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Defending a Risk Account of Scientific Objectivity

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Abstract

When discussing scientific objectivity, many philosophers of science have recently focused on accounts that can be applied in practice when assessing the objectivity of something. It has become clear that in different contexts, objectivity is realized in different ways, and the many senses of objectivity recognized in the recent literature seem to be conceptually distinct. I argue that these diverse 'applicable' senses of scientific objectivity have more in common than has thus far been recognized. I combine arguments from philosophical discussions of trust, from negative accounts of objectivity, and from the recent literature on epistemic risks. When we call *X* objective, we endorse it: we say that we rely on *X*, and that others should do so too. But the word 'objective' is reserved for a specific type of reliance: it is based on the belief that important epistemic risks arising from our imperfections as epistemic agents have been effectively averted. All the positive senses of objectivity identify either some risk of this type, or some efficient strategy for averting one or more such risks.

1. Introduction

In this article I defend a risk account of scientific objectivity. The account is meant to be a largely descriptive or even a semantic one; my aim is to draw together ideas presented in recent discussions, and to clarify what we philosophers of science do when we identify distinct, applicable senses of objectivity or call something objective. However, the article also has a prescriptive side to it. I believe objectivity to be a useful notion for philosophy of science, and I will argue that the risk account shows why and how this is the case.

When discussing scientific objectivity, many philosophers of science have recently focused on accounts that could be used in practice when assessing the objectivity of something. Particularly the critique of one influential account, the value-free ideal, has led to the recognition of several other senses of scientific objectivity (see e.g. Longino [1990], [2001]; Janack [2002]; Douglas [2004]; Tsou, Richardson and Padovani [2015]; Axtell [2016]). For instance, we may call a result objective when a research community agrees on it; or we may call a method objective when it produces the same results regardless of who uses it. As Heather Douglas ([2004]; [2009]) has noted, if one wants to avoid certain strong metaphysical or epistemological commitments, the recognized senses seem to be conceptually distinct. However, all these senses of objectivity have normative force; they are not just descriptions of ways in which people use the words 'objectivity' and 'objective', but identify instances where we seem to be justified in calling something objective.

My aim is to show that the diverse 'applicable' senses of objectivity recognized in the recent literature do in fact have more in common than has as yet been recognized. I propose an account that brings some unity to the discussion: it covers all these senses of objectivity, and clarifies why they have normative force. I argue that when we call *X* objective, we endorse it: we say that we rely on *X*, and that others should do so too. But the word 'objective' is reserved for a specific type of reliance: it is based on the belief that important epistemic risks arising from our imperfections as epistemic agents have been effectively averted.

I will start by discussing the diverse applicable and often contextual senses of scientific objectivity recognized in the recent literature. I then proceed to examine a suggestion made by Arthur Fine ([1998]), and Heather Douglas ([2004], [2009]) among others, namely that all the different senses of objectivity indicate a shared basis for trust. This idea is based on an important intuition, but it needs to be refined. If we distinguish between trust and reliance by saying that trust can be betrayed or let down, whereas reliance can only be disappointed (Baier [1986]; see also Hardwig [1991]), it becomes clear that when trying to identify what the different senses of objectivity have in common, we should talk about reliance instead of trust.

I then examine negative accounts of objectivity, in which objectivity is taken to mark the absence of something—notably some form of subjectivity (Daston and Galison [2007]). By combining such negative accounts with the idea of epistemic risks (Biddle and Kukla

[2017]; Biddle [forthcoming]), I argue that the different positive senses of objectivity identify either an epistemic risk arising from our imperfections as epistemic agents, or some efficient strategy for averting one or several such risks.

I then proceed to defend the risk account of scientific objectivity. I argue that it clarifies why the different, applicable senses of objectivity are often related to some specific context, and helps us to analyse what happens when people disagree about the objectivity of something. Before concluding, I present two examples of contexts where the account can be fruitful: it helps us to re-examine the question of objectivity in humanities research and in the social sciences, and it helps us to distinguish between two basic ways in which the democratisation of scientific knowledge production may threaten its objectivity.

2. Applicable Notions of Objectivity

The ideal of objectivity is often considered to be a cornerstone of scientific inquiry, one on which the epistemic authority of science rests. At the same time, it appears to many as an unattainable goal. The modern notion of objectivity has ofen been taken to contain the idea that objective knowledge is knowledge about the object, untainted by distortions caused by our subjectivity. In other words, an objective scientific claim or theory would tell us correctly about the objects in the world—it would be 'faithful to facts' (Reiss and Sprenger [2016]). For many reasons, philosophers have doubted the possibility of us securing such knowledge. For instance, one can follow Duhem and Quine and emphasize the empirical underdetermination of scientific theories—or choose a Nietzschean or Foucauldian approach and stress how the indissoluble links between knowledge and power taint science. Despite such fundamental doubts, the notions of objectivity and objective knowledge have persisted. They are central in many important debates not only in the philosophy of science, but also in epistemology, metaphysics, and even ethics.

In line with the ideas embedded in the modern notion, objectivity has often been taken to have both an ontological and an epistemological aspect: an objective knowledge claim would tell about the objective world existing independently of human observers; the reality behind the phenomena we perceive (Fine [1998]; Reiss [2014]; Axtell [2016]). This duality is visible in the continental tradition, where discussions of objectivity in science have often focused on the ways in which we construct our objects of study (see e.g. Megill [1994]). And it is clear in the analytical tradition, where some pronounced realists have claimed that the ontological aspect of objectivity is needed for the epistemic one to make sense (Nagel [1998]). The conclusion remains the same: we can never be certain that our results really capture what we hope they would. We seem to have no certain access to the 'Really Real', as Elizabeth Lloyd ([1995]) has termed this elusive goal. In fact, objectivity has for a long time been an issue because we cannot be sure that our claims and theories are objective in this sense.

In contemporary philosophy of science, objectivity is the subject of a lively discussion that has little to do with the fundamental doubts described above. The ontological question has been mostly set aside. Instead, the focus is on developing a less ambitious, but more usable and sustainable account of objectivity as an epistemically normative notion—or several such accounts, if necessary (Douglas [2004]; Tsou, Richardson and Padovani [2015]; Axtell [2016]). What is wanted is an account that we can actually apply when trying to determine the objectivity of something.

Such an account has quite a different set of desiderata than the ideal notion that also encompasses the ontological aspect. An applicable notion of scientific objectivity should not imply that the results of objective research would be certain, as we need an account that allows us to be fallible. Otherwise it would not be usable. Such an account should not claim that we can have objective knowledge only of objects that exist independently of human beings—we must be able to assess the objectivity of knowledge claims made about constructed phenomena dependent on human conceptualisations. Moreover, such an account should not represent objectivity as an on–off feature—objectivity has to be a degree concept. We must be able to say that something is more objective than something else without claiming either to be perfectly objective, and we need to be able to say that doing something increases or decreases the objectivity of our work. As many philosophers of science have tried to develop such accounts, they have come up with several different situations in which—so they claim—we are justified in calling something objective. I am attempting to develop an account that accounts for them all.

The recent discussions of objectivity have often been based on critiques of an influential account of objectivity: the value-free ideal. According to this ideal, non-epistemic values must not influence the gathering of evidence or the acceptance of scientific theories. The account seems to be practicable; it has often been used when assessing the objectivity of research. However, as noted, it has been heavily criticized. Feminist philosophers of science have long argued that non-epistemic value judgements can be necessary in scientific reasoning (e.g. Keller [1985]; Harding [1986]; Haraway [1988]). As Helen Longino ([1990], [2001]) has stressed, we cannot ensure that the background assumptions we base our research on are value-free, and research without background assumptions is impossible. So the value-free ideal is unattainable. And to avoid a situation where a whole community of researchers takes a certain set of values so much for granted that they do not realize how it distorts their results, research communities should foster diverse values (Haraway [1989]; Longino [1990], [2001]). Longino ([1996]) and Phyllis Rooney ([1992]), among others, have also questioned the distinction between epistemic and non-epistemic values. According to them, it is not as clear as we would like to think, and neither is the value-free ideal. Moreover, using value-laden concepts may be unavoidable in many fields of study (e.g. Dupré [2007]). In recent discussions, it is particularly the argument from inductive risk that has convinced many philosophers of science that non-epistemic values have an important, legitimate role to play at all stages of scientific research. Inductive risk is the risk of wrongly accepting or rejecting a hypothesis: it is a risk a researcher takes when

deciding to make the inductive leap from evidence to either accepting or rejecting a hypothesis. As many philosophers of science have argued, non-epistemic values must often influence such decisions, as the consequences of being wrong must be weighed when determining whether the evidence is strong enough or not. (Rudner [1953]; Hempel [1965]; Douglas [2000], [2009].)

Most of what has been said recently about objectivity in philosophy of science is related to the discussion of values in science. What is particularly noteworthy here is that it has lead philosophers of science to recognize many distinct, applicable senses of objectivity that do not rest on the value-free ideal. When we call *X* objective, *X* can be, for instance, a researcher, research process, knowledge claim, or research community. And the ways in which researchers attempt to ensure the objectivity of some *X* differ vastly depending on the field or discipline in question. Also, we philosophers of science use the adjective 'objective' in many different ways, which often tend to be tied to some specific context. Marianne Janack ([2002]) has recognized thirteen conceptually distinct senses of objectivity used in contemporary philosophy of science, and Douglas ([2004], [2009]) distinguishes between at least eight senses of process objectivity alone. She divides these senses into three modes: in the first one the focus is on the researchers' interactions with the world, in the second on their individual thought processes, and in the third on social processes. Most of the senses recognized by Janack and all of the ones recognized by Douglas are applicable: they can be used when assessing the objectivity of some *X* in some context. For instance, in convergent objectivity we call objective a result we can reach through many independent avenues. In procedural objectivity the research process has been so designed that a researcher can always be replaced by another, and that will not change the result. And in interactive objectivity a research community follows practices that ensure effective critical discussions and debates (Douglas [2004], [2009]).

This leads us to the problem to which I am attempting to offer a solution. The different applicable senses or notions of objectivity seem to be conceptually distinct (Douglas [2004]; Daston and Galison [2007]; Tsou, Richardson and Padovani [2015]; Axtell [2016]). They can complement each other, but they can also be set against each other: two people disagreeing about the objectivity of some *X* often refer to different senses of objectivity. According to Douglas ([2004]; [2009]), the notion of scientific objectivity is 'irreducibly' or 'inherently' complex: 'None of these meanings is logically reducible to other meanings (unless one is willing to make rather strong metaphysical or epistemological presumptions). While there are no reducible meanings here, neither are the senses unconnected.' (Douglas [2004], p. 465.) In other words, when the ontological aspect of objectivity is set aside, the notion seems to lose its unity. If one does not want to claim that the different senses of objectivity all ultimately follow from or guarantee faithfulness to facts, one is left with an irreducibly complex notion. Nevertheless, as Douglas ([2004]) notes, it still seems to be one notion, not several, and when people assess the objectivity of some *X*, they often implicitly refer to several different senses simultaneously.

In her own answer to the problem, Douglas follows Arthur Fine ([1998]) and suggests that in all cases objectivity indicates 'a shared basis for trust in a claim' (Douglas [2009], p. 123): 'Common to all the uses of objectivity is this sense of strong trust and persuasive endorsement, this claim of 'I trust this, and you should too.' It is this commonality that underlies the usage of objectivity in its various guises.' (Douglas [2009], p. 116.) However, I believe that there is more to say than this: by combining ideas presented in philosophical discussions about trust and reliance, the negative senses of objectivity, and epistemic risks, it is possible to offer a more precise account that saves the unity of the applicable notion of scientific objectivity.

3. Reliance Instead of Trust

The intuition that objectivity is somehow linked to trust is shared by many, and understandably so. However, I would like to argue that it is a shared basis for reliance, not trust, that we should talk about when trying to identify what the applicable senses of objectivity have in common: when we who use any of those senses call something objective, we state that we rely on it, and that others can safely do so too. This change will help us to distinguish between situations in which our assessment is based simply on trust, and situations in which we rely on more than mere trust.

According to Fine, objectivity is 'that which in the process of inquiry makes for trust in the outcome of inquiry' (Fine [1998], p. 127). Naomi Scheman ([2001]) also links objectivity to trust, or rather trustworthiness: according to her, when we call a claim objective, we present it as scientifically authoritative and say that others too should accept it. As most people are unable to verify scientific claims independently, and many may have fully rational reasons for mistrusting scientists, they must be given the opportunity to recognize scientists as trustworthy. Researchers must offer them a real chance to have rationally grounded trust in science.

Both Fine and Scheman talk explicitly about larger audiences: it is laypeople who should be able to trust the outcomes of science. Moreover, unlike Fine and Douglas, Scheman makes it explicit that trust is needed because the laypeople who should be able to accept something as objective cannot always independently verify that it is indeed worthy of being accepted. Their acceptance can be based only on more or less rationally grounded trust in the researchers. Douglas, on the other hand, is interested in the different senses of objectivity identified by philosophers—so when she talks about a shared basis for trust, she is mainly interested in the viewpoint of the person making the assessment that something is objective, and in expert audiences capable of assessing the claim.

Now if a lay community's rationally grounded trust in *X* increases, from the point of view of an expert it is not necessarily the case that the objectivity of *X* would have increased. Let us imagine a research process that has been designed so that a researcher can always be replaced by another, and that will not change the result. This makes the process objective in the procedural sense. Researchers using the process then initiate a systematic campaign of science communication in order to increase transparency, make

the process understandable, and thus gain the rationally grounded trust of lay audiences. If they succeed in their aim, has the objectivity of the process increased? Members of the lay audience may indeed be more capable of making an informed assessment than they were prior to the campaign. But from the point of view of the expert, the objectivity of the process does not seem to have changed, as the reasons for relying on it have not changed.

We can distinguish between trust and reliance by saying that trust can be betrayed or let down, whereas reliance cannot: it can only be disappointed (Baier [1986]; McLeod [2015]).[1] This makes trust into something that we have towards people, and perhaps groups or communities, but not processes or results. We can rely on a process, but as a process cannot betray us, we cannot trust it.

Trust is needed in science, as researchers cannot always verify their colleagues' results, but have to simply trust them (Hardwig [1991]; see also Rolin [forthcoming]). So trust may also be needed when we accept someone's assessment that *X* is objective—or when our assessment that *X* is objective rests on our confidence that a researcher, research group or a community does indeed follow some specific practices. However, in some other cases such an assessment can be based, for instance, on our thorough understanding of the workings of an equipment: we believe that we can rely on it. In other words, our reliance on *X* can be based on trust, but especially in the case of expert assessments, it can be based on other things too. So objectivity indicates a shared basis for reliance, rather than trust.

But even if we replace trust with reliance, we do not yet have satisfactory means of identifying what the different senses of objectivity have in common. As Julian Reiss and Jan Sprenger ([2016]) note, objectivity cannot be identified with features that promote trust in science, as trust can be misguided. The same applies to reliance: this kind of 'instrumentalism' about scientific objectivity would be a difficult stance to sustain. For example, as Kristina Rolin ([2002]) observes, when assessing experts, both lay audiences and researchers can err in questionable ways, for instance by categorically mistrusting women as experts. Similarly, we may rely on a knowledge claim, a theory, a process, etc., for epistemically dubious reasons—think of craniology.

Of course no one has suggested that felt trust be used as an indicator of objectivity. However, the observation by Reiss and Sprenger does point out a problem: Janack and Douglas have identified several shared bases for trust—or rather reliance—that seem to justify the assessment that *X* is objective. We have good reasons to rely on *X*. But we can also rely on something without having good reasons for doing so. Do the good reasons identified by Janack and Douglas have something in common? I believe they do. This becomes clear when we approach objectivity as a negative term.

[1] I use the notion of reliance here without endorsing any reliabilist theory of knowledge. I wish to present an account of scientific objectivity that is compatible with several different theories of knowledge.

4. Epistemic Risks Arising from Our Imperfections as Epistemic Agents—Instead of Subjectivity

Lorraine Daston and Peter Galison ([2007]) have noted that when new senses of objectivity have emerged in science, they have been related to newly recognized forms of 'scientific subjectivity' and epistemic worries arising from these. For instance, at the end of the 19th century the development of photography and microscopes revealed that researchers had been inadvertently idealising the objects they were supposed to observe—a drop of water was not as perfectly symmetrical as they had supposed. Daston and Galison conclude that the normativity of the notion of scientific objectivity has historically been based on the duty of scientists to avoid some form or forms of subjectivity.[2]

However, it may be misleading to talk about subjectivity here. Historically, such newly recognized worries have indeed often been closely linked to errors made by individual researchers, and thus to subjectivity. More recently, however, it has become clear that the lack of objectivity does not necessarily originate from us as individual subjects. Collective biases (e.g. Longino [1990], [2001]) and cognitive biases typical to us as human beings have also been getting attention recently. Moreover, as many accept that non-epistemic values have legitimate roles in science, banishing subjectivity entirely seems problematic as an aim, as those values are necessarily somewhat subjective (Douglas [2004]). So philosophers of science now often use 'biased' rather than 'subjective' as the opposite of 'objective' (e.g. Jukola [2015]). Ian Hacking ([2015]) talks simply of epistemic vices. Following Daston's and Galison's lead, he holds 'objective' to be an entirely negative adjective: it marks the absence of this or that vice. I too agree that the unity of the applicable notion of objectivity is found in the negative side of the notion. However, I would like to talk about epistemic risks rather than vices, and to point out that not all epistemic risks count.

Let us return to the different applicable senses of objectivity Janack and Douglas have described. In each case they mention or clearly imply a threat or several threats that are being averted; a contrast or contrasts to objectivity. These contrasts include illusions, subjectivity, idiosyncrasies, and collective biases. I think it is unnecessarily harsh to call something like being fooled by an illusion a vice. We human beings are in many ways imperfect as epistemic agents. For instance, we are prone to be fooled by certain types of illusion. I prefer to say that the different contrasts to objectivity are all what Justin B. Biddle and Rebecca Kukla call epistemic risks.

An epistemic risk, in the broad sense of the term Biddle and Kukla endorse, is 'any risk of epistemic error that arises anywhere during knowledge practices' (Biddle and Kukla [2017], p. 218). They start their treatment of this notion from the discussion of inductive risks that was mentioned earlier, and note that though inductive risk is an

[2] This negative approach answers Fine's ([1998]) claim that inferring the objectivity of a result from the objectivity of a research process would be an example of the process–product fallacy. If the process is designed so as to avoid some specific form of 'scientific subjectivity', it is not a fallacy to conclude that the product is not distorted by that form of subjectivity.

important epistemic risk, it is far from the only one researchers have to cope with. They distinguish several different types of epistemic risks, starting from the risk of having mistaken beliefs, or 'alethic risk', and the risk of simple reasoning error, or 'analytic risk', and ending with complex 'phronetic' risks that must bee managed in light of values and interests—for instance, risks related to operationalisation, concept formation, or model choice. Scientists face phronetic risks when doing things that are preconditions for empirical reasoning, or a necessary part of it. Inductive risk is one of these, but as Biddle and Kukla argue, not the only one. It is not possible for scientists to proceed in their work without continually facing and mitigating diverse epistemic risks. As there is a large variety of such risks—Biddle and Kukla's typology is not meant to be exhaustive—the strategies for managing them also vary. (Biddle and Kukla [2017]; Biddle [forthcoming].)

The risks that interest us here belong to a specific subset of epistemic risks. We start talking about objectivity only when facing certain kinds of risks. For instance, when the results of an experiment are incorrect because of malfunctioning equipment, we do not worry about objectivity—we just say that the results should not be taken into account. So epistemic risks arising from the imperfections of a machine usually do not induce us to talk about objectivity. Neither do occasional errors of reasoning: only recurrent errors can count as threats to objectivity, particularly ones arising from cognitive biases typical to us as human beings. So only some, but not all analytic risks count. Alethic risks can count if it is due to our own failings that we hold mistaken beliefs. Likewise, of the epistemic risks related to observation, only those count that arise from our deficiencies as observers. And many, perhaps even most phronetic risks seem to be similarly related to potential worries about objectivity. For instance, one of the most important epistemic risks related to concept formation is that the concepts may, unbeknown to us, end up reflecting some bias of ours.

So it is only when the epistemic risk is related to our own failings, and is hard to avert, that we start talking about objectivity. Illusions, subjectivity, idiosyncrasies, and collective biases are important epistemic risks arising from our imperfections as epistemic agents. By talking about these epistemic risks we can give a more precise account of the basic idea of Daston and Galison, while avoiding the problems related to the notion of subjectivity.

5. The Risk Account of Scientific Objectivity

So I would like to suggest the following: When we who use any of the applicable senses of objectivity call *X* objective, we endorse it: we say that we rely on *X*, and that others should do so too. But the word 'objective' is reserved for a specific type of reliance: it is based on the belief that important epistemic risks arising from our imperfections as epistemic agents have been effectively averted.

For example, when we say that a research community is objective in the interactive sense, we state that we rely on the community, and that others have good reason to rely

on it too, as it follows practices that ensure effective critical discussions and debates—which we take to be an efficient strategy for averting many individual and collective biases. Or when say that we do not doubt the objectivity of a result because we can reach it using different research approaches independent of each other, what we are in fact claiming is that this is an efficient strategy for ensuring that the result is not just a fantasy, or an illusion created by our theoretical assumptions. And when we call a method objective in the procedural sense, we state that it has been designed in a way that screens out the possibility of individual biases or idiosyncrasies distorting the results. The different senses of objectivity have normative force in so far as they offer good reasons for relying on the *X* that is called objective. All the different applicable positive senses of objectivity do this by making an essentially negative claim. They identify epistemic risks arising from our imperfections as epistemic agents, and/or effective strategies for averting them.

The risk account chimes in with the ways in which we use the adjective 'objective'. As noted, malfunctioning equipment does not induce us to talk about objectivity. But if we refuse to take the malfunction into account even after becoming aware of it, and continue using the results, questions about objectivity may arise. Or if we design software that repeats some widespread bias (e.g. facial-recognition software might have racial bias problems because of the sets of images used in the training of the algorithms), we may talk about objectivity—precisely because it is our bias that the software is repeating. Or if archaeological data from an area is first interpreted in a certain way, and then newly discovered data make it clear that the interpretation cannot be correct, we do not talk about objectivity; we just say that the former interpretation was wrong. But if archaeologists do not pay attention to the new data and continue to endorse the old interpretation, then questions about objectivity do arise. So objectivity is related to our failings as epistemic agents.[3]

We can now spell out several issues related to the applicable notion of objectivity. The account helps us to understand why the different, applicable senses of objectivity are often related to some specific context, and why objectivity does not imply certainty. It also allows comparisons, making objectivity a degree concept. Moreover, it helps us to analyse what happens when people disagree about the objectivity of something, and

[3] I have just claimed that when a machine malfunctions, we do not talk about objectivity. However, the current rapid progress in the development of AI, and particularly machine learning, may change the situation. As AI systems are used increasingly in science, and as the ways in which AI works are not transparent to humans, an AI system may gain the role of an epistemic agent in scientific research. Algorithmic AI systems, or any other AI systems, do not constitute perfect epistemic agents: it is not always possible to detect why a particular trained model is effective in completing its task, and such a model may also fail in unpredictable ways (see e.g. NSTC [2016]). This may bring about new, unforeseen ways of being imperfect as an epistemic agent—and, as a result, new epistemic risks. It remains to be seen whether the notion of objectivity will be applied to such cases, or whether we continue focusing on individual and collective human agents. This does not in any substantial sense threaten the risk account, but it may either bring about new senses of objectivity, or require a small alteration in the account. Either we accept AI systems as agents that may be sources of bias, and begin assessing their objectivity, or the account should be changed so that it only covers the imperfections of human agents.

even makes it clear why such disagreements can be rational. And finally, it brings unity to the notion.

The risk account clarifies why objectivity is a contextual matter. First, we are imperfect as epistemic agents in many ways: we are prone to wishful thinking; we can be fooled by illusions; as communities we may share ideas and values without even realising we do so; et cetera. Our imperfections lead to many different kinds of epistemic risks. It is plausible that some of them are particularly pressing in some contexts, other ones in others. So what the important risks are depends on the context. Secondly, the account separates strategies developed for averting some risk from the risk itself. There is no need to think that a single risk could not be averted in several different ways. Some are more appropriate in some contexts, others in others.

The risk account also makes it clear why calling *X*, for example a method, objective does not imply that the results of research would be certain. Something unrelated to objectivity (e.g. malfunctioning equipment) may skew the results. Or the strategy used for averting some risk may fail, or it may not be as efficient as we believe it to be. Or there may be risks we have not identified. Or we may later conclude that the risk was not as important as we thought, so averting it did not significantly increase the overall objectivity of *X*. Daston and Galison ([2007]) describe how newly recognized epistemic risks have led to new conceptions of objectivity—but also how older senses of objectivity have dissolved or lost their salience as new regimes have gained importance.

The risk account allows for comparisons: it is possible to say that *X* is more or less objective than *Y* without claiming that either is perfectly objective. We may deem the strategies used for averting epistemic risks more or less reliable, and one may be able to avert one recognized epistemic risk or several. So objectivity is a degree concept. However, comparing the objectivity of *X* and *Y* in two different contexts is at the very least difficult. To do so one would have to recognize two sets of important risks arising from our imperfections as epistemic agents: one set for *X* in one context, and another for *Y* in another. Then one should assess how well the risks are averted in each case, and finally compare these assessments. If both the sets of important risks and the strategies used to avert them are very different, the comparison can easily become meaningless. However, sometimes even such comparisons are possible—for example, when comparing a young research programme with a well-established one. The latter will most likely have approved strategies for averting the epistemic risks deemed important in its context, whereas the former will not yet have had the time to develop its strategies.

As noted, when assessing the objectivity of some *X*, people quite often disagree. We can now identify some components of such disagreements, and recognize how value judgements influence them.

What we take to be an important risk depends on different things in different contexts. The threshold after which we are satisfied that the risk has been averted effectively

enough depends on the context too. Both epistemic and non-epistemic values influence such assessments, which can lead to disagreements.

First, as Thomas Kuhn ([1977]) argued, individuals may legitimately differ about the application of different epistemic values to concrete cases. For instance, two scientists can disagree whether to value the accuracy of a theory over its scope in some case, or its consistency over fruitfulness. Such disagreements can lead to differing assessments of the most important epistemic risks to be averted in that case: one of the scientists would focus particularly on such risks arising from our imperfections as epistemic agents that threaten the accuracy and consistency of the theory; the other would be more worried about risks that somehow threaten its scope and fruitfulness. Now there may be unavoidable trade-offs between two incompatible strategies, each of which would be particularly good for averting some specific risk. So one has to decide which risks to prioritize. In other words, if scientists can rationally disagree about the application of epistemic values in some context, they can also rationally disagree about the objectivity of some *X* in some context.

Secondly, as has been made clear in the discussion of inductive risk and epistemic risks, the aims of a research project or programme, or the anticipated context of use of the results, must often influence our assessments of the importance of some risk, and of the threshold after which we are satisfied that the risk has been averted effectively enough. In some contexts, non-epistemic values must play an important role in such assessments. (Rudner [1953]; Douglas [2000]; [2009]; Biddle and Kukla [2017]; Biddle [forthcoming].) In policy-relevant research, for instance, non-epistemic values must often be taken into account when determining epistemic risks that have to be avoided particularly carefully—and different sets of values will lead to differing assessments. In other contexts, such as some forms of basic research without any foreseeable applications, both the important risks and the threshold after which we are satisfied may be determined on more purely epistemic grounds.

So a dispute about the objectivity of some *X* in some context can be based on differing assessments of what the important risks are that must be averted in that context, or of the efficiency of the strategies used, or of the threshold after which we should deem a strategy effective enough. For example, an archaeologist could consider the results of a research project objective because the methods used are objective in the procedural sense: the results stay the same even if different researchers repeat the process. However, other archaeologists could disagree because they believe that ethnocentric bias embedded in the background assumptions of the methods can make researchers blind to the existence of some important evidence or interpretive resources. So before this threat is averted, one should not rely on the results of the project, nor an overall picture of the research subject drawn from them (see Wylie [2015]). In such a case the disagreement stems from different assessments of what are the important risks in the context of that archaeological project. The strategy the first archaeologist finds satisfying will avert individual biases and idiosyncrasies. But it is not efficient against collective bias.

So the risk account clarifies several issues related to the applicable senses of objectivity. It also brings unity to the notion. As Douglas ([2004], [2009]) points out, none of the recognized positive senses of objectivity is reducible to the others. This becomes quite understandable when we realize that each sense identifies either some specific risk, or an efficient strategy for averting some important epistemic risk or several risks arising from our imperfections as epistemic agents. Many different epistemic risks arise from our imperfections as epistemic agents; for instance, our work can be too subjective, or we can succumb to collective bias. In fact, every new insight about the ways in which we are imperfect as epistemic agents can bring forth new senses of objectivity. Moreover, even a single risk can be averted in several different ways. For example, we can try to ensure that an object posited by a theory is not just an illusion by using several independent methods of observation, as in convergent objectivity, or by using the object as a tool, as in manipulable objectivity (Douglas [2004]). As our failings as epistemic agents give rise to several epistemic risks that can be deemed important in some context, the number of distinct senses of objectivity is bound to be large.

6. Is This Useful?

My aim has been to produce an account that covers the diverse, applicable senses of scientific objectivity recognized in the recent literature. The result is an account that links objectivity strongly to reliance, imperfect epistemic agents, and epistemic risks. This raises two interrelated questions. First, is this kind of notion of objectivity a necessary one at all—or are the different senses of objectivity discussed here ultimately redundant? Secondly, even if the different senses of objectivity are worth defending, does this account add anything to their applicability and usefulness? After all, what is wanted in the literature I am discussing is an applicable notion with normative force. The account clarifies why the different senses of objectivity have normative force, but is it itself useful in any of the diverse contexts where we would actually want to assess the objectivity of something?

Several philosophers have suggested that we could simply drop the notion of objectivity, because it is too muddled to be of any use. For instance, Hacking ([2015]) warns us against using the noun 'objectivity', as he takes it to be an unnecessary elevator word. As philosophers of science, we should concentrate on being of some use in discussions that aim to assess whether some *X* in some specific context is objective or not, and for that purpose we only need the adjective, not the noun that only philosophers tend to use.

This may be quite right if one uses a notion of objectivity that combines the epistemic aspect of objectivity with the ontological one. However, I do not think Hacking's criticism applies to the applicable senses of objectivity recognized in the recent literature and discussed here. Their meanings are restricted enough for them to be genuinely useful in the epistemic assessment of *X* in diverse contexts. Moreover,

philosophers should be allowed to have etic notions, and *objectivity* can be a valuable one.

Or can it? The senses of objectivity the risk account covers offer reasons for relying on research processes, communities, methods, etc., and as Douglas ([2004], [2009]) notes, ultimately on the produced knowledge claims. So, to quote Sandra Harding, do they add anything 'to assessments of the truth, 'verisimilitude,' reliability or productive power of research results'? (Harding [2015], p. xii.)

If we accept the risk account, objectivity is a weaker notion than truth or verisimilitude: when we call a claim objective, we do not necessarily contend that it is true, or even claim that it appears to be true or real. We only claim that we have very good reasons to rely on it.[4] Calling *X* objective also differs from claiming that it has productive power—an objective knowledge claim can easily be unimportant, or an objective process redundant. Even if we take *X* to be objective, we may conclude that we have no need for it, or do not wish to use it. So it remains to show that even if objectivity is closely related to reliance, it is a useful notion in its own right. This I find unproblematic: it is good to have an evaluative notion that focuses on the epistemic risks arising from our imperfections as epistemic agents.[5]

But even if the different senses of objectivity are useful, does the same apply to the risk account itself? Can it be used for anything besides describing what the different senses have in common? In order to illustrate that it can, I will now briefly describe two contexts where I believe the account can be applied in fruitful ways. I will first address the old and well-known question of objectivity in humanities research. Then I will turn to the new question of objectivity in the context of the democratisation of scientific knowledge production.

Doubts about the possibility of objectivity in the humanities and in the social sciences using qualitative methods have usually arisen from comparisons to the natural sciences. The risk account allows us to distinguish between two ways in which such comparisons can be misleading.

First, one may identify objectivity with specific strategies for averting epistemic risks, ones that have been developed in the natural sciences—such as controlled experiments. This would obviously be a mistake. Although those strategies are not usually available, for instance, in historiography, because of the nature of the research subject, it may be possible to avert the same risks using other strategies.

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[4] Similarly, calling something objective is a stronger statement than calling it justified. Some craniologists might have been justified in believing what they did, but when assessing the objectivity of their work, we assess it from our perspective, and find it unsatisfactory.

[5] I should note that there is no conflict between Harding's ([1986]) own take on objectivity and the risk account. The 'strong objectivity' sense of objectivity identifies epistemic risks arising from our imperfections as epistemic agents: if knowledge production is exclusively in the hands of socially dominant groups, researchers are easily blinded to relations of power and the ways in which these relations can distort the knowledge produced. Such risks are particularly important in contexts where the prospective use of the research results will affect the lives of socially marginal groups. In order to avert the risks, their viewpoints should be taken into account in the knowledge production.

A second, more important error is that of focusing solely on those epistemic risks that have been found important in the natural sciences and thus received attention there. As Daston and Galison ([2007]) describe, the history of the most influential senses of objectivity in the natural sciences is based on a set of different conceptions of subjectivity. All of these conceptions focus on *individual* subjects. And the same is often the case in discussions about the possibility or impossibility of objectivity in the humanities and some of the social sciences. There are longstanding disagreements on whether the interpretative methods, unavoidably value-laden categories and notions, and narrative modes of expression used in fields like historiography, sociology, and all the disciplines using ethnographic methods make these disciplines hopelessly subjective and doomed to being politically biased (see e.g. Novick [1988]; White [2000]; Montuschi [2014]; Axtell [2016]). However, if we understand these epistemic risks, subjectivity and value-ladenness, as arising from our imperfections as individual epistemic agents alone, this may prevent us from paying due attention to other important risks and to strategies developed for averting them. As noted, the relevance of different risks arising from our imperfections as epistemic agents may vary in different contexts. For instance, collective biases are often taken very seriously in humanities research and in the social sciences. This might be because they pose a particularly important epistemic risk in many of the fields in question, or perhaps the risk is just easier to recognize in these contexts. In any case, it should be noted that historians have been very aware of the risk of Whig historiography for a long time, and ethnographers of the risk of ethnocentrism—both being types of collective bias. It is not only individual political bias but collective political bias that is treated as an important risk in historiography, sociology, and ethnographic research, because when such a bias is shared by the whole research community, it can go undetected (Daston [2014]; Axtell [2016]).

Controlled experiments are not a particularly effective strategy against collective bias. In fact, any procedure that is meant to screen out just the subjective biases and idiosyncrasies of individual researchers is fairly inefficient against collective bias (Longino [1990]; [2001]). It matters little that you may change researchers, repeat an experiment, and still get the same result, if your theoretical background assumptions, and as a result the whole setting of the experiment, are biased. For example, procedural objectivity did not prevent the study of primates in the 1950's from being sexist (Haraway [1989]). Research was based on male-dominance schemas, which distorted the results, and even more importantly, the overall picture of primates drawn from them.

Now let us compare the primatology of those days with the humanities and social sciences of the same era. Some of the latter fields had, if not perfect, at least advanced strategies for averting collective biases. For example, in ethnographic research[6] it was

[6] Cultural anthropology is sometimes classified as a social science, and sometimes as a humanities discipline. Some other fields that use ethnographic methods, such as ethnology and folkloristics, are typically taken to belong to the humanities.

already by that time customary to avoid the epistemic appraisal of the knowledge systems of the cultures being studied, and the beliefs of the informants. This was done in order to avoid ethnocentrism. Epistemic appraisal of a knowledge claim is always based on an interpretation of that claim. However, the interpretation of claims such as 'My brother is a parrot' can be a complex task, requiring thorough understanding of the knowledge system and conceptual framework where the claim is uttered, with which the researcher is originally not familiar (see Risjord [1993]). If an ethnographer would start evaluating such knowledge claims right away, the assessment would most likely be based on an ethnocentric interpretation, and would thus be misleading. That is why such assessments were methodologically avoided (Asad [1986]; Koskinen [2015]).

So while the primatologists did not even notice that sexism skewed their results, ethnographers had quite sophisticated strategies for averting ethnocentrism. If we talk about objectivity in humanities research and in the qualitative social sciences, but focus only on individual biases, such strategies developed for averting collective bias easily go unnoticed. However, they should be taken into account when assessing the overall objectivity of research done in these fields.—And the risk account is useful when clarifying why this is the case.

Let us now turn to the second short example: objectivity in the context of the new forms of research where researchers try to democratize scientific knowledge production, such as citizen science and participatory research. The risk account can be used for distinguishing between two basic ways in which such a change in the context of research may threaten its objectivity.

First, as the context changes, some new epistemic risk arising from our imperfections as epistemic agents may become important. For example, researchers in many fields have expressed the worry that people participating in citizen science projects may have political goals or other partisan interests related to the research subject, and this may end up making the results biased (see e.g. Rise of the citizen scientist [2015]). This may happen even in contexts where the risk of such biases has not been important previously. So the change in the context may necessitate the implementation of strategies for averting a risk that has become newly important.

Secondly, which might be less obvious and thus more interesting, when the context changes, established strategies for averting epistemic risks can cease to function satisfactorily. As noted, a strategy efficient in one context may not work or may even be impossible to implement in another context. For instance, the practice of avoiding the epistemic appraisal of the informants' beliefs has been an effective way to avoid ethnocentrism in ethnographic research. However, when former informants become co-researchers in participatory projects, the practice becomes untenable. It is neither respectful nor epistemically acceptable to avoid the epistemic appraisal of the knowledge claims of some members of a research team (Koskinen [2014]). A new strategy for avoiding ethnocentrism must be devised.

The risk account of scientific objectivity mostly just describes what the different applicable senses of objectivity have in common. But I hope these two examples demonstrate that the account can also prove useful in some of the contexts where we wish to actually assess the objectivity of something.

7. Conclusion

My aim has been to bring some unity to the discussion of objectivity: to cover, with a single account, all the applicable senses of scientific objectivity recognized in the recent literature, and to clarify why they have normative force. This I have done by combining ideas presented in philosophical discussions, and by adding one observation.

I started from the intuition that the diverse positive senses of objectivity are all related to trust (Fine [1998]; Scheman [2001]; Douglas [2004]). However, if we take into account the literature on trust both in epistemology and in philosophy of science (McLeod [2015]; Hardwig [1991]; Rolin [forthcoming]), it becomes clear that trust is not quite what we should concentrate on. I suggested that the intuition be refined. First, instead of trust, we should talk about reliance. When we call *X* objective, we say that we rely on it, and that others should do so too. Secondly, instrumentalism about objectivity will not do: objectivity cannot be identified with just any features that promote trust or reliance, as both can be misguided (Reiss and Sprenger [2016]). In order to understand when the reliance is well grounded—that is, what gives the different senses of objectivity their normative force—I turned to the negative accounts of objectivity, in which objectivity marks the absence of something (Daston and Galison [2007]; Hacking [2015]). I then defined this 'something' as epistemic risks (Biddle and Kukla [2017]; Biddle [forthcoming]) arising from our imperfections as epistemic agents. When we call *X* objective, we believe that important epistemic risks arising from our imperfections as epistemic agents have been effectively averted.

The risk account of scientific objectivity I have defended here identifies imperfect epistemic agents, context-dependent epistemic risks, and strategies developed for averting them as integral elements of objectivity. Every positive, applicable sense of objectivity recognized in the recent literature makes an essentially negative claim. They all identify either an epistemic risk, or some efficient strategy for averting one or more important epistemic risks that arise from our imperfections as epistemic agents.

This article has been a mostly descriptive exercise. But I also hope to have made it clear why I hold the applicable notion of scientific objectivity to be worth defending, and to have demonstrated that the risk account can be useful in the analysis of questions about objectivity in different contexts.

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