Mess in Science and Wicked Problems

Jutta Schickore
Department of History and Philosophy of Science and Medicine, Indiana University

This paper discusses the claim that science is "messy." Part I argues first, that a good portion of today's discussions about messy science is just a portrayal of familiar features of science in new terms. In the paper, I refer to this as "messy science talk." Second, Part I draws out rhetorical functions of messy science talk, namely the denigration of science in the popular media and the celebration of the maverick. Part II identifies one way in which it is enlightening to think about mess in current science, namely in reference to the problems that scientists need to address. It also shows that we do not need an entirely new conceptual inventory to analyze these problems. "Mess" and "wicked problems" were a theme in operations research and theories of social planning in the 1970s. These older analyses can illuminate important characteristics of today's scientific problems. Wicked problems cut across different disciplines, engage different stakeholders (including non-scientists), are fluid, and cannot even be clearly formulated. They are urgent and need to be addressed before sufficient evidence is in.

1. Introduction

Nowadays, we often hear, or read, about "mess" or "messiness" in science. In books like Helga Nowotny's *An Orderly Mess* (2017), messiness is described

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as the new background condition of science, society, and our personal lives. John Law's *After Method: Mess in Social Science Research* (2004) calls for new research methodologies better to accommodate a messy world. There are blogs and articles discussing messy science (Aschwanden 2015; Gould et al. 2014). Mess even appears as a lemma in scientific encyclopedias (Ferrales 2020). Science educators state that messiness should be foregrounded in science teaching (Metz 2005, 2014; Turner and Khalilah Shamsid-Deen 2005). Sometimes messiness is invoked to suggest that established philosophical concepts and arguments are not very useful for the analysis of science (Currie 2014; Lipton 1999). Nevertheless, the claim that there is mess or messiness in science is rarely spelled out in detail, and the implications of messy science for philosophy of science also remain vague.

Is science messy? And if so, is this really a novel insight, or is "messy" just a new label for well-known features of science? Sometimes, finding or devising new concepts for characterizing things, activities, processes, events etc. can be enlightening because the new concept captures a feature of that thing, event, etc. that had not been noticed or properly understood before. Sometimes, the adoption of a new analytic term, e.g., "exploratory experimentation," is felicitous in this regard. Yet it is not always the case that the process of giving a new name to a practice generates novel analytical insights or explanations (Gingras 2010).

The notion of messy science certainly does generate a raft of questions. Is all science messy or just some? Is current science messier than past science? Is messy science bad science or good science? Does messiness in science make philosophical analysis less useful to science, and if so, what could be done to make philosophy more relevant to science? Does messy science require a renovation of philosophy of science; does it even make philosophy of science superfluous altogether? These questions sound worthwhile at first; still, it is prudent to ask: Do the designations "mess in science" or "messy" have any surplus value for science studies?[1] Or are they really just re-naming something we already know and have discussed, are they perhaps even trivial ideas in a fancy—untidy—dress?

In Part I of this paper, I show that a significant part of today's concern with mess in science is just shorthand either for familiar characterizations of scientific practice or for challenging the normative claims or conceptual inventory of philosophy of science. In the paper, I refer to this kind of shorthand as "messy science talk" because it oversimplifies and thus obscures rather than enlightens. I also draw out the main rhetorical functions of messy

1. In this paper, I use "science studies" as an umbrella term for philosophical, historical, cultural, sociological…analyses of science.

science talk, namely denigrating science in the popular media and celebrating the maverick.

In Part II, I take my cue from operations research and theories of social planning in the 1970s. At that time, it was common to characterize certain research problems as "messy" or "wicked"—not just hard, complicated, or ill conceived. Drawing on this literature, I argue that these older analyses of the features of messes or wicked problems can illuminate today's science. Specifically, they can capture important characteristics of today's scientific problems and help pin down what is special about them. Yet, given the looseness of current messy science talk, I recommend that we should stick to the original language and characterize these problems as wicked rather than messy. In conclusion, I suggest that the philosopher's problem of how to analyze science might itself be a wicked problem.

2. A Starting Point

I want to begin with a rather old-fashioned notion of philosophy of science and its role for science, namely Hans Reichenbach's notion from *Experience and Prediction*. Many philosophers of science today will consider it outdated. I am not endorsing it, but it is a useful foil against which to clarify and illustrate what is at stake in some discussions about messy science, and whether the recent discussions about it are really bringing up anything new.

According to Reichenbach, science in the making (by which he meant just thinking, not doing) encompassed "rather vague and fluctuating processes [which] almost never keep to the ways prescribed by logic and may even skip whole groups of operations which would be needed for a complete exposition of the subject in question" (Reichenbach 1938, p. 5). Philosophy of science "intends to construct thinking processes in a way in which they ought to occur" (p. 5). In other words, scientific thinking is deficient, and philosophy of science provides the best tools to complete the task of science. As Reichenbach put it, philosophy of science [he used epistemology] gives "a better way of thinking than actual thinking" (p. 6). He also emphasized the "great distance" between logic and actual thinking. It would thus be a "vain attempt" to construct a theory of knowledge that would be at the same time "logically complete and in strict correspondence" with actual thought processes (p. 5).

In short: 1) Scientific activity is essentially "thinking." 2) Scientists' thinking is "rather vague and fluctuating." 3) Scientists' thinking needs to be tidied up. 4) It's the job of philosophy of science to do this, to "*replace* actual thinking by such operations as are justifiable, that is, as can be demonstrated as valid." (p. 7; emphasis added). The underlying assumption is that the world is logically ordered and as such, knowable. Philosophical analysis is required to uncover this order. 5) We therefore should not expect that the final philosophical exposition of scientific activity will correspond with actual scientific thinking.

On Reichenbach's view, day-to-day science is less-than-ideal science ("vague and fluctuating" thinking). In what way and why scientists' thinking is "vague and fluctuating" are questions outside the scope of philosophy. Nevertheless, philosophy of science is eminently important to actual science. Reichenbach envisioned that philosophical reconstruction of scientists' thinking completes the project of understanding the world and its logical order, leaving actual scientific practice behind. The tacit assumption here is that the world is, in fact, ordered.

3. Night Science

Revisiting the Reichenbachian view is useful because it provides a background against which we can sort out different charges tied to the claim that science is messy and against which we can distinguish the well-worn from the novel. Consider, for instance, the claim made by science educators that scientific practice is messy. If they do not just make the innocuous point that science activities for kids should be fun, they typically highlight the difference between the lab class that is part of a science student's education and the real-world research lab. The editor of the journal *The Science Teacher* observed: "…we all look for straightforward, easy-to-manage lab activities that verify a scientific principle, preferably within an hour's time. Labs that produce clear and unambiguous results in a short timeframe are appealing, but at what cost? Science itself is a messy process… scientists rarely conduct investigations that can be completed in an hour or so, where a predetermined answer is achieved simply by following directions" (Metz 2005). Here, "messy" practice means something like "emulating the real research world, yet inconvenient for science teachers." In teaching contexts, students will typically experience and replicate demonstration experiments and learn textbook knowledge; they rarely experience the "night science" that "wanders blind" and "hesitates, stumbles, recoils, sweats, wakes with a start" (Jacob 1998, p. 126).[2] On this view, messy is a diagnosis of how science—day-to-day science—*is*. Day-to-day science is not as well-ordered or tidy as science textbooks suggest, as non-scientists may assume, or as misguided ideas about "The Scientific Method" may lead one to expect. Actual research is tortuous, not straightforward, produces imprecise and ambiguous, not clear and unambiguous results; it takes a long time, not just an hour, and is roving and roaming, not following a discernable linear path.

The diagnosis that actual research is messy is not terribly enlightening. For professional analysts of science and scientific practice, the realization that

2. Jacob owes the distinction between night science and day science to Gaston Bachelard.

day-to-day scientific activity falls short of epistemic ideals is old news. The science educators' diagnosis that scientific practice is messy is in fact a reiteration of the same point that Reichenbach already made about vague and fluctuating scientific thinking, yet it is couched in new terms and re-evaluated. Calling science messy is to cast into question those of us who thought that proper scientific practice is as well ordered, systematic, and methodical as the science textbook suggests…but who would still defend these assumptions nowadays? In the diagnostic mode, the notion "messy" does not pick out specific features of objects, concepts or practices. Instead, the designation "messy" for day-to-day practice is typically given *ex negativo*, messy means *not* certain, not unified, not strictly general, not predictable, not precise, etc. In the diagnostic mode, the term "messy science" reminds us that in day-to-day practice, scientists don't always follow a straight investigative pathway; their arguments are not always completely logical, their terminology is not always precise, research problems are not always well-defined.

This was once an important point to make in science studies, namely in the early days of ethnographies of laboratory work (e.g., Knorr-Cetina 1981), of studies of research notebooks (e.g., Holmes et al., eds. 2003), and the discovery process (Holmes 1974, 1991). In 1979, Bruno Latour and Steve Woolgar, for instance, described laboratory life and scientists' opportunistic and ad hoc ways of conducting research in (then) unusual detail; analyzing how the lab members attempted to create order out of seemingly bewildering conglomerates of materials, devices, and inscriptions (Latour and Woolgar 1979). They teased their readers not so much by providing lots of details but by using economic models and anthropological terms for their account of the opportunistic and ad hoc processes of knowledge generation they reported to have observed at the Salk Institute, thus provoking all those readers who might have thought that everyday science is somehow detached from, and superior to other social practices. They nettled some of their readers quite effectively.[3]

Since the 1970s, lab ethnographers have often described the ad hoc, opportunistic ways in which scientists approach research problems: how they use whatever resources happen to be available in the lab to find solutions to the problems they are trying to solve. Inspired by Harold Garfinkel's *Studies in Ethnomethodology* (1967) or Claude Lévi-Strauss's anthropology, they introduced various terms to characterize this practice, such as "tinkering" or "bricolage."

Today, the larger point—the point that day-to-day scientific activity does not have to be, and typically is not, neatly ordered to generate new knowledge—is widely accepted. Simply describing day-to-day scientific practice as messy is, in

3. See also Latour's "Cartesian" study *Science in Action*, which is modeled—tongue in cheek—after Descartes' *Principles of Philosophy* (Latour 1987).

fact, black-boxing it, thereby obscuring the more specific insights analysts have gained about it in the last few decades.[4]

4. Normative Philosophy of Science is Presumptuous

There is another brand of messy science talk that is directed against normative philosophy of science. Again, it is helpful to consider this charge against the background of the Reichebachian position. On Reichenbach's view, science needs philosophy of science as an aid to realize its goal to provide an accurate, complete epistemic account of the natural world. Philosophers of science complete the task of science by making scientific thinking even less "vague and fluctuating" than scientists' own reconstructions of their arguments do. The notion that science is messy sometimes encapsulates the claim that normative philosophy of science is presumptuous in its goal to tidy up scientific thinking and to complete the task of science.

What is at stake here is who is in a position to decide what messy or properly ordered science is. Who has that authority—scientists or non-scientists? And what warrants this authority? In this context, the notion of messy science strikes me as gratuitous. Analysts of science do not have to subscribe to any particular notion of messy (or indeed well-ordered) science to reject normative philosophy of science.

Notably, in some areas of gender and identity studies, this move against normative frameworks for knowledge-making takes on a more iconoclastic function, as it is combined with a charge against the framework itself and against efforts to apply and enforce it. The concern about the legitimacy of normative philosophy re-emerges in gender studies as a broader challenge against "binary logics." The *International Encyclopedia of Human Geography*, for instance, features an entry "Mess." In it, the practice of focusing on mess is described as a methodological choice and as a critical stance from which to characterize established ("Western") ways of knowing and seeing and to identify these "binary" and "linear" logics as practices of ordering, tidying up, controlling, and, as such, acts of suppression (Farrales 2020, p. 62; see Manalansan 2015). Here, the critique of logicist frameworks for the reconstruction of knowledge becomes an act of unmasking the hidden ideological purposes of certain methodological assumptions – their power to suppress certain social groups and their practices. Logicism and formalism are revealed as norms that dominate, override, or annihilate.[5] Mess is presented as a

4. A similar point can be made about "messy data", another term that has recently gained traction. Messy data covers a multitude of characteristics that the careful analyst may want to keep separate: incomplete, noisy, uncertain, ambiguous data or data from diverse sources. And, as in the case of messy science, we do not have a clear sense of the complement – what is "good" data? And who has the authority to validate?

5. Feyerabend's methodological anarchism in *Against Method* belongs in this category. For him, method stifles progress (Feyerabend 1975).

good thing, embracing mess as a liberating act; yet again, the focus is on the norms and their functions, not on the mess itself: The point here is that binary logics do, but should not, model how all of us should think. But does anything else go? If not, we are still left with the question: who can legitimately claim normative force over scientific thinking and practice?

5. Familiar Territory

So far, we have covered familiar territory: Each variant of the claim that science is messy discussed so far could be rendered in more traditional analytic terms, and the resulting point was innocuous or widely accepted or both. As a diagnosis, the claim that the process of knowledge generation is messy does not have any surplus value. If we question the normative philosophers' conviction that they are in a better position than the scientists to put science in order, the designation "messy" for knowledge in the making is gratuitous. The point that scientists themselves have the authority to negotiate what properly ordered science is does not become more convincing if we insist that science is messy. The Reichenbachian picture of knowledge and its generation is not being replaced, but the different elements of the picture are being re-evaluated and the scope and tasks of (philosophical) analyses of science are being redefined in the process.

Other concerns are about the traditional conceptual inventories of philosophy of science. If the analysis of messy science, night science, vague and fluctuating thinking, science in the making, or whatever we want to call it falls within the scope of philosophy of science, and not, as Reichenbach thought, outside it, then the disciplinary boundaries between philosophy of science and history or sociology of science are called into question. If philosophy of science morphs into history or sociology, what analytic framework should be used for analyzing science?

The underlying worry that is sometimes couched in messy science talk is the following: There is no reason to think that philosophical conceptions, stripped of their normative function, are particularly suitable tools for the job of analyzing scientific activity in its entirety. If we want to analyze scientific activity *in toto*, warts and all, traditional philosophical frameworks are simply too narrow, as scientific activity comprises more than just thinking and argumentation. To analyze practices of doing research, research methods and methodologies, as well as social interactions among scientists and between scientists and other social groups, we need to develop, or adopt, additional conceptual tools. And perhaps, to paraphrase Philip Kitcher, the (messy) workings of science exceed "the thin fantasies of philosophical imagination" (Kitcher 2014, p. 109).[6]

6. In the paper in question, Kitcher commented on the "messiness of life" more generally—it exceeds the "thin fantasies of philosophical imagination."

The worry is exacerbated if we, as analysts of scientific knowledge and practice, aim to contribute to science. Here the worry is that even if analysts focused more on stuff and doing things, philosophical logics, concepts, and distinctions are too clean, too precise, too formalized and thus too remote from actual, messy science to be useful or informative to scientists. Messy science talk invokes the concern that the most developed philosophical positions are out of touch with real-life scientific issues and only of interest to professional philosophers. The worry is that the tools that philosophy of science provides cannot serve to resolve issues arising from actual scientific practice in such a way that the solutions have a bearing on that practice.

If relevance to scientific practice is the goal, the criticism encapsulated in messy science talk is plausible. After all, philosophical discussions have the tendency to get ever more narrowly focused on quite detailed problems. Even if they are occasioned by problems arising in scientific practice, over time, philosophical contributions no longer respond to scientific issues but to other philosophers' arguments.[7] Contributors to these discussions may end up with very sophisticated answers to certain very specific problems that originated in messy science, but a million other issues remain unresolved. And faced with those sophisticated expositions and solutions, we most likely need to turn to HOPOS to uncover the scientific issues that occasioned philosophical discussions. Moreover, philosophical discussions have the tendency to generate several different answers to philosophical problems, whereby none of these answers is without drawbacks. Again, even though the original problem may have emerged in scientific discussions, each of the solutions creates its own momentum, and none of them wins. Many articles in the *Stanford Encyclopedia of Philosophy* illustrate this: there are good reasons to embrace (or reject) realism as well as antirealism, Bayesianism as well as frequentism, and so on. In each case, we have a number of different answers to a question, all more or less plausible, none ultimately satisfactory, so how should these answers possibly help scientists resolve their day-to-day issues? Yet again, calling these issues messy does not illuminate the situation.

6. In Praise and Contempt of Messy Science

Calling science messy serves other purposes beyond describing the practice of doing science or challenging philosophy's authority and conceptual inventory. Notably, the term messy is so equivocal that it can be put to

7. The concept of explanatory power, for instance, was introduced in the 1960s to better capture criteria and processes of theory choice. Since then, the concepts have multiplied and become ever more specific. Philosophers of science seek to specify formal conditions of adequacy (Schupbach and Sprenger 2011), explicate explanatory power in a Bayesian framework (Cohen 2018), or seek to distinguish different dimensions of goodness of explanation (Ylikoski and Kuorikoski 2010), for example.

use for the opposite purposes. Dictionaries tell us that messy means: involving, accompanied by, or generating disorder; untidy; dirty; disorganized or otherwise imperfect. According to the dictionary definition, messy implies something unpleasant or not quite right, something that should be avoided or tidied up. In actual contexts of use, however, messy is a much more equivocal term than the dictionary definitions suggest. While calling something "messy" is often meant as a criticism and a request for tidying up, it is sometimes an endorsement of sorts.

Consider, for instance, the article "Are Scientists Too Messy for Antarctica?" published in *Scientific American* (1993). The author, John Horgan, complains that scientists sometimes make a mess in the literal sense: They pose a danger to the environment. Human interlopers dump food waste, junked machinery, PCBs and radioactive waste on land and sea around the South Pole, causing the degradation of the delicate continent. Horgan raises a serious concern, but he does not pose a difficult theoretical or philosophical problem. He just demands, very legitimately, that scientists need to clean up after themselves.

The opposite view is that it is fun to mess around. It is cool to muck about with Erlenmeyer flasks, chemicals, and agar-agar in petri dishes. The phrase "messy science" frequently comes up in the context of K-12 science education. The hope is that, having done the Mentos and Diet Coke experiment and similar activities in science class, kids may decide to take up a science career in the future. Both notions of mess are innocuous and uncontested.

There is the notion that messing around has a liberating function—encapsulated in the popular idea of the "creative chaos" that is the birthplace of the next big breakthrough. This is a claim about the generation of new knowledge. In the Reichenbachian framework, this would be the claim that actual thinking (and doing) replaces operations and arguments we previously assumed were justifiable and could be demonstrated as valid with new arguments and operations. Instead of assuming that "vague and fluctuating" thinking needs rectification through proper reconstruction, one would say that loose, associative, unrestrained thinking leads to entirely new frameworks, superseding the old ones.

To cash this out in a theory of creativity, one would need to demonstrate that mess, confusion, Jacob's experience of "night science" is not just an occasional or a frequent, but indeed a necessary step for theoretical or methodological innovation. Given the numerous attempts to elucidate processes of discovery and human creativity, I doubt that this can be easily demonstrated.

An alternative to praising messiness and celebrating mavericks is to suggest that science never gets out of the quagmire; it's messy through and through. In the popular media, "messy" is often equaled with "broken" (Balling 1994). Such articles on the alleged "messiness" of science insinuate that there is something wrong with science today; that scientists do not quite know what they are talking about; that they are confused, at a loss, unable to agree; or even that they deliberately abuse their scientific work to promote a certain political agenda. The allegation is that because science is "messy" in this sense, it does not deserve funding and should not form the basis for policy decisions.

The bad popular image of messy science has evoked responses in defense of messiness–not necessarily in terms of praise, but aiming at rectification of popular misunderstandings. Those responses often come from scientists (Aschwanden 2015). There are numerous blogs and articles in popular science journals showing that this negative image of "messy, broken science" rests on common misunderstandings of how science really works[8]—it rests on a misguided idea that everyday science, or, more broadly, science in the making, is governed by formal, logical and mathematical principles or follows the notorious "steps of the scientific method": Purpose/Question – Research – Hypothesis – Experiment – Data/Analysis – Conclusion.[9] Here current scientists are working through ideas that scholars in science studies—and, indeed, past scientists—have examined for a long time.

Nevertheless, even though the insights about the characteristics of laboratory activities are not new, there is one important general point to take away from reading those popular discussions about messy science. We need to realize that we, as analysts, are part of the context we are analyzing. Reflecting on the rhetorical uses of the term "mess" helps us see that introducing or using suggestive or provocative labels for science and scientific practices comes with responsibility. Who is supposed to learn or benefit from the analysis of science – scientists? Non-scientists? Professional philosophers, policy makers, or potentially any responsible citizen and member of the public? Are analysts of science doing their work as a service to science or as a service to humanity? Whatever their purposes are, are they well served by calling science messy? What is our responsibility as analysts, are we obliged to consider the long-term consequences of our analyses; are we even in a position

8. Conversely, scientists who say of their colleagues that they made a mess do so to insinuate that these colleagues don't know how to do science properly ("you won't believe how messy that research is"). Perhaps not surprisingly, I only have anecdotal evidence for this; it is not something one would say in a publication. This seems to be how scientists typically understand the term. For other scientists, messy science is just something they think they can't deal with ("that's just messy").

9. The notion that there are discernible steps of the scientific method is, in fact, promulgated by science educators (see Helmenstine 2020). A quick search on Google turns up countless colorful diagrams, depicting the six—or four, or five, or seven, or eight, or nine—steps of the scientific method. For a historical treatment of the concept of scientific method in educational contexts, see Rudolph 2005.

to consider them? Following Justin Biddle and Rebecca Kukla's terminology, we could say that analysts of science offering accounts of science are faced with phronetic risks (Biddle and Kukla 2017, p. 217).[10]

7. Messy Worlds

The last section showed that some commentators on science exploit the term "messy" to denigrate science. Those who want to undermine the authority of science insinuate that properly ordered science (whatever this is supposed to be) will never be fully realized. Others exploit the ambiguity to celebrate a certain scientific persona, the maverick genius, who will create a new order. Yet others, worrying about the bad image that science has in the popular media might insist that scientists' thought is never vague and fluctuating (there is no mess to begin with). In each of these perspectives, the underlying assumption is that (proper) science is fundamentally a process of ordering, and that properly ordered science is the goal that needs to be reached or at least approximated for science to merit our support and our funding.

This assumption underlies not only popular discourses on science. It is, in fact, one of the orthodoxies of Western science and philosophy. The tacit assumption is that proper science needs to be a process of ordering because the world is, fundamentally, ordered—logically ordered, and as such, knowable, as Reichenbach presumed. That idea has permeated philosophical theories of science and knowledge for centuries. Until today, philosophers, lab ethnographers, and other analysts of science of different stripes keep providing various conceptual frameworks that are supposed to help account for the process of creating order. Whether "messy science" is good, bad, or just the way day-to-day science is, ultimately, that mess will, and should, disappear, as scientists stabilize their experiments, give support for their hypotheses or abandon them, and come to agreements about what theories and models to accept. The common assumption is that science strives for the creation of order out of mess: Night science will become well-argued and well-ordered day science; young experimenters will become serious scientists; ad hoc and opportunistic lab activities will be regularized and presented in well-argued papers of standardized format, and so on.

Alas, this point is a little too general to be instructive. Consider, for instance, three wildly different accounts of order in science are Paul Hoyningen-Huene's work on systematicity (2013), Philip Kitcher's program of well-ordered science (2001), and Michel Foucault's archeology of the order of things (1971). It is

10. STS scholars, notably Bruno Latour, have begun to reflect on the Science Wars in this vein (see Kofman 2018). Some scientists are beginning to reflect on their responsibilities as tellers of stories about science (see Wilson 2017).

not a very enlightening exercise to try to pinpoint what these approaches have in common.

Let us try out a different question. Do we really have reason to suppose that science is concerned with ordering? Is there perhaps something wrong with the intuition that good science is properly ordered science? This idea informs radically different approaches to science, ranging from Nancy Cartwright's philosophy of physics (1999) to John Law's theory of the social.

In *After Method: Mess in Social Science Research*, Law asks: "If [the social world] is an awful mess…would something less messy make a mess of describing it?" (2004, p. 1). Is messy science good science because only messy science can adequately describe messy phenomena in a messy world? Law's suggestive question evokes the idea that only messy science could adequately deal with its (messy) target. Disregarding for a moment the claim that messy science is good science, it is quite plausible to think that the social world is not well ordered—this is more plausible, in fact, than the opposite.

Cartwright's claim is less provocative and more profound because she challenges our fundamentalist intuition that the basic laws of physics apply everywhere. What if the world is a messy place? Along these lines, the term "messy world" is used as a generic term, implying that traditional philosophy of science concepts of law or explanation need to be rethought (Lipton 1999; see Rueger and Sharp 1996; Currie 2014).[11] All in all, however, the question of whether the world is messy or well-ordered is another very general question that it is hard to discuss it in a meaningful way. So let us return to messy science.

8. Wicked Problems

References to "messes" are frequent in the recent literature on issues in science policy and environmental studies. Examples for messes include environmental research on flooding (Donaldson et al. 2010); the challenge of deciding upon policy interventions to support community resilience (Forrester et al. 2019); the study of water (Jackson and Buyuktur 2014); field research in human geography (Davies et al. 2012); and legislative issues related to climate change (Lazarus 2009). All these studies focus on particular kinds of problems—problems that are complex, urgent, cut across many different fields, require input from the biomedical or

11. I should add that Law's question is a red herring because it implies a realist, representational account of science. In fact, he is trying for something much more radical. He offers an ontological interpretation of science whereby scientists, through their activities *qua* scientists, co-construct the world they investigate, describe and explain. Law has predecessors in social constructivist theories of science who made similar suggestions without couching them in the language of mess.

physical and chemical sciences as well as the social sciences, and engage and involve non-scientists. Such entangled problems are the interesting messes in today's science.

In *After Method: Mess in Social Science Research*, Law discusses social phenomena such as alcoholic liver disease, its causes and treatment; the introduction of water pumps in a developing country; or fallout monitoring after a nuclear disaster, claiming that the social sciences are ill-equipped for analyzing them. In contrast to Law, I think that the older literature in the social sciences does provide resources for dealing with those interesting messes. The social science literature from the 1970s offers some conceptual resources that strike me as still valuable for this purpose. At that time, it was common to characterize operations research and social planning as "messy" or "wicked." I think that these older analyses of the features of messes can illuminate not just legislative or policy interventions (Lazarus 2009; Forrester et al. 2019) but important characteristics of today's scientific problems more generally.[12]

In the 1970s, theorists of social planning proposed to distinguish the social sciences from the natural sciences because the kinds of problems treated in each domain seemed to be so different that the research methods had to be different, too. In a classic article on social planning, Horst Rittel and Melvin Webber claimed that the natural sciences dealt with problems which were "definable and separable and may have solutions that are findable" (Rittel and Webber 1973, p. 160). According to the authors, it was a misunderstanding to think that problems in the social science resembled natural science problems, and it was therefore wrong-headed to measure the social sciences against the natural sciences. The social scientists needed to leave the mindset of the natural scientists behind; "the social professions were misled somewhere along the line into assuming they could be applied scientists—that they could solve problems in the ways scientists can solve their sorts of problems. The error has been a serious one" (Rittel and Webber 1973, p. 160). Social planners needed to realize that their problems were inherently different from problems in the natural sciences.

It was then, that the concept of mess entered the discussion of social science research methods. In his 1974 study *Redesigning the Future*, Russell Ackoff proposed the concept of "mess" as a term for a "system of problems"—urgent

12. In *Scenes of Inquiry*, Nicholas Jardine has argued for a problem- and question-centered approach in the study of science, but this is not quite what I have in mind. Jardine investigates "the ways in which new questions are brought into being and old ones dissolved" (Jardine 1991, p. 3); in other words, he shifts the analytic attention from processes of validation and justification to processes and contexts of knowledge generation. My suggestion, by contrast, is that we should understand messy problems as particular kinds of problems.

societal problems that needed to be addressed, such as "the race problem, the poverty problems, the urban problem, and the crime problem" (Ackoff 1974, p. 21). Independently, Rittel and Webber described these kinds of challenging social problems as "wicked" (Rittel and Webber 1973, p. 155). They argued that social planners dealt with inherently wicked problems, problems that have no unambiguous formulation and no unambiguous solution.

This is different from the notion that scientific knowledge is preliminary, fallible, and open to revision. The point that scientific knowledge is fallible is typically treated as a deep epistemological point that does not threaten our ability to act on our research outcomes. In practice, we could be reasonably certain that our results and theories, once agreed upon, are sound; at any rate solid enough to base further research and actions on them. The notion of wicked problem challenges this relative certainty. Reversing the arguments for division the 1970s, I propose that we utilize the notion of a wicked problem more in our analyses of current science. The notions of mess and wicked problems as they were introduced in the 1970s help us grasp more firmly what is special about large portions of today's science. Many problems in the natural sciences today are in fact not so different from those planners' problems—they, too, are inherently wicked.

Of course, science has always been entangled with society, and historians and sociologists of science have examined this entanglement for decades. They have shown that seventeenth century natural and experimental philosophers had to please wealthy patrons and to deal with the Church or that eighteenth century experimenters put up spectacular shows for the public and that pubs and clubs were venues for science, too. Nineteenth century researchers had to face critical and suspecting members of the public who questioned science and scientific research because they seemed unethical (like vivisection) or theologically presumptuous (like evolution) or pointless (like foundational physics for its own sake). Arguably, however, the problems that these researches dealt with were more easily recognizable as scientific problems—funded, encouraged, or restricted by society and with societal implications—than, say, stem cell research, gene editing, research on orphan diseases or stream restoration. Wicked problems are such that the difference between science and the public domain becomes elusive.

For Ackoff, messes have a number of common features.[13] The problems in the mess are interrelated; a mess does not inherently break down into simpler problems but different researchers will break down the situation

13. Ackoff acknowledges the pragmatist tradition, especially Dewey, as a source of inspiration.

differently; attempts to solve the simpler problems independently of one another will often intensify the mess; and problems and solutions are always in flux, "*problems do not stay solved*" (Ackoff 1974, p. 31). Last but not least, messes require cooperation from diverse stakeholders.

Rittel and Webber's characterization of mess overlaps with Ackoff's but is more detailed; they describe altogether ten properties of wicked problems.[14] Like Ackoff, Rittel and Webber emphasize that wicked problems are ill defined, never defined definitely, and "never solved. At best they are only re-solved-over and over again" (Rittel and Webber 1973, p. 160).[15] Because wicked problems are both unique and elusive, the assessment of solutions also presents a challenge: There is no immediate solution, yet little opportunity to learn by trial and error, as the problem keeps shifting. Like Ackoff, Rittel and Webber also stress that wicked problems do not exist in isolation; each can be "considered to be a symptom of another problem." Finally, they remind us of the ethical dimensions of social research, noting (with a sidelong glance to Popper) that the planner "has no right to be wrong" (p. 166).

The notion of "wicked problem" strikes me as a productive extension of the established conceptual inventory in science studies. Wicked problems can be usefully contrasted with Joan Fujimura's (1987) "doable problems". In addition, the notion of wicked problems is a useful complement to discussions in philosophy of science and cognitive science about heuristics, bounded rationality, and inductive risk.

Doable problems are hard, multifaceted problems, which require articulation and the organization of work. Fujimura introduces the notion of doable problems to draw attention to the broader contexts of scientific research: It takes more than just good instruments, techniques, and methods to solve a research task. She argues that "technology alone does not make problems doable" (Fujimura 1987, p. 258). The framework of doability assumes that it is an important part of scientific work to align the experiment, the laboratory, and the social world. A doable problem requires proper alignment of all three levels of organization. By putting the articulation tasks in the foreground of the analysis, Fujimura presents the scientist as the main agent in the nexus of organization; it is the scientist who orchestrates the alignment.

14. They called them wicked in an attempt to stay away from moral discussions, likely in response to West Churchman's brief reference to wicked problems as mischievous and evil (Churchman 1967). Rittel and Webber use the term "not because these properties are themselves ethically deplorable. We use the term 'wicked' in a meaning akin to that of 'malignant' (in contrast to 'benign') or 'vicious' (like a circle) or 'tricky' (like a leprechaun) or 'aggressive' (like a lion, in contrast to the docility of a lamb)" (Rittel and Webber 1973, p. 60).

15. Law's notion of fluid problem situations resonates with this idea.

Wicked problems, by contrast, are such that the scientist's agency is limited by other stakeholders outside the laboratory as well as by other agents who conceptualize the problem differently and have a different understanding of its overall significance. Moreover, wicked problems contrast with doable problems in that not only the problem definition but also the problem situation keeps shifting. Problem situations are fluid, diffuse, slippery, and changing.[16]

The concept of "wicked problem" also strikes me as a useful addition to recent discussions about inductive risk. In the last few years, analysts of science have plausibly argued that inductive risk is just one type of a broader set of phronetic risks (Biddle and Kukla 2017). The interpretation of certain problems as wicked captures another lingering impression from the inductive science literature, namely that these risks arise more often in some contexts than in others. It shifts the focus to the kinds of situations that put the scientists at phronetic risk.

Similarly, by changing the focus to the kinds of tasks that need to be tackled, the notion of wicked problem complements discussions about heuristics and complexity in science. In epistemological perspective, complexity has mainly been discussed in two ways: as a generative force (e.g., by Stuart Kauffman) and as a constitutive feature of biological and physical systems that necessitates heuristic strategies of reasoning (e.g., by Bill Wimsatt). Stuart Kauffman characterizes a creative strategy for approaching difficult problems, the "patch procedure." He assumes that a hard task can be divided into "a quilt of nonoverlapping patches," whereby the patches are then tackled separately (Kauffman 1995, p. 252). Like the theorists of social planning, he assumes that those patches are interdependent and that a solution in one patch will affect others. Yet the emphasis is different. Theorists of social planning worry about the extent to which complex issues can be actively managed. Kauffman's emphasis is on the generative power of the patchwork quilt. He argues that: "if the entire conflict-laden task is broken into the properly chosen patches, the coevolving system lies at a phase transition between order and chaos and rapidly finds very good solutions" (Kauffman 1995, p. 253). Accordingly, Kauffman has termed this domain the domain of the "adjacent possible," the expansion of what we already know by recombining it with novel elements from adjacent domains.[17] On this view, the emergence of novel knowledge is largely a contingent process. The

16. The distinction between complicated and fluid problem situations can be extracted from Law's work. The world is complicated, there are too many parameters; our knowledge remains partial and piecemeal, situated and indexed. Situations are fluid, diffuse, slippery, changing, while our accounts are static snapshots. There are thus two different reasons why the world exceeds our capacity to know it.

17. See Nowotny 2017, p. 55. Rheinberger describes this process as hybridization of experimental systems (Rheinberger 1997).

framework of wicked problems shifts the emphasis to the conditions, needs, and pressures that constrain the play of the patches and to the possibility that we may not be in a position properly to choose the patches. Because wicked problems are urgently in need of solutions, the framework of patching and local optimization—with unanticipated outcomes—strikes me as too optimistic to account for these problems. Kauffman assumes that the patches "coevolve to find excellent solutions" (Kauffman 1995, p. 267); the notion of wicked problem acknowledges that we cannot wait for the excellent solution to a problem that we may not even have specified correctly.

Wicked problems are complex in a way that resonates more with Bill Wimsatt's approach to complexity (which is itself informed by economic and social theorizing from the 1960s and 1970s). Like Rittel and Webber, and following Herbert Simon, Wimsatt emphasizes that complexity implies limited possibility of decomposition into component parts and a plurality of possible decompositions with no definitive way of choosing among them (Wimsatt 2007, pp. 179–186). The notion of wicked problem adds to the work on heuristics and bounded rationality because it emphasizes the fluidity of problems as well as the fact that many research problems nowadays concern diverse stakeholders and require multiple kinds of expertise.

Moreover, paying attention to wicked problems as the target of heuristic strategies can put the urgency of our research questions in focus. These problems must be solved on the basis of insufficient evidence. The inductive risk literature describes many situations in which we cannot afford to wait until we have reached even relative certainty. Heather Douglas's now classic study of research on the carcinogenic power of dioxin (Douglas 2000), which kicked off the recent revival of debates about inductive risks, discusses the intrinsic uncertainty of research on dose-response relations testing the potential harms of certain pollutants such as dioxin. We may be confident that if the scientists could only go on studying the issue, they, and we would end up with a reasonably certain assessment of the potential harms—but in the meantime people may have died from cancer or else the industries bearing the costs of the regulations have been negatively impacted. Along these lines, other contributions to the inductive science literature have emphasized the urgency of problems that force us to act before enough evidence is in.

The problem of climate change is a wicked problem in every sense discussed here.[18] Climate change is a highly complicated thing with a multitude of factors to monitor and measure, which generate a wealth of data and models that need to be understood, evaluated, and pooled. Patching

18. In fact, in policy contexts, climate change has been described as a super-wicked problem, precisely because of its urgency (Lazarus 2009; Levin et al. 2012).

does not seem to be a promising strategy both for pragmatic reasons (the different model outcomes need to be combined) and because the assessment of societal, political, and ethical implications of climate change requires input from people with very different skill sets and expertise. Food and water insecurity, damage to ecosystems, potential health impacts, and economic inequalities need to be taken into account in decisions about what to monitor and measure and whom to entrust with this task. Amplifying feedback of warming effects and tipping points whose impact on climate we cannot exactly calculate might change the entire problem situation. And, of course, as the current political discussions about climate change make painfully clear, different stakeholders have vastly different views about how to approach these decisions.

It is also illuminating to think of the CRISPR-Cas9 technology of genetic editing as wicked problem. On the one hand, it promises numerous scientific, technological, and medical applications including editing of disease-causing genes and producing resilient fruit crops. On the other hand, the technology is in itself not completely understood. Like climate change, CRISPR-Cas9 technologies raise numerous questions about risks and long-term effects of their application, ethical and political concerns about inequity and inequality of access to treatments, and so forth. More examples can easily be found,[19] but these two may suffice for the purposes of this paper.[20]

As these examples show, the concept of wicked problem cannot be strictly defined, and the boundaries between wicked and (merely) hard problems are not rigid. But defining the term is not the point. Rather, the properties or characteristics of wicked problems that Rittel and Webber identified serve asinstructive heuristic tools for the analysis of similar problems, and, I

19. See the literature cited at the beginning of this section, Horn and Weber (2007, p. 3), and recent studies of inductive risk for more examples.

20. I wrote this paper before the Covid-19 pandemic hit. It is instructive to think about the recent developments in terms of wicked problems. Finding the structure of the virus SARS CoV-2 is not a wicked problem. It appears that the task was not even particularly hard because the researchers could make use of established approaches and technologies (see Walls et al. 2020). Understanding and dealing with the pandemic, however, is a wicked problem. It is urgently in need of solution, yet we know little about the spread of the virus, the fatality rate, and effective protection. The problem situation changes—almost daily, it seems. Understanding the pandemic poses phronetic risks for scientists who need to consider the implications of the concepts they choose to report their findings, of the background assumptions they use in their models, and of the choices they make when they include or exclude certain data. Understanding and dealing with the pandemic requires multiple kinds of expertise and involves numerous stakeholders with vastly different interests—including vulnerable and not-so-vulnerable groups of the general population, national public health institutes, government leaders and agencies, educational institutions, businesses small and large, while global coordination of research and data management is needed.

contend, prove to be useful. By highlighting urgency, fluidity, the limits of decomposition, and the multiplicity of stakeholders, the conceptual inventory of "wicked" problems allows for an informative answer to the question of what is special about today's scientific inquiry.

9. Conclusion: Wicked Problems and the Philosopher's Predicament

Science can be, and has been, described as messy in many ways: there are fun activities for kids; tortuous research paths; ad hoc, opportunistic practices; fluid, hybrid, complicated objects; ill-defined and complex research problems, and sometimes scientific work creates environmental hazards. Science can be, and has been called messy for many purposes: to undermine its authority, to unmask and weaken the normative power of philosophical frameworks, to distinguish processes of knowledge generation from their presentation, and to celebrate creative genius. It is sobering to remind ourselves of the diverse and even opposing rhetorical functions of messy science talk. As a diagnostic tool, by contrast, the notions of mess and messy science are gratuitous; sometimes they obscure rather than illuminate our understanding of scientific practice.

The concept of "wicked problem" has the surplus value that messy science talk lacks. Not only does it pick out features of current science that are unprecedented, but it also comes with additional conceptual tools that are helpful for its analysis. Wicked problems share a number of features: They are not just hard—i.e., difficult to answer—but we are not even sure how to ask the right question. Wicked problems are not inherently divided into simpler, smaller problems. Taking a piecemeal approach to these problems is typically not an option. Wicked problems concern numerous groups of stakeholders with multiple interests and require input from experts with diverse kinds of expertise, whereby these groups will often frame the problem differently. Wicked problems are urgent, which means that they need to be solved before enough evidence is in; we cannot wait for a "play of patches" to unfold. Wicked problems are fluid; the problem situation changes before we have found a satisfactory solution to the problem.

Earlier I described some of the concerns about messy science as a repetition of old discussions about the scope and normative power of philosophy of science. We could characterize these discussions as boundary work (Gieryn 1983) because they involve negotiations about disciplinary authority: At stake is who and what belongs to a certain field or does not belong; who is, and who is not, allowed to publish in philosophy journals, present at certain conferences, and so forth. If we think of the negotiations as an attempt to solve a wicked problem, it is possible to give a richer account of what is at stake in this boundary work.

It is by no means clear how we, as analysts, should deal with scientific research problems that are wicked in the sense outlined above. Which strategies or analytic approaches are open to us as we are dealing with a wicked problem? Should analysts try to insert themselves into wicked problem situations as additional stakeholders with unique expertise? Are philosophers of science in a position to determine what properly ordered science is and how it can or should be implemented? Would the epistemic ideals that philosophers are so fond of defending be genuinely helpful to those dealing with wicked problems and trying to find workable solutions? After all, the nature of wicked problems is such that the unrestrained process of clarification, more clarification, and even further clarification of concepts, theories, or standards of evidence, the compartmentalization of the issues, etc. appears futile or even counterproductive. Should we, as analysts, just stand back and try to understand for its own sake what science is and what scientists do, rather than intervene in the process?

Those questions can be considered wicked because they cannot be tackled in isolation, none of them can ever be solved; the target (science) is elusive and ever-changing; and the discussions about science engage different stakeholders, including science analysts and scientists from different fields and at different career stages, scientists, broader publics, and university administrators. These stakeholders will have very different ideas about the nature, task and scope of philosophy of science. Admittedly, the wicked problem of how best to analyze science is a rather modest specimen of its kind, as it is far less urgent than those wicked problems I considered earlier. It is relevant to our careers and scholarly pursuits, but the fate of humanity does not depend on its solution.

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