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EXPERIMENTAL SKILLS AND EXPERIMENT APPRAISAL

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ABSTRACT

Traditional philosophy of science believes that scientists can achieve agreement on every experimental result provided it can be replicated in an appropriate way, that is, reproducible with the same experimental arrangement and procedure. By analyzing the role of skills in experimental appraisal, I explain why in fact scientists do not always have consensus on experimental results despite their replication attempts. Based on a detailed analysis of a historical case, I argue that experimental replications inevitably involve a process of skill-transference, which is frequently not articulated in linguistic discourses. Hence, it is very difficult to make identical replications if experimental reports are the only resources. Furthermore, I argue that, because transferred skills have to be integrated with scientists' prior experience, skill-transference is sensitive to contextual factors, which can prevent scientists from reaching consensus on experimental results by influencing the effectiveness of communication in experiment appraisal.

INTRODUCTION

Every student of science agrees that experiment is the foundation of theory testing, because experiment is supposed to supply objective knowledge of the world. However, experimental results themselves are not unproblematic. Most experimental instruments, procedures, and findings now widely accepted as reliable have experienced a period in which their legitimacy was controversial. Even after these instruments, procedures, and findings become conventional, their legitimacy may later be challenged under new circumstances, especially in scientific debates. Hence, not every experimental result can be qualified as objective knowledge. Whether an experiment, especially a newly designed one, can provide objective knowledge about the world is a question that requires careful examinations. Experiment appraisal, that is, evaluating the legitimacy, the reliability, or the accuracy of experimental instruments, procedures, and findings, is an important topic for the philosophy of science.

Some contemporary philosophers of science have addressed the issue of experiment appraisal. Karl Popper, for example, notes that an experimental result must satisfy a couple requirements in order to be qualified as objective knowledge. First, this result must be displayed by a genuine physical effect, which is observable not only in a psychological but also a materialistic sense. To be more specific, it should be an observable effect "occurring in a certain individual region of space and time," or involving "position and movement of macroscopic physical bodies."1 (1959, 103)

Second, more importantly, an acceptable experimental result should be reproducible. Popper maintains that "[w]e do not take even our observations quite seriously, or accept them as scientific observations, until we have repeated and tested them. Only by such repetitions can we convince ourselves that we are not dealing with a mere isolated 'coincidence', but with events which, on account of their regularity and reproducibility, are in principle inter-subjectively testable." (Ibid., 45)

However, Popper also realizes that a reproducible result may not need to be actually reproduced (Ibid., 87). The key to demonstrate the reproducibility of an experimental result, according to Popper, is to provide clearly written instructions for its replication, so that the result "can be regularly reproduced by anyone who carries out the appropriate experiment in the way prescribed." (Ibid., 45) He thus recommends that experimental processes should be expressed in clearly written descriptions. Those who conduct an original experiment should present the experiment by describing the experimental arrangement in detail so that anyone with relevant techniques can replicate it. Those who have doubts about the original experiment should construct a counter-experiment with contradictory results, and publish instructions telling others how to repeat their new experiment (Ibid., 99).

If scientists follow this methodological guideline carefully, and if what they are dealing with is a physical effect involving position and movement of macroscopic bodies, they should be able to reach agreement about the experimental result, or at least to determine whether there is any difference in their experimental arrangements or operations. Otherwise, Popper says, language would no longer be "a means of universal communication." (Ibid., 104)

However, the results of some recent studies of scientific experiments suggest that replication attempts, even those that strictly follow Popper's methodological guideline, do not always produce agreement among scientists about experimental results. Based on detailed analyses of experimental discoveries in contemporary physics, for example, Franklin and Howson report that experiment replications are neither necessary nor sufficient for the validation of experimental results (Franklin and Howson 1988, 426). Also, based on interviews with a group of biochemists, Mulkay and Gilbert note that scientists frequently have different conceptions of what a proper experiment replication should be, and that replication attempts may not bring about agreement among scientists though the experimental results they are dealing with are observable physical effects (Mulkay and Gilbert 1986, 22).

The purpose of this paper is to examine the complexities involved in experiment appraisal, and to explore some of the fundamental features of experiment replications. In the following sections, I first illustrate the complexities in experiment appraisal by analyzing an historical case: the debate over the result of a prismatic interference experiment in the early 1830s. The main issue of this debate concerned what exactly happened in the prismatic interference experiment. Despite the fact that the experiment produced an observable physical effect, conflicting reports about the experimental result per se still existed after a series of replication attempts. This historical episode vividly shows that replicating an experiment "in the way prescribed" cannot always verify the experimental result even if it is a genuine physical effect.

I then explore one of the crucial features of experiment appraisal that has been underestimated by Popper: the involvement of experimental skills. I argue that experiment replications inevitably require a process of skill-transference, which is frequently not articulated in linguistic descriptions. We should not expect that experimental processes can be described in clearly written instructions so that others can reproduce experiments "in the way prescribed." Moreover, I argue that those transferred skills have to be integrated with people's prior practices or experiences. Thus, even if clearly written instructions have been given, even if these instructions have been carefully followed, and even if the experimental results are genuine physical effects, scientists still may not be able to reproduce the same experimental result because of their different prior practices or experiences.

THE DEBATE ON THE PRISMATIC INTERFERENCE EXPERIMENT

In Britain, the early 1830s was a critical period for the development of optics. Since Newton's endorsement in the late seventeenth century, the particle theory of light, which claimed that light is composed of tiny particles, had dominated the field of optics in Britain for more than a hundred years. During this period, the wave theory of light, which regarded light as waves, was very unpopular. The dominance of the particle theory, however, became shaky at the beginning of the nineteenth century. In the late 1820s, a group of British scientists, most of them Cambridge-trained physicists, adopted the wave theory. Beginning in 1830, these newly committed wave theorists started to publish their results, both theoretical and experimental. A heated particle-wave debate then began.

In 1832, Baden Powell, Savilian Professor of geometry at Oxford and a committed wave theorist, published an article in the Philosophical Magazine on several experiments about diffraction and interference (Powell 1832a). One of the experiments that Powell described in detail was originally proposed by Augustin Fresnel. This was an experiment using two plane glasses inclined at a very large angle to demonstrate the phenomenon of interference by reflection. Powell repeated this experiment with some modifications. In addition to having two plane glasses inclined at a very large angle as Fresnel did, Powell placed a prism in front of the glasses, in the position where the two reflected rays were supposed to intersect (fig.1). Using sunlight as the light source, he found that, after being refracted by the prism, the two reflected rays continued to produce interference fringes -- a series of parallel alternating light-and-dark lines. He also found that the pattern and the positions of the interference fringes did not change after the interception by the prism. Powell believed that the results of this prismatic interference were entirely consistent with the wave theory.

Powell's experiment on prismatic interference drew the attention of Richard Potter, an amateur physicist at the time.2 Although he was a merchant at Manchester, Potter devoted his leisure time to the study of optics, conducting experiments to measure the reflective power of mirrors. Since he found that neither the particle theory nor the wave theory was

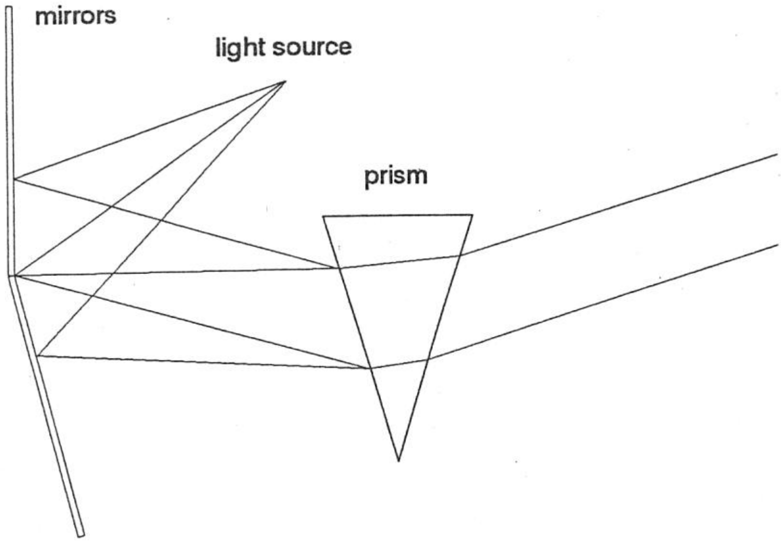

Figure 3.1 Powell's prismatic interference experiment

able to explain the experimental results he obtained, Potter did not commit himself to either theoretical tradition in his early optical researches.

After reading Powell's article, Potter replicated the prismatic interference experiment. Instead of using sun light as the source, Potter employed homogeneous light produced by a colored solution. He observed that some portions of the reflected rays, which should have interfered without the prism, did not interfere after being refracted by the prism. On the other hand, he found that interference took place between other portions of the reflected rays. Using an eye-glass to observe the interference fringes directly, Potter found that the interference fringes moved toward the thick side of the prism when he withdrew his eye and the eye-glass further from the prism. In February 1833, Potter published a paper in the Philosophical Magazine, reporting his experimental findings. As shown in figure 2, Powell had reported that the interference fringes produced after the refraction by the prism were unchanged, and the central band of the interference fringes was still on the line mn. However, Potter reported that different portions of the reflected rays were involved in the interference, and that the central band of the interference fringes was on a new line pq (Potter 1833a, 82).

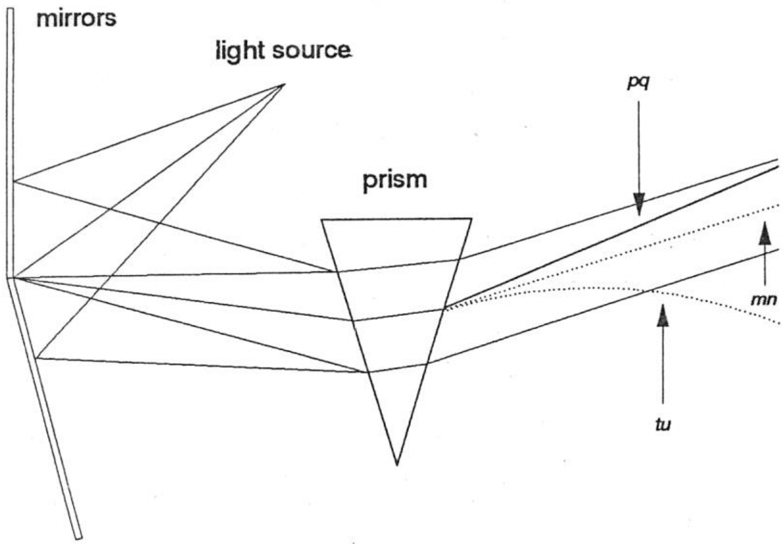

Figure 3.2 The result of Potter's replication

Potter also found that this experiment on prismatic interference could be used to determine the velocity of light in refractive media. The positions of the interference fringes in this experiment were determined by the path differences of the intersecting rays. These path differences were affected by the prism because rays of light changed their velocities in refractive media. Hence, the velocity of light in the prism could be calculated based upon the position of the central band of the interference fringes. Since the two rival theories of light had different predictions of the velocity of light in refractive media, a test of these theories could be made by comparing their predictions with the measures.

The particle theory assumed that light moved with an increased velocity when passing through refractive media, in a direct ratio to their refractive indices. According to this assumption, Potter found, the central band of the interference fringes in his experiment ought to be seen along the line tu in figure 2, which was far from the facts shown by the experiment. On the other hand, the wave theory assumed that light traveled with a decreased velocity in refractive media, in an inverse ratio to their refractive indices. According to this assumption, Potter demonstrated, the central band of the interference fringes in this experiment should coincide with the intermediate line mn, which was still not compatible with the experimental results, although better than the particle theory's prediction. Therefore, neither the particle theory nor the wave theory of light gave a correct prediction of the velocity of light in refractive media. These experimental results, Potter claimed, constituted a fatal objection to both theories of light (Ibid., 94).

Potter's attack prompted strong reactions from the wave camp, including one from George Airy. As Lucasian Professor at Cambridge, Airy was one of the most influential figures among the wave theorists in the early 1830s, and had been known to fiercely counter-attack every challenge from the particle camp. Airy published a comment on Potter's experiment in the Philosophical Magazine, just one month after the appearance of Potter's article. In his remarks, Airy first cast doubt on one of the most important experimental conditions in Potter's work -- the light source. Airy insisted that Potter must not have used homogeneous light as the source in his experiment. Airy listed two reasons to support his allegation. First, interference by reflection required a light source with very high intensity, but so far all homogeneous sources could only produce very faint light. Second, if homogeneous light had been used in Potter's experiment, Airy reasoned, it would have produced a series of bright and dark bars with equal intensity, and no one could have determined where the center of the fringes was (Airy 1833a, 164,162).

If the light source was not homogeneous but heterogeneous, Airy argued, then the center of the fringes was not at the point where the two intersecting rays had equal paths -- the line mn in figure 2. Airy emphasized that his analysis of the positions of the interference fringes was theoretically neutral, having nothing to do with assumptions about the nature of light. According to Airy, if a heterogenous light source was used, each homogeneous ray composing the reflected heterogeneous light would produce its own group of bars. Due to the impact of the prism, the bars produced by each color would have different breadths and different displacements moving slightly toward the thick end of the prism. When these different groups of bars coincided with each other, they constituted the center of the fringes with a displacement toward the thick end of the prism, although the group of interference fringes as a whole actually did not move (Ibid., 162-4).

Airy realized that the phenomenon he said he could explain was not identical with the one Potter claimed he had observed in the experiment. Potter said that he had seen the displacement of a group of the interference fringes as a whole, while Airy only accounted for the shift of the center of the fringes. But Airy insisted that Potter's observation must be wrong because of an inappropriate observation technique he employed. To illustrate this point, Airy presented to his readers an "instructive experiment." This was also an experiment on interference with reflection, in which two pieces of glass were connected with hinges. By slightly inclining one piece of glass while fixing the other, the center of the interference fringes would move while the position of the group of fringes as a whole remained unchanged. However, Airy noted, if the experimenter had not been continuously observing the change of the fringes, for example, if he left the eye-glass to adjust the angle between the glasses, he might not be able to distinguish the differences between a shift of the center and a move of the group as a whole, because he could not tell whether the rest of the fringes really had moved. A continuous observation, thus, was a key for achieving reliable results. Airy suspected that, without any discussion of this issue in his experimental report, Potter must have been unaware of the problem and not been continuously observing the change of the fringes when he withdrew his eye and the eye-glass from the prism. Therefore, Potter's observation was not reliable (Ibid., 164-5).

Potter was very unhappy with Airy's remarks. He immediately published a reply in the 1833 Philosophical Magazine, in which he complained that Airy's analysis of his experiment had completely missed the point. In response to Airy's charge about his experimental setting, Potter provided details about the light source he had used in the experiment. It was the red light produced by a solution of "iodine in hydriodic acid," which gave much purer and more intense light than red glasses did. Even according to the standard adopted by wave theorists like Fresnel, Potter claimed, the light source in his experiments was satisfactorily homogeneous (Potter 1833b, 276-7).

Potter also held that the observation techniques he used were reliable and would not create the confusion that Airy had described. One of the advantages of his techniques, Potter claimed, consisted in the use of a reference to show the displacement of the interference fringes. This was a group of diffracted lines caused by the edge of one of the glass mirrors during the observation process. To illustrate this point, Potter presented a diagram (fig.3), in which lines ef represented the diffracted fringes produced by the edge of the mirror, and ab and cd were the different positions of the whole interference fringes he observed at different distances from the prism. By introducing this reference, Potter said that he could be certain about the movement of the interference fringes as a whole, and did not commit the observational mistake that Airy had

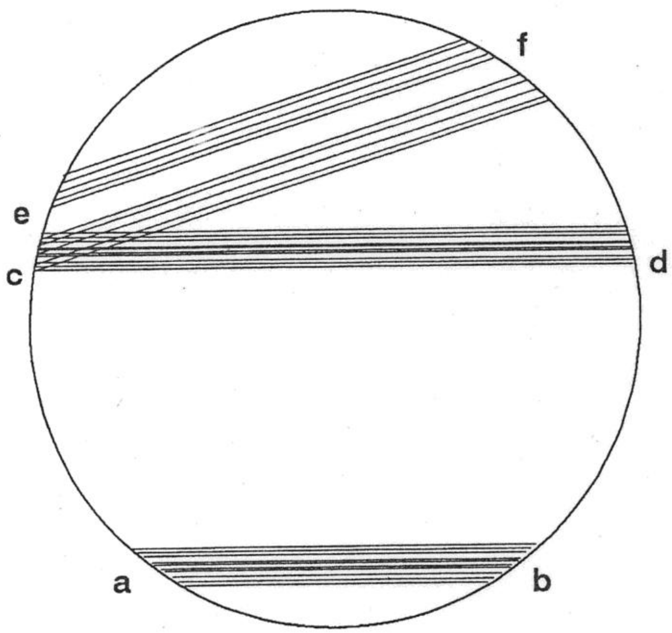

Figure 3.3 Potter's observations of the fringes

suggested (Ibid., 287).

The most powerful defense Potter presented, however, was his announcement that he had successfully replicated his experiment in front of Powell. He claimed that he had repeatedly replicated his experiment at Powell's residence in June 1832 (Ibid.). There is no further evidence to verify Potter's replication attempts. But from a paper Powell published in December 1832, it is evident that Powell had known of Potter's experimental results, and, surprisingly, adopted a very positive attitude toward Potter's work (Powell 1832b, 436).

The dispute between Potter and Airy finally centered on a very simple question: What had actually happened in these experiments? Or, more specifically, had the group of interference fringes as a whole really moved in these experiments or had they not? Potter's claim concerning his successful replications in front of Powell forced Airy to replicate the experiment of prismatic interference. In his replication, Airy used a new observation method to determine the displacements of the fringes. His new idea consisted in using an eye-glass with a wire fixed in its focus, both attached to a slide on a bar. By proper adjustment of the bar's direction, Airy was able to keep the image of the wire focused upon one of the fringes when he looked through the eye-glass, even though the distance between the eye-glass and the prism varied. With this method, Airy reported that, while the fringe under the wire shifted only a half of its breadth, the center of the fringes had gradually moved through a distance of twelve double fringes (Airy 1833b, 451).

Airy's new observation device did not convince Potter. After reading Airy's report of the replication, Potter immediately complained that Airy did not give sufficient information to enable others to verify the result. He pointed out that Airy's description of his new observation device was not sufficient for further replications, unless the angle between the bar and the incident rays, together with other data, were known. Potter also charged that Airy's observation device created unnecessary "intricacy," because it introduced a new object, a wire fixed in the focus of the eye-glass, as the reference. This "intricacy," according to Potter, could be avoided by using a reference that had been given by the experimental arrangement. A reliable observation can be obtained by measuring the position change of the interference fringes with respect to the diffracted fringes caused by the edge of one of the mirrors (Potter 1833c, 333). For these reasons Potter concluded that Airy's replication could not be reliable. Thus, after several rounds of exchanges, Potter and Airy still did not reach agreement on what really happened in these experiments. Specifically, they simply did not agree with each other on whether the position of the group of fringes as a whole moved when they were observed from different distances.

The attempts to replicate the prismatic interference experiment, which produced more than ten experimental reports from Powell, Potter, Airy and others between 1832 and 1833, did not yield any agreement. On the one hand, the wave theorists in the debate were confident that, through their replications, the problem had been successfully solved by the wave theory. Airy even predicted that, if Potter continued to study this subject, he would very soon become a wave theorist (Airy 1833a, 167). On the other hand, Potter regarded the results in his replications as a solid evidence against the wave theory, and claimed that it was harder and harder for him to accept the wave theory (1833b, 277).

These completely opposite judgments stemmed from Airy's and Potter's different observations of what really happened in the experiments. The discrepancy could perhaps have been resolved through performing the experiment in front of the two scientists. But in a letter to William Hamilton on April 1833, Airy expressed his reluctance to continue the debate with Potter or to verify the experimental findings in question.3 One reason suggested for Airy's retreat was that the debate had become too personal.4 However, a more plausible reason was that Airy just did not have an interest in meeting with Potter. There were great differences between Airy and Potter in terms of their social and intellectual status. In the early 1830s Airy had been one of the most successful and prestigious scientists in Britain. Potter, on the other hand, was an unknown amateur who had no formal training in science. Such differences could create a barrier to a face-to-face meeting between Potter and Airy, which might have helped them determine the details of their experimental settings and resolve their differences.

EXPERIMENTAL SKILLS AND CONTEXTUAL FACTORS

The debate between Potter and Airy on the prismatic interference experiment indicates that the process of experiment appraisal is much more complicated than what Karl Popper has described. Although the prismatic interference experiment did produce a real physical effect (interference fringes involved only position and movement of macroscopic bodies), scientists failed to reach agreement about what this physical effect was despite several replication attempts. Potter and Airy simply did not agree with each other on what really happened in the experiment: the former insisted that he saw the displacement of the group of the interference fringes while the latter maintained that only the center of the fringes shifted.

The unsettled debate on the experiment raises some important questions. Why did Potter and Airy fail to reach agreement on the result of the experiment, which was a real physical effect? Or, if they were in fact dealing with different physical effects by conducting different experiments, why did they fail to detect their differences and resolve the debate? The deadlock between Potter and Airy suggests that they had experienced a communication failure that hindered them from reaching consensus on the experimental result. If so, what were the factors that caused the communication failure?

The peculiar inconclusiveness of experiment appraisal in the debate on the prismatic interference experiment might partly result from the conventional style of reporting and representing experimental findings. In early nineteenth century Britain, there was no standard format for reporting optical experiments. Most experimental reports on optics published in academic journals were relatively simple, usually lacking detailed descriptions of experimental procedures, instruments, and results. This was particularly true for those publishing in the Philosophical Magazine. Unlike the Philosophical Transactions, the Philosophical Magazine provided only a very limited space for publications. Within an average length of three to five pages, it was quite difficult to portray an experiment in detail, or to provide the necessary information for experiment replications.

Moreover, scientists in the early nineteenth century lacked adequate techniques to reproduce optical images in their experimental reports. Before 1860 when photographic techniques became available for book or journal illustrations, scientists in optics were limited to sketches and engraved diagrams. But these techniques could not accurately represent the details of optical images, especially the variations of the intensity of light. In the debate on the prismatic interference experiment, less confusion would have been created if Airy had been able to reproduce his observation in his report with an accurate illustrative technique, rather than just giving a verbal description. For example, Airy described his observation as follows: "[O]n receding from the prism, the fringes remain stationary; while . . . the centre of fringes passes gradually and rapidly from the centre of the mixture of light to its border." (Airy 1833b, 451; original emphasis) According to this description, the center of the fringes experienced a spacial displacement, moving from one location to another. However, William Whewell, who agreed with Airy on the experimental result, had a different description of the same phenomenon. After witnessing Airy's experiment, Whewell wrote down his observation as follows: "As you withdraw the eyepiece, you see the bars, not move, but grow on one side and dim on the other so that the centre shifts." (Hankins 1980, 150; original emphasis) According to Whewell's description, there was no spatial movement but rather changes of the intensity of light. This confusion could have been eliminated by using an appropriate technique such as photography that could capture the optical phenomenon in detail and correctly present it to the readers.

These limitations increased the difficulties in experiment replications, if one had only the information from published experimental reports. To complete the replication process for experiment appraisal, intensive communication, especially informal exchanges, between scientists was necessary. In terms of their functions in experiment appraisal, there were significant differences between formal communication (experimental reports and published replies or comments), and informal communication (private conversations and private correspondence). In the debate on the prismatic interference experiment, formal communication might be able to assure those who did not have direct experience of the experiment in question and did not intend to replicate it that its result was reliable. However, it was not enough to persuade those who had been directly involved in the debate, because the experimental reports and published replies did not supply the detailed information sufficient for experiment replications. Only informal communication that aimed at information exchange between relevant scientists could complete the process of replication. These informal exchanges, however, depended upon a series of contextual factors. As indicated above, the differences in intellectual and social status between Potter and Airy might have prevented them from further private communication, even though Airy was willing to reply publicly to Potter.

The format of experimental reports, the techniques of presenting optical images, and the intellectual and social status of scientists were the contextual factors that contributed to a communication failure between Potter and Airy in their appraisals of the prismatic interference experiment. Our historical episode clearly indicates that contextual factors played a significant role in the evaluation of the prismatic interference experiment. However, was the involvement of contextual factors in this historical case merely contingent, or inevitable in the sense that it reflected an essential feature of experiment appraisal? If the answer is the latter, then a related question also should be asked: how are contextual factors in general involved in the process of experiment replication?

One way to answer these questions is to examine the distinct characteristics of experiment appraisal. For a long time, philosophers have recognized that there are some fundamental differences between two kinds of intelligent activities: knowing how and knowing that. As noted by Gilbert Ryle more than forty years ago, "there are many classes of performances in which intelligence is displayed, but the rules or criteria of which are unformulated." (1949, 30) Examples of these performances, or the activities of knowing how, include a wit who knows how to make good jokes and how to detect bad ones, but cannot tell us or himself any recipes for doing so; or, a well-trained sailor who can tie complex knots and discern if someone else is tying them correctly, but who is probably incapable of describing in words how the knots should be tied. By contrast with from knowing that (in which intelligent operations involve the observance of rules), knowing how involves the intelligent activities in such a way that "[e]fficient practice precedes the theory of it; . . . Some intelligent performances are not controlled by any anterior acknowledgments of the principles applied to them." (Ibid.)

Another writer who comments on the differences between knowing how and knowing that is Michael Polanyi. He labels the products of knowing how as tacit knowledge or skills. One example Polanyi uses to illustrate the characteristics of tacit knowledge or skills is the practice of cycling. In this case, the major task for a cyclist is to keep balance. According to our knowledge of physics, we know that, in order to compensate for a given angle of imbalance, we must take a curve on the side of the imbalance, of which the radius should be proportional to the square of the velocity divided by the tangent of the angle of imbalance. We may write down this requirement in the form of a rule, but learning this rule certainly does not make one know how to cycle. In fact, the majority of cyclists would not be able to describe in words this rule, although they know quite well how to keep their balance (Polanyi 1969, 144). Explicit knowledge of rules in this case may be completely ineffectual. On the other hand, Polanyi does not exclude the possibility that some cyclists may improve their skills in cycling by studying the rules written down in manuals or taking instructions of experts. But he insists that, when they come to action, they have to "reintegrate" this explicit knowledge of rules with their prior performances (Polanyi 1966, 11). This knowledge of rules has to be reapplied in a new situation, one in which the knowledge of rules itself does not specify its application conditions. Hence, the key to success in these cases is not the knowledge of rules but people's prior practice or experience that shapes the applications of rules.

The process of experiment appraisal involves a variety of activities that clearly belong to knowing how rather than knowing that. Everyone knows that very specific skills are needed for experiment operations. These include the skills for designing experiments, calibrating and operating instruments, measuring observational parameters, presenting experimental results, and so on. In our historical case, the major skills needed for operating the experiment included those of setting up the arrangement for interferences by reflection, producing a homogeneous light source, observing the interference fringes, and measuring the changes of the fringes. These experimental skills were employed in the original prismatic interference experiment first conducted by Powell and the later replications made by Potter and Airy, but none of them specified these experimental skills clearly in the form of explicit descriptions or instructions. One example is the skill of directly observing the image of the interference fringes with an eye-glass. Quite obviously the success of this observation technique relies on the relative positions of the eye-glass and the observer's eye. But nothing had been said on this issue in the whole debate. It seems that people should have known how to do so before they made the observation. Lack of explicit descriptions of this skill may not be accidental. It is quite possible that even these scientists themselves might not know how to describe this skill through a list of instructions, or that they might regard such a description as unnecessary, because they themselves did not master this skill in this way, or because they simply assumed this skill as part of the necessary expertise of every competent scientist.

In addition to the skills for experiment operations, there are more specific skills involved in the process of experiment replication. Many studies have indicated that a replication is not simply a matter of repeating an experiment identical with the original.5 For those replications with the purpose of confirming the original experiment, an exactly identical experimental design usually provides a very limited support. In these cases, replications require redesigns of the experimental settings. For those replications with the purpose of disconfirming the original, though a completely identical setting is theoretically recommended, scientists usually need to make adjustments because of practical constraints. In these cases, replications should include a justification of the alterations. To make appropriate adjustments of experimental arrangements and to give convincing justifications of these adjustments require very sophisticated skills.

The involvement of experimental skills has a profound impact on the effectiveness of communication in the process of experiment replication. In the cases in which the experimental skills are totally tacit, namely, where there is no articulation of them at all in experimental reports, scientists have to figure out these skills by themselves, and misunderstandings occur easily. A good example in the prismatic interference experiment was the skills involved in producing a homogeneous light source with colored solution. At first Potter employed these skills in his experiment, but gave no description of them, making them completely tacit. When Airy tried to replicate Potter's experiment, he had to determine this tacit knowledge. From Potter's later paper we know that he used the solution of "iodine in hydriodic acid" rather than colored glasses to produce homogeneous light, because the former could generate purer and more intense light than the latter. But Airy did not know this. When he tried to identify the tacit knowledge behind Potter's light source on the basis of his own experience, which was quite probably limited to the uses of colored glasses, Airy concluded that Potter did not use homogeneous light in his experiment at all! Clearly, this was a misunderstanding of Potter's experimental setting. But this was not Airy's fault, because Potter had left his experimental arrangement tacit. And this was not Potter's fault either, because he had every reason to assume that a first-rank researcher in optics like Airy would have the skills of producing homogeneous light that he did.

In those cases in which experimental skills are partially tacit, namely, where some descriptions of them have been provided, obstacles to effective communication still exist. The descriptions of experimental skills, even in the form of clearly written rules, have to be "reintegrated" into scientists' prior performances, as Polanyi has suggested. An example of this kind of complexity in the prismatic interference experiment was the skills involved in observing the interference fringes with an eye-glass. Potter did provide some details of his observational device. While the skills involved in determining the size and power of the eye-glass remained tacit, Potter clearly described the way he operated the device and the reference he used. Airy did not miss these explicit descriptions in his replication. But when he "reintegrated" this articulated knowledge with his prior practices, he inferred that in Potter's operation the eye must have left the eye-glass when withdrawing from the prism, which would bring about unreliable observations according to his experience. Airy designed an "instructive experiment" to show that keeping the eye focusing upon the eye-glass was necessary for observing the change of the interference fringes; later he adopted a new device that recorded a different experimental result. Hence, partial expressions of skills cannot eliminate the obstacles in communication.

For the sake of argument, we can even assume that in some cases skills can be articulated in a very explicit form such as a list of rules.6 Even so, the difficulties in communication remain basically the same. The simplest example to illustrate this point is the case of following a rule of arithmetic such as "add a 2 and then another 2 and then another and so on." Although this rule has been fully articulated in language, several uncertainties remain when it comes to its applications, if we isolate this rule from our prior practices. For example, writing "82, 822, 8222, 82222," "28, 282, 2822, 22822," and even "8^2, 8^{22}, 8^{222}, 8^{2222}" may be said to be cases of following this rule. But in our daily life, we seldom apply this rule arbitrarily, because we integrate this rule with our prior practices, which provide a guideline for its applications. We obey this rule in a normal way because there are regular practices of following the rule in that way. We are trained to do so and through such training we firmly believe that what we do is simply the way it should be done.7 Hence, integration with prior practice is crucial for every kind of skill-transference.

In short, the need for experimental skills imposes several constraints on the communication process in experiment replications. First, these constraints stem from the fact that skills are seldom fully articulated in experimental reports. An effective skill-transference then may not be achieved merely through formal communication like writing and reading experimental reports. Informal communication, including private conversations and personal contacts, is crucial for the success of skill-transference. Whether an informal communication is possible, however, in turn relies upon a series of contextual factors, especially the personal relationship between the scientists involved. Furthermore, constraints on the communication process in experiment replications also stem from the fact that integrations with prior practices are essential in the process of skill-transference. Consequently, contextual factors, such as the personal, social, and intellectual experience of the relevant scientists, must be involved in the process of skill-transference.

Therefore, if we take the important role of skills in experiment appraisal into account, we will understand why no agreement was reached on the experimental results of the prismatic interference experiment despite a series of replication attempts. In the appraisal of the prismatic interference experiment, the experiment produced a real physical effect, and both Potter and Airy tried to replicate each other's experiment "in the way prescribed." The disagreement between them, in the final analysis, resulted from the fact that skills necessary for the experiment were not articulated fully in the experimental reports, and that scientists had to integrate the knowledge presented in experimental reports with their prior practices in the process of skill-transference. Although the dispute between Potter and Airy could have been settled if the experiment had been performed directly in front of them, certain contextual factors, in particular, the differences in their intellectual and social status, finally led to a deadlock in the communication between them.

CONCLUSION

Popper presents an overly simplified picture of experiment appraisal. According to Popper, as long as an experiment produces a genuine physical effect, clear instructions of how to reproduce the experiment have been given, and scientists do replicate it in the way prescribed, they are likely to reach agreement on the acceptance or rejection of the result as physical evidence. If they do not agree with each other at the beginning, they can simply continue the replication process, and sooner and later resolve their differences. Popper is confident that an experiment displayed by real physical effects is bound to generate agreement among scientists through replication attempts. If not so, he claims, "scientific discovery would be reduced to absurdity, . . .[and] the soaring edifice of science would soon lie in ruins." (1959, 104) Following this line, Popper emphasizes the importance of giving clearly written instructions for further replications, which is necessary for achieving appropriate experiment replications. How to give accurate descriptions of experimental operations and results becomes a central issue in Popper's methodology for experiment appraisal. At the same time, factors such as scientists' personal and social characteristics are largely ignored.

However, in the appraisal of the prismatic interference experiment, we find that scientists reached no agreement, although the experiment produced genuine physical effects and the scientists did try to replicate the experiment in the way prescribed. As I have pointed out in the last section, what Popper has overlooked is the involvement of skills in the process of experiment appraisal. Experiment replications require an effective transfer of skills among relevant scientists, which usually are not fully articulated in linguistic descriptions. Therefore, appropriate experiment replications cannot be achieved solely by reading linguistic descriptions or instructions. Furthermore, the process of skill-transference is highly sensitive to contextual factors that can determine the effectiveness of communication essential for experiment appraisal. Because of these contextual factors, an experiment displayed by genuine physical effects may not always generate agreement among scientists in experiment appraisal.

This new understanding of experiment appraisal has several practical implications for the methodology of experiment appraisal, particularly regarding the methods for achieving successful experiment replications. First, we should not expect that we can fully understand an experiment only by reading the experimental report, no matter how accurate the report is. Some crucial skills involved in the experiment may not be articulated in the report. Secondly, in addition to formal communication involving the exchanges of linguistic descriptions of experimental procedures and results, informal communication is also necessary. Sometimes, an experiment can only be fully understood by directly witnessing the whole experimental process, or by personal contacts with the experimenters. Lastly, beside the details of experimental designs, operations, and results, the persons who conducted the experiment are also important for the process of experiment appraisal. Their characteristics, including personal and social features, can play an important role affecting the process of experiment replications.8

NOTES

1. This is Popper's "material requirement" for "basic statements," which are used to describe both observational and experimental results. This requirement is connected with the fact that, without helps of instruments, we can only reliably detect the displacement of macroscopic bodies. Experiments in fields such as microphysics and psychology, hence, need special instruments to convert microscopic or psychological effects into genuine physical effects, namely, the displacement of macroscopic bodies.
2. Later Potter received formal education at Cambridge, graduated in 1838 as a sixth Wrangler, and occupied a professorship of natural philosophy at University College, London from 1841 to 1865. See Dictionary of National Biography, Vol. 16, p.219.
3. Hamilton to Adare, (April 22, 1833), in Graves (1882, vol.2, 44).
4. This was the viewpoint of Hamilton. In the same letter to Adare, Hamilton wrote that "Airy is right, I think, to stop, for it was in danger of becoming too personal a matter".
5. For more discussions, see Franklin and Howson (1984), Collins (1984), Mulkay and Gilbert (1986).
6. To what extent skills can be articulated in language is controversial. For information about the relevant debates, see Sanders (1988, 5-6).
7. For more discussion on why practices are prior to the formulations and followings of rules, see Wittgenstein (1958, 75-88).
8. I am very grateful to Peter Achinstein, Peter Barker, Nathan Tierney, and Bill Angelett for many valuable comments on the drafts of this article.

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