while a lot has happened in the scientific community and its relationship to society since the 1940s, the importance of internal ethical norms for an effective self-steering of science has never been explicitly denied. One could assume that within science itself, a new adapted ethos has emerged, taking into account the changing (or diversifying) character of science and the changing (or diversifying) position of science(s)-in-society. Such an alternative ethos need not be a set of unreconcilable counter-norms as proposed by Ziman (2000) for post-academic science; it can also be understood as an extension or incremental adaptation that builds on Merton's CUDOS. The programmatic writings of post-normal science theorists (Funtowicz and Ravetz 1993) seem to side with this latter option.

The following contribution revisits Merton's outline of the scientific ethos against this background. More specifically, I ask how the scientific ethos outlined by Merton in 1942 is being invigorated and possibly adapted in contemporary post-normal science contexts (exemplarily: technology assessment) that come with distinctly new functionalities, including but also transgressing the primary rationale of "exten[ding] certified knowledge" (Merton 1942: 117). I thereby combine the Mertonian outline of a scientific ethos and his conception of science as a distinct social system with a focus on practices and the thesis of a further differentiation of science. I do not argue for a fading of scientific ethos, but rather for its (local) adaptation in post-normal science contexts. In such contexts, the boundary between science and society is bridged via distinct practices, but within these practices it is at the same time reinvigorated and becomes even more relevant.

Before presenting my own research in more detail, I will give an overview of the state of discussion about Merton's ethos of science with a view to literature that addresses historical change in science (including general shifts as well as selective differentiation) and related change and specificities in the prevalent ethos. The presentation of these approaches and findings directly links to some methodological remarks on researching scientific ethos and allows for introducing my own empirical case and approach. A consecutive section summarizes the results of my study. In the final section, I discuss these findings with a view to my general research question. The aim of this contribution is not so much to define a concrete list of post-normal science norms, but rather to better understand post-normal constellations, which require a high degree of context sensitivity, reflexivity, and reactivity and demand that practitioners deal with partly contradictory imperatives on a daily basis. Last, but not least, I hope to illustrate that a distinct (if partly fuzzy, partly ambiguous) institutional ethos also plays a formative role in contemporary post-normal settings.

OPERATIONALIZING CUDOS FOR THE RECONSTRUCTION OF VARYING EPISTEMIC CULTURES

Merton's ethos of science has been both heavily referenced and criticized during the past decades. For the task at hand, I do not aim at discussing the context of its formulation (see e.g., Turner 2007) or the history of its scholarly uptake. Instead, I want to focus on its productive operationalization in empirical investigations of contemporary scientific practice and culture. In doing so, I start from the general assumption that it is worthwhile to focus on the workings and character of a distinct scientific ethos and methodically feasible to reconstruct aspects of it. I thus go with Merton's theses that (a) the scientific system is in some respects differentiated from other societal subsystems, thus exhibiting some autonomy and specificities, that (b) it is nevertheless to be understood as a *social* subsystem and thus suited for sociological analysis, and that (c) it is to be understood as a social *subsystem*, implying that science and social structure are not randomly associated. All three of these theses have become central cornerstones of science and technology studies, including the sociology of scientific knowledge, the anthropology of science, and ethnographies of scientific fields as well as theories of and discourses on science-in-context (from systems theory to socio-cultural histories of science).

I also stick to Merton's initial presentation of CUDOS that takes norms (or "institutional imperatives") as pars pro toto for ethos, including norms, cultural values, mores, ideas, standards, and imperatives.[1] These are not necessarily codified but "expressed in the form of prescriptions, proscriptions, preferences, and permissions," "legitimatized in terms of institutional values," "transmitted by precept and example and reinforced by sanctions," "in varying degrees internalized by scientists (…) fashioning their scientific conscience," "scientific mind," or "superego" (ibid.: 116). Giddens and Sutton (2013) define norms as "rules of behaviour that reflect or embody a culture's values, either prescribing a given type of behaviour or forbidding it," thus constituting one element of science understood as a socio-cultural system. Within the "New Sociology of Knowledge," scientific culture, socialization, and mindset have mostly been researched in the plural.[2] I argue that this was only in

[1] Merton's outline stays slightly fuzzy in this respect; the subtle differences between ethos, mores, norms, imperatives, and standards are not addressed. For the context at hand, I want to note that ethos does not refer to just any "rules of the game," but to collectively held moral ideals that serve as guidelines for individual actions and their appraisal for "a man of science."

[2] Flecks's thought styles (1994[1935]), Snow's (1961) Two Cultures, Becher's (1989) academic tribes, Knorr-Cetina's (1999) epistemic cultures, Meyer and Molyneux's (2010) epistemic communities, or the German school of higher education research into socialization and enculturation taken up by scholars like Arnold and Fischer (2004).

exceptional cases a move to question a joint normative basis of science that was distinct from other social systems; in most cases, the plurality of cultures to be reconstructed was rather a methodological necessity as the distinct characteristics of a culture and the cultural dimension of science as such could only be reconstructed in comparative ways, necessitating the comparison of more than one "tribe" or "field."[3] Still, these juxtaposed tribes and fields were seen as, in principle, comparable and thus of a sufficiently similar kind. The anthropological approach helped to better understand and research normative systems or "an individual's normative orientation" that were neither "fully knowable, since many of a social system's norms remain latent until they are challenged or violated" (Anderson et al. 2010). Anderson and colleagues (2010) thus recommend ethnographic or anthropologic approaches, but they note that "[n]one of these approaches has been used specifically and deliberately to investigate the normative structure of science [as such]" (ibid.: 373). The discourse on Mertonian norms still runs somewhat in parallel to a discourse on epistemic collectives and cultures, with both discourses necessarily addressing normative repertoires and structures of science, but unfortunately not joining forces in reconstructing a shared scientific ethos. Mertonian norms are instead being researched mostly via quantitative, hypothesis-testing surveys. These tend to test explicit support for or perceived adherence to CUDOS, resulting in "exact numbers," with the unsolvable problem that "Merton relied on simple labels to represent broad normative principles, but (…) such labels are subject to widely differing interpretations."

Interestingly, the quantitative studies make room for differences between proponents of different disciplines, career stages, or age groups, and thus for the possibility that the scientific ethos and its role might differ in some details within the scientific landscape and across historical times. This adaptation is admittedly in some conflict with Merton's initial socio-political ambition to argue against "a random association of science and social structure" and for an unquestionable and somewhat exclusive fit between *the* ethos of science and a distinct socio-political order (namely democracy). Merton's original ambition requires us to assume essentiality and immutability on the part of science and its ethos, at least to some extent. It seems irreconcilable with later, more relativistic stances. But this relativistic turn in itself might have something to do with fundamental changes in scientific ethos.

LITERATURE ON ALTERED OR DIVERSIFIED SCIENTIFIC ETHOS

This empirical study is certainly not the first one to bring the role and distinct character of collectively held norms in a scholarly field to the fore. It is also not the first one to address their historical dimension. Especially in the context of concepts that denote widespread paradigmatic change in science, the stability of norms over time is put up for discussion. The list of usual suspects includes conceptions of Mode-2 science (Gibbons et al. 1994), post-normal science (Funtowicz and Ravetz 1993), Open Science / Science 2.0 (taken up e.g. by Cohoon and Howison 2021; and Bucchi 2014), and technoscience (Forman 2007; Nordmann et al. 2011) as well as triple helix (Etzkowitz and Leydesdorff 1997), post-academic, and post-industrial (Ziman 2000) research. Moreover, historical incidences like the "Climategate" controversy prompted debates about adequate scientific norms and

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[3] Or the comparison of one's own unconscious conceptions with those of a different field. Already in 1944, Feibleman specifies in this vein: "The ethos cannot be felt by the individual as existing in himself, and it is rarely felt by him as existing in his own social group. It can more easily be apprehended explicitly by individuals from social groups of which they are not members. The flavor, the qualitative aroma, of a social group is more easily detected by foreigners simply because it is fresh (odors make a strong first impression) and because the foreigner is not apt to be full of preconceived acquaintance with the details of the organization of the social group."

role sets (Grundmann 2013; Bray and von Storch 2017; Brüggemann et al. 2020; Cohoon and Howison 2021). Pertinent empirical investigations build on diverse methodological approaches, from questionnaires[4] to documentary analysis,[5] from mixed methods[6] to ethnographic work[7] and action research,[8] targeting various (post-)academic settings[9] and national contexts.[10] Some authors target rhetorical strategies; others study the explicit endorsements of norms; yet others investigate self-reported behavior or aim at reconstructing implicit, tacitly held beliefs. In many cases, these various themes blend in one way or another. Nevertheless, the different methodological approaches all have to deal with the fact that implicit norms, explicit endorsement of norms, and observable/reported behavior relate to each other, but are still different things—or, put differently, the violation of a norm by an individual or even collective action alone, as might be observed in qualitative studies or reported in quantitative surveys, does not necessarily negate its existence. In his theory of social structure, Merton himself has made room not only for the case of conforming to given cultural values and institutional means of their implementation, but also for deviating from them by mode of innovation, ritualism, retreatism, or rebellion (Merton 1996[1938]: 132-152). Recent empirical investigations into CUDOS—especially the hypothesis-testing quantitative sample—thus mostly support Merton's ethos as they find that the majority of respondents subscribes to CUDOS and reports on adherence to CUDOS in scientists' behaviors. But they also face methodological difficulties, as they cannot fully translate high subscription and observation numbers alone into a final prove of CUDOS.

The situation becomes even more complicated, if the deviation from the norm seems to become the rule and if deviant behavior is not sanctioned in dissident collectives. Such might be the case with shifts in ethos along fundamental shifts in scientific culture and function in general or with the formation of alternative sub-cultures and sub-communities. Evidence of such cases is mostly presented in qualitative studies. Ziman (2000) prominently characterizes the proposed historical shift from academic to post-academic science as an "undramatic revolution" (ibid.: 68) and thus refers to a fundamental shift in scientific culture in general. Ferretti and Guimãres Pereira (2021) discuss the DIY movement as an opt-out of the established system and its ethos. In both variants, new norms can still be depicted as adding to and coexisting with (rather than superseding) the classic set. They can conflict with the classic set or represent its unproblematic extension. Especially in the former case (but to some extent also in the latter), the resulting pluralization of norms (and, even more importantly, primary values) results either in ethos ambivalence in which choices have to be made (consciously or unconsciously) on a constant basis—ambivalence stretching the dimensions of action as well as identity and sometimes referred to as "work," "struggle," or "trouble" in other literature[11]— or in further differentiation within science relating to differing ethos regimes, thus fragmenting (cp. Bucchi 2014) or "unbundling" (Macfarlane 2011) the "republic of science" (Polanyi 2000).

[4] E.g., Macfarlane and Cheng (2008); Anderson et al. (2010); Bray and von Storch (2017); Bieliński and Tomczyńska (2018); Kim and Kim (2018); Ferretti and Guimarães Pereira (2021).

[5] Holten (2010), Kønig et al. (2017); Grundmann (2013); Cohoon and Howison (2021); Henze (2021).

[6] Anderson et al. (2010).

[7] E.g., Maxwell and Benneworth (2018); Koehrsen et al. (2020).

[8] Temper et al. (2019).

[9] E.g., Holten (2010) for the US Bioeconomy Institute; Kønig et al. (2017) for post-normal Science documents.

[10] E.g., Macfarlane and Cheng (2008) for the UK; Anderson et al. (2010) for the US; Bieliński and Tomczyńska (2018) for Poland; Kim and Kim (2018) for South Korea.

[11] Another kind of ethos-related "struggle" Anderson et al. (2007: 11-12) label as "dissonance": it arises in an academic milieu in which observed "normal" behavior does not align with one's principled basis for one's decisions.

In some cases, authors also advocate the integration of existing, initially incompatible ethos into one consolidated form by parts of a collective. Etzkowitz (1998), for instance, argues that a new persona of entrepreneurial scientist successfully supersedes the classic academic persona, albeit not being taken up by all scientists. He sketches experiences of conversion in individual scientists' biographies that suggest:

> the transmutation of ambivalence—the opposition between two principles, one primary, the other secondary—into consonance and the reformation of ideological elements into a consistent identity. Entrepreneurship is made compatible with the conduct of basic research through a legitimating theme that integrates the two activities into a complementary relationship. For example, scientists often say that monies made from commercializing their research will be applied to furthering their basic research interests (ibid.: 827).

Similar judgments of successful integration also resonate in historical conceptions like that of "renormalization" by Metlay (2006) or the configurations of the "heterogenous engineer" of the technoscientific "seamless web" (Law 1987) and the "scientific entrepreneur" in Shapin's (2008) "moral history of a late modern vocation."

Accounts of fundamental shifts in ethos go hand in hand with the acknowledgement of new functions of and roles for science in society, adding to and competing with a primary rationale of "exten[ding] certified knowledge" (Merton 1942: 117). New functionalities and roles are sought in roughly two respects: Whereas technoscience as well as triple-helix, post-industrial, and post-academic science span the boundary of the public and the private, of basic and applied research, and of epistemic and engineering practices, and they meet increasing calls for "delivering economic returns" as well as "social responsibility" and "responsiveness," other emerging modes of science such as post-normal and transdisciplinary science span the boundary between scientific research and societal decision-making by engaging diverse publics and addressing diverse polities, following calls for evidence-for-policy and participatory decision making. The new functions and roles—if subscribed to and integrated by the pertinent scientific communities—are likely to result in new institutional imperatives. Public engagement practices as well as policy advisory practices react to and come with their own values, quality criteria, norms, and fallacies (as well institutions, communities, roles, and identities). Clark and Majone (1985), for example, suggest "adequacy," "value," "effectiveness," and "legitimacy" as qualitative criteria for science-for-policy. Policy scholars such as Weimer and Vining (1999) or Pielke Jr. (2007) define distinct roles in advisory practice that link to distinct contexts, paradigms, and standards (like avoiding "stealth advocacy" as "issue advocate"). In their analysis of "the ethos of post-normal science," Kønig and colleagues (2017) enlist explicit codes suggested by proponents of post-normal science and suggest the five-letter acronym TRUST[12] to denote the normative sets of Transparency, Robustness, Uncertainty management, Sustainability, and Tolerance. As post-normal science is understood to draw on normal science, "both CUDOS and TRUST will be intertwined in this practice. Therefore, PNS needs to confront, deliberate, and balance norm conflicts such as disinterestedness versus accountability and transparency; universalism versus pluralism and democratization" (ibid.: 22).

In all these cases, the social sciences and humanities (SSH) play a critical role not only as topical experts, but also as boundary experts in charge of organizing public engagement, making sense of the public discourse, or relating meaningfully to political contexts and reflecting on engagement and

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[12] "Whereas CUDOS (recognition) is what one achieves by complying with the norms and values of normal science, TRUST (the public trust in science advice) is what can be regained by the ethos of PNS." (Ibid.: 21).

advisory practices. Still, Merton's original outline does not provide us with a position on a potential ethos of SSH. Nor does any of the above cited contributions directly address a potential differentiation of ethos within the academic landscape along the "two cultures" (Snow 1961).[13] The lacuna could be explained with the specific view of the Mertonian ethos to grasp a universal scientific ethos beyond all disciplinary fragmentation and therefore to render a discipline-specific formulation of ethos meaningless, if not counterproductive. The discipline-specific aspects of scientific ethos would then be addressed under a different label, not as "ethos of science" but as (field-specific) "epistemic cultures," or as an "ethos of intellectuals" in the case of the humanities and arts, thus fragmenting the theme along separate discourses. Another option is that a distinct SSH ethos is seen as simply not relevant enough to be researched. The missing search for a valid arts and humanities ethos certainly has important repercussions in the context of multidisciplinary collaboration. It furthers the risk of "epistemic domination by technological disciplines in both funding streams and research content" (Maxwell and Benneworth 2018: 2) within mission-oriented research programs like the European Horizon 2020 program; it also impoverishes the general discussion of scientific ethos, for example when it comes to more recent takes on "universalism" or "disinterestedness."

RE-CONSTRUCTING AN ETHOS OF TECHNOLOGY ASSESSMENT PRACTITIONERS "FROM WITHIN"

In the following section, I report on findings from a project conducted at the Institute of Technology Assessment (ITA) at the Austrian Academy of Sciences. Technology assessment (TA)—like other inter- and transdisciplines (cp. Kastenhofer et al. 2011)—has been institutionalized only some 30 years after Merton's treatise on the scientific ethos, first with the foundation of the Office of Technology Assessment at the United States Congress in 1974, and later (from the 1980s onwards) with the creation of several technology assessment facilities at European parliaments, universities, and academies of science. As "an analytic and democratic practice that aims at contributing to the timely formation of public and political opinion on societal aspects of science and technology" (van Est and Brom 2011), TA represents a post-normal or Mode 2 science—especially when performed at an academic institution. It subscribes to a bifold focus on both scientific research and advisory practice. With its meanwhile longstanding tradition and high degree of institutionalization, TA can serve as a valuable case for researching institutionalized imperatives. Targeting transdisciplinary practice at ITA allows for addressing the question of whether there is such a thing as a distinct ethos of post-normal science. To do so, I build on "research from within" (Sikes and Potts 2008; Trowler 2011) performed at ITA, dedicated to questions of successful policy advice.[14] ITA was created in 1994 as an institute of the Austrian Academy of Sciences (cp. Nentwich and Fuchs 2021). It operates based on basic funding (covering 16% of the research projects and all general costs), competitive research funds (38% of the research projects, funded by the European Commission, the Austrian Research Promotion Agency, or the Austrian Science Fund) and commissioned research (46% of the research projects, cp. Kastenhofer et al. 2019). Research is commissioned by public entities like Austrian ministries, the Austrian and European Parliament, or the Austrian Chamber of Labour. For want of a better term we will call these funding institutions "clients"; in most cases, the client and its publics are also the main addressees of the project results, while society at large is the targeted beneficiary.

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[13] Some quantitative surveys include a disciplinary split in their analyses. But they do not make room for a specific adaptation of the CUDOS set beyond differences in levels of endorsement for the various disciplines.

[14] "Policy Advice at ITA," Pol[ITA], 2016-2018 (cp. Kastenhofer et al. 2019; Bauer and Kastenhofer 2019; Kastenhofer and Bauer forthcoming).

Our "research from within" included all scientific staff at ITA[15] in one way or another: as project members with various tasks, as interviewees, and as participants in internal workshops and group discussions. It also addressed collaboration partners and clients via a series of interviews.

The in-depth interviews, group discussions, participatory observations, and "observing participation" touched upon internalized imperatives and normative conflicts carried by TA practitioners and led me to reconstruct these in a secondary analysis with a view to Merton's CUDOS conception.[16] As mentioned above, scientific norms as part of epistemic cultures are "never fully knowable" and become tangible only in distinct situations, such as "when: (a) one enters the social system for the first time (...); (b) one moves from the social system into a related but different arena (...); (c) in times of environmental change (...); and (d) when violations or the potential for violation of latent norms leads to more deliberate articulation of the norms"[17] (Anderson et al. (2010: 374). In all of these cases, tangibility is effectuated by situations of cultural "estrangement." Reconstructions of epistemic cultures have highlighted this situation and built on it, acknowledging that such estrangement can either take place during socialization, enculturation, and initiation of "novices," when scientists switch fields and thus epistemic cultures (including their distinct "institutional imperatives"), when individual scientists violate prevailing norms, or when the science system undergoes changes that lead to the estrangement of established participants. Moreover, ethnographers of science can make use of their own socialization "outside" the field they study and thus confront themselves and scientists they encounter in the field with contrasting norms, mores, and illusions.

This analysis also makes use of situations of estrangement that render ethos more tangible. With a "research from within" approach, it cannot rely on cultural estrangement based on differences between the epistemic cultures of the ethnographers and the ethnographed—as both were part of the technology assessment field when the project was conducted. Still, the material reveals several sources of estrangement *within* the field that obviously motivated interviewees to refer to aspects relating to ethos in a broad sense. Moreover, the set of interviews I will focus on here[18] relates to distinct lines of advisory work conducted in close collaboration with other research institutes as well as with clients from polity. Parts of this work included participatory activities with stakeholders and/or publics. Five in-depth interviews addressed TA practitioners (mostly the projects' principal investigators); another ten interviews addressed collaboration partners (with universities as well as with one small service enterprise) and clients (governmental agencies, ministries, and the like). The interviews thus depict the relationship between TA, "normal science," politics, and publics, especially their relative functions, roles, and norms.

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[15] Ca. 20 TA practitioners from diverse academic backgrounds, ranging from biology and chemical engineering to sociology and philosophy (see also Kastenhofer and Bauer, forthcoming. TA projects are performed in varying interdisciplinary teams and combine scientific research with advisory activities.

[16] Moreover, it brought identity struggles to the fore that are summarized and discussed in Kastenhofer and Bauer (forthcoming). A consecutive internal project took an even closer look at TA and normativity ("TAN: TA and Normativity", cp. Nierling and Torgersen 2020).

[17] The latter situation is frequently related to Durkheim's (1995) notion that "the significance of a norm is indicated by the extent of moral outrage or indignation that ensues when a norm is violated" (quote taken from Brey & Storch 2017).

[18] The empirical material covers four project lines (PL) of TA work at ITA: the first centers on a distinct socio-political issue related to technological innovation (PL1), with a primary view to gathering expertise and fact finding; the second centers on serving public interests via providing the respective client with high quality information (PL2); the third centers on the governance of a distinct technoscience (PL3); the fourth centers on a distinct participatory method, adapted to various contexts (PL4).

As mentioned before, the relevance of collective normative referents in TA came to the fore in an empirically grounded way (Corbin and Strauss 2008) only *throughout* the completion of the field work. Instead of directly asking participants about their normative stance (a move that would have ignored the fact that collectively held norms are only partly explicit and become tangible only in specific situations), I developed an increasing sensitivity for normative issues in my field work and searched empirical material *ex-post* for sections that relate to a collectively held ethos. With this analytical step, it was essential to choose the right level of abstraction, distinguishing between the field's main objectives, its normative dimension, and its tools to realize these objectives. I looked out for instances of normative comments, (moral) satisfaction, and minor or major (moral) outrage, sampling along emotions as well as content. Codes included normative ambivalence, disappointment, controversy and conflict, stories of success or failure, attributions of quality, and functionality of TA activities. The resulting references were analyzed with an aim to reconstruct the kinds of ethos that underlay the various statements. Thus, a distinct perspective on the workings of ethos in TA was established. Moreover, by including interviews with TA practitioners as well as with collaborating scientists and TA's clients/addressees, the material provided insights into views from the inside as well as the outside. The results were summarized in close reference to Merton's CUDOS, outlining specificities and amendments, hinting at additional functionalities of TA that relate to its close interaction with polities and publics.

THE POST-NORMAL ETHOS OF TA AND ITS SOCIAL FUNCTIONS

"Extended" communism

In Merton's original conception, the norm of "communism" refers to sharing "the substantive findings of science" as "a product of social collaboration" with the scientific community or "scientific fraternity" (Merton 1942: 122). He also notes that "[t]he scientific communism of the scientific ethos is abstractly incompatible with the definition of technology as 'private property' in a capitalistic economy" (ibid.: 123). When revisiting this conception, two aspects are conspicuous: first, the realm in which scientific findings ought to be shared has expanded, so as to encompass society at large. In parallel to Merton's partly epistemic, partly moral legitimation of communism (collaborative input warrants sharing of output), recent open science movements and open innovation strategies argue not only that communism is an epistemic prerogative, but also that public investment in science warrants sharing its results; similarly, recent post-normal science asks for an "extended peer community" along an epistemic and moral rationale (more robust findings for better risk governance in society). I borrow the attribute of extension from this literature. Secondly, the discrepancy between scientific communism and industrial secrecy still serves as a bone of contention in contemporary TechnoScienceSocieties (Maasen 2020). With an increasing convergence of scientific and engineering practices and a consecutive clash of ownership cultures, it poses not only a moral but also a practical problem.

As for the practice of TA, communism *beyond* the scientific community has played a central role from its very beginning. Not only is informing polity part of its raison d'être in all contexts where TA is institutionalized as parliamentary TA or as an advisory body to governments; informing publics or society at large is also perceived as a central duty of TA. This ethos is present in TA proponents' substantive propagation of open science and in institutional codes of conduct and publication strat-

egies. It was also voiced by TA practitioners in our internal project. One interviewee depicts TA practitioners as "experts not only for producing, but also for communicating knowledge" and highlights that they hold the respective expertise as well as the resources (I PL3/3).

With communism beyond the scientific community, additional values come into play, including: extended transparency, accessibility of reports for the lay public ("the visions and recommendations that are developed [in participatory exercises] get published as such; they are publicly accessible. Transparency is the best impact we can achieve."), balanced provision of information, reader friendliness for lay participants, and time pressured decision makers. A lot of effort is dedicated to alternative formats (the "Dossier" and "Infogramm" series), modes, and contents of exchange with a view to foster engagement and prevent bias or power imbalance as best possible.

> In project [X] we work with information videos and these are very difficult to prepare; because they must not be manipulative, but should provide balanced information in a concise way. (…) And they should also be funny. (I PL4/6)

> Everything should be kept short and crisp, with a very pragmatic approach. Because nobody has time to read. I understand that this bothers the scientists. (I PL4/3)[19]

> We need condensed versions, not lengthy treatises. The higher in the political or ministerial hierarchy, the more confined is the appetite for reading. (I PL3/2)

With close contact between clients and TA practitioners in some advisory projects over longer periods of time, communication often becomes a two-way street resulting in an ongoing dialogue and collaborative networks that expand over time ("[this participatory method] is an intelligent synapse between customers, citizens, affected populations and experts." I PL4/3), so much so that TA practitioners emphasize that some projects "are not projects, but processes" (I PL3/3, I PL3/2).

On the other hand, industrial property rights and military secrecy pose fundamental problems for TA. Emerging technological innovations such as bio- and nanotechnology applications or military drones are themes highly relevant to, but not fully accessible to, technology assessment activities. If participatory methodologies are patented or licensed, they are not fully available in innovation regimes. As a result, TA's service to liberal democracies is restricted and options for responsible innovation are hampered.

"Diffracted" universalism

Since Merton confidently wrote that "[t]he acceptance or rejection of claims entering the lists of science is not to depend on the personal or social attributes of their protagonists; their race, nationality, religion, class, and personal qualities are as such irrelevant" (Merton 1942: 118), ensuing sociologies of science have seen constructivist, relativist, positionalist, and epistemological turns. As a result, the initial formulation has not been discarded as an institutional imperative, but it has been refined in many respects as an epistemological presumption. TA practitioners' accounts reflect their awareness of these more recent epistemological refinements, but they also add aspects that come with the post-normal ambition to contribute to the solution of societal problems and with the resulting close interaction with distinct publics, polities, and persons. The insights of TA should not only answer to

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[19] Internally, the institute's advisory formats are also referred to as "two-pagers" or "four-pagers."

standards of truth, but also to standards of cultural and social robustness ("[When interacting with participants, I also have to be aware that] I am a woman, of my age, from a specific cultural background"). It does not suffice to "be universally right"; TA should also foster a climate of mutual respect and be heard and considered in highly contextual decision-making processes. Whereas Merton sees universalism "rooted deep in the impersonal character of science," the success of TA has very person-centered components. Thus, TA's ethos differs in some respects from normal science, a difference that I suggest can be considered a "diffraction"[20] (a term borrowed from physics and mobilized of late by feminist science and technology scholars such as Karen Barad or Donna Haraway)— the original ideal picture of universalism is diffracted by additional layers or lenses of post-normality and Mode 2. Such diffraction of a normal science ethos can cause occasional conflicts in collaborative projects. In interview I PL1/2, a collaboration partner reflects on a past difficult project:

> [T]here were some really academic partners, really interested in statistics and so on; and there were some partners from the [agency X] and so on, which were mainly interested in other kinds of questions. I can remember very difficult discussions (…) the partners from the social sciences, well, first they were academics so they had to publish - and to publish something new and rather academic, and not always policy-oriented; and so they [decided] to develop a model that they wanted to test with hypotheses and so on; (…) these were kinds of things that were not so easy for us to use because it's not always really relevant for policy-making. (…) I understand that you want all these questions to help your model, but now we have a [participatory] method, we are here for one day and you cannot expect that [the participating citizens] will answer all these questions. (PL1/2)

Whereas TA practitioners do not defy universalism, they are also committed to including publics and achieving political impact. Other than having a quest for universal truths, they must answer to mostly national remits and depend on local specificities. Very importantly, the impact of TA relies on local political windows of opportunity, or on what has been labeled as "Kairos" in literature on Mode 2 science (Holten 2010) or post-normal science (cp. Ferretti and Guimarães Pereira 2021). References to such "Kairos" are omnipresent in the interviews. Sometimes developments at the TA institute coincide with demands from polity to get input on pressing issues, resulting in a productive collaboration. Other times, no political decisions are pending and there is no interest in input from TA. Sometimes an issue is already addressed at the transnational level, and it is TA's role to raise the local polities' awareness of this issue and thus co-create demand for political discussion and expert input. In these processes, local specificities (the national public discourse, the institutional landscape, individual decision makers, and government personnel) play an important role as do personal contacts (knowledge about TA practitioners' expertise, personal trust, and sympathy), as one collaboration partner from polity puts it:

> [I]t is good to know someone personally, not having to write [formally] to the institute's director to explain one's problem; if I know an expert in this or that field of expertise, I can address him/her informally or we meet accidentally at a conference and can chat about current issues and developments. (I PL4/4)

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[20] My initial choice was to speak of "bounded universalism," but rather than putting universalism in its place, "diffracted universalism" goes with changing conceptions of and strategies for safeguarding a robust relation between research subjects, research objects, and claimed truths; in other words, to achieve transpersonal, generalizable formulations, the research as a person has to be taken into account rather than made irrelevant. This methodological move is paradigmatic in most qualitative sociological or ethnographic research.

"Diffracted" disinterestedness

The fate of the institutional imperative of disinterestedness in post-normal science seems very similar to that of universalism. Again, the more recent social epistemology of science as well as recent shifts in innovation regimes have added some tweaks to how we perceive the relation between science and interests, without discarding the whole norm as such. Disinterestedness still counts as a cornerstone of basic science, albeit leaving room for exceptions and relativizations. The ethos of disinterestedness is complemented by calls for making unavoidably persevering interests transparent (e.g., when clinical trials are funded by the pharmaceutical industry or when academics hold shares in private companies) or by claims that interests are not necessarily a bad thing and that epistemic, private, and public interests are not necessarily at odds. Such developments also relate to new practical ontologies of science, reconfiguring the relation between "pure" and "applied research," as exemplified by new funding program rationales that target specific research qualities like responsibility or specific outcomes like fostering economic competitiveness or sustainable development. But throughout all these shifts, disinterestedness has remained as a (albeit "diffracted") cornerstone of science's institutionalized ethos.

The practice of TA relates to the above-mentioned shifts in two ways: as a *Mode 2 practice*, it is intrinsically but also explicitly oriented towards societal values pertaining to a "socially responsible technology policy."[21] Projects address issues such as privacy, security, inclusion and empowerment, justice, equality, health and environmental risks, sustainability, democracy, and good governance (cp. Kastenhofer et al. 2019), and TA practitioners occasionally transcend the role repertoire of normal science by acting as agenda setters or issue advocates (cp. Bauer and Kastenhofer 2019). TA is thus invested in serving the public good as opposed to partial and private interests or an ignorance of public interests. As a *post-normal science*, TA is directly confronted with lay clienteles, and thus with the danger or "possibility of exploiting the credulity, ignorance, and dependence of the layman" (Merton 1942: 125). It also interacts directly with stakeholders and decision makers. Against this backdrop, the normal peer review processes institutionalized for scholarly communication and scientific project approval do not suffice. The more it becomes a matter of course that TA is not practiced in an interest-free setting and, in many cases, confronted with highly controversial issues and antagonistic stakeholder camps, the more TA practitioners seem to experiment with additional approaches to quality control, expose their work to extended skepticism (see next section), and thus safeguard a healthy relation to vested as well as public interests.

Moreover, the interviews with principal investigators at ITA and with their clients show that safeguarding autonomy is central in the public assessment of their authority and the legitimacy of their policy advice. Both groups of interviewees state that the institute being independent (from stakeholders and private money) and ITA's impeccable reputation to open-mindedly strive for the public good is just as important as the topical, interdisciplinary, and methodological expertise held by its staff members.

> Together with [our client] I was in [X] at this council where the first attempts had been presented on how to address [this technology] in technology assessment. And [our client] said instantly that ITA should be in charge. That we needed an independent institution, especially independent from industry, that was also accepted and approved of by the general public. That this was of utmost importance. (I PL3/4)

In some cases, ITA was selected by clients to outbalance an otherwise industry-driven innovation process; in other cases, ITA was selected because of its extramural affiliation (that is, not with the same research organization at which the technology at stake was being developed). ITA's relatively closer tie to polity than other research institutes and existing personal contacts also figured in some selection processes.

The English term "at arm's length" maybe best depicts the sensitive juggling acts at the institutional and individual level that come with this kind of "diffracted" disinterestedness. While one PI thought it was very important to keep individual interactions with clients as formal as possible (opting for the polite and distanced German "Sie"), another PI interacted on amicable and informal terms; but both interviewees shared a high awareness of keeping the balance between distance and interpersonal proximity, between abstinence and engagement. They were aware that any breach in this dimension could harm their own and their institute's reputation, possibly irreparably. This attitude also implied gaining the potential clients' attention for a specific issue, but not elucidating too much interest; motivating further research calls, but not necessarily applying for funding in the same call; aiming for presence in the public media, but not at all costs; or refusing what got pejoratively called "acceptance research" ("Akzeptanzforschung")—activities that were merely meant to foster public acceptance of technological innovations; and, last but not least, it implied a good deal of constant individual and collective reflection on the role of stakes and stakeholders in TA processes as well as methodological experimentation with how to involve stakes and stakeholders in participatory processes.

Interestingly, all collaboration partners and clients seemed to tacitly agree on this role set and interaction rituals with almost no exception. The ethos of a disinterested science, be it normal or postnormal, was shared by all, and its functionality and instrumental necessity in innovation governance was unquestioned.[22]

"Extended" organized skepticism

The previous sections already brought up the need for an "extended skepticism" by way of an "extended peer review" of "extended facts." Extensions pertain to the dimensions of TA work to be scrutinized, the organization of review processes, and the selection of reviewers. Scientific and social robustness as well as social and political relevance are addressed; review takes place not only during project submission and the publication of outcomes, but also throughout the project's implementation by internal and external advisory boards; reviewers and board members stem from multiple disciplines and, in some cases, from outside of academia. With extended advisory boards, the function of safeguarding quality goes hand in hand with informing societal actors and establishing expert networks. This extended functionality goes with TA's remit to produce more than mere facts (see also the next section).

> It is our ambition to develop visions [during this participatory exercise], but also to establish a respectful interaction, empowerment; we define distinct quality criteria [for this process], we write scripts collaboratively, [A] supported us a lot with [project X], [B] reviews them internally (…) With project [Y] we decided on having an additional external project board,

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[22] Only once did a TA practitioner ponder that the respective ministry might not have understood why ITA did not submit a proposal to the very call it had advised the ministry to launch.

representing addressees and other topical research institutions. We had a meeting every three months [with these relevant actors]; we also invited them to review the texts we prepared internally for communicating our results to the public and to give feedback. (I PL4/6)

Internal and external advisory boards perform extended review throughout the whole process; they also give feedback on the accessibility and applicability of results and at the same time serve as relay stations to further distribute outcomes and secure impact beyond the research community. Thus, skepticism is partly extended, partly diffracted, and related to functions other than quality control.

"Diffracted" originality and "extended" relevance

After Merton's 1942 publication, two further suggestions for institutional imperatives in science have been put forward quite prominently: relevance and originality. One might argue that these categories are not on the same level as communism, disinterestedness/autonomy, or organized skepticism, as they represent knowledge qualities rather than institutional qualities. Together with the objective to produce "true knowledge," they serve as criteria for peer review (or organized skepticism). Scientific output should thus be "new (i.e., original), true (i.e., scientifically sound), and interesting (i.e., relevant)." Nevertheless, these additional categories shall be taken up here because they also serve to illustrate how normal and post-normal science differ in fundamental orientations.

With the double ambition to produce facts and to advice society and polity, originality sometimes takes the back seat. In some cases, societal decision makers are reminded of already established scientific facts, such as climate change and the urgency to take adequate steps. In other cases, their existing appraisal of a situation is simply proven right. In these situations, addressees can respond with disappointment, as they had awaited astonishing new insights from an academic institute like the ITA. Such disappointment was reported by our interviewees especially in relation to participatory projects. The client had expected revolutionary findings while the TA practitioners had sought something completely different, something that they thought more important to establish relevance and impact in the respective context: establishing mutual trust and understanding and finding new and creative ways of interaction. One client concludes after the participatory exercise that (disappointingly, but expectedly):

> public participation has two dimensions: one is certainly useful, namely when it comes to trying new things out, to see where people are affected, and the like; the other dimension, thinking things through is an individual process, richness of ideas is not higher in the populace; really good, well thought through ideas do not come from there. (I PL4/3)

Thus, participatory processes seemed not easy to sell and sometimes even triggered conflicts with academic peers.

> [t]here was this event in Florence where we discussed some first results with a group of experts and we invited [two very well-known experts]. I discussed with them and they were both critical about the results. [The academic expert] was critical in the sense that she said, 'Well, I didn't learn much. (...) nothing new, and we don't need that; we really now need some focus, a specific answer, and this is not enough!' So, she was really disappointed, she said, 'Well, it was a waste of time for me to [come]!' and so it was really hard; [The practical expert] said, 'Okay, it was interesting, but in a way these are things that we could more or less expect.' And so he also had the feeling that it was a lot of effort for quite interesting results, but not that

new for the policy-making. (...) it's something related to these participatory processes which are not easy to sell. (I PL1/2)

Thus, TA's ambition somewhat differs from both purely academic and political expectations. This "diffraction" is most easily explained with TA's efforts for more than epistemic relevance, namely for societal impact. Even before societal impact began to be a quality criterion applied within research evaluation exercises, it was very common for TA practitioners to measure the worth of their work with a view to changing society for the better and making the world a better place (cp. Hessels et al. 2009). This orientation towards impact is also perceptible in our interview series. Interviewees discard project reports as byproducts, not because they concentrate on "high impact" journal publications, but because they envision other, more effective ways to change societal perspectives and make-ups, e.g., by including publics and decision makers directly in the process as participants or as advisory board members, or by establishing mutually trusted and engaged expert networks and commissions. They lighten up when recounting how they got initially skeptical stakeholders on board with their process and could convey to them the value of the quality they strived for, in many cases by participating in person rather by rational argument alone.

DISCUSSION: TA'S POST-NORMAL ETHOS

This qualitative analysis of TA practitioners, their collaborators, and clients' take on TA practice has illustrated first and foremost that the classic ethos of science as depicted by Merton 1942 still figures centrally, even in a post-normal science field like TA. This outcome confirms other studies, especially those with a quantitative approach. The analysis also shows how institutional imperatives of science like disinterestedness (and thus the differentiation of science as a social subsystem from other social subsystems such as politics or economy) are stabilized by expectations and necessities from publics and polities, in line with discussions about boundary organizations and boundary work bridging the science-policy divide. Second, this analysis has shown that some changes from a normal to a post-normal science regime can be captured as extensions of the classic ethos, including: "extended" communism, "extended" organized skepticism, and "extended" relevance. Such extensions have already been put forward by early theorists and proponents of post-normal science (Funtowicz and Ravetz 1993) in cases of high epistemic complexity, high scientific uncertainty, and high societal risks. Third, the presented analysis points to some fundamental shifts that cannot be captured adequately as mere extensions. I have labeled these as "diffractions": "diffracted" universalism, "diffracted" disinterestedness, and "diffracted" originality. Taking a step back, orientation towards societal relevance and impact seems to be the strongest and most ubiquitously related to such diffractions. As the quest to change the world for the better has permeated scientists' ambitions and scientific research programs, holding on to academic ivory towers as strongholds of fundamental universalism, disinterestedness, and relevance has become ambivalent at best.

This ambivalence is not unique to late modernity, but it does affect the science system at an unprecedented scale. The advent of knowledge societies has not only altered the quantitative presence of science, but also multiplied its roles and functions in society (cp. e.g., Hessels et al. 2009) beyond the mere "extension of certified knowledge." This multiplication of function has gone hand in hand with the emergence of new epistemologies and ontologies and has necessitated adaptations in science's institutional imperatives. Post-normal scientists like TA practitioners seem to have come to terms with this new, partly extended, partly diffracted ethos and its new ambivalences, but open

questions and conflicts arise in collaborations with normal science as well as with clients and addressees. How should societal impact be assessed (cp. the discussion in Mitchell et al. 2015; Miettinen et al. 2015; Schäfer et al. 2021)? How should trade-offs between scientific originality and societal relevance, and between scholarly communication and communication to/with publics, be taken into account? And last, but not least, how should post-normal science and late-modern polities deal with values, norms, and interests, while adhering to evidence-based standards of decision making (cp. exemplarily Nierling and Torgersen 2020)? If post-normal sciences' ambition is to contribute to changing the world for the better—whose world and who's better? In these respects, post-normal science not only depends on a strong internal ethos nurtured by political awareness, ontological scrutiny, and debates about quality. It also depends on an effective and resilient democratic system that helps with delivering answers to these questions, be it by securing the accessibility and transparency of relevant information (e.g., on emerging technologies and applications), by furthering participatory decision making,[23] or by providing adequate boundary-spanning organizations between science and policy (like arm's length advisory bodies, structures, and guidelines). With every attempt at establishing a new TA facility in yet another nation state,[24] these critical aspects come to the fore.

Finally, there remains much room to discuss whether a post-normal science ethos still represents an ethos of science *in a strict sense*. Opinions will, inter alia, depend on the definition of science applied to this question. Is the label "science" reserved for an institution that focuses solely and uncompromisingly on the "extension of certified knowledge"? Or does the label encompass activities and practices that follow a dual ambition like extending certified knowledge AND constructing a new world "atom-by-atom" (technoscience), or providing relevant expertise to decision makers and civil societies (post-normal science)? If following the latter route, a differentiation of such technoscientific or post-normal science spheres from other societal spheres is still possible (as I tried to showcase), but the corresponding extended and diffracted normative set comes with additional ambiguities and contradictions. If an adapted ethos were not in place or did not result in sufficient self-regulation, the alternative would be to install (additional) political measures to guarantee the realization of the additional functions. We have seen such moves, if not from polity in a strict sense, then from an emerging hybrid science policy ecosystem. Initiatives such as Ethical, Legal, and Social Implications (ELSI) research and, consecutively, Responsible Research and Innovation (RRI) have been implemented in major funding initiatives of the European Commission; assessment guidelines for research projects and research organizations have been adapted to include new dimensions such as public outreach and societal relevance; and many funding agencies now request open access publication formats. One might conclude that the belief in science's self-regulation has waned and with it the belief in an (effective) ethos of science. Other recent developments point to yet another direction: more and more scientists raise their voices to intervene in the unfolding climate catastrophe, calling for immediate political action as concerned scientists. Similar moves could be observed during the unfolding COVID-19 pandemic. With that, an era might be approaching in which "nothing in science makes sense except in the light of humanities' survival," to adapt Dobzhansky's famous dictum, and all institutional resources, including science's ethos, would re-orient *within* science towards this overarching goal. With that, another era could dawn, not one of post-normal science, but of "survival science," with probably yet another institutional and normative constellation.

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[23] Albeit with this aspect, a tension between direct democracy and representative democracy, and between public consultation organized by governments and parliamentary decision making, has to be noted.

[24] TA is currently undergoing a phase of worldwide expansion, furthered by the GlobalTA network and initiative (https://globalta.technology-assessment.info/, last accessed 6 June 2022).

ACKNOWLEDGMENT

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Author biography

Karen Kastenhofer works at the Austrian Academy of Sciences' Institute of Technology Assessment as a senior scientist. She identifies professionally as an ex-biologist, a science and technology studies scholar and a TA practitioner. Her research focusses on techno-epistemic cultures and techno-science governance in the realm of the life sciences, on the socio-epistemic cultures of STS and TA, and on the innovation regime contemporary epistemic cultures and communities are embedded in.

ARTICLE

The "Normative Structure" of Social Science:

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

Steffen Hillmert

steffen.hillmert@uni-tuebingen.de

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

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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Author biography

Steffen Hillmert is Professor of Sociology at the University of Tübingen. His research interests include various aspects of the life course, education, work, social inequality and stratification, research methods, and the sociology of science. Amongst others, recent work has focused on the perceived legitimacy of affirmative action measures in the university context.

BOOK REVIEW

Solovey, Dayé:
Cold War Social Science

Philippe Fontaine

philippe.fontaine@ens-paris-saclay.fr

"Never judge a book by its cover!" they say. I shall not. Yet, the cover is the first thing you see and Christian Dayé and Mark Solovey, the editors of *Cold War Social Science: Transnational Entanglements*, made a sensible choice: a minimalist front cover picturing a notice board reading: "You are leaving the American sector." The four-language board suggests that it was located in one of the Allied zones of occupation following the Second World War. For a book placing special emphasis on the transnational dimension of Cold War social science, one could hardly think of a better symbol. I take it that most readers will get the message at once: they are invited to go beyond US-centered narratives.

For contemporaries of the postwar era, the message on the notice board was no doubt less metaphorical. When I first saw the book's front cover, I was reminded of a memorable story. In April 1950, the would-be economist and Nobel laureate János Harsányi (later John Harsanyi), his future wife and her parents, left illegally the increasingly Sovietized Hungary. Following a grueling and dangerous journey by foot from Budapest, they eventually reached the Austrian eastern border. From there they headed towards the four-power occupied Vienna, this time by bus, and eventually landed in the Russian zone. They were not lucky enough to see one of the notice boards marking the entry or exit of the various Allied sectors, but a bystander suggested they go over to the other side of the street, which was in the American sector. They did. Leaving the Russian sector was the first step towards a mind-changing experience—a process that will imply regularly re-visiting the foregone past in light of a changing present. In a sense, that is what Dayé and Solovey want their readers to do, though the context is altogether different: re-revisit the past of the social sciences in the Cold War now that the erosion of the American dream has prompted increased attention to the *oubliés de l'histoire*.

As is well-known, in the past thirty years or so, the history of the social sciences since 1945 has often concentrated on the Anglo-American world, relegating the transnational dimension of social science in the Cold War to footnotes. It must be admitted that inadequate attention has been paid to other sites, their own logics and interests and their role in acclimating the US and UK social sciences to a variety of situations. Nor has much been done to explain the way US and UK social scientists, their approaches, ideas and practices were transformed through exposure to new cultural environments. When efforts were occasionally made to go beyond the Anglo-American world, the story was often over-determined by the alleged irresistibility and imperturbability of the dominant side.

Studying this collection of essays, readers will reach the conclusion that other, decentered, histories of the social sciences can be written that challenge the one-way conception of international social scientific exchange and favor instead multivocal narratives. "In following developments in Cold War social science across diverse national contexts, including the United States and Soviet Union as well as many others around the globe," the editors write in their introduction, "this volume is also inspired by recent scholarship that has urged us to rethink certain fundamental points about how we should understand … the Cold War itself" (p. 3). Helped by the crumbling of Cold War triumphalism and the emergence of a multipolar world, the transnational perspective has gained ground to the point that diversifying observation points is gradually becoming second nature for historians of the post-war social sciences.

Those who contributed a chapter in *Cold War Social Science: Transnational Entanglements* are not the usual suspects, but their conclusions are no less valuable if only because they shed light on episodes which are still poorly known or often analyzed from a US-centered perspective. At the same time, we should not expect a transnational history of the social sciences to be built in a day. Following the "transnational turn" in history, Johan Heilbron, Nicolas Guilhot and Laurent Jeanpierre (2008) laid down a few markers for the social sciences, and *Serendipities* itself has made a special effort in that direction, but there is still a long way to go. On this inadequately signposted path, Dayé and Solovey's introduction and the eleven chapters comprising the volume prove useful.

Going through the essays composing the collection one after the other is not the best way to convey its contribution to the history of recent social science. Despite the editors' successful effort to weave together the various threads in the introduction, the reader would inevitably be submerged by the variety of viewpoints, topics and characters. It is better to see the volume as the outcome of an ongoing conversation producing mutual enrichment despite left-over questions and unsolved misunderstandings. Then, the quality of the collection is shown by the capacity of its essays to respond to each other within the framework defined by the editors.

That framework is defined as "the general subject of transnationalism in Cold War social science" (p. 4). More precisely, Dayé and Solovey identified three central themes that form the skeleton of the collection:

- the role of institutions in promoting transnationalism;
- the impact of transnationalism on Cold War social science;
- and the impact on transnationalism on the way their practitioners see the nature and meaning of the social sciences.

Those interested in the themes above will find much of interest in the collection, but I would like to depart from the editors' organizing themes to insist on two important aspects of the collection.

First, there is the issue of the relationship between the personal transformations undergone by social scientists confronted with other cultures and the way they approach society. Interestingly, half of the essays in the collection examine their subjects through the story of individual social scientists. Not all focus on the scientific personae of social scientists, as does Begüm Adalet when she examines the careers of Dankwart Rustow and Frederick Frey, two US-based scholars in comparative politics; but all insist on the necessity of questioning the separation of the scientist and the person and exploring instead the causes and consequences of the changing worldview of the individual immersed in another culture. Clearly, social scientists learn to adapt to new cultures and their personal change affects in turn the way they approach their new environment as much as their culture of origin. Think of the American library scholar and entrepreneur Eugene Garfield who developed the Science Citation Index (SCI) as a commercial product. As Elena Aronova shows, Garfield benefitted from the interest of Soviet researchers to give his project a second wind after it failed to convince US scientists and policy makers that it could help them keep abreast of increased scientific information. Likewise, Garfield's experience of Soviet information management as much as his capitalist instincts gave him a significant advantage as he pursued his efforts in the US, and as SCI eventually became an indispensable tool for many scientists there.

Consider, likewise, the US anthropologist Charles Wagley and the University of Chicago-trained anthropologist Felipe Landa Jocano. As shown by Sebastián Gil-Riaño, the work of the former depended on his involvement with transnational networks, including various strata of Brazilian society. As a result, Wagley's social scientific work can hardly be dissociated from his more general experience of Brazilian society. His personal attachment to Brazil—his wife was Brazilian—placed him in a better situation to appreciate the cultural specificity of the groups he studied and encouraged him to try to see the world from a non-US viewpoint. From this perspective, it seems difficult to examine the making of Wagley as a Cold War area studies expert without recognizing the influential part taken by a number of Brazilian actors and institutions in the process, and it would be a mistake to limit the influences on his studies to the orientations of US anthropology or to geopolitical and national security concerns alone. Jocano offers another interesting example, though his was that of a native Filipino returning home after graduate training at the University of Chicago. As the nation's most influential anthropologist, Jocano developed close ties with dictator Ferdinand Marcos and, as a result, he was in a good position to propagate the message of Western modernization theory in his home country. Here again, however, his story reveals more complexity than expected, for his vision of modernization was informed by the willingness to take the nation's pre-modern heritage into account. Christa Wirth shows the relevance of the Cold War in Jocano's life and work, but in so doing she makes clear that his experience within the Philippine Studies Program at the University of Chicago, or his endorsement of structural-functionalism, did not imply unconditional adherence to US Cold War interests or their embodiment in modernization theory.

Finally, Per Wisselgren followed the Swedish social scientist Alva Myrdal as she embarked on a new adventure as head of UNESCO's Department of the Social Sciences in 1950. Myrdal's social scientific internationalism betrayed her adherence to a scientistic approach that one could easily connect with the orientations of US social science at the time. Myrdal's highly international profile, her experience of cultural differences and knowledge of various geopolitical contexts did not predispose her to thinking about the internationalization of social science as a process of Americanization. Instead, she believed that overcoming national isolation was the first step towards mutual understanding between nations, which implied paying special attention to the conditions of application of social scientific knowledge, its geographical and interdisciplinary specificity. That orientation amounted to

cultivating a decentered social scientific internationalism that reflected Myrdal's own position in the international social science community.

The above essays achieve a process of cultural decentering through the analysis of the life and work of social scientists whose personal experience of different cultures influences their overall vision of the world, which in turn influences the way they approach it as social scientists. The experience of new cultures is a total experience that covers the multiplicity of capacities in which individuals embrace the world and not just their effort to understand it as social scientists. This brings us back to Adalet's conclusion that the uncertainties unleashed by transnational (and other personal) experiences, and not just academic constraints, complicate the construction of scientific personae.

Another aspect of the collection which is worth pointing out is what can be described as the "political element" of social scientific knowledge. In its generality, the phrase covers a variety of interactions between politics on the one hand and the production and use of Cold War social science on the other. Here the transnational perspective proves especially illuminating. It shows that policy demands stand as a crucial factor in the production and transformation of social science because its results were used for ideologically opposed purposes by the protagonists of the Cold War. Likewise, it illustrates the role of social scientific theories as social interventions—their use in political arguments in Cold War debates.

Ekaterina Babintseva shows how the question of the efficiency of the learning process became crucial in relation to the objective of strengthening education. The Soviet approach to programmed instruction was inspired by Western scientific developments, the work of B. F. Skinner in particular, but, because of pressing matters of political significance, it underwent notable adaptations in the hands of psychologist Lev Landa and other scientists at the Soviet Council on Cybernetics, who developed a theory of algorithmic thinking. Soviet scholars found it difficult to rely on a form of behaviorism that eventually downplayed the fact that "the self has an agency in determining its own development" (p. 50). Accordingly, cybernetics seemed to offer better theoretical foundations than behaviorism for programmed instruction.

A perhaps stronger example of the influence of politics on social science in the context of transnational exchange of ideas is to be found in Zhipeng Gao's chapter on Chinese education. First, it shows how the educators of the People's Republic of China gradually substituted a pedagogy developed by the Soviet educator Ivan A. Kairov for the American-influenced and student-centered Deweyan pedagogy that had dominated education before the Chinese Communist Party ascended to power in 1949. Second, it describes another shift that responded to late-1950s economic and political demands: away from Soviet pedagogy toward a labor-based pedagogy. Here it is quite clear that Chinese education and psychology's reconceptualizations of human nature were often driven by political ambitions, which is not to say however that more individual factors played no role in the practical translation of these ambitions.

In Vítězslav Sommer's chapter, the political element of Cold War social science is also visible because the Prague Spring offers a convenient dividing line between the introduction of leisure studies in Czechoslovakia in the 1950s and their transformation following the intervention of the Soviet Union. From the 1950s to 1968, the development of leisure research in Czechoslovakia was marked by reformist ambitions and the recognition that social scientific knowledge could help build a socialist society, both of which prompted critical attention to Western social science. In the wake of the Prague Spring, political pressures, and not just political demands, impacted social science as a whole, with

the return of Marxism-Leninism as a general theoretical framework. Within leisure studies, the promotion of a socialist lifestyle often replaced empirical examination of its conditions, blurring the lines between social scientific research and policy recommendations. By the late 1980s, because of the mixing of genres between reformist sociology and sociological advocacy, many students of the socialist lifestyle had lost touch with the socioeconomic realities of Czechoslovak society.

Political pressures do not necessarily influence social science in the expected way, for its practitioners always retain agency when they respond to government and military agencies in need of information. Surprisingly, that may be true of non-academic producers of knowledge as well. A good case in point is provided by Simon Ottersbach's essay on the CIA-supported Radio Free Europe (RFE). Created in the early 1950s, this pro-Western broadcasting organization was meant to produce information on life in the Eastern bloc so as to counter its propaganda. One could hardly think of a more blatantly ideological objective. Yet, RFE turned into one of the main purveyors of social scientific knowledge about Eastern Europe in the first two decades of the Cold War. RFE served as a source of information for social scientists and more generally for the media. Such a mission is not necessarily incompatible with propagandist ambitions—to the contrary. At the same time, the interplay between social scientific research, communication studies and propaganda suggests that the history of the social sciences cannot too easily dismiss the role of organizations like RFE in the production and dissemination of knowledge. As Ottersbach argues, the process of building up trust with listeners made RFE's commitment to "getting it right" central to its reputation and culminated with the introduction of academic research methods into its Research and Analysis Department in 1960. Gradually, RFE became a privileged site for the production of social scientific knowledge and its use in broadcasting: it "thus became part of a transnational and transsystemic space for thinking and speaking about the East" (p. 111).

As we have seen, the influence of political demands and pressures on Cold War social science was varied and at times unexpected. However, this must not obscure the fact that it can also be useful to consider social scientific theories as political interventions reflecting conflicting visions of the place of knowledge in society, as does Markus Arnold in the concluding chapter to this volume. The influence of Cold War concerns notwithstanding, these political interventions reflected the growing significance of social science knowledge and expertise in the postwar era.

It is fitting to conclude this review with a reference to Margarita Fajardo's chapter on dependency theory. Her essay does not fit well with my division between chapters dealing with the relationship between scholars' personal transformations and their approach to society on the one hand and chapters covering the political element in social science on the other. It is unclear whether that essay stands as an illustration of a transnational history of the social sciences either. Fajardo's thesis that dependency theory was not a reaction against modernization theory, but rather the product of a transnational network gravitating around the United Nations Economic Commission for Latin American (CEPAL) and linking a number of protagonists in Chile and Brazil, proves convincing and innovative. On the other hand, it reminds us of the importance of geographies of knowledge. Taking seriously the idea of the formation of a "Latin American" social science during the Cold War does make sense for historians of social science willing to go beyond US-centered narratives. Yet, it also implies that historians give due consideration to the role of specific locations in the making of social scientific knowledge and explain how local knowledge is at times transformed into something more global (which was the case of dependency theory in the 1960s and 1970s as Fajardo shows). In other

words, historians of social science need to ask geographical questions. Understanding the transnational flow of people and ideas is central to the history of the social sciences as is the effort to situate them in the places of their making.

Dayé and Solovey's essay collection is instructive and at times illuminating. Its case studies help us go beyond our Western preconceptions to consider perspectives we are often too quick to dismiss for lack of knowledge, because of the language barrier or simply by habit. As the editors recognize, the volume has been inspired by recent scholarship that challenges the usual equation between Cold War social science and social science in the Cold War. Yet, I should like to underline a perhaps more practical inspiration that has increasingly marked historical enterprises since the end of the Cold War and even more so in the first two decades of the twenty-first century. What has been called the "transnational turn" in history amounts to shifting emphasis towards actors, ideas and practices from different geographical centers with a view to challenging the precepts of modernization theories. In the process, transnational approaches have contested not merely the precepts whereby given ends may best be attained, but also the very idea that these ends are defined by external forces only. The end of the Cold War, at least initially, may have encouraged Cold War triumphalist narratives, but the persisting crisis of Western democracies, by making new voices heard, has gradually complicated our assessment of past achievements. Those voices can help shed a new light on the way we think about the history of the social sciences after 1945. The question of whether they will help make the production and dissemination of knowledge a more democratic enterprise or just another side-effect of globalization remains to be answered.

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BOOK REVIEW

Merton Reloaded?

Tangi Audinet

audinet.tangi@gmail.com

A good textbook

In a clear and informed book, Charles Crothers, professor of sociology in Auckland in New Zealand, produces a comprehensive introduction to the sociology of Robert King Merton. In seven short chapters, Crothers follows the career of the American sociologist in a chronological manner and systematically elaborates a review of his work. The aim of the series in which the book is published, called *Reintroducing*, is to offer "concise and accessible books that remind us of the importance of sociological theorists whose work, while constituting a significant and lasting contribution to the discipline, is no longer widely discussed". But precisely this idea can be linked to the Mertonian's conception of the systematic of sociological theory which selects what is still alive in the sociological knowledge produced in the past (Saint-Martin, 2021). In a sense, the sociologist of Columbia was a figure of choice to inaugurate this new editorial agenda in which the history of sociology serves the sociological research nowadays. In this review, I will follow the chapters' chronological order and critically discuss some of their significant points.

First, Crothers noticed that Merton fall into the social and intellectual process theorized by Merton himself, called *OBI* (obliteration by incorporation), which means that some Mertonian concepts are actually used by sociologists, but without mentioning its original author. It is not the first Crothers's book on Merton (see Crothers 1987), and he knows very well the work of this fascinating intellectual figure. In the first chapter, he offers a multi-faced portrait of the sociologist based on the Mertonian exegesis. This plural portrait is a worthy idea because as Eviatar Zerubavel pointed out: "there were several Merton's" (Zerubavel, 2019: 70). For many, he was simply an epigone of Talcott Parsons, an excellent theorist and the father of functionalism in sociology. And according to Alan Sica, the most of Merton's work is already outdated (p. 5). But according to Crothers, Arthur Stinchcombe and Piotr Sztompka Merton is a central figure and an "unintended general social theorist" (ibid.). Despite those controversies, the only point on which all the exegetes probably agree is that Merton was a really good writer (p. 6). Crothers then offers a short guide to navigating into the Mertonian labyrinth and completes his introduction with a short biography.

A Magic Trick: how Meyers became Merton

Meyer Schkolnick was born in 1910 in Philadelphia, in an immigrant Jewish and anarchist family. The young boy, educated in American values, grew up in a city with a "mix of Irish, Italians, and Russians" (p. 12). From an early age he was passionate about books, especially enjoying Laurence Sterne's *Tristan Shandy*, which prepared him for future intellectual explorations (*ibid*). The young teenager was very interested by the sleight and soon became the new Houdini of the City. But his main magic trick probably concerned his name, because at this moment the young Meyers decided to become "Robert King Merton" as we know him now. Like many other great sociologists[1], Merton was initially trained in Philosophy. But he switched to Sociology, this science in which one can find "the joy of discovering that it was possible to examine human behavior objectively and without using loaded moral preconceptions" (Merton, quoted by Crothers p. 13).

Harvard and the formation of a historical sociologist of science

In the very stimulating intellectual context of the 1930s University of Harvard, the young student carried out most of its training "outside the field of sociology" (p. 21), especially in economics and economic history, history of science, philosophy, comparative religion, anthropology and also English literature (*ibid*). Under the direction of Pitirim Sorokin, a Russian emigrant who chaired the new department of Sociology, Merton discover the links between social and cultural phenomena. He was also a very passionate reader, and wrote some important reviews, notably on French Sociology (Merton, 1934) and German *Wissenssoziologie* or Sociology of Knowledge (Merton, 1937). Fascinated by Parsons' theory of social action, and assisted by the historian of science George Sarton, Merton worked on his Ph.D. on the English Science of the XVII Century with a Weberian perspective. He focused on "looking particularly at the various interactions between society and the development of scientific work" (p. 23). An original approach in this early work is the methodical use of the prosopography and the combination of theoretical propositions and empirical investigation (p. 24). In the 1940s, Merton began to build a sociological model to analyze the scientific world as a social activity founded on a specific *ethos* and set of moral norms. In addition to this model, he expanded upon the Durkheimian conception of anomie, worked on the sociology of deviance, and conceptualized the unanticipated consequences of action or *UCA* (p. 33).

Arriving at Columbia: An exotic duo and a charismatic teacher

In 1940 Merton obtained the post of assistant professor at the University of Columbia in New York. He worked with Paul Lazarsfeld in the newly created *Bureau of Applied Social Research*. Those years constituted an intense collaboration based on an "intellectual seduction" (p. 39), between Merton, the theoretician, and Lazarsfeld, the empiricist. But those years were also for Merton a transition between scholarly research in the libraries and applied social science funded by the government or private sector. In this mostly collective scientific enterprise, Merton learned to construct sociological explanations by using statistics and large data sets, and produced some innovative methodological tools such as focus groups. Professor Merton quickly became a superstar: "His teaching became famous, attracting repeat attendance at classes, outsiders dropping in, and leading to several published

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[1] For others examples see Norbert Elias, Pierre Bourdieu, or Karl Mannheim.

accounts of his teaching success" (p. 41). Several testimonies from past students attested to Merton as an excellent teacher. Crothers writes on this point:

> "students learned that there should always be an interplay between theory and empirical research, and therefore, their recommended career choice was to be *both* theorists and empirical researchers. They learned to critically examine theoretical work; to respecify and reconceptualize earlier theory and empirical findings, so as to cumulate these efforts." (p. 42).

As Richard Swedberg pointed out, Merton also taught theorizing in sociology (Swedberg, 2014), which is not to learn Grand Social Theory but consists of "a special way of interaction with data" (p. 43). With Merton, one moved from learning sociology to thinking like a sociologist.

"Sociology of Science and Sociology as Science"²

In the main period of his career, from the 1950s to the 1980s, Merton followed two main strategies. First, "he returns to his 'roots' or 'first love': sociology of science" (p. 77) and pursued the development of this sub-field of the sociological research. Secondly, as President of the American Sociological Association since 1957, he tries to extent the recognition of the discipline. He thought that sociology was at an early stage, but was convinced that one could collectively do a lot to make this discipline a rigorous scientific enterprise. And if the sociology of science was his main interest it was because this subfield had the peculiarity to be "self-exemplified": all that it says can be returned on itself by "a mirror effect" (Saint-Martin, 2013: 29). In other words, Merton tried to make sociology become a real science by producing a scientific sociology of science. On this point, Crothers writes: "Merton's later sociology of science centered on the key internal features of science as an institution" (p. 90-91). The main idea is that the social organization of science follow a normative structure in which Merton distinguishes four norms:

- "Communism: all scientists should have common ownership of scientific goods (intellectual property), to promote collaboration; secrecy is opposite of this norm;
- Universalism: scientific validity is independent of the sociopolitical status/personal attributes of its participants;
- Disinterestedness: scientific institutions act for the benefit of a common scientific enterprise, rather than for the personal gain of individuals within them; and
- Organized skepticism: scientific claims should be exposed to critical scrutiny before being accepted: both in methodology and institutional codes of conduct" (p. 92).

As Pierre Bourdieu observed, these norms are not realistic (Bourdieu, 2004). Despite this, it is interesting to note – because it is not often mentioned – that Merton had also elaborated some counternorms, such as solitariness, interestedness, and dogmatism (p. 93); norms which tend to prevent the healthy development of science. Even though these norms are more ideal than real, the analysis of the social structure of science constitutes an advancement in the sociology of this specific activity.

Merton also produced a sociological analysis of the process of discovery. Crothers writes:

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² Calhoun (2017).

"Central to this image of science was the idea that scientific discoveries were given to the scientific community by the discovers but in return for an acknowledgment (through citations) that the discovery was the symbolic property of the discover" (p. 91).

The Mertonian sociology of science is a social world with a clear split between the few and the many: "While many scientists work quietly at assigned puzzles, the motivation for intense scientific activity is the reward of recognition which flows from having been the first to make a discovery" (p. 92). In Mertonian view, science is a social space characterized by intense competition and unequal positions among scientists. Another point concerns the process of production and diffusion in science. Merton was interested in writing, publication and communication, and conceived scientific work as a continuous process of rearrangement (p. 95).

Time and Words matter

After 1984, Merton stopped teaching. But this event didn't end his career. He wrote on "social time" during this late period, and was especially interested about how decision about timeframes affected the action of individuals: "different time preferences lead to rather different decisions" (p. 112). He also elaborated a research program on sociological semantics. This program aimed to scrutinize the uses of words or expressions through social groups and history, in order to understand how and why their meaning differs, and why sometimes some words disappear. This work relied on multiples sources like dictionaries and texts databases (p. 114) and was achieved into the book *The Travel and Adventures of Serendipity: A Study in Sociological Semantics and the Sociology of Science*, co-written with Elinor Barber, and published posthumously in 2004.

A Grand Social Theory?

In the 6th chapter, Crothers seeks to convince the reader that a general social theory can be reconstructed through Merton's work. But this aim can appear problematic given that Merton had carefully refuted the notion of Grand Social Theory in his discussions of Parsons' works. Following Merton's view, this kind of general theory of society risks being useless, or simply wrong. According to him, the more one adopts a wide scope of analysis, the more one risks producing interpretations that do not 'fit with facts'. A more satisfactory way of reinvesting the Mertonian heritage in Sociology is to look at Merton's research program. Crothers shows that this is characterized by some specific features such as problem finding, conceptualization, conceptual clarification, construction of generalizations and typologies of the 'middle range', and a productive return to the classics (as Crothers's book seeks to do) (p. 122-123). Another aspect of Merton's sociology which continues to be of interest is the question of the social character of the individual (p. 133). Here, psychology and sociology are fundamentally interrelated. The construction of the personality structure in childhood and its transformation throughout one's entire life is a central point to understand how the social environment always shapes our personality.

Scientist as humanist

Crothers opens the final chapter with a very strange idea: "Ironically, there is little need for sociological exposé of Merton's life as I would argue it bore only lightly on his intellectual career […]" (p. 154). The fact that the aim of a textbook differs from an intellectual biography is absolutely clear. But it is another matter to argue that there is no link between Merton's life and his scientific production. Of course, there are links, and the other chapters show that the social environment was crucial to his intellectual development. If it is not the aim of this book to reconstitute precisely the intellectual habitus and the self-concept (Gross, 2008) of Merton himself, it is however not a good reason to argue that the sociology of sociology is useless here, especially with regards to a sociologist who had defended the self-exemplification of the discipline, and used his sociological theory in order to understand his own life (Merton, 1994; on this point see also Saint-Martin 2016).

Despite this sociological disavowal, Crothers points out two key elements in Merton's intellectual style. First, he wrote only essays, based on a very important rhetorical dimension. This part of the sociological prose is interesting for the history of sociology, but also for contemporary scholars who probably have much to learn from the Mertonian style. Indeed, for Merton, words matter. In trying to be the most rigorous scientifically but also very careful in humanistic erudition, Merton probably appears as the best figure of the "third culture" that Wolf Lepenies identified within the sociological enterprise (Lepenies, 2008). Furthermore, the Merton's reviewing papers are still valuable, because for him reviewing was a noble intellectual activity, and this practical art of reading others appeared to be the best way to write better.

Merton as touchstone

The latest book of Charles Crothers possibly has both the advantages and disadvantages of a textbook. On one hand, it combines clarity and synthetic capacity. But on the other hand, it sometimes reifies a sociological work based on intellectual exploration into a too strict conceptualization. If the chronological construction of the book has a pedagogical interest and the inter-titles are valuable, the listing and the multiplication of typologies are not always readable and sometimes impede a deeper understanding of Merton's oeuvre. Furthermore, there are sometimes annoying repetitions[3] and a few problems in references[4], even though we appreciate the detailed bibliography for each chapter and the final index.

In the end, one could ask: What is the relevance of Merton's legacy for sociology today? I think that the answer is clear: being systematic matters. It seems that what is remembered from Merton (with or without his name) in the sociological cognitive memory today is still valuable (systematic, *OBI*, Middle-range theory, theorizing, self-fulfilling prophesy, serendipity, sociological semantic), and what is forgotten is maybe outdated because since the main works of Merton, societies have considerably changed, and sociological research too. Ultimately, Merton appears foremost as a "classic" of sociology in the sense "of touchstone", a word used by Arthur Stinchcombe to designate "a good example of doing a beautiful scientific work" (Stinchcombe, 1982:2).

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[3] See for example p. 51 and p. 168 on the differences between American and European ways of thinking.
[4] For example Saint-Martin, Arnaud (2014), « Robert K. Merton épistolier, ou la gestion de l'influence par correspondance », is quoted two times page 170. The author maybe wants to refer to (Saint-Martin, 2013), but this book is absent from the bibliography.

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BOOK REVIEW

Bortolini:
A Joyfully Serious Man

Wolfgang Knöbl

direktion@his-online.de

There are probably a couple of reasons a considerable number of biographies of great social scientists have appeared in recent years, especially on the English-language book market. First and foremost, of course, is the simple biological fact that many of the figures who shaped the international social sciences after 1945 have lived to an astonishing age and thus died only a short time ago. With the passing of the authors mentioned above, hitherto unknown sources became accessible, from which interesting biographies could be reconstructed, especially since the life courses of quite a few of these figures had been highly complex. It should be remembered, for example, that some of these intellectuals were socialized in the extreme left-wing milieus of New York City or Chicago or came from Germany and had to go into exile in the 1930s. Certainly this is what Martin Krygier reports in his biography of Philip Selznick, or Jeremy Adelman or Daniel Bessner do in a similar way in their biographical accounts of the lives of Albert O. Hirschman and Hans Speier. Occasionally, however, biographies are written from the view that the respective discipline is in a form of crisis and that it might therefore be worthwhile to look back at the work and the personality of some rather heterodox representatives of the discipline as Charles Camic recently did with his biography of Thorstein Veblen.

All this does not seem to apply to the "hero" of Matteo Bortolini's biography – at least not at first glance. For although Robert Bellah was undoubtedly a great figure of the social sciences, known far beyond the boundaries of his discipline, his life does not seem to have been particularly exciting, especially since he spent most of his time in Berkeley, California. And his work can hardly be called heterodox in any way, at least not if one considers the fact that Bellah had been the master student of Talcott Parsons at Harvard, so that his scientific socialization took place in the context of what was later called the "orthodox consensus", which then possibly also explains the fact that Bellah later rushed from success to success and became one of the great public intellectuals of the USA.

Matteo Bortolini, an Italian sociologist at the University of Padua who has published extensively – particularly on the history of Italian sociology – thus took a considerable risk when he decided to write Bellah's biography. For at the beginning of what became decades of research, it would not have been known that Bellah was to have had a much more fractured curriculum vitae than might be expected. In short, in this brilliantly written book, based on many archival sources and many interviews (including conversations with Bellah himself), Bortolini succeeds in presenting to the reader a quite unimaginable intellectual figure and in a way that makes Bellah's oeuvre more than accessible. In short, the risk taken by Bortolini was worth it!

Robert Bellah, born in 1927, probably did not have an entirely happy childhood and youth. His father had become a moderately successful journalist and publisher of a local newspaper in Oklahoma in the mid-1920s, but he sold his newspaper in the summer of 1929 before the family lost its fortune in the stock market crash of October and decided to move to Los Angeles to live with relatives of Robert Bellah's mother. The father, however, left the family during this phase (he was to commit suicide a few years later as a bigamist), so that Bellah grew up fatherless under somewhat precarious circumstances in Los Angeles. As an individual eager to learn, however, Bellah was able to enroll at Harvard in February 1945, which was immediately interrupted by his enlistment in the military, where he performed writing services during the demobilization phase at the end of the war. Unlike many social scientists who were to become influential after 1945, Bellah was thus too young to take an active part in the war in any form. What he did share with quite a few, however, was his left-wing political orientation. As Bortolini shows, Bellah was enthusiastic about Roosevelt's New Deal as a young man, and during his career moved further and further to the left, eventually openly showing his sympathies for the Soviet Union by the end of the war. When he returned to Harvard after his military service, Bellah became a member of the John Reed Society and the undergraduate branch of the Communist Party (CP) at Harvard. But it was also clear that he was interested in all kinds of intellectual currents, not least in Freud and psychoanalysis, which soon led to his expulsion from the CP. It was during this phase that he met Talcott Parsons, in whose intellectual environment he was to develop further. In 1950, Bellah, who had married shortly before, enrolled in a double PhD program in sociology and Far Eastern languages at Harvard, thus entering an institutional structure in which the ideas for his famous book *Tokugawa Religion: The Cultural Roots of Modern Japan* (subsequently published in 1957), were to take shape.

The genesis of this book was anything but happy, however, as Bellah was denied a research trip to Japan because of his former membership in the CP. McCarthyism and the problematic domestic political climate in the U.S. thus began to have a considerable influence on Bellah's career, so that – as Bortolini explains – he was forced to remain an armchair scholar with little field experience. Eventually, he even left the U.S. for a short period and went to McGill University in Montreal to escape political pressure and further questioning by the FBI. Bellah, according to Bortolini, did not compromise himself politically, he did not name other CP members to the FBI and thus showed himself to be a person of high integrity.

At McGill, Bellah made contact with the famous historian of religion Wilfred Cantwell Smith and his Institute of Islamic Studies, but remained closely associated with Clifford Geertz at Harvard, so that his study of Japan emerged under a variety of influences. For even though this study was undoubtedly heavily influenced by Parsons and his AGIL scheme (a systematic depiction of certain societal functions fundamental for sustaining social life), Parsons was by no means the only key figure for Bellah. On the recommendation of C. Wright Mills, of all people, who was certainly no friend of

Parsons, Bellah's attention was drawn to the work of Paul Tillich, a man who was to have a considerable influence on his own understanding of religion. In any case, it was clear that Bellah, who returned to Harvard in 1957 (having turned down job offers from McGill), continued to be opposed to simple notions of secularization and – as is apparent in his book on Tokugawa religion – always insisted on the autonomous causal role of culture and religion. Presumably, it was this last point that brought sharp criticism from Barrington Moore at Harvard, although Bellah's book on Japan certainly also had a somewhat tense relationship to the emerging modernization theory of the time (something with which Barrington Moore was familiar, although this did not prevent him from severely attacking Bellah's culturalist position).

After the publication of the book on Japan, Bellah spent some time abroad, and his travels brought him into the intellectual environment of American Studies. Indeed from the early 1960s, Bellah became increasingly interested in the peculiarities of American culture and democracy, thus preparing the text and the concept with which he became famous also in the non-academic world with his book *Civil Religion in America* (published in 1967). Although he had become Associate Professor at Harvard a year earlier, Bellah left the institution and thus the intellectual orbit of Parsons in order to join the Sociology Department at Berkeley, where he would remain until the end of his life.

At that time, Berkeley was as intellectually vibrant as Harvard, since the former had been aggressively recruiting new sociological scholars, including Erving Goffman, Nathan Glazer, and Neil Smelser. These sociologists joined existing Berkeley "stars", such as Seymour Martin Lipset, Philip Selznick, Robert Nisbet and Herbert Blumer. And in this environment, which was additionally influenced around the same time by the student movement, Bellah had to establish himself, which he did unabashedly, even with tendencies that were opposed to the main figures within the Department. And this was quite evident when he – in accordance with his concept of civil religion – tested the limits of science and questioned the boundaries between the social sciences and theology. Likewise, Bellah (according to Bortolini) defied what he saw as the excesses of the leftist student movement at Berkeley and published a reader on the work of Emile Durkheim, and later made an effort to undermine certain established forms of differentiation between science and religion. As an example, in 1969 at a symposium in Rome, in the presence of important sociologists of religion, Bellah attracted in an almost scandalous manner the criticism of his colleagues, who sensed a betrayal of science in his approach. Bellah did not allow himself to be irritated by this; rather, new readings of Durkheim's sociology of religion strengthened his emphasis on the autonomy and efficacy of symbolic systems and led to a position (one described by Bortolini as 'symbolic realism') in his 1970 book *Beyond Belief: Essays on Religion in a Post-Traditionalist World*. This position, similar to that of his friend Clifford Geertz, demonstrated Bellah's increasing detachment from Parsons' functionalist horizon of thought, and subsequently led to a personal distancing from his former teacher. Bellah could not (and probably did not want to) gain real institutional power with such a theoretical position, even if he had a number of students who would later become famous – from Jeffrey Alexander to Robert Wuthnow and Ann Swidler.

The late 1960s and early 1970s can certainly be described as another formative phase in Bellah's work, especially since these years were also marked by professional and personal twists of fate. On the one hand, Bellah's appointment as a permanent member of the Institute for Advanced Study in Princeton was a failure and the site of a very public, unpleasant "mudslinging battle" over claims about the unscientific nature of Bellah's work. On the other hand, Tammy, one of the four daughters of the Bellahs, committed suicide in 1973, while Abby, another daughter, died in a car accident in 1976. These personal events caused a lot of disruption, and some previously hidden issues became

public. For example, during this period, Bellah openly sought homosexual experiences and finally confessed his homosexuality, although the consequences were mitigated somewhat because he had been living with his wife Melanie for years in an open marriage, which enabled the partners to explore their sexuality outside of the usual legal context. In his professional life, Bellah began to withdraw from American Studies, becoming increasingly interested in new religious movements, including the spiritual currents supported and promoted by the hippie movement. This latter was one of the reasons that Bellah, unlike many of his friends and colleagues, refused to condemn American counter-culture from the outset.

It was in this context that Bellah began to prepare the work that would become his most famous book, a "bestseller", even though he did not write it alone – *Habits of the Heart: Individualism and Commitment in American Life*. The book was written at the time when Bellah's former mentor, Parsons, with whom he had reconciled, died, and when he became Head of the Sociology Department at Berkeley, thus moving into a position which, on the one hand, entailed power, but which, on the other hand, did not induce him to seek excessive influence. Bellah certainly acted according to the principles of a pluralistic understanding of science; he supported Michael Burawoy as well as Jürgen Habermas, even if he did not succeed in bringing the latter to Berkeley.

The book, *Habits of the Heart* was a success selling 400-500,000 copies in the U.S. Bellah became a sought-after speaker who was able to demand significant honorariums, and he came to the attention of politicians. But Bellah was just as disappointed by President Jimmy Carter as he was – much later – by Bill Clinton and his policies, with Bellah's writings displaying an increasing pessimism with regard to the future of U.S. politics and society. In conceptualizing his 1985 work, Bellah had been heavily influenced by Aristotelians such as Alasdair McIntyre, Charles Taylor, and Joachim Ritter, and become increasingly aware of the importance of Republican (and biblical) ideas and ideals. Under the influence of these thinkers, Bellah rejected Kant's radical dualism of norms and desires, focusing instead on virtues and practices, and thus began to emphasize the role of institutions for the functioning of the social fabric, which then also led him to a rapprochement with the Catholic Church. Bellah's pessimism had probably a lot to do with the fact that – as the follow-up study *The Good Society,* published in 1991, revealed – it had become increasingly unclear to him and his co-authors how 'good' social structures could be designed in concrete terms and – above all – which collective actors were to drive them forward and implement them. The consequences of the neo-liberal policies of Ronald Reagan and his Republican successors seemed to be too devastating, so that it became increasingly difficult to point to convincing political alternatives. This, however, did not prevent Bellah from repeatedly criticizing U.S. foreign policy, for example in the wake of 9/11, and to highlight its devastating effects on American society.

One might assume – although Bortolini does not express it in this way – that it was this disillusionment which, after the turn of the millennium, led Bellah to increasingly bury himself in the history of mankind. In the 1960s, Bellah had written important essays on religious evolution, and he returned to this theme in the last decades of his life, considering religion as an aspect of symbolic development. He emphasized that no development belonged only to the past, but is always carried forward, in some modified form, to a later period – "Nothing is ever lost"! As an old man, Bellah began a research project that he knew he would never be able to complete – an investigation of the history of humans as symbol-using animals. In this he connected to the Axial Age-debate conducted by historians and historical sociologists (which since the 1980s has been decisively shaped by Shmuel Eisenstadt), but also to evolutionary biology and anthropological insights, especially those of Merlin Donald. *Religion in Human Evolution: From the Paleolithic to the Axial Age* was published in 2011

and became Bellah's last major work. Bellah's final years – he died in 2013 – were filled with several sad events, (for example, his wife Melanie and his friend Eisenstadt died on the same day), yet melancholy is alien to his work. On the contrary, the analytical power of Bellah, who never shied from tackling great tasks and taking considerable intellectual risks, continued to be evident during these years. And it seemed as if he could and would extend his research on the history of mankind forever, as if more books written by Bellah could and would be added to this volume.

All this is vividly presented to the reader, and Bortolini's excellent biography does complete justice to Bellah. One might prefer to see greater elaboration in parts, or wish for more information about the influence of Tillich's thoughts on Bellah's sociological reasoning; more background on the reasons for Barrington Moore's criticism of Bellah; more detail about the period when Bellah began to distance himself from modernization theory, etc. At the same time, however, it should be emphasized that Bellah's long life and his rootedness in many forums of discussion and intellectual circles mean that a complete intellectual biography is almost impossible. In this respect, such criticism should not be taken too seriously. For it can hardly be denied that Matteo Bortolini has written an impressive biography on one of the most important figures in the field of the international social sciences – and he has done so in a way that readers will certainly wish that biographies of the same quality on other public intellectuals could be available soon.

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Wolfgang Knöbl, Director of the Hamburg Institute for Social Research. Most recent book: *Die Soziologie vor der Geschichte. Zur Kritik der Sozialtheorie.* Berlin 2022: Suhrkamp.