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The experimenters' regress reconsidered: Replication, tacit knowledge, and the dynamics of knowledge generation

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A B S T R A C T

This paper revisits the debate between Harry Collins and Allan Franklin, concerning the experimenters' regress. Focusing my attention on a case study from recent psychology (regarding experimental evidence for the existence of a *Mozart Effect*), I argue that Franklin is right to highlight the role of epistemological strategies in scientific practice, but that his account does not sufficiently appreciate Collins's point about the importance of tacit knowledge in experimental practice. In turn, Collins rightly highlights the epistemic uncertainty (and skepticism) surrounding much experimental research. However, I will argue that his analysis of tacit knowledge fails to elucidate the reasons why scientists often are (and should be) skeptical of other researchers' experimental results. I will present an analysis of tacit knowledge in experimental research that not only answers to this desideratum, but also shows how such skepticism can in fact be a vital enabling factor for the dynamic processes of experimental knowledge generation.

1. Introduction

While science is widely held to be characterized by methodological rigor and rational argumentation, many will agree that scientific practice, like other kinds of practices, also involves tacit skills and practical knowledge, which are acquired as part of scientific training through long processes of instruction, repetition, and enculturation. Yet, the nature of tacit knowledge (and of practice more generally) is not easy to pin down, as tacit knowledge is not readily accessible to the philosophical tools of logical and conceptual analysis (or to the more recent experimental philosophy, for that matter). As a result, there are to date very few attempts within the philosophy of science to come to grips with tacit knowledge, or with the concept of scientific practice more generally.[1] Moreover, the notion of practice itself, though common in social theory, is riddled with problems.[2]

Despite these problems, some sociologists of science (in particular Harry Collins) have drawn skeptical conclusions from the existence of tacit knowledge, suggesting that if every scientific action or judgment involves tacit knowledge, and if such knowledge is not accessible to rational explication, there will inevitably be an 'a-rational' aspect to scientific decision-making, which makes it hard to see how scientific controversies can ever be regarded as decisively resolved. While this argument has over the decades been met with fierce opposition within philosophy of science, critics (such as, most prominently, Allan Franklin) have typically not denied the existence of tacit knowledge as such. However, in their attempts to argue against relativism and for the rationality of science, it seems to me that they have not sufficiently addressed this crucial premise of Collins's argument. In this article, I will evaluate the status of this crucial premise. I will show that (something like) tacit knowledge plays an important role in scientific practice (and does indeed raise skeptical questions). However, I will challenge Collins's specific philosophical conception of tacit knowledge. Moreover, in contrast to both Collins and Franklin, I will shift the focus of analysis away from questions about the *justification* (or

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[1] But see Rouse (2002) and Soler (2011) for exceptions to this statement.

[2] See for example Turner (1994) for a devastating critique of dominant conceptions of practical knowledge, and Rouse (2006) for an overview of the various usages of, and problems with, the term "practice."

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validation) of specific scientific claims, instead aiming at a normative analysis of the *production* of scientific knowledge.

Before delving into my argument, let me add a few words about the intended scope of my thesis. Harry Collins is particularly well known for his analysis of the early debate over the detection of gravitational waves, but also discusses other disciplines and even parapsychology. Allan Franklin, on the other hand, has largely focused on cases in the history of physics. In this paper, I will focus my attention specifically on the cognitive and behavioral sciences. However, I contend that the analysis provided here can be generalized to other research fields characterized by conceptual openness and epistemic uncertainty.

I will begin (in Section 2) by recounting the debate between Collins and Franklin, arguing that tacit and explicit knowledge cannot be attributed to different stages of the investigative process, but are rather intertwined throughout the entire process of research. In Section 3, I will continue on this theme, arguing that Collins, with his account of the experimenters' regress, is on to something, insofar as scientists, at every stage of research, have to make judgements as to whether their concepts apply, and it is impossible to explicate unambiguous criteria of how to apply concepts correctly. However, I will present a different (and novel) analysis of why this situation gives rise to skepticism, having to do with the deep epistemic uncertainty and conceptual openness that characterizes much experimental research in the cognitive and behavioral sciences. Section 4 presents a case study (about the Mozart Effect) designed to illustrate the analysis from the previous section, and to show how scientists can fruitfully and rationally utilize this skepticism to navigate the situation of epistemic uncertainty in which they find themselves. I will also indicate the role played by tacit knowledge here. In Sections 5 and 6, I will elaborate on my usage of the expression "tacit knowledge," specifically situating it vis-à-vis recent work by Collins (2010) and Stephen Turner (2014).

2. The experimenters' regress

A prominent and notorious example of a skeptical argument that has made recourse to tacit knowledge is Harry Collins's "experimenters' regress" argument. I take the argument to consist of two parts (only one of which invokes, strictly speaking, a regress): Collins first posits a circle between judgments of the validity of a measurement device and judgment of the validity of a measurement result.[3] As Collins notes, "we don't know if we have built a good detector until we have tried it and obtained the correct outcome! But we don't know what the correct outcome is until … and so on *ad infinitum*" (Collins, 1985, p. 84). Collins himself refers to "[t]he existence of this circle" as "the 'experimenters' regress'" (Collins, 1985, p. 84). However, it bears stressing that a circle is not the same as a regress. The regress only enters Collins's argument once scientists try to *justify their judgments* about a given outcome or about the quality of their data. It is here that Collins appeals to tacit knowledge, arguing that "[e]xperimental ability has the character of a skill that can be acquired and developed with practice. Like a skill, it cannot be fully explicated or absolutely established" (Collins, 1985, p. 73). It follows that for every scientific judgment there will be an inexplicable remainder, making it impossible to reduce a scientific justification to an algorithm. This becomes critical when scientists disagree in their judgments, because on Collins's analysis such a disagreement cannot be rationally adjudicated.[4]

Collins lays the groundwork for this (second) part of his argument in the course of a discussion of *replication* (in chapter 3 of his 1985 book), though he does not intend the implications of the argument to be restricted to problems of exact replication.[5] Putting Collins's argument in more abstract terms, his claim, in a nutshell, is that if a given experiment, B, produces a result that conflicts with a previous experiment, A, this can in principle mean either one of two things: (i) the results of A and B disagree about the hypothesis under test, or (ii) experiment B in fact failed to properly replicate experiment A. The question is whether it can be determined which of the two interpretations is the correct one. If the outcome of experiment B disagrees with those of experiment A, does this refute A's results, or does it mean that it failed to replicate A, such that it cannot confirm or refute its results? Famously, Collins' answer to this question has been that there is no way of deciding between these two possible scenarios (Collins, 1985). While the scientist who conducted experiment B may insist that her results refute the results of A, the scientist who conducted A may say that B is not a replication of A, and hence, that B did not succeed in addressing the subject matter under investigation. Moreover, if both parties keep insisting on their points of view, there is no rational procedure that could decide the matter.

The argument just outlined has two implications, one skeptical and one relativist: The *skeptical* implication is that there is no way to resolve disagreements about whether an experiment has succeeded. Hence, we have reason to be skeptical that we will ever know the true answer to questions like the above. The *relativist* implication is that when disagreements actually do get resolved, there have to be mechanisms other than rational argumentation (e.g., mechanisms of power relations) to account for this. Here my focus will be on the skeptical first part of the argument, not on the relativist second part.[6]

The two conclusions of Collins's argument have been quite unacceptable to many philosophers of science. Most prominently, Allan Franklin (1989) responded by pointing out that even though strong skepticism cannot be decisively refuted, a detailed analysis of specific scientific practices reveals it to be rather implausible. In this vein, he maintained that scientists have "arguments designed to establish, or help establish, the validity of an experimental result or method" (Franklin, 1989, p. 438). Such arguments involve "epistemological strategies," which include (1) appeals to a well-corroborated theory of the apparatus, (2) the use of different experimental apparatuses, (3) the demonstration that the same apparatus can detect similar phenomena, (4) the test of predictions about the results of an experimental intervention, and many others (the list is open-ended). Franklin's point was that the existence of such epistemological strategies shows that scientific controversies of the kind described by Collins are in fact resolved by rational means. In turn, Collins (1994) has emphasized that it was not his

[3] Famously, Collins has illustrated this with the example of Joseph Weber's apparatus for gravitational wave detection.

[4] In distinguishing between the circularity and the regress parts of Collins's "experimenters' regress" argument, I hope to highlight that philosophers of science, in their responses to Collins, have typically only taken up the circularity problem, thereby not acknowledging the central status of tacit knowledge to the actual regress part of Collins's argument. In this article I hope to draw attention to this neglect.

[5] While Collins argues that exact replication will be desirable when one attempts to disprove someone else's experimental results, the case of gravitational wave detection turns on whose judgment about the proper functioning of their respective gravity wave detectors has higher credibility (regardless of whether one aspires to be an exact replication of the other).

[6] It is easy to see that Collins's skeptical argument constitutes a modern-day version of Duhemian underdetermination (see also Rasmussen, 1993). In this paper I will provide an interpretation of this underdetermination that simultaneously gives a proper place to a particular kind of tacit knowledge and reveals how the underdetermination in question can be transient and epistemically productive.