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Snow White and the Wicked Problems of the West: A Look at the Lines between Empirical Description and Normative Prescription

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Abstract

This article discusses the relationship between the origins of the concept of post-normal science, its potential as a heuristic and the phenomenon of complex science entailed policy problems in late industrial societies (wicked problems of the West). Drawing on arguments presented in the early works of Funtowicz and Ravetz, it is proposed that there is a fundamentally empirical character to the post-normal science call for democratizing expertise, which serves as an antidote to late industrial poisoning of the fairy tale ideal (Snow White) of a clean divide between science and politics. Post-normal science extended-peer-review methodology is interpreted as a

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response to a crisis in the governance of science. Rather than viewing extended-peer-review processes as products of the post-normal science discourse, here the post-normal science discourse is understood to provide a heuristic lens through which existing complex science/society collaborations and conflicts can be better understood. Two different post-normal science situations—management of mega-contamination in Bitterfeld, Germany, and eviction of the Parakuiyo Maasai from the eastern wetlands of the Usangu plains of Tanzania—are presented and the implications of their de facto extended-peer-review structures are discussed, to illustrate this heuristic potential.

Keywords
post-normal science, extended-peer-review, wicked problems, Tanzania, Bitterfeld

Introduction

This article is intended to develop methodological theory concerning the circumstances and practices of post-normal science. Its overarching purpose is to systematically discuss the relationship between two central topics within post-normal science: a normative prescription to democratize expertise (Funtowicz and Ravetz 1990, 210) through participatory science and extended-peer-review methods (Funtowicz and Ravetz 1994a); and the description of complex late-industrial science/society relationships that give rise to both scientific methodological and governance of science challenges that might potentially be addressed through the democratizing of expertise.

The problematique addressed in the following pages can be described as what Rittel and Webber (1973) term "wicked problems": where, in late-industrial societies (that is to say, in the so-called West) straightforward planning problems have all been more or less solved and the ones still remaining unsolved are "wicked" —persistent, complex, and difficult or perhaps even impossible to solve. As a short hand for acknowledging both their complexity and their late-industrial character, we might call the problems of post-normal science "wicked problems of the West": meaning (a) that they are technologically entailed (late-industrial) *and* (b) that their status as a wicked problem is related not only to their technological character but also to their social context (Allen et al. 2001; Giampietro 2004; Farrell 2008).

In the following sections, it will be argued that there is a fundamentally empirical character to the concept of post-normal science, which concerns

itself with wicked problems of the West, and that concerns about it harboring an undertheorized normative bias (Lorenzoni, Jones, and Turnpenny 2006; Turnpenny, Lorenzoni, and Jones 2006) are misplaced. Calls for the democratization of expertise (Funtowicz and Ravetz 1990, 60-5, 210) are discussed here with regard to their merit as empirically based normative recommendations arising from within a methodology discourse where fact/value ambiguities in twenty-first century science for policy are the main concern. Rather than understanding unreflective normativity (such as has been observed recently in the field of climate change—see The Economist 2010; Mosher and Fuller 2010) as a symptom of undertheorized engagement with post-normal science problems, it is proposed here that unreflective normativity can lead to misunderstanding of post-normal science situations, which can, in turn, give rise to poor quality extended-peer-review processes.

Although the argument presented here is intended to speak to the general topic "governance of science" (Jasanoff 1990; Kates et al. 2001; Habermas 2003; Farrell and Ravetz 2005; Weingart 2009), the specific focus is *not* the normative post-normal science recommendation to democratize expertise but rather its logical antecedent: the observation that conventional differentiations between the role of science and the role of society (sic Ravetz 1971) are insufficient to requirements when a community is faced with a post-normal science problem (sic Wynne 1988; Funtowicz and Ravetz 1990). The argument is developed in three consecutive steps: (a) elaboration of a proposition—early post-normal science conceptualizations of the problem of fact/value conflation in late-industrial science are discussed and contextualized, providing a basis for interpreting the concept of post-normal science as empirical in origin; (b) four basic empirical observations of the post-normal science discourse are then reviewed and explicit links between these empirical origins and the post-normal science concept of extended peer review are laid out, which justify its application as a heuristic; and (c) the link between the concept of extended peer review and the call to democratize expertise is discussed and the heuristic potential of focusing attention on extension of scientific peer-review processes (whether intentional or phenomenological) is illustrated by presenting sketches of two profoundly different late-industrial science entailed policy situations (wicked problems of the West), both of which can be understood, heuristically, as phenomenological instances of post-normal science extended peer review: (a) efforts to remediate mega-contamination in the German city of Bitterfeld and (b) the decision to evict the Parakuiyo Maasai from the eastern wetlands of the Usangu plains in Tanzania. The article concludes with some

reflections on the implications this argument may have for further development *and assessment* of extended-peer-review methods and methodologies.

Basis for and Elaboration of a Proposition

Deterioration of belief in a clear distinction between scientific facts and political values is not a new theme in the modern academic discourse. Latour and Woolgar (1979) explicitly addressed this in their study of the Salk Institute in California and, although he argues against the interpretation in a postscript to the second edition of his historic text *The Structure of Scientific Revolutions*, the stability of this distinction was at least called into question in the 1960s by Kuhn (1962[1970]). Although discussed occasionally (e.g., Camus 1953[1951]; Marcuse 1991 [1964]; Ravetz 1971, 1990; Foucault 1973; and Toulmin 1990), the historical and practical context for this deteriorating belief (wicked problems of the West) has received considerably less attention than has the dismantling of the fairy tale (Snow White). However, in their 1990 text *Uncertainty and Quality in Science for Policy*, Funtowicz and Ravetz address this problematique from an empirical perspective, as a practical, methodological problem associated with carrying out the work of providing highly technical analysis of complex risk and environmental management problems to policy makers in late-industrial societies.

> Previously it was assumed that Science provided "hard facts" in the numerical form, in contrast to the "soft," interest driven, value-laden determinant of politics. Now policy-makers increasingly need to make "hard" decisions, choosing between conflicting options, using scientific information that is irremediably "soft" (Funtowicz and Ravetz 1990, 1).

As a response to these new conditions, they present the normative recommendation that, for certain types of problems, scientific knowledge production should be democratized (Funtowicz and Ravetz 1990, 60-8). When reading this early text, it is clear that their proposition is based on a preceding, empirical observation. The need for new methods for preparing and presenting analysis of complex late-industrial scientific problems is specifically and explicitly linked to the *character of those problems*: "... (and here is our crucial distinction) the sciences which are required to *solve* the problem are systematically different from those that *created* the problem in the first place" (Funtowicz and Ravetz 1990, 65-6, emphasis original). Furthermore, they themselves explicitly relate these recommendation to the empirical situation under observation: where, "paradoxically,

the powers achieved by computers for policy related research increase rather than decrease the role of craft skills and judgements in the management of uncertainty" (Funtowicz and Ravetz 1990, 24), and where "... official expertise is contested and compromised ... [and] encounters technically incompetent criticism from outsiders" (Funtowicz and Ravetz 1990, 65). Here they are describing a post-normal science situation where "normal" puzzle-solving science has become obsolete:[1]

> oncology, epidemiology and even radiological protection, are radically different from nuclear physics and heat engineering ... [;] these new sciences, however much they mature technically, can never become "normal" in Kuhn's sense of having a dogmatic consensus that enforces adherence to a closed set of rules for puzzle-solving (Funtowicz and Ravetz 1990, 66).

The Numeral Unit Spread Assessment Pedigree (NUSAP) system presented in this 1990 text is intended to serve as a methodological tool for dealing with these post-normal science situations. Its main function is to disclose to nonexperts the limitations of a given scientific assessment of a given complex problem (Funtowicz and Ravetz 1990; van der Sluijs et al. 2005).[2] The development of this approach is a methodological response to a specific set of empirical conditions, which van der Sluijs et al. (2005, 481) summarize as follows: "Usually controversies surround these problems, in which three interrelated factors play a key role: uncertainty in the knowledge base, differences in framing of the problem, and the inadequacy of the institutional arrangements at the science-policy interface." Under these conditions, the peer community reviewing the technical quality of a piece of scientific analysis is *automatically* extended, and the question is not if, but rather how peer-review relationships between scientists and nonscientists can be managed in ways that favor production of good quality descriptions of complex problems.

Shortly after publishing their major text, Funtowicz and Ravetz (1991) contributed a chapter to the first collected edition of works specifically addressing the then newly emerging discourse on ecological economics. Although much of what they raised there is already elaborated in greater detail in their 1990 text, it is in 1991 text that they coin the term "post-normal science" where:

> The inherent limitations of traditional problem-solving strategies are revealed by a structural feature of the new global environmental issues. For in these, decisions depend on evaluations of future states of the natural environment, resources, and human society, all of which are unknown and unknowable (Funtowicz and Ravetz 1991, 144).

Here the concept appears as an artifact with a history: a *response* to the inability of normal science to support the development of useful descriptions of complex human/technology/nature relationships in late-industrial societies (Farrell 2008, 2010). Nowhere in these early texts, nor as far as I can see, in subsequent works, does their elaboration of this concept *purport to resolve* the host of contentious issues that arise in extended-peer-review situations. Rather, it offers a conceptual frame within which the character and dynamics of these situations can be placed and through which the complexity of their dynamics can be better understood and constructively managed, as with the applications of methodological innovations such as NUSAP (sic, van der Sluijs et al. 2005). In this respect, the concept is perhaps best understood as a heuristic, which can be helpful for developing both theory concerning science–policy interface relationships and practical extended-peer-review methods and methodologies, as opposed to being viewed as a program, agenda, or comprehensive theory in its own right.

Three Empirical Observations with Practical Implications

A few early texts on the topic of post-normal science, one from Ravetz (1971) and several from Funtowicz and Ravetz (1990, 1991, 1992, 1993), can be used to sketch out the basic ideas in the discourse. There are many others that could be referenced here but a detailed history of the concept is not my aim. Instead, the purpose of the following review of selected early post-normal science texts is to substantiate the proposition outlined above: that the concept of post-normal science has it roots in an empirical as opposed to a normative discourse. This is done through engagement with the following three arguments found in these early texts, which have strong resonance with the three conditions outlined van der Sluijs et al. (2005, cited above): (a) conventional peer-review methods are ill-equipped for conducting quality checks on descriptions of some types of late-industrial scientific problems (wicked problems of the west); (b) in these situations technical matters are reviewed by an extended community of adjudicating peers and quality checks on scientific output concerning these kinds of problems acquire an inherently political (value criteria) hue; and (c) this means that quality control on such scientific output needs to draw on combined judgments about both the social value and the technical adequacy of the science in question (sic, Ravetz 1971), and it is for this reason that

extended-peer-review needs to combine technical and political judgment, thereby democratizing expertise (sic Lee 1993).

Conventional Peer-Review Methods are Ill-Equipped to Deal with Wicked Problems of the West

Without proposing that it constitutes "the" origin of the discourse, Ravetz's (1971) major work *Scientific Knowledge and its Social Problems* clearly contains ideas that have played a central role in framing the concept of post-normal science. There, he presents a history of mostly nineteenth and twentieth century scientific knowledge production and a related philosophy of science analysis that focuses largely on the relationship between quality control and method. Among other things, the text details the emergence of a mismatch, between peer-review practices originally developed to serve the gentlemens pursuit of nineteenth century science and the modern practices of industrialized scientific knowledge production, which came to be standard throughout the world in the twentieth century.

In discussing the principle of quality control in science, Ravetz (1971, 157-62) argues for a distinction between the application of adequacy and of value criteria in the assessment of scientific knowledge production; where adequacy criteria (is it right?) can be set within the puzzle solving rule structures of "normal" science but value criteria (what should we study?) must be set by society. This distinction, which Ravetz (1971, 159) discusses specifically with respect to the principle of peer review, reflects a presumption of fact/value *dis*ambiguity: "philosophical questions of the possibility and nature of scientific knowledge" are judged through reference to "criteria of adequacy"; whereas "criteria of value," which are related to "the social activity of science" are used to adjudicate "the choice of problems to be investigated." Later, these same two categories reappear in the graph used by Funtowicz and Ravetz (1985, 1991) to illustrate the concept of post-normal science (Figure 1): albeit in a slightly different form: as decision stakes (criteria of value) and systems uncertainty (criteria of adequacy).

Under "normal" conditions, scientific peer review can be understood as the application of adequacy-oriented quality control criteria and science policy debates, regarding, for example the setting of national and university research agendas, can be understood as value-oriented quality control processes (Ravetz 1971). The intrusion of value-oriented social issues into the technical domain of science proper, an attribute of post-normal science problems, can be understood to undermine the presumption that a

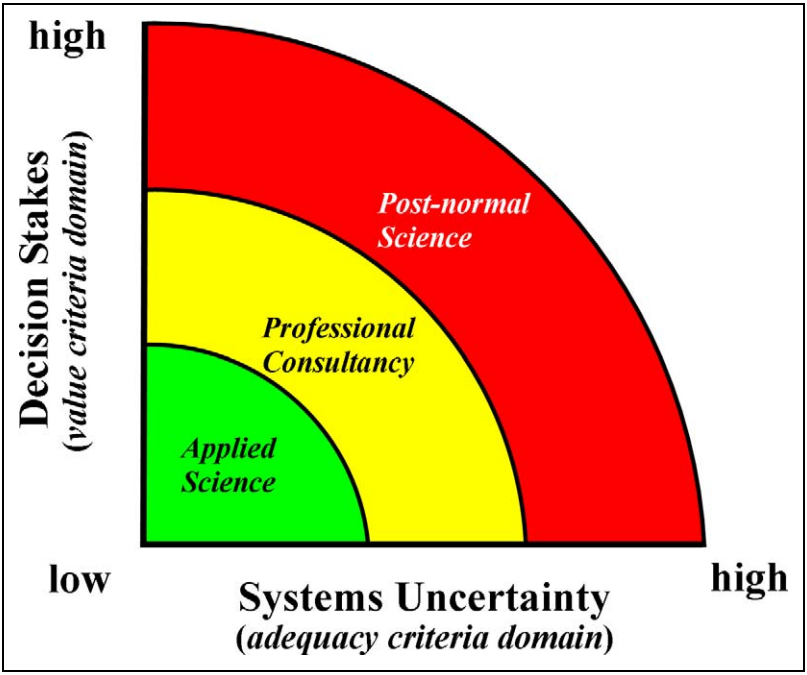

Figure 1. A post-normal science perspective in perspective. Source: Underlying image from Funtowicz and Ravetz (1990), included with permission; criteria domains drawn from Ravetz (1971), added by the author.

conventional peer-review process is sufficient for judging the technical quality of a piece of scientific work. Moving from the bottom left to the top right of Figure 1, keeping in mind the two types of quality criteria discussed by Ravetz (1971), a progression can be observed: from normal science situations, where technical quality can be judged using conventional peer-review methods to post-normal science situations where conventional peer-review processes, which are concerned only with criteria of adequacy, are ill-equipped to moderate complex debates that include a mixture of contentions regarding *both* the technical and the social quality of a piece of scientific work.

Under post-normal science conditions, criteria of adequacy and of value, as Ravetz describes them, are no longer distinct. Deciding whether a given technology should be developed (a value judgment) may be contingent upon the assessment of highly technical scientific data (an adequacy judgment)

concerning its functionality and the associated risks. For example, the well-established practice of manufacturing carbon nanotubes (Iijima 1991; Colbert et al. 1994) has produced a new social (value criteria) issue, as it now appears that these may have performance-enhancing impacts on the early development of plants (see Khodakovskaya et al. 2009). Since there is still substantial uncertainty regarding how carbon nanotubes operate in biological settings (Popov et al. 2007; Khodakovskaya et al. 2009), value judgments regarding whether to develop agricultural applications of the technology can only be made *in conjunction* with adequacy judgments regarding the reliability of the science informing those value judgments. Here, due to high epistemological uncertainty, adequacy data and value criteria fold in upon each other: "[t]he traditional fact/value distinction has not merely been inverted; ... the two categories cannot be realistically separated" (Funtowicz and Ravetz 1993, 751) and the basic criteria for judging the quality of a piece of scientific work change (Funtowicz and Ravetz 1985, 217).

Similarly, human societies are confronted with "the impossibility to define in absolute terms the optimal way to sustainability" (Giampietro 2004, 77), not only because adequacy criteria concerning what is ecologically optimal are unclear but also because sustainability cannot be viewed as a purely technical problem but must be addressed as a politically charged, complex social issue, where value criteria are also unclear. When humans set out to make decisions under such conditions, the conventional social role of science as "truth-sayer" is both methodologically *and* politically problematic (Luks 1999; Farrell 2008).

Funtowicz and Ravetz propose that, when such situations arise, a new picture of the social role of scientific knowledge production is needed, *in order to correctly understand and effectively engage with the empirical problems at hand* (1991, 144). Under post-normal science conditions, the idea that there exists a single, externally objective (even if Ideal) "truth," that can be successfully uncovered, becomes as mythical as the fairy tale character Snow White; "we have seen the man behind the curtain and objectivist realism is now compromised" (Allen et al. 2001, 476). At the one extreme, social and political controversy leads to underspecified problems, for which multiple, even contradictory descriptions may be equally plausible; at the other, uncertain scientific knowledge concerning the structure of even the most clearly defined of problems can lead to underspecified solutions. In both cases, conventional peer-review quality control procedures are inadequate because they are designed to verify the correctness of a result that cannot be correct in the conventional sense of the word (Funtowicz and Ravetz 1994b, 1997; Funtowicz and O'Connor 1999).

An Extended Community of Adequacy Judges

The terms "extended" and "peer review" have a significant history in Ravetz's (1971) early arguments, which can help clarify their meaning in his later work with Funtowicz. Reaching back to Ravetz's (1971) description of "facts and their evolution," we find an explicit description of extended facts and of the extended communities of individuals responsible for the evolution and reification of those facts. He argues that:

> [t]hose facts which continue in being long enough to become knowledge must do so by a process of extension analogous to that of tools: they must be seen to be relevant to problems in other fields of enquiry (Ravetz 1971, 199). Indeed, its [(scientific knowledge's)] special character results from the complexity and interconnectedness of its materials [scientific facts], as they evolve through the complex and fallible social processes of their use and *adaptation* (Ibid: 209, *emphasis added*).

Recalling this early description of extended facts, it seems reasonable to propose that what is 'extended' under extended peer review is the community of individuals with a role to play in describing and deciding what does or does not constitute a scientific 'fact.' That is to say, the community of individuals with the internal authority to judge a piece of scientific work based on adequacy criteria (is it right?) is extended to include individuals who are normally responsible for making external quality judgements based on value criteria (is it useful?).

That there are methodological implications to this empirical observation is apparent even in Ravetz's (1971, 145-80, 273-88) arguments, where, he discusses the professional assessment of colleagues in different disciplines as a situation that could today be described as extended peer review:

> the system of quality control in science ... the assessment of a research report by the referee of a recognized journal, for authentication as a published paper (Ravetz 1971, 280) relies upon a very informal ranking of journals in a field by their prestige (Ravetz 1971, 281), prestige in turn functions as the "cash value" of the scientist's intellectual property as assessed by his colleagues (Ravetz 1971, 283) while within the universities, the assessment of a man [or woman] and of his [or her] work ... will have a very different basis from that within a field of science ... colleagues will judge him [or her] from personal experience (Ravetz 1971, 285).

Here, Ravetz can be understood to be describing what we may call a first-order (within the *academe*) extended peer community and associated

extended-peer-review process, where the technical quality of the work being reviewed is of interest to a scientist's colleagues, even though they lack the ability to judge that quality purely through reference to criteria of adequacy. The difference from the peer review of nineteenth century gentlemen's science is a strong differentiation of disciplinary fields, which we today take for granted. In the nineteenth century, this was not so pronounced. At that time, peer review was more general. Any reasonably competent scientist could more or less expect to be able to judge the adequacy of any scientific work. Here, the advent of extended-peer-review processes, far from being normatively motivated, is cast as an empirical consequence of the ways in which the practice and operational context of science changed during the twentieth century.

Also in this 1971 text, Ravetz describes what we might call a second order (between science and society) type of extended peer community and peer-review process, which is somewhat more consistent with what is today usually discussed under that heading. He proposes that the industrialization of scientific knowledge production has changed the relationship between typically value judgment-oriented reviewers (i.e., patrons, government authorities, other intellectuals, etc.) and the technical content of the work that scientists carry out. Here we see the transformation of the product of scientific work from the gentlemanly status of universal knowledge, produced out of curiosity, for the good of humankind, to a more vulgar status of personal, private, intellectual property (in the form of patents and publications). Under the conditions of industrialized scientific knowledge production, "in order to do any research at all, he [or she, the scientist,] must first apply to the institutions or agencies that distribute funds for this purpose; and only if one of them considers the project worth investment can he [or she] proceed" (Ravetz 1971, 44). New procedures for deciding which scientific research work is worth conducting thereby begin to impinge upon how the research itself is carried out: "the goals of a career in science can change from being a series of successful research projects made possible by a parallel series of adequate contracts, to being a series of successful research contracts made possible by a parallel series of adequate projects" (Ravetz 1971, 46). In such situations, where funder specifications become a key factor determining *how a research project will be organized and executed*, quality controls based on value criteria intrude into the technical domain associated with adequacy judgments, stipulating not only *what* should be studied but also *how*.

To illustrate the complexity and novelty of these emergent extended-peer-review processes, we can take a brief look at the initial controversy

surrounding the publication of Bjørn Lomborg's (2001) now infamous book *The Skeptical Environmentalist*: an empirical phenomenon that can also be understood as an emergent, phenomenological extended-peer-review process containing both first- and second-order aspects. Following publication of this book, the international weekly newspaper *The Economist* ran a feature essay in its Science and Technology section "The truth about the environment" written by Lomborg, *by invitation*, introducing the text (*The Economist* August 2, 2001, print edition). Later, in the wake of swelling international controversy, in its January 31, 2002, print edition, the paper ran an article "Defending science," which argued in support of Lomborg and against scientists who were taking issue with his book. This was later followed by a series of further small pieces, as the paper tracked the progress of the formal complaints registered against Lomborg with the Danish Committees on Scientific Dishonesty ([DCSD] *Udvalgene Vedr ende Videnskabelig Uredelighed* [UVVU]). In these articles, it is the voice of the *Economist*—clearly positioned outside of science proper—that speaks and one of the things it speaks about is the adequacy of Lomborg's claims, "They are largely true in our opinion" (*The Economist*, January 31, 2002, print edition).

In the English language version of the 2002 Annual Report of the DCSD, the formal complaints against Lomborg are gathered under the heading "*Three complaints from natural scientists about scientific dishonesty by a social scientist in his treatment of natural science topics*" (DCSD 2003, 47 emphasis original), illustrating that this controversy can readily be understood as an emergent instance of what I have termed above first-order (within the *academe*) extended peer review. At the same time, the formal review undertaken in response to these complaints and the extensive subsequent discourse, in both the academic and popular press, illustrate that debate about the adequacy of Lomborg's science has also extended well beyond the *academe*, becoming a second-order (science and society) extended-peer-review process: on January 6, 2003, the DCSD handed down its decision, acquitting Lomborg "of the accusations of having acted in a manner considered scientifically dishonest. But … stated, at the same time, that he had clearly acted contrary to good scientific practice[3]; on Febrauary 13, 2003, Lomborg filed an appeal against the DCSD's decision with the Danish Ministry of Science, Technology and Innovation; on December 17, 2003, the Ministry judged the DCSDs decision to be invalid, because it had concerned itself with Lomborg's *competence* (adequacy criterion) as opposed to the *ethics* (value criterion) of his behavior, and sent the case back to them to decide if it should be reexamined or dropped; on March

14, 2004, the DCSD announced its decision to drop the case, citing a lack of time and financial resources for convening an interdisciplinary peer-review panel that would be suitably qualified to distinguish between instances of incompetence (criteria of adequacy) and of unethical behavior (criteria of value) on the part of Lomborg in the preparation of the book.

Debate surrounding Lomborg's book is still quite lively and discussion of the case as an instance of emergent extended peer review could easily constitute a paper in its own right.[4] However, it is the character of the debate, as opposed to the details of its content that is immediately relevant to the argument presented here: while certainly comprising various normative prescriptions (not least among them being Lomborg's own) here extended peer review is encountered as an emergent phenomenon, not a planned and designed product.

A Need to be Able to Combine Value and Adequacy Judgments

In another early article, Funtowicz and Ravetz (1992) make a distinction between "internal quality [which] can be assessed by criteria developed within a field of practice" and "external quality, which is defined by a relationship with a broader community ... [and which can be] conceived in such terms as fitness for purpose" (Funtowicz and Ravetz 1992, 964). Proscribing methods that can be used to produce scientific problem descriptions of good external quality, which perform well when measured against the value criterion of fitness for purpose (sic Funtowicz and Ravetz 1992, 1994a), can be understood as the basic aim of extended-peer-review methodology.

Since the specification of purpose is a political, as opposed to a scientific task, with this idea of external quality, political discourse is introduced as a methodological tool of post-normal science (Luks 1999; Farrell 2008). However, by the same token, production of poor external quality scientific descriptions, ones that are, for example, not useful for addressing the concerns of the individuals confronted with a particular science entailed problem, can be understood to be the outcome of poorly designed or poorly managed extended-peer-review processes.

When the snow was white, scientists could claim the right to be heard as policy advisors based on their objectivity. However, under post-normal science conditions, this right to speak with authority becomes problematic. If values, which are judgments that derive from the individual and which may differ from person to person, are mixed up with facts, truth is no longer

universalizable. The democratic theory implications are summarized by Pellizzoni (2003) as follows:

> the impossibility of establishing with mutually acceptable approximation what the outcomes of a choice will be prevents a justification in terms of its being in the interest of everyone affected. Agreement on the "best argument," on a shared justification of a course of action cannot therefore be reached, even in the most favourable and equitable conditions (Pellizzoni 2003, 209).

Failure to recognize the empirical, circumstantial as opposed to intentional, character of the fact/value conflations that increasingly present in late-industrial political debates concerning environmental issues can leave those involved in these debates with a distorted view of their own position within these situations. Taken at face value, calls for the parity inclusion of nonscientists in debates on technical aspects of complex environmental management issues are difficult to distinguish from simple politically motivated appeals for access to a position of power. However, when the empirical context within which these debated arise is also taken into account, calls for the democratization of scientific expertise can and should be distinguishable from ordinary politicking. The development of procedures that help to make this distinction possible is one of the tasks of post-normal science methodology.

The Poisoning of Snow White

The de facto extension of the peer community judging the adequacy of a scientific work undermines the fairy tale expectation that objective scientists provide hard facts for reference in value-laden political debates about clearly defined problems. The wicked problems of the West have poisoned Snow White, and it is this poisoning that necessitates the democratizing of expertise (sic Funtowicz and Ravetz 1990): not only because scientists may be silenced by politics but also because valid political concern may be silenced by appeals to uncertain science.

Helping to make visible the relationships between scientific and political claims concerning wicked problems of the West makes it easier to distinguish between arguments based in facts (adequacy debates), arguments based on opinions (value debates), and those that have taken on a political tone due to the uncertainty of the facts and/or controversy over the values at play (post-normal science debates). The next task, deciding how to proceed

in post-normal science situations, can be supported by drawing out insights about the post-normal science structure of a specific problem under consideration: illustrated here through reference to one in Germany and one in Tanzania. Both cases are centered on wicked problems of the West—where severe environmental impacts associated with late-industrial human activities have created highly politicized, scientifically intractable problems. Both involve de facto extension of the peer community judging the adequacy of scientific understanding about the problems at hand and neither situation has been handled through recourse (or even reference) to participatory science methods. My aim in presenting them here is not to claim that the associated decision processes should have been different or to proscribe how they could be improved. It is to show how looking at these cases through the heuristic lens of post-normal science can help to reveal questions regarding the quality of both the science and the politics at play, which might not have been raised otherwise.

Bitterfeld

Although the name Bitterfeld is little known outside the German-speaking world, within Germany the name of this city has become synonymous with the environmental contamination legacy of industrialization. Founded as a manufacturing city in the early middle ages (c. 1200), in what was then central Germany, the history of Bitterfeld is a microcosm of the history of modern industrialization. As early as the 1870s, Bitterfeld was a center of machine manufacture for the European industrial revolution and a leading center for chemical manufacture (Mühlenberg and Kurt 1991; Banzhaf 1999). At the turn of the twentieth century, the Agfa (Aktiengesellschaft für Anilinfabrikation) factory, in Bitterfeld's sister city of Wolfen, invented, at nearly the same time as the Eastman Kodak factory in upstate New York, color film (Holz 2007) and "[a]t the peak of industrial production in the 1970s, as many as 32,000 people were employed in the two main production facilities, Bitterfeld Chemical Works and the film factory Wolfen" (Heidrich et al. 2004a, 144). The industrial waste that resulted from these activities filled the air of the city with sulfurated coal ash and spills and dumping of chemicals left something on the order of 200 million liters of affected groundwater, contaminated with a great variety of mostly persistent, organic compounds, as well as a selection of heavy metals and other less offensive but nonetheless toxic chemicals (Heidrich et al. 2004b).

Wickedness of the problem. Two key moments in Bitterfeld's history make it clear that the management of the city's contamination is a wicked problem of the West. The creation of this mega-contamination site is, for all intents and purposes, an irreversible situation. This is a problem that can not so much be solved or corrected as handled (Weiss, Teutsch, and Dau 2004). The sheer volume and hydrogeological complexity of the contamination at the Bitterfeld site makes reliable predictions regarding its long-term status at best highly problematic and more realistically, practically impossible (Farrell et al. 2008): there is high uncertainty and the measures of what constitutes a good outcome are not obvious. In addition, the fall of the Berlin wall, in 1989, lead to abrupt cessation of industrial activities throughout eastern Germany, a massive spike in unemployment and eventually to concerted, government-directed efforts to redress the extreme environmental damage in the Bitterfeld-Wolfen area. This included the allocation of billions of Deutsch Marks (Bryson 1992; Merkel 1997) for the physical work of controlling migration of the contamination and for the Snow White like scientific work of developing new ways to solve the unsolvable problem of how to clean up this site.

Although there is good scientific understanding of the basic character and general impacts of the contamination at the site (sic Weiss, Teutsch, and Dau 2004; Heidrich et al. 2004a, 2004b) one of the things that is clearly understood is that the science is uncertain. Hundreds of millions of Euros that have been allocated over the years, for the control and remediation of the contamination of Bitterfeld and concrete human health hazards persist: decision stakes are high.

Extended peer review is phenomenological. It is very difficult, if not impossible, to develop an accurate picture of how the residents of this city experienced the establishment of remediation programs, since a large part of the population left the area after the fall of the Berlin wall (Banzhaf 1999; Flämig et al. 2007) and since the need to clean up the contamination and to remove people from harm's way was practically beyond debate (value criteria were clear). However, on a trip through the city in 2006, I saw a sheet hung out in front of a house near the main industrial complex—an area that was slated for rezoning from residential to industrial—with the words "let us stay, this is our home." Huge sums of money were poured into the technical side of the problem, for remediation of a contamination situation matched in scale only by major chemical disasters like Chernobyl and Bhopal, but the controversies of Bitterfeld are quiet ones, taking place behind closed doors.

There was no participatory science informing the research on how to manage the contamination in Bitterfeld and there was no coordinated dialogue between remediation scientists and local stakeholders. The city, once heralded as the darling of the German Democratic Republic (GDR) (Lange and Schreck 1959), while showing signs of recovery as an industrial park, has more or less lost its social identity. Recently, in response to the massive collapse in population, the town hall in Bitterfeld was closed and administration of the city's government was combined with that of Wolfen's to create the new municipality Stadt Bitterfeld-Wolfen.

Insights. The decision to leave the city of Bitterfeld partly unremediated, turning instead to rezoning, cannot be understood as either entirely political or entirely scientific. Here a complex decision has been made by an extended community of concerned individuals, including the German and Saxony-Anhalt government authorities with responsibility for funding and administering the clean up of the mega-site. A solution has been identified, which the stakeholders involved in the decision-making process find to be fit for the purposes of managing the remediation funds wisely, limiting the detrimental impacts associated with the contamination and dealing with the enormous demographic changes that the city has experienced since the early 1990s. However, there remains the question as to whose purposes are the appropriate reference purposes for addressing this problem.

Should the people who inhabited that residence near the main contamination site be allowed to specify the purpose of the extended-peer-review assessments that led to the decision to rezone their homes? Should they be allowed to take their own choices about exposure to the potential hazards associated with continuing to live there? The question itself is a political one—concerning how, where, and when a community of experts and nonexperts should go about democratizing expertise—and I do not propose to answer it here. Instead, I simply wish to highlight that using the concept of post-normal science as a heuristic makes it possible to talk, in a formalized way, about the authority of these people to speak on complex matters of great concern to them, which might otherwise quite easily be classified as technical and not open to debate.

The Usangu Plains

Moving from the womb of European industrialization to the wilds of Africa, our second illustrative case brings us to the high plains of southern Tanzania: an area not typically associated with late-industrial problems. However,

a closer look at the story of the migration and eviction of the Parakuiyo Maasai of the Mbarali District, who were evicted from these lands in the name of nature conservation, reveals a strong connection between the problems faced by this small pastoralist tribe and the industrialization of both Tanzania and the global economy.

> In the period from May 2006 to May 2007 large numbers of Sukuma agro-pastoralists and IlParakuiyo, Taturu and Barabaig pastoralists and their livestock have been evicted from the Usangu Plains in Mbarali district, Mbeya region. It is estimated that more than 400 families and 300,000 livestock have been forcefully moved, and a high number of livestock died and other[s were] lost during the operation. Thousands of cattle died after being confined at Igawa camp for weeks without pasture and water (Mwarabu 2008a).

The first round of evictions was carried out by the government of Tanzania in 2006 and 2007, to stop the pastoralists from watering their cattle in the eastern wetlands of the Usangu plains. In January 2009, those members of the tribes who had managed to establish themselves in the Kilosa and Kilombero districts, in the coastal Morogoro region, were once again evicted (Mwarabu 2009, personal communication). For these pastoralists, the second round of evictions basically meant the loss of their remaining livestock, the deterioration of their extended families, and finally, the devastation of their tribes.

As with the preceding illustrations, the aim here is not to take sides in this debate but to illustrate how the use of the concept of post-normal science as a heuristic can reveal overlooked aspects and hidden biases within a situation that may appear, on the surface, to be mainly a technical scientific problem.

Wickedness of the problem. At first glance, the technical problems here seem relatively straightforward. The Maasai were watering their cattle at the edge of and within the wetlands, and this was leading to water logging, which damaged the hydrology of the system (Mtahiko et al. 2006; Malley et al. 2007), reducing the downstream outflow from the area. They were evicted based on the entirely valid claim that their activities were damaging this sensitive and at-risk ecosystem, which is a major source water body for the Great Ruaha river. The Great Ruaha waters the Ruaha National Park and associated big game reserve and eventually feeds into the Metra and Kadatu hydroelectric dams. In recent years, it has been drying up (Mtahiko et al. 2006; Malley et al. 2007).

However, on closer examination, it becomes clear that the science underpinning the case for evicting the Maasai (Mtahiko et al. 2006) entails a great deal of uncertainty. For example, it is not possible to explain why the river is drying up. There are simply too many key factors involved for any one of them to be singled out as the main cause of the problem: downstream dams, agriculture upstream in what used to be the western wetlands, climate change induced drought, the possible onset of an ordinary cyclical drying phase in the region, and pastoralist trampling of eastern wetlands. Furthermore, there are still simple empirical puzzles that remain unsolved, since the observed drops in rainfall are not necessarily correlated with the patterns of drying seen in the river (Mtahiko et al. 2006; Malley et al. 2007). System uncertainties are high and adequacy criteria are not clear.

The social situation is more obviously complex, involving a number of opposing stakeholders: decision stakes are high and value criteria are unclear. In the treaty of 1904, agreed between the functionaries of the Monarchy of England and Lanana, then leader of the Maasai federation, Lanana agreed to vacate certain lands, mainly Nairobi (the place of cool clear water) and the Rift Valley, in exchange for the guarantee that no Europeans would occupy the Maasai's remaining lands "so long as the Masai as a race shall exist" (Ainsworth 1911).[5] However, establishment of big game reserves and national parks in the areas assigned to the Maasai under this treaty has led many of them to spend more time further south, now reaching as far as the grasslands of the Usangu plains. That is to say, the Usangu plains are not traditional Maasai lands. However, since the Maasai are nomads, the idea that they have fixed traditional lands is itself problematic, adding further to the social complexity of the situation. Then there is the fact that this region already had a settled population when they arrived, who were known at the beginning of the colonial period for the exceptionally high-grade salt they manufactured from brackish well water, which has been traded for thousands of years, perhaps as far east as Asia (Sutton 1990, 17) and currently hosts large refugee populations from Congo and Burundi (Kajembe et al. 2003; Malley et al. 2007). Finally, the Tanzanian big game reserves in the Ruaha region, which are an important tourist attraction and income source for the economy, are also being adversely affected by the drying up of the river, which is affecting wildlife population levels (Malley et al. 2007).

Extended peer review is phenomenological. Options for reducing pressures on the Great Ruaha river system are limited. Rice cultivation to the West, while certainly having impacts on the system, is providing food for the residents of the plains (estimated at circa 200,000 people in the year 2000, as

per Kajembe et al. 2003) and also for export to other parts of Tanzania and other countries in the region (Kajembe et al. 2003; Mtahiko et al. 2006; Malley et al. 2007). Stopping this upstream diversion of water is politically ill advised, as it would expose a large human population to food insecurity. At the other end of the river system, downstream hydroelectric dams, which are certainly contributing, through evaporation, at least somewhat to water loss in the system are central to the industrial activities of Tanzania, providing roughly half of the country's electricity, when they are functioning (Mtahiko et al. 2006, 498). Closing the dams is not an option, although it may be a necessity, if the river runs dry.

Seen in this light, through the heuristic lens of post-normal science, the decision to evict the pastoralists from the Usangu plains, rather than to reduce other pressures on the system, cannot be defended as an impartial one, based on objective assessment of a fixed and clear set of scientific facts. What looks, on the face of it, to be a conflict between environmental conservation and the rights of indigenous peoples, when viewed as an extended-peer-review process, is revealed to be a tightly tangled set of late-industrial issues: dependency on the electricity generated from the Metra and Kadatu dams, colonial dislocation of the Maasai into these more southern regions of Tanzania, displaced persons from conflicts in Burundi and Congo and tourism associated with the National Parks and big game reserves, where wealthy sightseers from the industrialized world come to see a form of wilderness that no longer exists in their own lands.

Insights. The eviction of the Parakuiyo Maasai from the eastern wetlands of the Usangu plains was a police action, enforcing policy to protect the wetlands (Pastoralists Indigenous Non-governmental Organisation's Forum 2006; Mwarabu 2008b, 2009). By framing (value judgments) the problem as one of immediate preservation of current water uses and protection of the eastern wetlands, the government was able to make the eviction of the pastoralist into a technical matter. However, depending on whose perspectives are taken into account, the Maasai's, the government's, the settled inhabitants of the region, the refugees settling there, or the tourists and tour operators, the fitness of this decision looks different.

Conclusion

The arguments presented above have been concerned with the character of the relationship between the empirical description of and normative prescriptions for dealing with what I have termed here wicked problems of the

West, where post-normal science conditions prevail. The aim has been to provide a systematic review of the relationship between the empirical observations made by Funtowicz and Ravetz in the 1970s and 1990s, the concept of extended peer review and the normative recommendation that expertise concerning post-normal science problems should be democratized. I have set out to demonstrate, both argumentatively and with illustrations, that informed application of the post-normal science concept of extended peer review need not be focused always on the quality and success of stakeholder consultation forums. Instead, I have proposed that the concept constitutes a promising heuristic that can be used to differentiate among the various fact- and value-based arguments being advanced within a complex discourse concerning one or another wicked problem of the West. Although an informed heuristic application can be undertaken without moving on to normative design, I would propose that an informed approach to normative design questions cannot be carried out without attention to the heuristic potential of the concept.

Previously, when the snow was white, normative scientific recommendations were acceptable only on the basis that they related to "the facts": the job of the analyst was to assess the facts of the situation and on that basis to make normative recommendations. However, wicked problems of the West are poison to the Snow White fairy tale of scientific objectivity. Under such conditions, giving normative scientific advice for policy can no longer be viewed as a value-neutral technical recommendation. It has become a political act (Luks 1999; Farrell 2008). In this respect, efforts to formalize the management of extended-peer-review processes through the development of participatory science methods can be understood as attempts not to give up on but rather to shore up the integrity and the quality of the associated scientific work: as a response to situations where the empirical conflation of facts and values may be exploited to advance a politically unjust and/or scientifically dubious position. When employed as a heuristic, the concept of post-normal science helps to reveal the structure of extended-peer-review situations, making it possible to explicitly discuss who is advancing or being silenced by an argument that needs to be evaluated, according to both adequacy and value criteria. Although this by no means guarantees the democratizing of expertise, what it can do is inform that work, so that it can be done well.

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Notes

1. Kuhn (1970[1962]) introduced the concept of "normal science" as part of his theory that scientific knowledge progresses through socially constructed paradigm shifts, from normal science (the rules are agreed, science is puzzle solving) to revolutionary science (important rules are called into question) to new normal science (new rules are accepted, science returns to puzzle solving under new rules). A clear illustration of the theory in practice is the Copernican revolution, where Copernicus' idea of a solar system was largely ignored (not in the rules), then Galileo was deemed a heretic (rules called into question) and today the solar system is taken as an obvious and foundational part of scientific knowledge (new rules).
2. Funtowicz and Ravetz (1990) have dedicated an entire book to elaborating the concept of NUSAP, and it would be cavalier to attempt to explain the methodology here. Generally, one can think of it as a procedure whereby information concerning the measurement tools (Numeral, Unit), precision and reliability (Unit, Spread), judgments (Assessment), and expert authority (Pedigree) contributing to the creation of a piece of scientific knowledge are presented in a format that can be easily understood, evaluated *and judged* by nonscientists and/or by scientists not expert in the field of research being addressed.

3. All information and quotes concerning the decisions of the Danish Committees on Scientific Dishonesty (DCSD) were taken from the Committees' Web site at http://forsk.dk/ (accessed March 2005). Although this summary text is no longer available on the Internet, the two DCSD Annual Reports for 2002 and 2003, which reference the complaints and the associated DCSD decisions, can be found at http://en.fi.dk/publications/2003 and http://en.fi.dk/publications/2004 (accessed March 2009) and full citations for these reports are included here in the references.
4. Detailed information about the current status of this debate can be accessed via Lomborg's own Web site (http://lomborg.com/faq), via a post-normal science discourse blog on the topic, at the Post Normal Times (http://www.postnormaltimes.net/blog/archives/the_lomborg/) and via a very comprehensive English language Wikipedia entry (http://en.wikipedia.org/wiki/Bjørn_Lomborg), which includes a long history section, where the debate surrounding the development of entry can also be observed.
5. While Edward VII was regent of the United Kingdom in 1904, when the treaty came into force, it was negotiated by functionaries from both his and Victoria's regencies.

Authors' Note

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