

# Checking and Making Public a Result in Collaborative Science: The Case of the Neutrino Velocity Measurement\*

Hidenori Suzuki

## Abstract

This paper analyzes the case of neutrino velocity measurement by the OPERA experiment, in which the result that the speed of neutrino is faster than light was presented and later corrected. Based on fieldwork, this study aims to elucidate the actual situation of collaborative knowledge production and draw lessons for scientific practice. The issues of confirmatory work and making public a result are addressed. It is pointed out that in some cases strict internal checks such as independent measurements are necessary beyond trust and indirect assessment, even in high-energy physics where epistemic dependence is inevitable. Regarding the condition for strict checks, the extraordinariness of the result and comprehensive understanding of technologies matter. As for making public a result, this study presents a model of decision-making for publication which shows that the issues of checking and publication intersect. In particular, the impact of the result and the manner of the presentation are important factors to consider. In addition, the timing of publication can be influenced by such factors as priority and accountability. Institutional aspects must be paid considerable attention for proper analysis of scientific knowledge as well as for its sound development.

Keywords: Fieldwork, High-energy physics, Trust, Checking, Publication

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## 1 Introduction

On September 23, 2011, an international collaborative experiment OPERA (Oscillation Project with Emulsion-Tracking Apparatus) announced that they precisely measured the velocity of high-energy muon neutrinos and obtained the anomalous result that neutrinos were faster than light. Specifically, they reported the following:

An early arrival time of CNGS muon neutrinos with respect to the one computed assuming the speed of light in vacuum of  $(60.7 \pm 6.9 \text{ (stat.)} \pm 7.4 \text{ (sys.)})$  ns was measured. This anomaly corresponds to a relative difference of the muon neutrino velocity with respect to the speed of light  $(v-c)/c = (2.48 \pm 0.28 \text{ (stat.)} \pm 0.30 \text{ (sys.)}) \times 10^{-5}$ . (Adam et al. 2011)

To put it simply, the result indicates that neutrinos arrived approximately 60 ns (nanoseconds) earlier than predicted at the speed of light in vacuum, in a beam line about 730 km.<sup>\*1</sup> This means that neutrinos are about 0.0025% faster than light.

However, several months after the public announcement, the OPERA collaboration found defects in their instruments and, in June 2012, reported that the speed of neutrino was consistent with that of light (Adam et al. 2012). These circumstances were extensively reported by the media and drew worldwide attention both from the scientific community and from the general public.<sup>\*2</sup>

The author, a philosopher, conducted fieldwork in the OPERA experiment, mainly from 2010 to 2014,<sup>\*3</sup> focusing on tactics to conduct or manage an experiment with limited resources, or *methodologies* of experiments, considering them to be practical knowledge acquisition methods in contemporary science. It is crucial for scientists, especially in collaborative experiments, to have methodologies for various activities to achieve their purpose with limited *resources* (time, funds, people and things).<sup>\*4</sup> The case of the neutrino velocity measurement provides valuable materials for examining the structure of collaborative science, or how matters of group management affect the quality of knowledge production. Its study has implications for issues regarding methodologies: how to deal with anomalous results in particular and what kinds of factors contemporary scientists have to consider in collaborative practice in general.

Regarding this case, the collaborators have their own insights and various interpretations depending on

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<sup>\*1</sup> “CNGS” means “the CERN Neutrinos to Gran Sasso.” See the next section for an overview of the experiment.

<sup>\*2</sup> See, for instance, Brumfiel (2011), Cartlidge (2011b), and Palmer (2011a) and their subsequent reports.

<sup>\*3</sup> I began participant observation in a laboratory that participated in the OPERA collaboration, taking neutrino oscillation research (its main purpose) as a subject. The start was joint research with an OPERA collaborator who was interested in the philosophy of science. While we were conducting the study, the result of the neutrino velocity measurement was presented, and it became my research target. Although I was not a member of the OPERA collaboration, I could have dialogues with collaborators attending some meetings of the collaboration.

<sup>\*4</sup> Suzuki and Naganawa (2022) argues that the concept of methodologies of experiments enables to understand a complementary relationship between justification of the results and resource-managing activities, which are sometimes deemed as conflicting two aspects between rational and social.

how they were involved and what values they possessed. I have conducted hearings and discussions with scientists, taking seriously what they themselves think and listening from as diverse positions (e.g., in institutions, careers, and values) as possible. I have also listened to and exchanged opinions with about 20 OPERA collaborators, including senior and young researchers from both Europe and Japan. They hold (more or less) diverse opinions regarding to what degree and at which point there was a problem with the collaboration.<sup>\*5</sup> Granted that this approach has certain limitations in scope, it nevertheless reveals various aspects of collaborative science in actual situations and contributes to the epistemological discussion in the philosophical domain. By examining the analysis of this study, further studies on collaborative scientific practice can improve its validity and generalizability.

As for the distance from scientists, it is important question whether there was any problem with the OPERA collaboration regarding the neutrino velocity measurement in which a presented result was later corrected. Certainly, problems can be recognized as far as this study indicates *better ways* for knowledge production, which have a normative connotation. However, the purpose of this study is neither to make a ruling on the collaboration nor to offer prescriptive judgments to scientists. Instead, this study adopts a future-oriented stance to elucidate the factors that operate in contemporary scientific practice and to draw lessons or methodologies from the case that working scientists can refer to and utilize.<sup>\*6</sup>

Thus, I take a naturalistic stance that inquires into knowledge acquisition methods continuously with the context in which scientists conduct and reflect on their research. In this point, the direction of this study is different from laboratory studies by anthropologists or sociological studies (Latour and Woolgar 1979, 1986; Collins 1985) in which there is little awareness of exploring how knowledge production activities should be.<sup>\*7</sup> Meanwhile, some literatures from social studies of science will be referred and considered as long as they can be linked to discussions in this study in a meaningful way.

The first focus of this paper is the issue of *confirmation*<sup>\*8</sup> or confirmatory work in the collaboration, especially that of *trust*.<sup>\*9</sup> In the field of epistemology, it has been widely recognized that epistemic

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<sup>\*5</sup> In this case, I learned a lot from partial observations and informal conversations in ongoing circumstances (including physicists outside the collaboration) as well as from interviews and discussions after the situation settled down. I then presented my analysis to the scientists and received their feedback. The information was anonymized for this paper and I got confirmation from persons concerned as far as possible for using their information in the study. Note that the analysis presented in this paper is not the official view of the OPERA collaboration and does not represent any subgroup of it, but is of the author. Regarding the method of fieldwork, see Section 4.1.

<sup>\*6</sup> One can also take issue with the communities surrounding collaborations, such as other experiments, research institutes, and the media. However, I focus on the problems of collaboration whose management is relatively easy to modify. That is, it is necessary to consider what working scientists can do even in a harsh environment.

<sup>\*7</sup> Issues concerning social constructionism or relativism are not addressed in this paper. For related discussions, see Franklin and Perovic (2023), Boyd (2021), and Suzuki and Naganawa (2022).

<sup>\*8</sup> In this paper, the word “confirmation” or “verification” is used not in the sense of proving something is true, but to mean checking or examining whether something is true or in proper condition.

<sup>\*9</sup> In the field of philosophy, scholars consider various aspects of trust such as belief, will and emotion. It is also argued

dependence (or reliance on the work of others) is inevitable in scientific practice where research is commonly carried out by groups. In relation to that, trust in science and the nature of collective knowledge are the subjects of much discussion (Hardwig 1985, 1991; Lackey 2014; Fricker et al. 2019; Simon 2020). However, it is not thoroughly investigated *in what condition, in what manner, and to what extent* trust matters or strict internal checks (i.e., cross-checking, such as independent measurements by other collaborators) should be conducted in the actual context of collaborative science. This study concretizes these points.

Subsequently, this paper addresses the issue of *publication*, or how a result is made public, which in this case matters no less than the issue of confirmation. As we will see, the way of presenting a result is crucial for collaborative knowledge production; that is, the methodology of presentation has significance. This study presents a model of decision-making for publication and shows how the issue of publication intersects with the issue of confirmation. Seeing the intersection is important both for scientific practice and for its understanding.

The remainder of this paper is structured as follows. Section 2 provides an overview of the OPERA experiment and its neutrino velocity measurement. Section 3 analyzes a lesson of cross-checking based on the reflections of several OPERA collaborators, while Section 4 locates the view on the necessity of strict checks into existing philosophical studies. Section 5 specifies the applicable condition of the lesson (i.e., when to go beyond trust), which is to analyze the methodology with limited resources. Section 6 addresses the problem of publication and indicates some points to be cared in collaborative knowledge production, with the model of decision-making for publication. Section 7 describes the scope and limitations of the model. Finally, Section 8 provides general remarks and prospects regarding epistemology, while Section 9 concludes the paper.

## 2 Overview of the OPERA experiment and its neutrino velocity

### measurement

#### 2.1 The OPERA experiment

The OPERA experiment was designed for the purpose of definitely confirming the neutrino oscillation,

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whether there is a sense in which one trusts in technological artifacts besides merely relying on them (Simon 2020). This study does not commit to any specific theory of trust, assuming that its general usage (including that of working scientists) is basically consistent with various philosophical accounts of trust. That is, in the context of scientific practice, a scientist trusts another person roughly in the sense that the former believes what the latter says or does, usually based on the latter's competence and honesty. This implies that one relies on the work of others without checking them by oneself, especially without carrying out an independent measurement to make sure.

a phenomenon in which neutrinos change from one type (flavor) to another while flying, by directly detecting the appeared tau neutrinos. In this experiment, muon neutrinos were produced by injecting protons into a carbon target at an accelerator of CERN (Conseil Européen pour la Recherche Nucléaire / the European Council for Nuclear Research) in Switzerland (Guler et al. 2000). The beam was directed toward the OPERA detector situated underground at the Gran Sasso National Laboratory (LNGS) of INFN (Istituto Nazionale di Fisica Nucleare / the National Institute for Nuclear Physics) in Italy, approximately 730 km away. This detector consisted of about 150,000 emulsion cloud chambers (ECCs), in which sheets of nuclear emulsions (a kind of photographic film) and lead plates are alternately stacked, and electrical detectors. The neutrino interactions in the detector produce charged particles. The tracks of these particles in nuclear emulsions were analyzed with automatic scanning devices and the type of neutrinos was identified through detailed analysis.

The OPERA collaboration was composed of approximately 160 researchers from 11 countries (as of 2011), mainly in Europe and Japan. Its proper beam radiation began in 2008. The collaboration achieved their main purpose in 2015 by detecting five tau neutrino interactions, which were statistically sufficient for discovery (Agafonova et al. 2015). As a final result, five more tau neutrino events were added (Agafonova et al. 2018).

Meanwhile, the OPERA collaboration also conducted the measurement of neutrino velocity utilizing the long baseline, which is the target of this article. This measurement was initiated by a part of the collaboration, and nuclear emulsions did not play an important role there; that is, the neutrino velocity measurement was a *byproduct* for the OPERA experiment. However, it drew more worldwide attention than the oscillation analysis by obtaining and presenting an anomalous result in 2011.

## 2.2 The neutrino velocity measurement by OPERA

Prior to the OPERA experiment the velocity of neutrinos had not been precisely measured at the laboratory level, while the MINOS (Main Injector Neutrino Oscillation Search) experiment presented a statistically insignificant result in 2007 (Adamson et al. 2007). Some OPERA collaborators began preparations for the measurement in 2003 and upgraded the time synchronization system with a global positioning system (GPS) receiver and a cesium (Cs) atomic clock in 2008 for the precise measurement. The time of the atomic clocks in the two institutes about 730 km away was synchronized at the 1 ns level, and the flight distance was determined with 20 cm uncertainty.

The OPERA collaboration analyzed the data for about 16,000 neutrino events and obtained the unexpected result which indicates that the neutrinos arrived approximately 60 ns earlier than expected for the speed of light in vacuum; that is, neutrinos are about 0.0025% faster than light.

The neutrino velocity measurement was originally the theme of a collaborator's dissertation, the defense of which was held in May 2011. In June, the result was shared with OPERA collaborators at a

collaboration meeting, and a task force was organized to conduct internal checks.

In this measurement, it was necessary to accurately determine the baseline (distance), the neutrino production time on the accelerator side, and the neutrino interaction time on the OPERA detector side. For instance, the reliability of the GPS and the statistical method concerning time distributions of events mattered. The response characteristics of the apparatus and the signal delays of the cables needed to be precisely understood, regarding both that are used in CERN and in LNGS. Other external factors that can affect the result were also assessed. (Although there are many technical details, this study describes only the necessary experimental contents for understanding the following discussions.)

For several months afterward (the first checking period), confirmation activities found no errors or effects that could explain the result of superluminal neutrinos. At the collaboration meeting in September, the representatives of OPERA decided to make public the result in a particular way. Thus, on September 23, 2011, the OPERA collaboration held a seminar at CERN and a press release, while the paper, which was signed by most of the collaborators, was published in the electronic preprint arXiv (Adam et al. 2011). The result was not presented as a final result, but the purpose of inviting scrutiny and the continuation of studies were mentioned.<sup>\*10</sup>

After the public release, the main focus was the beam, or the issues on the accelerator side, for both OPERA collaborators and external scientists. Specifically, since the spill width (i.e., the extraction time) of the proton beam was 10.5  $\mu\text{s}$ , which is relatively long in the nanosecond-level measurement, the correspondence between protons on the accelerator side and neutrino events on the OPERA detector side mattered. In response, from October to November 2011, the beam was switched to a bunched beam that makes short pulses with less indefiniteness. The superluminal result did not change, and the revised paper was submitted to a peer-reviewed journal, *The Journal of High Energy Physics*.

Afterward, intensive checks of the OPERA detector were carried out from December 2011 to February 2012 (the second checking period), when the beam was stopped by the accelerator shutdown in the winter. The problematic points were found there. Two problems were found in the OPERA detector side, and what was crucial for the faster-than-light result was the optical fiber problem. That is, a delay of 74 ns had been occurred in the 8.3 km optical fiber that sent a GPS signal to the underground OPERA master clock at the LNGS. The problem was that the connector of the fiber was incorrectly plugged in. That created a slight gap between the fiber and the light detector (the photodiode), causing a decrease in the amount of light that the detector received. Consequently, an electrical voltage was delayed in reaching a threshold and the master clock registered the arrival time of the signal with the delay. Thus, the clock of the OPERA detector

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<sup>\*10</sup> Cf. the following comment: "Given the potential far-reaching consequences of such a result, independent measurements are needed before the effect can either be refuted or firmly established. This is why the OPERA collaboration has decided to open the result to broader scrutiny" (CERN press release 2011). Meanwhile, some confidence in the measurement itself can be observed in the press release. Indeed, they have analyzed about 16,000 neutrino events and obtained a significance of 6  $\sigma$ , while 5  $\sigma$  is normally required for claiming a "discovery."

side became slower by several tens of nanoseconds, making appeared the arrival time of the neutrinos earlier, i.e., their speed faster, to that extent.<sup>\*11</sup>

Subsequently, working together with the LVD (Large Volume Detector) experiment conducted at the LNGS (Agafonova et al. 2012), from the change in the timing of the cosmic ray muons that penetrated the two experiments, it is revealed that the fiber connection problem had occurred in around mid-2008 (the fiber is considered to have been improperly reconnected by someone after instruments had been checked in 2007). The report on the two problems and the joint research was made in a workshop at the LNGS on March 28, 2012.<sup>\*12</sup>

Finally, after fixing the problematic points, the bunched beam was shot in May 2012. In Neutrino 2012 (The XXV International Conference on Neutrino Physics and Astrophysics) held in June, the result that the speed of neutrinos was consistent with that of light was presented with reports from other experiments. In October of that year, the paper with the corrected result was published (Adam et al. 2012). It is stated there that “[t]he results of the study using CNGS muon neutrinos with an average energy of 17 GeV indicate a neutrino arrival time compatible within errors to the one computed by assuming the speed of light in vacuum” (p. 29).

### 3 The structure of trust and a lesson for cross-checking

In OPERA’s velocity measurement, the most critical part that affected the result was the optical fiber. However, there was another problematic part; the master clock. I examine the circumstances regarding the latter, where an issue of trust is clearly observed, and draw a lesson for cross-checking, that is, how and to what extent the checking by another person or subgroup in the collaboration should be performed. A similar analysis is possible for the fiber problem, although the situation regarding the part is relatively unclear.

Note that despite the fact that the effect of the master clock problem made the flight time of the neutrinos appear 15 ns longer (i.e., when corrected, the speed of the neutrinos increases), the problem is no less important than the fiber problem in that the part was not thoroughly checked and there was a non-negligible error. If the effect had been on the opposite side, then it could have been critical for the result.

#### 3.1 The necessity of actual measurement

The master clock problem consisted of two points: (1) the drift of the master clock occurred since the

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<sup>\*11</sup> The problem was noticed from the work that took off each fiber and remeasured it. After reconnection, a time difference occurred, with the place of the fiber being different from the one seen in a photograph taken before. Another problem, i.e., the time drift of the OPERA master clock, will be discussed in the next section. For a helpful explanation of the two problems, see Strassler (2012).

<sup>\*12</sup> Afterward, OPERA’s spokesperson and physics coordinator stepped down (Reich 2012).

frequency of the oscillator did not correspond to its specification, and (2) its effect became non-negligible because the reset of the clock (i.e., synchronization with the GPS) was not by the planned 1 ms but by 600 ms (the cycle of data acquisition).

Regarding this problem, a representative of an OPERA subgroup that got involved with cross-checking for the neutrino velocity measurement stated,<sup>\*13</sup> “Regarding the latter [the master clock problem], it was the part where the Japanese group pointed out severely during the first checking period, but due to also the assumption of the person in charge in Europe, it was thought to be no problem because of 1 ms reset [(2) of the above], and we were persuaded by that ‘explanation,’ too ... What is common to both [the fiber problem and the master clock problem] is that there were errors in parts where we were persuaded by ‘logic’ or ‘explanation.’ That made us realize again that as long as we are experimenters, it is important to measure honestly where the measurement is possible.” (Nakamura 2012, translated and supplemented by the author). He thus expressed regret for their own role concerning verification and referred to the importance of the actual measurement. Besides, the person engaged in cross-checking in the subgroup reflected that at least they should have required *the evidence* (in this case, evidence of 1 ms reset) more critically.

According to “the person in charge in Europe,” with respect to the reset ((2) above), it was not made in accordance with the design by an engineer, and he regretted the trust to the engineer stating that he should have followed till the end his tough nature that never trust.<sup>\*14</sup> He also mentioned that the 1 ms synchronization was overkilling design and that the more problematic point was that the frequency of the oscillator used in the measurement did not correspond to the specification ((1) above). Actually, two spare oscillators of the same type did correspond to the specification. Thus, the lesson for checking that the actual measurement should be done applies here too, and we can add that the actual measurement of *the real thing* used in the experiment is important.

The same lesson can be drawn regarding the fiber problem. The person in charge above stated that he trusted that the collaborators (or someone) could not manipulate parts of the system by detaching and badly reconnecting cables without warning any expert. Also, as the representative of a subgroup reflected, the actual measurement was important in cross-checking for the fiber, too. Although fibers had been measured in different ways before 2008, independent measurements were not carried out in the first checking period. Thus, we can say that the actual measurement is required beyond trust.

### 3.2 Trust in the context: Several qualifications

However, we should be careful in stressing or accepting unqualifiedly the point that it is problematic

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<sup>\*13</sup> One person in the subgroup was a member of the OPERA task force. Note that the comment below is not an official statement of the subgroup, and there are various opinions even inside it.

<sup>\*14</sup> Personal correspondence on September 25 and October 18, 2013.

to trust others by their words, that is, by “logic or explanation.” First, scientists cannot help but trust their colleagues at several points in the collaborative work that has a huge amount and many kinds of tasks and requires division of labor. Conducting actual measurements at every point is unrealistic in terms of *the limitation of resources* (time and manpower). This issue is discussed in more detail in Section 5.

Second, simply not trusting others can naturally lead to trusting only oneself, that is self-reliance or overestimating oneself, which is epistemologically risky. The point is that while a certain degree of skepticism (nontrusting attitude) on the checking side is desirable, trust by *the checked side*<sup>\*15</sup> in those who conduct checks is indispensable for elaborate cross-checking. Indeed, it is important for those who are checked to not trust themselves and to urge others to conduct cross-checking by opening the information fully and listening sincerely to criticisms by others (trusting their ability and sincerity). Moreover, with regard to “logic or explanation,” discussions must not be refrained from, but rather need to be conducted thoroughly and critically, sparing sufficient time and effort until the part where the actual measurement was not done can be pointed out.<sup>\*16</sup> Much like trust, cross-checking involves *interpersonal* relationships, and the attitude of both sides of the checking activity matters.

It should be noted that a scientist belonging to the checking side is sometimes the one who is checked as well. The person in charge in Europe was on the checking side relative to the engineer but was on the checked side relative to the task force and other collaborators. Also, the person of the task force was the checked person in relation to other collaborators. Hence, it is important for each scientist to adopt a proper attitude depending on the side they belong to regarding the point at issue. Consistent trust or distrust can be detrimental to collaborative knowledge production.

In sum, the lesson for cross-checking is as follows. For the checking part, “Do not simply trust the checked part, but independently measure the real thing (or at least critically require evidence),” and for the checked part, “Do not trust yourself, and open the information fully to the checking part.” Scientists are required to use these tactics depending on which part they belong to with respect to the matter in question.

However, it is hard to always take those attitudes (especially for the checking part), and it is not necessarily desirable in view of limitations of resources. That is, checking work must be done with limited people and time. In Section 5, we consider when to apply the aforementioned lesson and develop it into a

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<sup>\*15</sup> Regarding the concept of the checked (or checking) side, it is natural to think from the viewpoint of division of labor. That is, when one is in charge of a task, especially takes raw data working closely with the instrument, the person is on the checked side (and other collaborators are on the checking side) for it. However, in actual practice, it can be unclear whether an individual belongs to the cross-checked part or the cross-checking part, as described below.

<sup>\*16</sup> Indeed, some collaborators reflected that the other collaborators could not fully access information and that there was not enough time to do so. A senior collaborator pointed out that richer internal notes describing the technologies for collaborators could have made internal checks smoother. Although it is an important question what influences information and communication technologies (e.g., video meetings and e-mails) have on collaborative checking activities, it is not pursued in this study.

methodology, or a practical knowledge acquiring method, for conducting or managing an experiment *with limited resources*.

Before proceeding to that point, we discuss in the next section what can be said, from the viewpoint of the lesson of cross-checking, about previous research on trust by scholars on science. The point is that independent measurement is required on some occasion (which will be specified later) even in the collaborative experiment where division of labor and epistemic dependence are common.

#### 4 Managing epistemic dependence in collaborative science

Scientists, especially experimenters, are required as experts to provide reliable and novel (or significant) information in their research field, which also has importance for society. Accordingly, one may expect that they have stricter standards than ordinary people for scrutinizing experimental results and accepting knowledge claims.

Meanwhile, in contemporary science, most of the research is conducted by groups with division of labor requiring different expertise, in order to handle many and diverse tasks. In such a cooperative structure, there is epistemic dependence, that is, the reliance on the work by others within a research team (as well as others in the scientific community) and no one person is able to fully understand the whole research. In the background, there are the constraints that scientists must obtain valid results *with limited resources*. In fact, if they check each other's work thoroughly, then it would take too much time and effort. Moreover, even if one researcher *trusts* the others, a certain degree of reliability can be ensured. Hence, it is rational to trust to that extent both for knowledge production by the research team and for scientific progress in general.

Epistemology was traditionally individualistic in its nature, putting value on thinking for oneself and requiring vigilance rather than trusting. However, contemporary scholars have argued that even in science where skepticism is an ethos, epistemic dependence and trust are common (Hardwig 1985, 1991; Fricker 2002; Simon 2020; de Ridder 2022; O'Connor et al. 2024). Then it becomes a question whether a person, a scientist in this context, can be said to have scientific knowledge whose justification is not fully possessed by the individual alone. On the one hand, one may think that the individual scientist can *know* a scientific proposition without having evidence of it by oneself, typically on the basis of testimony of others. On the other hand, it is argued that scientific groups (teams or communities), not individual scientists, *know* the proposition as its evidence is possessed by the groups. In relation to this, issues of *collective knowledge* in science that is not reducible to individual knowledge have been much discussed in social epistemology (Lackey 2014; Fricker et al. 2019).

Nevertheless, there are relatively few case studies on concrete situations concerning trust that step into the actual scenes of scientific practice by fieldwork. As we observed, scientists do not necessarily and

should not simply trust their collaborators based on trustworthiness, at least on some occasions. Then the following questions arise: In which cases do and should scientists trust (or distrust) their collaborators? If they do not simply trust by words, what tactics do they have to increase the trustworthiness of their colleagues or ensure the reliability of work by others?

In considering these questions, it is useful to refer to Wagenknecht (2015) that conducted an empirical case study of a collaborating scientific practice by an interdisciplinary team.<sup>\*17</sup> Critically examining her views, I situate my analysis in philosophical discussions based on the lesson of cross-checking in Section 3.

#### 4.1 Dialoguing practices and explanatory responsiveness

Wagenknecht observed that epistemic trust is inherently incomplete and that scientists do not completely trust in their practice. That is, epistemic trust does not solely solve the problem of epistemic dependence in the sense that scientists cannot tell which testimony to rely on. She also pointed out that scientists resort to particular strategies, such as dialoguing practices and the probing of explanatory responsiveness, to mitigate the limitations of epistemic trust.

*Dialoguing practices* are practices in which a scientist asks questions to another scientist (expertise), and the latter explains these questions and has them understood. Even though the scientist asking the questions lacks disciplinary expertise, that scientist is able to measure the explanations against certain expectations. These expectations can be founded on experience, but they also concern the argumentative structure of the explanation: “Does the explanation offered sound ‘rational and logical’?” (Wagenknecht 2015, p. 172).

In dialoguing practices, a speaker’s ability that Wagenknecht calls *explanatory responsiveness*, or “the willingness and skill to address the listener’s epistemic needs” plays an important role. In this regard, an explanatorily responsive speaker commits themselves to the listener’s informational needs. Only with their professional sensitivity for dynamics of dialoguing can the addressee perceive the explanations as complete. Thus, the speaker helps the listener assess the explanation on the grounds of background knowledge.

According to Wagenknecht, these strategies serve to indirectly assess the quality of a contribution made by a collaborator and to increase the collaborator’s trustworthiness. Thus, she argued, scientists

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<sup>\*17</sup> Wagenknecht employed qualitative methods, especially interviews, coding and interpreting the data (cf. Wagenknecht 2016, Chapter 3). I basically agree with her approach of going back and forth between concrete empirical insights (on scientific practice) and abstract philosophical concepts. Although I conducted some interviews as well, I did not directly quote them. There were circumstances that it was difficult to hold formal interviews with recording in this case, when things were going on. Meanwhile, informal conversations with scientists were of great help. With regard to handling what scientists said as ‘data,’ there are several issues to be cared, e.g., differences in opinion among practitioners and changes in their recognitions (during the course of time or as a result of opinion exchanges with the analyst and other collaborators). These issues of research method cannot be discussed here (for introducing empirical method, see Wagenknecht et al 2015). My general stance is that the naturalistic approach, rather than a priori arguments, is fruitful that improves fieldwork while conducting it, aiming to refine it as an empirical method.

manage epistemic dependence, which should be taken seriously since it reflects their professional experiences.

## 4.2 Limitations of dialoguing practices and going beyond trust

Although the analysis of the case of the neutrino velocity measurement in Section 3 supports the incompleteness of trust and the existence of dialoguing practices as actual activities, it also indicates that dialoguing practices are insufficient *as strategies* for epistemic scrutiny. In this regard, recall the statement of an OPERA collaborator regarding the case: “there were errors in parts where we were persuaded by ‘logic’ or ‘explanation.’” In that case, it is a point of reflection or regret that the scientists took the explanation and understanding for ‘cross-checking.’ Borrowing an expression of a member of the task force, in the first checking period “‘cross-checking’ was not actually cross-checking,” the latter meaning the strict check, such as an independent measurement.

Admittedly, dialoguing practices make it possible to assess the speaker by criteria such as coherence and whether important points are addressed, even if the ‘listener’ or the checking part cannot understand the details of the point at issue. Such probing processes enable scientists to take a more careful stance, i.e., a healthy skepticism, than simply trusting their collaborators as trustworthy. Furthermore, some errors can be removed due to the revelation of problematic points in these processes. However, these practices are basically ‘confirmation’ work that is insufficient for or weak as cross-checking (if we admit that cross-checking has a degree of strength). An important lesson in our case is that scientists should recognize that dialoguing practices are only the *next best* activities to corroborate or falsify results.

As for the role of explanatory responsiveness, if it is understood as a requirement for the speaker, it seems consistent with my view on the lesson of cross-checking for the checked part, that is, not to fall into self-reliance but open the information fully. However, there is an important difference.

The question is whether explanatory responsiveness ensures the reliability of a knowledge claim. If the person who asks the question lacks expertise (as normally assumed in the setting of epistemic dependence), they are expected to be unable to adequately judge what is ‘important’ and what response is ‘proper’ with regard to the issue. As long as the questioner’s epistemic needs are not appropriate, it is insufficient that the respondent addresses them. Even though understanding the explanation does not entail accepting it, it is problematic that the listener can be deceived by the response skills of the speaker, sharing (possibly wrong) assumptions of the latter. Rather, it may be epistemologically harmful that the speaker possesses sensitivity and is good at explanation.

Accordingly, it is problematic to regard dialoguing practices and explanatory responsiveness as tactics of checking.<sup>\*18</sup> At least in some contexts, “logic” or explanation by words has a limitation, and not

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<sup>\*18</sup> “Impersonal trust” and “hierarchical authorship,” which Wagenknecht (2015) finds supplementary functions, are also insufficient to do the job of strong cross-checking. The problem of authorship requires further analysis in relation to

questions for understanding but critical discussions that lead to the acquisition of further evidence are necessary.

Notably, in light of interpersonal relationships in checking activities, a more active attitude should be put significance for the checking part; on this side, it is desirable to have the possibility to surpass the speaker or the checked part exposing their wrong assumptions, pointing out problems or directing them to further confirmatory work by critically requiring evidence. Meanwhile, on the checked side that explains, it is important to have the willingness to conduct additional confirmatory work as needed and to take the stance that they are ready for them. Furthermore, if the checking part feels that confirmatory work is insufficient, then they may embark on independent measurement and step into the area of the person in charge or even take the place of the checked part (so to speak, it is “invading another’s territory”).

Thus, to ensure the reliability of a knowledge claim toward epistemic dependence and incompleteness of trust, scientists are occasionally required not to indirectly assess their work or increase the trustworthiness of their collaborators but to undertake independent checks *beyond trust* while spending additional resources if necessary.

#### 4.3 Keeping the option of independent checks in view

It may be objected that as Wagenknecht focuses on cases where independent checks are impossible taking interdisciplinary collaboration as an object, the above considerations do not make objections and are at most supplementary. I admit that the lesson of strict cross-checking has qualifications as a methodology with limited resources for the condition of application, as we will see in the next section.

Moreover, we may admit that our observations regarding cross-checking do not necessarily deny Wagenknecht’s view but rather take it one step further. She argues that in collaborative scientific practice, trust is inherently incomplete, and dialoguing practices are required besides simple trust (based on trustworthiness). I take considerations of more careful tactics of scientists for when to *go beyond trust* and proceed to stronger cross-checking. Nevertheless, keeping the option of independent measurement in view makes a difference both for how to conduct collaborative work and for how to see scientific collaboration.

In the first place, even if one takes up a high-energy physics experiment (of ‘one’ field, or a monodisciplinary research group), a variety of tasks regarding detectors and analysis are carried out with division of labor that requires different types of expertise among collaborators. Hence, they must more or less go beyond their expertise and acquire others according to the circumstances of the experiment. In OPERA’s neutrino velocity measurement, because it was not the main purpose and an unexpected result was obtained, the necessity of learning measurement methods arose among collaborators other than those who had been centrally engaged in it. Consequently, some collaborators contributed to reveal erroneous points by embarking on independent measurements in the second checking period. The effort and time that

cost for acquiring the ability in question are a *matter of degree* there, and stronger checks can be required in accordance with that degree and necessity. That is, asymmetry in knowledge or differences in expertise is not necessarily fixed but can be dissolved in science in-the-making. Therefore, simply framing a case in terms of whether independent checks are possible does not match reality.<sup>\*19</sup>

Moreover, even when it is impracticable to directly assess others' work, there can be situations where one is required to go beyond dialoguing practices from the nature of the issue (e.g., obtaining an unexpected result), and it is also possible to do so to a certain extent. For example, the following tactics are considered: requiring stronger evidence, calling for the cooperation of other experts in that field, scrutinizing the result taking time (obtaining further resources), and limiting or refraining from the presentation as its check is insufficient. It is beneficial for practitioners of science to take into account these various coping methods in view of the difficult and unstable situations that they can be involved in. (Some of these tactics are explored in more detail in the following sections.)

Furthermore, a fundamental point regarding the situation of collaborative science should be noted in relation to the option of independent checks: it is not plausible that one may trust *a scientist* if the person is competent and honest or positively assessed by dialoguing practices and explanatorily responsive.

If the science is truly cutting-edge, it is highly likely that scientists would use methods that have never been tried before and confront unknown phenomena. In particular, if the scale of the experiment is large, as instruments and analysis methods are complex, what a scientist (or a subgroup) can comprehend is limited no matter how competent the person is. Rather, it is appropriate to think that a scientist can make a serious (elementary and critical) mistake even if the person is trustworthy regarding the part and withstands indirect assessments. Then, if a speaker or trustee fails to notice something regarding technologies or natural effects, the information becomes unreliable, as demonstrated by the two problems in the OPERA detector. This presents a serious problem for scientists, at least for those who try to be leading scientists.

Fukushima (2017) discusses what he calls "defense systems" in science, i.e., laboratory governance, referee system, and replication, and describes how scientific misconduct pass through them. It must be emphasized that the case of neutrino velocity measurement is not a case of scientific misconduct, but the analysis here is applicable to the latter to some extent. In particular, regarding the lesson for the checking part, independent measurement by a collaborator or lab colleague is also effective (and dialoguing practices have a limitation) for revealing scientific misconduct such as fabrication. In relation to the point, it is worth noting that the importance of checking within the group is not limited to high-energy physics or large-scale experiments. Certainly, it is quite likely that confirmatory work in the collaboration is crucial in the

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<sup>\*19</sup> Wagenknecht (2014) acknowledges "the gray zone" between epistemic dependence where a scientist does not possess the expertise necessary to independently carry out (and profoundly assess) the piece of scientific labor their colleague is contributing and where the scientist does possess such expertise ("opaque and translucent epistemic dependence"). Nevertheless, she does not discuss the modification of epistemic dependence in fluid situations and several tactics in accordance with the degree of dependence.

large-scale experiments as replication is practically difficult. However, as Fukushima observes, replication is sometimes difficult even in the field of biology due to the differences in properties of experimental apparatus or research materials (pp. 241-2), and therefore we can say that checking within the group is also important in relatively small-scale experiments.

The point is that in *every scientific research*, especially in contemporary collaborative practice, one can be required to go beyond indirect strategies such as dialoguing practices to a greater or lesser degree depending on the situation and therefore to carry out independent checks at times. The next section specifies some conditions in which strict checks, as independent measurements, should be conducted.

## 5 When to go beyond trust: an applicable condition as methodology

The lesson for the checking part in Section 3, that is, ‘Do not trust by words, but actually measure the real thing,’ is not sufficient as a *methodology*, a tactic to conduct an experiment *with limited resources*. It is impractical, at least in every case, to cross-check all parts of a large-scale experiment with a huge amount of task and various divisions of labor (e.g., research and development of detectors and analysis of various events), as experiments have to be conducted with limited manpower and time. Here lies the issue of balance between reliability and resources.

In actual scientific practice, there are parts where one has to trust specialists or those in charge, yet many large-scale experiments can avoid critical mistakes. Hence, what is important is to judge appropriately *in what case and to what extent* strict checks such as independent measurements are required, or trust is permissible, in collaborative practice. This is to develop the lesson of cross-checking into the methodology of cross-checking. Below I present two points concerning an applicable condition of independent measurements from the analysis of the neutrino velocity case: the extraordinariness of the result and comprehensive understanding of technologies.

### 5.1 The extraordinariness of the result

Regarding the result of the ‘superluminal’ neutrino in our case, an epistemological principle was sometimes mentioned —*extraordinary claims require extraordinary evidence* (ECREE). This phrase was spread by an astronomer and science fiction writer Carl Sagan. While it can be point in dispute what this phrase exactly means,<sup>\*20</sup> a reasonable reading is that *consistency with already accepted knowledge system* matters in the acceptance of a knowledge claim. In this sense, an extraordinary claim can be assimilated

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<sup>\*20</sup> For instance, the quotation by an astrophysicist is introduced in Nosengo (2011). Deming (2016) understands it in terms of the existence of the large number of contrary observations. It may be expressed in terms of statistics. The issue will be complicated if we take theoretical elements into consideration and apply the principle (with degree) when the claim is at odds with, but not directly contradicted by, existing evidence (e.g., when some differences exist in the research target or in the experimental condition). We cannot address these points here.

into an “anomaly” that is quite unexpected by the scientific community. In particular, one has reason to doubt a claim to the degree that it is inconsistent with the background knowledge system and that the knowledge system is established by evidence, as the claim is implausible in accordance with that degree. (For example, we rationally require more evidence for such a claim as “I saw a ghost” than for “I saw a tree” in the ordinal situation.)

Indeed, the physics community, including the OPERA collaborators themselves, more or less held the view that the claim that neutrino is faster than light in a vacuum was extraordinary or anomalous in consideration of well-confirmed scientific knowledge: special relativity, the observation of the supernova SN 1987A in which neutrinos arrived almost at the same time as light (Hirata et al. 1987), and so on. (Meanwhile, there were some differences of opinions with regard to *the extent to which* it is extraordinary. For instance, some theory might be specially devised for the superluminal neutrino. Moreover, the result of the MINOS experiment, which had hinted at the faster-than-light neutrino at a statistically insignificant level (Adamson et al. 2007), might have influenced considerations on extraordinariness, at least after OPERA obtained the result.)

We can say that concerning such an extraordinary claim or result,<sup>\*21</sup> more strict checks than usual are epistemologically required, as it is highly probable that something is wrong.

The ECREE indicates that how much confirmatory or checking activities one is required to do varies in proportion to how much extraordinary a claim is. This has a practical implication: experimenters need to have, in addition to the recognition of the principle itself, some view on how much likely or unlikely their results are in order to conduct proper confirmatory work. Accordingly, although statements such as ‘experimenters should be loyal to their own data and not be biased by theories’ seem sound, this does not justify for experimenters to concentrate solely on their own experiments.<sup>\*22</sup>

Meanwhile, some caution is needed with regard to the ECREE. First, the OPERA collaboration recognized that their result was anomalous and conducted confirmatory activities conforming to the principle, at least to some extent. We will see this point in Section 5.2. Second, as Deming (2016) points out, the misuse of “aphorism,” for instance, to simply apply the ECREE just because a result is unexpected for the researcher, can suppress novel findings. Excessive conservativeness is no less detrimental to scientific progress than the presence of too many unreliable ‘anomalies.’ Third, which is important for practitioners, the ECREE should not be taken for meaning that strict cross-checking is necessary only when the result is quite inconsistent with the accepted knowledge system. Even for claims that are not completely extraordinary, independent checks are sometimes mandatory and actually conducted in collaborative

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<sup>\*21</sup> It can be said that OPERA did not *claim* the superluminal neutrino, which is related to the problem of publication. See Section 6.

<sup>\*22</sup> It can be said that even ‘experimenters’ need to have a *view of nature* in light of theories and results of other experiments or observations. It would be beneficial to have connections with theorists or scientists in other experiments, as the review and assessment of anomalous results may be beyond the capacity (or resources) of a collaboration.

experiments, especially when they have importance. One such example is the tau neutrino candidate event in OPERA's neutrino oscillation analysis.

In sum, although the ECREE is basically right in the sense that *stricter* checks are required for results that are anomalous than for those that are not (other things being equal), it is just one of the indicators to be taken into account. While incorporating the principle into the condition regarding the methodology of cross-checking,<sup>\*23</sup> we must be careful for emphasizing it in consideration of possible demerits.

## 5.2 Comprehensive understanding of technologies

Certainly, experiments can call for new theories by presenting anomalous or unexpected phenomena, but it is crucial there to have a well-established experimental system, i.e., that system is reliable enough.<sup>\*24</sup> This relates to another point regarding the condition of cross-checking: *comprehensive understanding* of technologies, that is, to have intimate knowledge of every instrument used in the experiment and to grasp their behavior and proper use. This affects the confirmation ability of the group, or how much evidence they can obtain, and is also related to resources, i.e., the time and effort that scientists invest to acquire the understanding.

In OPERA's neutrino velocity measurement, there was a circumstance that it was a byproduct for the experiment: its main purpose was to obtain the decisive answer to neutrino oscillation by detecting appeared tau neutrinos. As for the analysis of neutrino interactions, the OPERA collaboration had an accumulation of experience from past experiments in which some of the members had participated (Eskut et al. 1997; Nakamura et al. 1999), and accordingly was able to naturally discern parts where cross-checking must be carried out and to what extent. However, as the collaborators were relatively inexperienced in the neutrino velocity measurement with clock synchronization using GPS, that was not the case in this instance. Working with the outside institutions of geodesy and metrology for the important part of measurement, it was difficult for the OPERA collaboration to justly have enough confidence and take full responsibility for the result on its own. (This is not to criticize the challenging of the measurement itself that OPERA developed the method in cooperation with outside institutions.) A senior OPERA collaborator who did not consent to make public the result, put weight on the point that technologies that were "not in one's hands," such as the GPS, were used in the measurement. Hence, the degree to which scientists are acquainted with technologies at issue is crucial for whether to proceed based on trust or not.

Still, in the case of the neutrino velocity measurement, the story is a little more complex than that

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<sup>\*23</sup> The ECREE is related to the methodology. Regarding the case of the neutrino velocity measurement, the OPERA collaboration had planned to use a geostationary satellite for checking of the anomalous result, but this was stopped after problems in the instruments were found (i.e., when the result became nonextraordinary). Here we can see that the balance between the impact of the result and resources required for checking matters.

<sup>\*24</sup> "Reliability" here is used in a general sense, i.e., competence to measure what is to be measured or tendency consistently producing results that are likely to be true (cf. de Ridder 2022; Goldman and Beddor 2021).

something was overlooked in parts where experimenters were not well-acquainted with. Rather, the OPERA collaborators, quite rationally, held great concerns about issues regarding parts such as the accelerator side and GPS and carried out confirmatory work for them. However, critical problems were not found there after all but on the OPERA detector side, that is, parts with which they were relatively familiar. A member of the task force reflected that the more difficult parts (the accelerator side, statistical methods, GPS, and external effects) tended to be concerned with, and it took time to understand the instrumental parts (i.e., equipment of the OPERA detector).

The point is that collaborators had to *spare considerable resources* to parts with which they were not well-acquainted, and could not manage strict cross-checking as independent measurements for the detector side in the first checking period.<sup>\*25</sup> Hence, a comprehensive understanding of technologies matters; if experimenters grasp the entire experimental system, they can readily tell which points are to be checked and how. This means that confirmatory work can be accomplished with relatively few time and effort, or intensive checks become more feasible devoting resources to the points that concern them. In addition, if experimenters have the sufficient self-recognition that they have not achieved comprehensive understanding, it enables them to handle the obtained result carefully, for instance, taking time to understand and check (or being cautious about making public the result). Thus, we can say that if the understanding of technologies is not comprehensive, securing enough resources is all the more required for strict checking and making the system reliable.

In particular, it is impressive in our case that the two problems were both found on the detector side. Knorr-Cetina (1999) describes *self-understanding*, that is, the understanding of the detectors' behavior by experimenters, in terms of epistemic culture of high-energy physics (pp. 55-63). We can see that it is also important as a *norm* for experimenters; detectors ought to be understood and checked thoroughly with priority. Concerning the responsibility of the collaboration, it makes a difference whether the problematic points were found in detectors or in other parts. As the checking of equipment is difficult to conduct for scientists outside the collaboration, it becomes a precondition for presenting the result to the outside. This point is related to the issue of publication, which is investigated in the next section.

In sum, with respect to conditions where the lesson of cross-checking is applied, we can point out that actual measurements, and therefore resources for them, are required *when the result is unlikely in light of*

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<sup>\*25</sup> Another issue was the resource allocation for purposes of the collaboration. The OPERA collaboration was conducting the neutrino oscillation analysis in parallel and did not invest its full resources in the velocity measurement. The collaboration reported the observation of the second tau neutrino candidate at the Neutrino 2012, where the corrected result of the velocity measurement was reported. In terms of the relationship with the main purpose, one of the reasons why the intensive checks of detectors, which involve disjoints of some apparatus, were not carried out in the first checking period was (in addition to the factors discussed in Section 6.3) that if they were carried out before winter when the beam was stopped, it would affect data accumulation for the neutrino oscillation analysis.

*background knowledge and the group has not achieved a comprehensive understanding of (or is not sufficiently experienced in) the technologies used in the experiment.* As the methodology of cross-checking the requirement needs to be taken into account in considering whether to go beyond trust and to what extent. In other words, the balance between the unlikeliness of the result and technological comprehension matters.\*<sup>26</sup>

Accordingly, from the viewpoint of knowledge acquisition methods, it is important for scientists to have a wide perspective for correctly estimating the likeliness of a result and comprehensive understanding of technologies in order to appropriately deal with obtained results. (These can have other benefits for proposing and conducting the experiment.) If the collaboration does not have these, it is urgent to acquire them by obtaining and spending *further resources*. When the comprehensive understanding of technologies is accomplished, experimenters can rightly present the obtained result even if it is extraordinary. In this sense, the point of comprehensive understanding is worthy of emphasis.

These methodological considerations constitute an important finding for studies on collaborative knowledge production in the actual context, elucidating what elements (or variables) are to be involved in confirmatory activities. Working scientists can refer to the tactic with limited resources reflecting on their practices.

Meanwhile, as for the case of OPERA's neutrino velocity measurement, besides the problem of trust and checking, other issues must be analyzed regarding *making public* the result. Some points are indicated with prospects in the following sections.

## 6 The problem of publication and its intersection with confirmation

The problem of confirmation, such as trust or checking in the collaboration, constitutes at most half of the issues in the neutrino velocity case. The problem of *publication*,\*<sup>27</sup> that is, presenting (or not presenting) a result and how it is presented, matters substantially. In this section, I analyze how the issues of confirmation and publication are interrelated in contemporary scientific practice. In particular, I present a model of decision-making for publication that involves both elements of confirmation and publication. This model is expected to be utilized for the consideration of methodologies of making public results. Although the issue of publication is relatively not well-investigated in the epistemology of science, a wide range of factors affecting publication must be examined since they can have a crucial influence on the production of

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\*<sup>26</sup> We can see here the limitation of the statistical significance of a result. It is important to consider how well relevant theories or phenomena are confirmed (which may also be expressed statistically) and how firmly the system with which the result is obtained is established.

\*<sup>27</sup> The word "publication" is used in a broad sense which means making public a result or officially announcing it. That is, it includes not only publishing a paper in a preprint or an article in an academic journal, but also presenting a result at a seminar or a conference talk, possibly with press release.

scientific knowledge.

## 6.1 The limit of the problem of confirmation

The first thing to be mentioned, in a sense obvious, is that even if internal checks (i.e., checks by collaborators) are insufficient, no problem would occur if scientists do not *make public* the result outside the collaboration. Of course, assuming that the collaboration took confirmatory work was sufficiently done (for presenting in some way) based on trust, it would be natural to present the result although it can be epistemologically problematic as we have seen.<sup>\*28</sup> Importantly, however, in the case of neutrino velocity measurement, not every OPERA collaborator thought that internal checks were sufficient when it was decided to present the result.

There are the circumstances as follows. First, some leaders of institutions (subgroups) in the OPERA were against making public the result, and a few collaborators did not sign the paper (in arXiv, an electronic preprint) (cf. Cartlidge 2011a). Second, before the public release, an OPERA subgroup even pointed out as one of concerns that fiber delays needed to be remeasured, which was eventually the critical point. It should be noted that the OPERA collaborators themselves found erroneous points within half a year after the public release (i.e., within a year from March 2011 when the result was obtained) and corrected the result soon. Therefore, despite the abovementioned reflections by some collaborators regarding trust and checking, the confirmation ability of the group itself can be considered as not so problematic. (Indeed, the collaborators reflected on such publication practices as presenting a result and authorship no less than on confirmatory activities with regard to the case.) As a collaboration, there was a certain degree of awareness of the problem concerning confirmatory work before the public release.

The following question then arises: *why the collaboration came to make public the result*, by preprint and seminar in September 2011, in the situation where some collaborators remained skeptical or at least did not have much confidence in the result (in particular, why it was not waited until intensive checks of the detector were over). It seems hasty to announce the result at the stage before investing further resources for checking activities, not from a hindsight of outsider but from the awareness at that time of the group.

Thus, alongside the problem of confirmation, *the problem of publication* or decision-making<sup>\*29</sup> must be addressed. Below, in analyzing decision-making for making public the result, the relationships between the matter of verification and that of publication are elucidated.

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<sup>\*28</sup> The circumstances in which scientists have diverse opinions regarding how and to what extent the result is justified are not confined to the time when the unexpected result is obtained. Staley (2007) observes in his study of the CDF (Collider Detector at Fermilab) collaboration that collaborators disagree over what evidence supports the conclusion (concerning evidence for the top quark) and that agreement is a matter of degree although the collaborators agree on the conclusion and publication of the paper.

<sup>\*29</sup> Strassler (2012) comments on this case that “[t]he issue here is not the scientific errors themselves, but the bad judgment about how to handle a potentially sensational but probably wrong scientific result.”

## 6.2 Making public the result and confirmatory activities

To begin with, it can be said that the OPERA collaboration, or some representatives responsible for decision-making,<sup>\*30</sup> presented the result moderately and carefully. In the publication, the collaboration stressed the provisional characteristic of the result, stating that they announced the result to invite broad scrutiny and that they continued their studies. For instance, the following passage is explicitly included in the paper;

Despite the large significance of the measurement reported here and the stability of the analysis, the potentially great impact of the result motivates the continuation of our studies in order to investigate possible still unknown systematic effects that could explain the observed anomaly. We deliberately do not attempt any theoretical or phenomenological interpretation of the results. (Adam et al. 2011)

These wordings or precautionary statements were one of the reasons why collaborators who did not believe that internal checks were sufficient consented to make public the result.<sup>\*31</sup> Owing to them, it became easier for some collaborators to sign the paper (or these wordings “lower psychological hurdles”).

Nevertheless, the result was reported by the media extensively in sensational ways, for instance, that it may invalidate the cornerstone in Einstein's theory of special relativity that nothing can travel faster than light in a vacuum, or even that time travel may become possible (e.g., Brumfiel 2011 and Palmer 2011b). Accordingly, the result attracted public interest and the movement of the OPERA collaboration thereafter was frequently featured in the news and became the focus of worldwide attention. On this point, OPERA's spokesperson at that time commented: “as a result of the enormous media interest, the OPERA Collaboration found itself under anomalous and in some respects irregular pressure” (Ereditato 2012).

One can certainly discuss the problem with the media here, but I focus on the issue of collaboration to analyze how scientists, as leading scientists, could behave even in difficult circumstances. A collaborator stated that though it was expected that the presentation of the result would cause a sensation, it was unexpected that these precautionary statements were disregarded. Nevertheless, he reflected that this disregard itself, i.e., that the media responds in such ways, should have been expected. From this viewpoint, we can take up an issue of decision-making, in particular, whether the assessment by the collaboration of the impact that the presentation would have was sufficient. (This may affect the reputation of the collaboration when the presented result was wrong.)

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<sup>\*30</sup> The division of roles between senior staff and young researchers (including students) can matter, but it is not analyzed here.

<sup>\*31</sup> It was unusual for a collaboration to write such wordings in a paper. As a different but interesting example, there is a case of KOTO experiment (Shiomi et al. 2021). The collaboration reported anomalous events at Kaon2019 (The International Conference on Kaon Physics 2019) and later uploaded to the website of the conference a file of addendum (Shinohara 2020) which included the sentences such as “We did not claim the observed events as signals.” Those events were finally explained as the backgrounds (Ahn et al. 2021).

Now, from the points of inviting scrutiny and receiving pressure, we can see a structure in which publication is related to confirmatory activities. On the one hand, by presenting the result to the outside, confirmatory work may become more extensive, and the checking activities of the collaboration may be promoted with the cooperation of the scientific community, as seen in the switching to the bunched beam.<sup>\*32</sup> On the other hand, the circumstance with much pressure that the OPERA collaborators felt in, along with the need to explain and respond to the outside, could have hindered their activities (bringing tension within the collaboration).

In either case, we can see that whether and how to present the result, or the ways of handling information, *influence the quality of knowledge production through resources (funds, people, things and time) that can be devoted to confirmatory activities*. Hence, the management of information, including measures to the media and the control of collaborators, can be regarded as an issue of the tactic for conducting experiments with limited resources, that is, the *methodology of presentation*. Especially, more moderate and careful ways of presentation are worth investigating, as discussed in Section 6.4.

In addition, since a result is usually made public with consensus by most of the collaborators or at least the majority of them in the large-scale experiments, it matters whether to consent to announce the result and *sign one's name* when presenting it by the paper. In our case, it needs to be inquired why a large majority of the OPERA collaboration, at least more than 90%, signed on the paper (a preprint and the paper submitted to the journal) despite suspicions by several collaborators. This is the problem of authorship, which can also be analyzed in relation to knowledge production.

Regarding the publication of a result, social or institutional issues are crucial: the decision-making procedure of the collaboration, conventions or customs of publication (including press release and authorship) within the discipline, the credit given to the group that first found something (competition and cooperation among groups), and the collaboration's relationship with research institutes and funding agencies. Note that these elements, which are more or less mentioned below, constitute the foundational structure of collaborative scientific practice including confirmatory activities and can influence it generally, not limited to the case of anomalous results. Analysis that disregards these as 'nonepistemic' will distort our understanding of the scientific enterprise in the actual context and cannot reflect *knowledge acquisition practices* conducted in society.

### 6.3 A model of decision-making for publication

As a general point, both issues of confirmation and publication are involved in the decision-making for making public the result by the collaboration. Here to see how they are so, a working model of the

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<sup>\*32</sup> It matters that *extraordinary evidence requires corresponding resources*. That is, it may be that a single experiment is not able to obtain "extraordinary evidence" for the anomalous result and therefore they have to announce it to the outside at some point.

decision-making for publication is presented based on the neutrino velocity measurement case. Although this model is hypothetical one that is assumed to be refined by further case studies, it is useful for epistemologists and scientists to see the intersection of confirmation and publication and to consider ways of publication, i.e., what to be cared and which way is better.

Whether and how an obtained result should be made public are crucial questions for practitioners of science. The point is, in addition to the importance of the result, whether verification or checking of the result has attained a sufficient level *for publishing it to the outside*, which is critical for judging the acceptability of the publication. The answer to this question differs among scientists depending mainly on two components: *the recognized adequacy of verification* and *the standard that the scientist sets for publication*. It can be understood that basically when the former exceeds or meets the latter, the scientist reasonably decides or consents to make public the result (Fig. 1). (For the time being, I focus on the individual scientists. See Section 7 for issues in collective decision-making.)<sup>\*33</sup>

Note that both confirmation and publication issues are involved here. The recognized adequacy of checking is the scientist's recognition that *how well-confirmed* the result is, which is influenced by the elements discussed in Section 5:

consistency with the accepted knowledge system, reliability of the experimental system (for which how well scientists grasp the technologies used is crucial), and the exclusion of other effects.<sup>\*34</sup>

Meanwhile, the standard that the scientist sets for publication pertains to *how much confirmatory work is required* to make public the result or the requisite degree of verification for publication.<sup>\*35</sup> In the first place, the standard for publication is affected by the conventions and values in the research field.<sup>\*36</sup> In that background, the sense or way of thinking regarding the responsibility of scientists for making public is more or less different depending on the group or the scientist; that is, some may be conservative placing

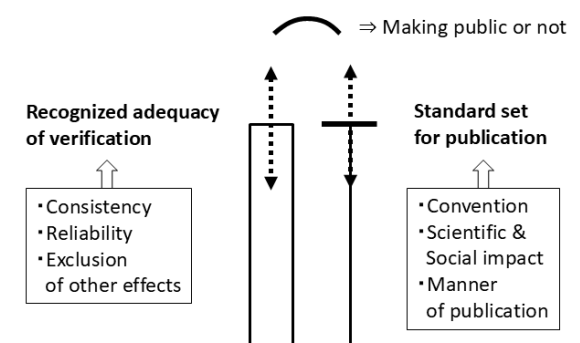


Figure 1. A model of decision-making.

When the recognized adequacy of verification exceeds the standard set for publication, the scientist consents to make public the result.

<sup>\*33</sup> While the decision-making for presentation is examined below, the model can be utilized for considering whether a collaborator consents to sign a paper. It is worth noting here that in OPERA's neutrino velocity case, whether or not to sign was confirmed with each collaborator *one by one*. However, it may be questioned how much a scientist is able to and do decide that by oneself. The issue of authorship has many points to be considered and is discussed in another article (Suzuki 2019).

<sup>\*34</sup> These elements can be revised or analyzed in more detail. See Franklin and Perovic (2023) for a set of strategies that provides good reasons for belief in experimental results (cf. Boyd 2021).

<sup>\*35</sup> Thus, the vertical axis in the figure indicates the subjective *degree* to which the scientist considers the verification to be adequate (in the case of "recognized adequacy of verification") or the verification to be required for publication (in the case of "standard set for publication"). This may be quantified in some way, but it is not attempted in this paper.

<sup>\*36</sup> It is beyond the scope of this paper to elucidate specific details of the conventions (e.g., statistical significance) which can vary depending on the field. Some points concerning differences by research field are mentioned in Section 7.

importance on certainty, while some may be liberal being more open to the possibility of error.

The next factor to be considered regarding the publication standard is the impact the result would have for the scientific community. On the one hand, scientists are responsible (and also motivated) for reporting novel results to the community. On the other hand, they have reasons to be careful for making public such results, e.g., the credibility of scientists matters. These points are described in more detail later.

In addition, as our case indicates, factors such as *the social impact* the publication would have and *the manner of publication* are related to the standard. The potential social impact of presentation provides researchers with the reason to set the standard for publication high in view of the pressure that they would receive. In the neutrino velocity case, social interest in the theory of relativity (or Einstein) is high, and it is relatively easy for the public to understand that neutrinos travel faster than light, compared with some technical characteristics of neutrinos. This provides a reason to be careful in presenting the result, since there is a high risk that the content would be popularized by the media and the collaboration would be exposed to worldwide attention. Especially when the result is wrong, the collaboration may be blamed for disappointing the public. Such an imagination is required for contemporary scientists.

The manner of publication is related to both its *content* and *form*. As mentioned above, regarding content (i.e., expression or wordings) of publication, in the neutrino velocity case, the emphasis on the provisional character of publication is considered to have lowered the standards of some collaborators. Meanwhile, regarding form of publication, the OPERA collaboration presented their result through the preprint and the seminar (that was webcasted) with the press release. Although it is customary in the high-energy physics community to submit a paper to arXiv prior to a peer-reviewed journal (Delfanti 2016), it would be a more cautious measure to hold a seminar after the paper has been accepted by a peer-reviewed journal.<sup>\*37</sup> That is, the extensive announcement of the preliminary result, with its social impact, provides a reason to set a high standard for publication, despite the moderate expression of the result.<sup>\*38</sup>

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<sup>\*37</sup> This is to receive endorsement from the community for making public the result. However, the paper with an anomalous result may be difficult to be accepted. There is another way of publication that announces the result when the paper is submitted to (but is not yet accepted by) a peer-reviewed journal.

<sup>\*38</sup> Sample (2013) presents an anecdote that the ATLAS and CMS collaborations preferred the word ‘observation’ to ‘discovery’ for that of the Higgs boson (cf. CERN press release 2012), and writes, “Prudent scientists tend to claim official discoveries only once their papers are published, that is, after the results have been fully scrutinized, peer reviewed, and given the thumbs up” (pp. 283–4). Subsequently, a reference by the chairman of the Cern Scientific Policy Committee to the case of superluminal neutrinos is presented: “There was the feeling that a mistake, which is always possible in forefront science, could give a fatal blow to the credibility of Cern, of the LHC experiments, and of particle physics at large, and with the recent memory of the case of superluminal neutrinos everybody wanted to be ultra-cautious.”

Before describing further elements that affect publication, it is helpful to see some features of the model. From this model, it is clearly understood that there are roughly two ways for making public the result when confirmatory work is not sufficient (Fig. 2). One is to have incorrect (or overestimating) recognition of the adequacy of verification or checking. That is, scientists can take a result for being sufficiently checked when it is not and make it public. (“Valid degree” in the figure can be understood in terms of what degree is regarded as valid by the scientific specialty community with sufficient relevant information.) The other is due to setting the standard for publication unjustifiably low. In this case scientists decide to present the result supposing its verification is sufficient for the specific publication where it is not, even though they correctly recognize the level of adequacy of verification. In particular, scientists can consent to make public the result (for some reasons to be specified) while thinking that there remains important confirmatory work to be done. Of course, both ways can be included in the collaboration’s decision-making, i.e., due to differences among collaborators, and also a collaborator can have a certain degree of mixing for both reflection points.

It is also understood that scientists with similar views on either the adequacy of verification or the standard for publication can make different decisions on publication because of different opinions about the other (Fig. 3).

Now, to further develop the model, note that scientists do not always present a result when they are convinced by it. As Sugiyama (2005) observed, scientists judge whether or not to make public their findings based on “importance” rather than “certainty.” Especially in high-energy physics, where experiments or observations often take many years to obtain final results, it is common practice that a collaboration presents a temporary result in the middle of the experiment even if the result is statistically insufficient. Papers with such results are often published in peer-reviewed journals and are not generally regarded as problematic (e.g., the paper on the observation of the first tau neutrino candidate event in the OPERA experiment (Agafonova et al. 2010)).

In relation to this custom, there is an important point where problems of confirmation and publication intersect, which is to be added to the model: the *timing* of making public the result (Fig. 4).

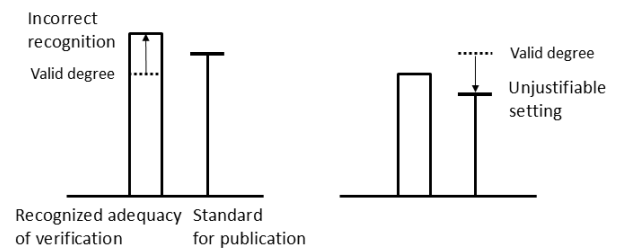


Figure 2. Two ways for making public the result whose verification is insufficient that are represented by the model of decision-making. The dotted line indicates the supposed valid degree.

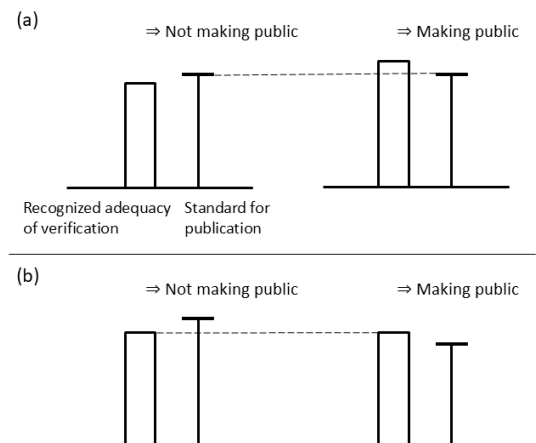


Figure 3. (a) shows that two scientists who share the standard for publication can decide differently due to different recognitions for verification. (b) shows that two scientists who hold the same view for verification can decide differently due to different standards for publication.

The timing of publication has to do with *how much confirmatory work (especially internal checks) can be conducted* for the result before making public. The necessity of some extent of rigorous checks and the requisite resources for confirmatory work can provide scientists with the motive to delay publication, or even not to make public the result. However, in practice, there are several factors that put pressure on or hurry up scientists for publication.

First, by making public a result early, it is possible that a result is highly evaluated and more credit is attributed to the researchers by the scientific community, having impact on the development of science. In particular, scientists can be driven to present results earlier to gain *priority* over other (possibly) competing scientists or collaborations. Especially when the result is of high impact (e.g., the Nobel Prize is awarded if true, as the superluminal neutrino is), scientists are given an incentive to hasten publication.<sup>\*39</sup>

Second, it can be difficult for the collaboration to keep the information confidential. If the information concerning an important result leaks to the outside, the collaboration may be put under pressure to explain it to funding and research agencies (e.g., INFN and CERN) and the public. Scientists are accountable, one might insist, for what they are doing, as they obtain considerable resources (especially in high-energy physics) from society.

Indeed, these factors are considered to work to some extent in the case of the neutrino velocity measurement by OPERA<sup>\*40</sup> (cf. Nature 2012). However, as it was not made sure by the fieldwork what really happened regarding these points and how they affected the collaboration's decision-making, they are described here only in a general way.

By these factors, scientists can make public results even if they recognize that confirmatory activities do not reach a standard required for publication, or subjective evaluation may be biased to justify the publication. Of course, it has a risk of damaging their credibility by making public a result whose verification is insufficient, being unable to devote enough resources to confirmatory work.

Thus, various elements, including institutional and social ones, are involved in whether and when to

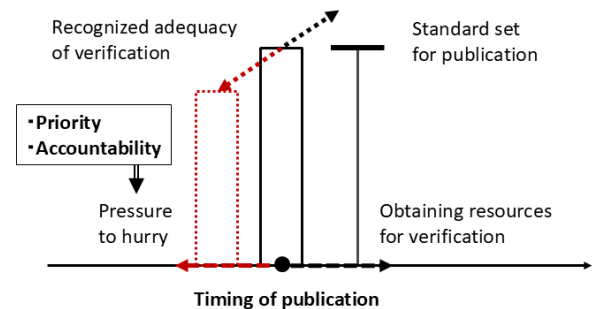


Figure 4. A model of decision-making for publication with the time axis. Scientists can rightly increase recognized adequacy of verification by obtaining resources for it, but there are factors that hurry up them for publications which make sufficient checking difficult.

<sup>\*39</sup> Credit affects the reputation of collaborations and the careers of individual scientists as well as funding for research institutes. Note that although the ambition or desire for credit may seem inappropriate as a motive of scientists (as opposed to pure curiosity), competition can be beneficial for scientific progress. Strevens (2003) evaluates the priority rule in terms of efficient allocation of resources.

<sup>\*40</sup> Several collaborators stated that some were misled by the misinformation that the MINOS collaboration upgraded their timing system. It should be noted that even if it were so, the problem is not of a specific few people but of the group as a whole, since the result was made public with consensus by the majority of the collaboration.

make public a result.

#### 6.4 A preliminary analysis of the methodology of presentation

It must be noted that although scientists may feel pressure for several reasons, they are not *forced* to present results or sign papers. Especially, they have the freedom to delay or refrain from presenting results for the reason that internal check is insufficient for publication. Accordingly, it is up to the collaboration or each collaborator to decide on or consent to making public a result in a certain manner and at a certain timing, deliberating in light of various elements whether the publication is beneficial for the scientific community including the public as well as for the collaboration.

However, as we have seen, there can be some practical reasons for making public a result. Therefore, more careful ways to make public results are worth considering. That is to analyze the methodology of presentation (i.e., a tactic of knowledge production that take resources into account) when the publication is urgent. It cannot be fully articulated here, but some options are suggested.

As mentioned above, the manner of publication concerns its content and form. With respect to the content of presentation, the result of the neutrino velocity measurement was presented with a certain amount of caution. However, a senior OPERA collaborator told that there could be a *more* moderate way of presentation. Regarding the anomalous result, it would put weight on possible problems with the instruments such as the GPS, which are still to be verified, suggesting the superluminal neutrino only as a possible interpretation, and the paper is not titled “Measurement of the Neutrino Velocity.” This can be seen as a candidate for the tactic that can rightly lower the standard for publication further.

Regarding the form of presentation, it should be recognized that after their public release (the extensive announcement by a seminar at CERN and a press release), the OPERA collaboration had to deal with and respond to both specialists and nonspecialists at the same time in tough circumstances. One other possible form of presentation is initially presenting the result to only a limited part of the high-energy physics community (e.g., at the LNGS workshop) and then, in cooperation with them, conducting confirmatory work and dealing with broader outsiders such as the scientific community, the media, and citizens (as it were, “involving the nearer of the outside”). Strassler (2012) also points out; “Instead of having a big public talk at CERN (thereby dragging other people into your mess) when you announce your result, you should announce the anomaly quietly, at a little mini-workshop.” This would enable more intensive confirmatory work with the help of other experiments and the OPERA collaborators may concentrate on their work by relieving pressure from the outside.

In terms of the point that the way of presentation affects confirmatory activities, which was seen in Section 6.2, we can say that these tactics improve the ability for verification by securing adequate resources for it. Granted that the presentation may still cause some stir, the responsibility of scientists would be lightened and the risk for the collaboration reduced. Moreover, if the result were correct, they would get the

credit they deserve, though not as much as when it was entirely accomplished by the single experiment.<sup>\*41</sup>

However, concerning the form of presentation, some collaborators suggested that it would be impracticable to present a high impact result such as the superluminal neutrino without a press release (the issue of accountability). That means it is really difficult to present the result for inviting scrutiny from related researchers without running a risk of confusing the public and being criticized. If so, delaying or refraining from presenting the result would be a rational decision for scientists as its verification is insufficient.

Meanwhile, it needs to be noted here that anomalous results might be buried by the negative attitude. In the case of neutrino velocity measurement, the result became nonanomalous in the end by internal checks of the collaboration. However, anomalous results may not be made public and pursued further, remaining incomprehensible. That would have some effects on scientific knowledge system.<sup>\*42</sup> Thus, the decision-making for publication has importance not only for knowledge production by scientists involved but also for the dissemination and development of scientific knowledge. Especially, the handling of anomalous results will be worth investigating including institutional aspects of making public a result.

In the field of high-energy physics, there can be an option to establish “anomaly forum” where scientists share the results that they cannot understand and hesitate to make public in the normal way. Such results would likely to be wrong, but have a high impact if true. It is epistemologically interesting what will happen to knowledge system when the results that have not been presented are collected. Scholars on science can contribute to the institutional design of the forum considering requirements such as tolerance for mistakes and priority rules, in accordance with the characteristics of the field.

We must admit that information is currently not enough for judging what are the viable options of publication and what their consequences would be in relation to research and funding agencies and the scientific community.<sup>\*43</sup> This is the reason why the above analysis is preliminary.

Comparative studies of various scientific publications, which analyze the context, the reasons, and the manner in which the results are presented (or not presented) and the aftermath, will be valuable for the practice of producing scientific knowledge as well as for the obtainment of its real image. Furthermore, the methodology of presentation needs to be *developed*, beyond “analysis,” with (future) practices of working scientists in view of institutional aspects. That is, what kind of relationship they build with research and

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<sup>\*41</sup> Indeed, it would be unfair for scientists to claim credit for a result that they cannot handle by themselves while denying their responsibility when the result was wrong.

<sup>\*42</sup> Suzuki (2017) discusses delays in scientific discovery due to various reasons and their possible influence on resulting knowledge system. In high-energy physics, it particularly becomes a problem that producing a result is difficult in terms of resources required. Tsou et al. (2014) describe various perceptions by researchers in different disciplines of ‘unpublishable’ research, such as negative results and replications, which may be worthy of publication.

<sup>\*43</sup> For example, how much of a difference regarding the responsibility of a collaboration will be there between widely announcing the result by itself (i.e., actively) and doing so upon request? Or is it practicable for collaborators to keep pretending to not know the result whose information is leaked?

funding agencies and other agents (i.e., their peers, the media and the general public) matters.

## 7 Considerations on the scope and limitations of the model

As mentioned earlier, the model in Section 6.3 (see (Fig. 1) and (Fig. 4)) is hypothetical one that is assumed to be developed by further research. I do not exclude other factors that affect publication. Especially, various circumstances can hinder publication regardless of scientific validity. Silver (2022) states that the war in Ukraine has influences such as removing institutional affiliations and slowing down submission.<sup>\*44</sup> In addition, the withdrawal of collaborators or the splitting of the collaboration may make publication difficult, which can be for scientific or other reasons.

Moreover, one may wonder *whether or to what extent the model of decision-making for publication is applicable to other cases* than the neutrino velocity case or those of high-energy physics. My own basic stance is that in order to know that it is necessary to conduct fieldwork on other cases including those of other scientific fields, such as biology and chemistry. However, considering some points regarding the scope and limitations of the model will be useful for encouraging future studies on various cases.

On the one hand, the model may have a certain bias or partiality, as it is constructed in analyzing the neutrino velocity case and abstracting from the relevant points. Therefore, we must be cautious about hastily generalizing it to other cases (i.e., there is a “n=1 problem”). On the other hand, special characteristics of the case do not necessarily mean that elements found there are not working in other cases. Rather we can say that structures that are normally invisible have been revealed due to the special circumstances of the case.

Now, let us look more specifically at elements on the model. As for “reliability” of the experimental system (or comprehensive understanding of technologies) concerning “recognized adequacy of verification,” it is also important for the oscillation analysis in the OPERA experiment as stated in Section 5.2.

As mentioned in that section, Knorr-Cetina (1999) describes self-understanding in terms of epistemic culture of high-energy physics. She contrasts it with “object-oriented” processing in molecular biology where researchers are mainly engaged with many forms of intervention in material objects of organisms (pp. 84-93). Strategy of investigations can vary depending on such factors as manipulability of the objects and reproducibility of phenomena.<sup>\*45</sup> It is an open question whether elements regarding recognized adequacy of verification differ between scientific disciplines in kind or only in degree.

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<sup>\*44</sup> I am indebted to an anonymous referee for drawing my attention to this example.

<sup>\*45</sup> As for the reliability of the experimental (or observational) system, “a blind injection” was used in the detection of the gravitational waves. That is “a false signal intentionally but secretly injected into the data stream to test the collaborations preparedness and ability to cope with a bona fide signal.” (Levin 2016, pp. 206-7)

With regard to the element of “scientific and social impact” in “standard set for publication,” the social impact would be given much weight in research areas such as life and environmental science than in particle physics, as the former is more directly related to social interests (benefits and risks). Nerlich (2013) examines as two cases of ‘hype’ the case of superluminal neutrinos and a case of climate science where conflicting groups published their findings on climate change. The point is that neutrino velocity case is called ‘honest hype’ in which “[t]here was no extra dramatization or sensationalism involved in making that claim which was in itself dramatic” (p. 52), and it is distinguished from ‘politicized hype’ of the climate papers whose findings were hardly earth-shattering and their importance lies in the politics of science. One may regard the difference as a matter of degree in consideration of a more moderate way of presentation in the neutrino velocity case (which Nerlich could agree). In light of “the medialization and politicization of science” Nerlich mentions (pp. 46-8; cf. Weingart 2017), research on the actual situations of *pre-publication* (publishing results before peer-review) and its effects in various fields becomes important, which is related to the elements of “manner of publication” and “timing of publication.”

What also matters concerning the limitations of the model is the scale of scientific research and the involvement of agents outside the researchers.

Although this study basically bears collaborative science with a certain scale in mind, the model of decision-making for publication itself seems applicable to individual scientific research (see note 33). However, the decision-making by group has additional features to be taken into consideration such as the rules for publication<sup>\*46</sup> and interactions among collaborators. That is, the *collective decision-making* is not necessarily simple sum of the individual decision-making. For instance, a result can be made public by some majority rule in the large-scale experiments even if some collaborators do not consent,<sup>\*47</sup> but unanimity may be practical norm in the small-scale collaborative research (and such an issue does not arise in individual research). Moreover, the decision of a collaborator can be influenced by the behavior of other collaborators. A scientist may consent to make public a result and sign a paper relying on the person in charge of the group or conforming to the majority of the collaboration, being unable to fully establish one's own view (e.g., due to insufficient information or a lack of expertise).<sup>\*48</sup> Scrutinizing collective

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<sup>\*46</sup> Collaborations in high-energy physics usually have a written constitution. In OPERA constitution, the approval procedure for papers is stipulated as well as other matters as management functions. It is yet to be investigated how such institutional aspects affect collaborators' practices of publication.

<sup>\*47</sup> In such a situation, the belief in a result can be ascribed to the group even if it is not true that most of its members believe the result. For the points at issue regarding summative and non-summative accounts of group belief, i.e., reductive and non-reductive accounts of group belief in terms of the beliefs of individual members, see Schmitt (2017) and Bird (2019). A representative of non-summative accounts is the joint commitment account developed by Gilbert (1987; 2004).

<sup>\*48</sup> There is a motive for unity in a collaboration to ensure the credibility of publication. Galison (2003) introduces a statement in authorship rules of the DØ collaboration at Fermilab: “Withdrawing individual names because of a lack of close involvement in some particular aspect of the analysis will tend to undercut the impact of any publication and is

characteristics of science is significant in view of increase in scale of the research group in contemporary science. It is also relevant for thinking about the responsibility of individual scientists in collaborative research.

It should be reminded that the decision-making for publication is influenced by the (actual or putative) behaviors of agents outside the collaboration such as other collaborations and research institutes, as we saw in relation to “priority” and “accountability.” Besides, when scientists publish a result in a peer-reviewed journal, it matters which journal to submit to, and the editorial policy and peer review criteria of the journal have relevance. These pertain to the element of “convention” in the discipline. Rowley et al. (2022) examines several factors influencing journal selection by researchers in different disciplines and levels of experience, e.g., journal reputation, reliability of reviewing process and fit with journal scope.<sup>\*49</sup> It remains to be examined how journal characteristics affect scientists’ considerations on the standard of publication and its timing.

Thus, it is a subject of future study to elucidate these points in detail and there is much work to be done for research on science. The model presented in this paper is intended to be a basis for further investigation and discussion. Meanwhile, it should be noted that making the model itself too concrete may impair its usefulness. Scientists can reflect on each practice of publication referring to the framework of the decision-making model, probably with modifications depending on the case, recognizing the factors that can influence their judgments and considering which elements to be cared and to what extent.

Issues of publication are important not only for collaborators but also for the scientific community in general, since the accumulation of published work principally constitutes scientific knowledge. The characteristics of the decision-making for publication by scientific collaborations are relevant when analyzing collaborative knowledge production and collective knowledge, which are stated as prospects in Section 8.2.

## 8 General remarks and prospects

In this section, I provide general remarks on the extent of the problem in the case of the neutrino velocity measurement by the OPERA collaboration and indicate the prospects of this study regarding epistemological research on collective knowledge.

### 8.1 General remarks on the problem

With regard to the issue whether and at what point there was a problem with the OPERA collaboration

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therefore to be strongly discouraged.” (p. 336)

<sup>\*49</sup> Cicchetti (1991) examines the reliability of peer review in various disciplines. Björk (2019) surveys acceptance rates of peer-reviewed journals (including open access journals) in different fields of science.

in the neutrino velocity measurement case, which is important for practitioners, there were various opinions both inside and outside the collaboration. My view is that the extent to which one regards this case as problematic depends on how high a standard one imposes on the collaboration (or themselves). That is, considering elaborate confirmatory activities and a certain degree of careful presentation, there were not gross deviations from the scientific method or customary practices of the scientific community. Accordingly, it is not fair to say that the OPERA experiment was especially problematic. However, as we have seen, there could be better ways to deal with the anomalous result, and therefore it must be said in this sense that *as a leading experiment* that should be an exemplar at the forefront, the collaboration had problematic points for this case.

Notably, the fact that critical problems were found in the instruments is problematic in light of the norm of self-understanding of the experiment. It is basic points for experimenters to comprehend the instruments they use. Internal checks on equipment by the collaboration must be given priority before the result was announced to the outside, as it is difficult for noncollaborators to perform such checks in terms of both ability and practice. That is, the premise that *what can be carried out inside has been sufficiently done* regarding confirmatory work is necessary for inviting scrutiny.

Moreover, with respect to the decision-making on the publication of the result, the assessment of its impact and the manner in which it is presented matter. Since the result of the superluminal neutrino is of high impact, it would be difficult for scientists to avoid suspicion, regardless of the facts, that they hastened the announcement of the result in order to gain priority.<sup>\*50</sup> A more careful handling of the result would be appropriate both for the collaboration and for the scientific community. In relation to the scientific community, the publication of unreliable information makes spare resources for other scientists and research institutes wastefully.

In the field of high-energy physics, the competition for weak bosons between the UA1 and UA2 collaborations and the episode of Carlo Rubbia, the head of the former and recipient of the Nobel Prize in Physics, are well-known (Taubes 1986; Sample 2013, Chapter 5). Although competitions are common in science, a passage by Pierre Darriulat, the spokesperson of the UA2 collaboration, is worth noting: “In retrospect I am proud we resisted the pressure that was exerted on us to publish faster than we thought we had to” (Darriulat 2004).<sup>\*51</sup>

Certainly, falsifiability is an important characteristic of science, and suppressing novel findings is detrimental to scientific progress. In this sense, anomalous results need special attention in terms of their potentially significant influence on the knowledge system and their possibility of being disregarded, as

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<sup>\*50</sup> In this sense, the ECREE can be construed, in addition to being an epistemological principle, as a practical principle for publication. That is, if scientists make public extraordinary results without extraordinary evidence, they would be criticized for hurrying for credit.

<sup>\*51</sup> Here lies the issue of *institutional design* for a reward system that concerns the ethics of science (such as research integrity, the virtues of scientists and utility), which is not addressed in this paper.

stated in Section 6.4. Nevertheless, it should be noted that if publication with insufficient checks becomes common, resources for scientific research may be dispersed, or at least the scientific community would be confused. In addition, it may also have negative influence not only on the credibility of the collaboration but also on that of the research field (such as neutrino physics) or the scientific community in general, although it must be carefully observed how much influence a single case would actually have.

My diagnosis for our case is that the problematic situation (i.e., publication at a time when verification was insufficient) arose not from a single critical error but from the accumulation of multiple points where each collaborator was involved in different ways, for both confirmation and publication. This, as well as the aforementioned issue of the standard that one imposes on the collaboration, explains the various opinions among the OPERA collaborators as to what matters regarding this case and what lessons are to be drawn from it.

Finally, I would like to stress that the issues taken up in this study are not of specific collaboration. It is valuable to analyze the structure of collaborative knowledge production and methodologies with limited resources in this case, as long as *any experiment* can obtain unexpected results and several elements concerning checking and publication are relevant to ordinal collaborative scientific practices in a contemporary institutional or social context. That is, what is revealed in the special case can be widely applied, even though some differences by the research area exist.

## 8.2 Prospects regarding epistemology

As a specific point, the issue of trust can be broadened from inside the collaboration to ‘trust’ in the collaboration itself.<sup>\*52</sup> Especially in high-energy physics, where large resources are necessary, the checking of collaboration from the outside has limitations, and a result is difficult to replicate. In such circumstances, the group’s credibility (i.e., whether it is trustworthy in ability and sincerity) can be significant for outside scientists to consider the result.<sup>\*53</sup>

As a more general point concerning epistemology, the explication of the interaction between the collaboration and its surroundings, such as funding and research agencies and the scientific community, will contribute to philosophical discussions on *collective knowledge*. The issue of collective knowledge is, roughly, whether and in what sense scientific knowledge is irreducibly collective in light of cases in which the group seems to *know* what any individual does not.<sup>\*54</sup>

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<sup>\*52</sup> See Miller and Freiman (2020) for the issue of trust in groups and its relation to the question of whether instruments are genuine objects of trust, and Rolin (2020) for trust in the social practices and institutions of scientific communities.

<sup>\*53</sup> In that sense, each collaboration is required some degree of publicity, and indeed, high-energy physics experiments usually have an internal review system for making public results. This is also the reason why a collaboration can be held responsible for publication.

<sup>\*54</sup> Wagenknecht (2016, Chapter 9), referring to de Ridder (2014), develops an account of collective knowledge. It regards mutual epistemic dependence among a group of individuals that allows only the group to provide a scientific

Following Bird (2014), two accounts of collective knowledge can be distinguished—the distributed model which focuses on the group’s division of labor and the commitment model which regards joint acceptance by the members as essential. Palermos (2022) argues that the two models are not necessarily competitive but can be complementary. I also think that we do not have to choose one or the other. The distributed model is considered to be suitable for understanding processes in which scientists collaboratively obtain a result using instruments. The commitment model can be utilized for analyzing formation of group views. In particular, the notion of joint acceptance has relevance to collective decision-making for publication mentioned in Section 7, e.g., in how individual knowledge or beliefs are related to collective acceptance and how it constrains members’ behaviors. Meanwhile, we should be cautious about drawing general implications from one or a few case studies.

Relatedly, there is a discussion regarding to which group collective scientific knowledge should be attributed. For instance, Wray (2007) limits apt candidates for collective knowledge to research teams from a viewpoint of organic solidarity or functional interdependence, whereas Rolin (2008) argues that scientific communities, both a scientific subfield and the scientific community as a whole, are also capable of having collective knowledge in terms of epistemic justification (cf. Cheon 2014; Dragos 2016a, 2016b).

As the main purpose of this paper is to analyze the case, I do not go deeply into those discussions here but suggest some relevant points from the standpoint that have conducted fieldwork, anticipating future developments of epistemological research.

First, it is important to put in perspective both knowledge production by research teams and their interactions with the broader community, such as each scientific specialty community and the scientific community as a whole. Collaborative science is carried out on the basis of knowledge system and technologies developed by the scientific community in the broad sense. Moreover, given that a knowledge claim is usually established as *scientific knowledge* after processes such as peer reviews and replication, institutional aspects of the scientific community cannot be disregarded. In particular, since there are some published results to be revised significantly or retracted (Marcus and Oransky 2017), it may be inappropriate to simply regard a group’s presentation of a result or publication of a paper as ‘knowledge’ production. Admittedly, one can require as a condition of knowledge that it is produced in a *reliable* way (Goldman and Beddor 2021), but it remains to be investigated what verification activities and institutions are required for a cognitive system, including artifacts (or instruments) and social organizations (Giere 2002), <sup>\*55</sup> to be reliable, which depends on contexts such as the consistency with the accepted knowledge system and characteristics of the research field.

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justification for a proposition as a necessary condition for collective scientific knowledge.

<sup>\*55</sup> Giere (2007) is opposed to postulating an epistemic subject (or a collective agent) and attributing such attributes as knowing and having a mind to distributed cognitive systems as a whole (for some concepts regarding cognitive systems, see Theiner 2017). Uygun Tunç (2023) argues that scientific knowledge is produced collectively (evidence-generation), but is possessed by individuals in terms of cognitive agency.

Second, it is worthy of attention what implications the information control by research teams has both on the conception of scientific knowledge and on practices of contemporary science. For instance, when a scientific collaboration observes an unknown phenomenon with a certain level of reliability but does not make it public for some practical reasons, what does it mean for “scientific knowledge”? The answer would vary depending on the view one takes regarding an epistemic subject of knowledge. Meanwhile, publication of uncertain results is non-negligible, as it can influence the direction of research by other scientists (with limited resources) and what is to be known about. In addition, it matters how research teams share information within the group. In the context of collaborative science, one may be careful for accepting that individual scientists can *know* something based on trust (which has practical importance) in view of such a case as the neutrino velocity measurement. However, this does not preclude us from thinking that nonexperts can have scientific knowledge relying on scientists, once it is established by the scientific community. That has implications for what we as laypeople know about.

Note that it is not guaranteed that there is only one sense of ‘knowledge.’ The term may be used properly (probably with degree) for both individuals and groups, and both research teams and the scientific community, depending on the questions one is concerned with. It would be challenging for social epistemologists to inquire, perhaps with psychologists,<sup>\*56</sup> into each characteristic and interrelationship of ‘knowledge’ or scientific knowledge *at multiple levels*, and examine the theoretical advantages of endorsing the concept of collective knowledge.

It might turn out that such a concept should be rejected. However, at least it is too early to deny its utility by a priori argument when the information of explanandum is not sufficient. That is, materials on scientific practices or *phenomena* that are to be understood and explained must be collected with priority. It is crucially important to accumulate steady efforts in intensively observing and analyzing a wide range of scientific activities in various contexts. Then we will be able to consider whether and how it is substantial and fruitful to discuss collective knowledge in the interaction with developing practices of science.

In this study, we saw that contemporary scientists have to care a wide range of issues for successful knowledge production, which requires extending the meaning of ‘knowledge acquiring methods’ to include the management of resources, such as researchers, things, funds, time *and information*. Thus, philosophers with epistemological concerns are expected to make their research more context-sensitive, i.e., more relevant to ‘practice’ in the actual scene, by conducting some fieldwork or associating their considerations with laboratory studies. These endeavors will enable more realistic and fruitful research on science that reflects concrete situations of knowledge production and possesses practical significance.

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<sup>\*56</sup> Karasawa and Todayama (2012) is a collaborative work by social psychologists and philosophers of science regarding research on group mind.

## 9 Conclusion

This study analyzed the case of the neutrino velocity measurement by the OPERA experiment based on fieldwork, regarding both its confirmatory work and decision-making for publication, in order to elucidate the structure of collaborative knowledge production in contemporary science and draw lessons for it.

This paper demonstrated that strict cross-checking, such as independent measurement within the group, is necessary beyond trust and dialoguing practices in collaborative science on some occasions. Since it is impracticable to carry out such strict checks all the time, the condition is to be specified as the analysis of methodologies, i.e., tactics for conducting research with limited resources. That is, strict cross-checking and resources for it are required when the result is extraordinary in consideration of background knowledge and comprehensive understanding of technologies is not sufficient (the latter has more importance).

Meanwhile, some collaborators consented to make public the result despite recognizing that internal checks were not sufficient. This indicates that not only the problem of confirmation but also that of publication matters for collaborative knowledge production. Indeed, these two problems intersect. In particular, whether and how to make public the result influence the activities of confirmatory work through resources available for scientists. More generally, a model of decision-making for publication, which includes the recognized adequacy of verification and the standard for publication as components, shows that it matters whether the verification of a result is sufficient *in terms of making public that particular result in a certain way*. That is, both elements of verification and those of publication are involved in the decision-making, and considerations of them are necessary. The impact of the result (for both the scientific community and the general public) must be taken fully into account, and regarding the manner of presentation both its content and form are relevant. It is also indicated that the timing of publication, or whether or not to make public a result at some point, can be influenced by such institutional or social factors as priority and accountability. These issues of publication are significant in that they concern not only credibility and reputation of scientific collaborations but also where the scientific community is headed.

In the present age, scientists are required to have *scientific methods* in the broad sense to successfully acquire knowledge. It is worthy of special attention what implications the information control by the research group, and therefore the institutional design regarding presentation, has for the development of scientific knowledge. More extensive fieldwork studies must be conducted so that social epistemology can reflect the actual circumstances in which scientists produce knowledge, if it is to be research on phenomena that occur in our world.

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## Author Information

Hidegori Suzuki (Mie University)