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The STS Challenge to Philosophy of Science in Taiwan

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Abstract This article discusses the relation between the tradition of the philosophy of science and the tradition of science, technology, and society (STS) studies in Taiwan. The swiftly rising popularity of STS studies has issued a challenge to philosophy of science, whose practitioners have amounted to a relatively small community. The STS challenge is measurable in the increase in the number of scholars who have lost interest in philosophical debates. They doubted that these sorts of debates were far from the reality of science, technology, and society. Since philosophical debate within STS studies is a form peculiar to philosophy of science, this lack of interest can be read as symptomatic of a general skepticism on the necessity of philosophy of science. This paper argues that philosophy of science can provide useful models for the development of STS studies in Taiwan. Furthermore, philosophy of science can benefit from the rise of STS studies, so long as philosophers respond to the challenge. This paper sets out several strategies for such responses, and suggests that the philosophy of science should play a crucial role. This suggestion raises the question of whether philosophers of science are more suitable interlocutors to the scholars of STS studies than are philosophers of technology.

Abstract 本文討論台灣的科學哲學傳統和科技與社會研究的傳統，兩者之間的關係。科技與社會研究在台灣快速崛起，對於相對少數的科學哲學構成一個挑戰；一些研究者認為某種哲學辯論對於理解科技與社會無所助益、質疑它們脫離社會現實。筆者認為，科技與社會的哲學辯論是一種特別的科學哲學，因此這種質疑標誌了對科學哲學必要性的一般懷疑。本文爭論，科學哲學可以對科技與社會研究的發展提供有用的模型；反過來說，它也能受益於對方，只要科學哲學家能認真地回應科技與社會研究的挑戰。本文提出

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幾個回應的策略，並建議科學哲學在科技與社會研究的發展中，扮演關鍵的角色。可是，這個建議引發一個質疑：科學哲學家會比技術哲學家更適合和科技與社會研究者對話嗎？

Keywords philosophy of science · STS · Taiwan · Thomas Kuhn · Paul Feyerabend · 科學哲學 · 科技與社會 · 台灣 · 孔恩 · 費耶阿本

1 Introduction

The relationship between philosophy of science—in particular, the tradition of history and philosophy of science—and science, technology, and society (STS) studies can resemble that between parents and children. Do STS studies have an Oedipus complex vis-à-vis philosophy of science? Or, as Hacking suggests, perhaps philosophy of science has a reverse Oedipus complex to STS.[1] This dynamic can also be found in Taiwan, albeit in a slightly different form. Several Taiwanese philosophers of science have supported and engaged with STS studies, yet the "child" has grown stronger than its "parent." STS studies are attracting more talented young scholars than is philosophy of science. This paper explores the discomfort widespread among philosophers of science in Taiwan as they interact with adherents of a new, some might say trendy, type of STS studies.

Philosophers of science in Taiwan are now facing a problem: how should they respond to the swift rise of STS studies? First, they need to grasp the nature of the situation: are they experiencing the twilight of the philosophy of science or is it rather a new dawn? They also must decide how to approach STS studies. Should they treat it as an enemy to be defeated, or as a provocative yet helpful friend? As being valuable, complementary to philosophy of science, the success of the new arrival need not ring down the curtain on the established discipline. On the contrary, it need not be negative at all: it signifies a tremendous chance, a crucial moment. Philosophers of science should see the success of STS as an opportunity to revitalize their own works as we seek—as we should—to participate in the development of a neighboring field. We can help create a broad space for cooperation and mutual learning. This leads to more questions: What is the meaning and content of the challenge presented by STS studies? How should philosophers respond?

Before telling the Taiwanese story, a brief outline of both philosophy of science and STS in the Western world will be helpful, both because they are source and because they can bring out the characteristic traits of Taiwan's experience. I shall focus on the scholarships that strongly influenced Taiwan.[2]

[1] Hacking used the metaphor of reverse Oedipus complex to characterize the relationship between Kuhn and the scholars in sociology of scientific knowledge (SSK): "I think his [Kuhn's] relationship to the young STS people was a little bit like Hobbes' feelings about Boyle . . . . Call it a reverse Oedipus complex. He saw that all these gifted young people were approaching this history of the sciences in a new way, profoundly influenced by his own work, but he didn't like the way they did it at all" (2008: 268–69).

[2] For Taiwanese work on the development of Western philosophy of science, see Chuang 1994; Yuann 2003; Fu and Chu 2001; R.-L. Chen 2003.

2 A Brief Outline of Western Philosophy of Science and STS Studies, as Viewed from Taiwan

The history of philosophy of science in Anglo-American tradition may be divided into three periods. The periods are distinguished by approach, subject matter, and inter-group competition. From the 1920s to the 1960s, logical positivists and empiricists led the way, but they were continuously challenged by Popper, the distinguished falsificationist, and his followers. In spite of their differences, the two schools shared a rigorously logical approach and an interest in the testing of theories. Their contentious focus was on whether positive confirmation or negative falsification is more adequate to define the testing of hypotheses or theories. Thomas Kuhn's *The Structure of Scientific Revolutions* opened the second period in 1962 by introducing the historical approach. Kuhn immediately faced criticism and resistance from historical rationalists such as Dudley Shapere, Imre Lakatos, and Larry Laudan. Still, they shared his historical approach and agreed that philosophy of science should try to determine how to interpret scientific development. Many debates over rationality, progress, and continuity took place. The third period began in the end of the 1980s and has continued until now. By and large, it is plural and complex. Philosophers of science have absorbed the historical approach, adding novel elements such as theories from cognitive science, structural analysis, model-based and case-based reasoning, and new sociological concepts. Scientific practices including experimentation, model manipulation, measurement, data mining, and other related activities have been subjected to increased scrutiny. Prominent studies include those of Ian Hacking (1983, 2002) on experimentation, manipulation, and historical ontology; of Nancy Cartwright (1989, 1996) on causality and causal models; of Ronald Giere (1988, 1999, 2006) on theoretical models and perspectivism; and of Joseph Rouse (1987, 2002) on laboratory power and naturalism. As for the traditional subject of philosophy of science, theory, philosophers with that interest have turned their attention to metaphysical issues of particular sciences, for example, physics and biology. Even though schools, approaches, and subjects have been plural and divergent in the third period, we can still identify two contentious focuses: naturalism versus antinaturalism and realism versus antirealism.

STS studies is a young discipline, not yet fifty years old. It stemmed from science studies, which in turn originated from Kuhn's *The Structure of Scientific Revolutions*. The strong program of the Edinburgh school first proposed in David Bloor's *Knowledge and Social Imagery* in 1976 is recognized as the pioneering theory. Four tenets of the strong program have directed the later development of STS. Some scholars of STS studies have proposed different taxonomies of theories or approaches for their own discipline. For example, the founder of actor network theory, Michel Callon (1995), utilized five indices from the basic frame of that theory (nature, actors, underlying dynamic, agreement, and social organization) to define four models of the dynamics of science: science as rational knowledge, science as competition in the market of knowledge, science as sociocultural practice, and science as an extended translation.[3] Nelly Oudshoorn and Trevor Pinch (2005) discern four classes of "approaches" by focusing

[3] The first one corresponds to the scientific image provided by philosophical empiricism and realism, the second by traditional sociology of science, the third by social constructivism, and the fourth by actor network theory.

on the relation between users and technology: the social construction of technology approach, the feminist approaches emphasizing diversity and power, semiotic approaches using the concepts of configuration and script (including the actor network theory), and the cultural media studies focusing on consumption and domestication.

As viewed from Taiwan, three basic theories or approaches have brought a strong influence: social constructivism of science and technology, actor network theory, and the risk theory of STS.[4] I shall offer a particular interpretation of the genealogy of STS studies later in this article.

Many philosophers of science responded to STS studies with vigorous rebuttals. In the early 1980s, a number of them launched a critical attack against Bloor's strong program (Brown 1984). They generally treated it as social constructivism or postmodernism drenched in epistemological relativism, which they viewed as a self-defeating doctrine. Lively criticism continued through the 1980s (Brown 1987; Nola 1988) and reached a climax with the science wars of the 1990s (Gross, Levitt, and Lewis 1996; Koertge 1998; Kukla 2000). Conversely, some founders of STS studies also have no good impression on philosophy of science in the analytical tradition.[5] After the science wars, the philosophers of science seem to have given up on criticizing STS studies, and vice versa. All dialogue seemed to cease. Here I am expressing a pessimistic attitude toward the relation between the two disciplines. By contrast, Joseph Rouse (2011) holds an optimistic view on a possible future convergence.

3 A Survey of Philosophy of Science and STS Studies in Taiwan

Why has STS issued a challenge to philosophy of science in Taiwan? Let us first consider the problem from an institutional perspective. There are no formal statistics on the number of researchers, institutions, centers, or students associated with the two disciplines. However, by my rough estimate, the number of those self-identified as STS scholars is two to four times larger than the number of philosophers of science (Table 1).[6]

These figures are naturally a product of the historical development of the two disciplines.

It has been about sixty years since Yin Haiguang 殷海光 introduced logical positivism to Taiwan. This had a profound influence on the development of Taiwanese

[4] The so-called risk theory of STS is based on Ulrich Beck's risk sociology. I use it to refer to the scholarships of risk communication, public understanding and engagement of science and technology, science and technology communication, technology and democracy, and so on.

[5] In a recent interview conducted by Taiwanese and Chinese STS scholars, David Bloor said, "Encourage history, encourage causal explanatory sociology and above all keep the philosophers out of it." The Taiwanese and Chinese interviewers laughed (Li, Hwang, and Huang 2010: 431).

[6] Several points must be noted. STS studies in Taiwan make up an interdisciplinary field whose researchers come from history, sociology, philosophy, anthropology, politics, law, nursing, and even engineering. There is no clear border separating this field from other related fields. Therefore, STS studies have not yet become a formal item recognized in the taxonomy of disciplines of the Taiwanese National Science Council. Readers may wonder why I have not compared STS studies with the philosophical establishment in Taiwan. Such a comparison involves entering the complicated history of philosophy, which would go beyond the scope of this paper. But the key point is that philosophy of science is also an extremely small part of the general discipline of philosophy in Taiwan.

Table 1 Estimate of Institutional Presence of STS and PS in Taiwan.

	STS studies	Philosophy of science
Institutions	1	0
Programs	1	0
Centers	3	0
Researchers	Approx. 30–40 (interdisciplinary, but self-identification with STS)	Approx. 10–20
Members of professional association	134	no professional association[7]
Graduate students (2000–2010)	?	< 10

philosophy of science from the 1950s to the 1970s. But this trend suffered attacks from the conservatives, New Confucians, and the partisans of logical positivism were oppressed by the Kuomingtang authorities.

An increasingly modern and democratic Taiwanese society encouraged a second generation of philosophers of science led by Lin Cheng-Hung 林正弘, who had received a doctoral degree from the University of California at Berkeley in 1985. He improved on Yin Haiguang's work by introducing Taiwanese scholars to Popper's falsificationism, Kuhn's theory of paradigm, scientific realism, and other related issues (Lin 1985, 1988). (One can now speak of a third generation, a group that includes the present author, who prepared his doctorate under Lin's supervision.) Another important figure in the second generation is Fu Daiwie 傅大為, who has studied Kuhn, Lakatos, Laudan, and the historical approach since earning his doctoral degree from Columbia University in 1986 (Fu 1992, 1995).

The most distinguished achievement of the second generation has been the publication of *Philosophy and Conceptual History of Science in Taiwan*, edited by Lin and Fu, which is included as volume 141 in the series Boston Studies in the Philosophy of Science. Throughout the 1990s, a period of dramatic political reform, Lin and Fu frequently contributed political and social comments on public affairs and Taiwanese society to newspapers.

Members of the third generation, who got their degrees after 1990, studied history of philosophy of science (e.g., Yuann 2007), realism and antirealism (e.g., R.-L. Chen 2003, 2008; Wang and S.-T. Chen 2008), the structure and development of theories (e.g., R.-L. Chen 2000, 2004b), model and experimentation (e.g., R.-L. Chen 2004a, 2007), and the methodology of economics (e.g., Chao 2007, 2009; S.-T. Chen 2009). *Naturalized Epistemology and Philosophy of Science*, coedited by Mi Chien-Kuo 米建國 and Chen Ruey-Lin 陳瑞麟, and *Representation and Structure in Economics* by Chao Hsiang-Ke 趙相科 are two important publications of this generation. In general, the period from 1990 has been marked by a plural and mixed style. It includes the

[7] Members of the International Logic, Methodology and Philosophy of Science Organization in Republic of China are mathematicians, logicians, natural scientists, analytical philosophers, and the like. But this is not in practice an open association, and it is basically inactive.

approaches, subjects, and issues of Anglo-American philosophy of science in the first, second, and third periods. It is worth mentioning that some members of the third generation have consistently expressed their concerns about public affairs in the 2000s.

To sum up, three external features of philosophy of science in Taiwan are apparent. (1) The role of the Anglo-Saxon tradition has been dominant. Most studies introduce, interpret, revise, and criticize one or another of the important philosophical theories of science. (2) Concerns about the public affairs are apparent enough to constitute a traditional "culture." (3) Philosophers of science are few in number, between ten (including general methodology and epistemology, history and philosophy of science, and philosophy of physics, biology, and economics) and twenty (including philosophy of mathematics, philosophy of psychology and cognitive science, and metaphysics of mind and world) practitioners; there is no professional journal for philosophy of science in Taiwan.

STS studies in Taiwan have shown explosive growth over less than fifteen years. The philosopher and historian of science Fu has acted as the principal advocate of STS since 2000. In addition, several historians of Chinese medicine such as Lei Hsiang-Lin 雷祥麟 and Chu Pinyi 祝平一 have made a great contribution to STS in Taiwan. Lin Chung-Hsi 林崇熙, whose master's thesis in history was supervised by Fu and who earned his doctoral degree from Virginia Polytechnic Institute and State University in 1994, is the first formal STS Ph.D. in Taiwan.

In the first decade of the twenty-first century, STS studies have become a new field, receiving organizational and research support from the government. Clearly this is an interdisciplinary zone, with practitioners from various traditional fields such as history, sociology, philosophy, public health, medicine, and even engineering. There are two professional STS journals in Taiwan. In 2000, Fu invited several historians of Chinese science, sociologists of medicine, and philosophers of science to establish the *Taiwanese Journal for Studies of Science, Technology and Medicine* (abbreviated as *STM*), sponsored by Taiwan's National Science and Technology Museum. *STM*, published in both Chinese and English, became the official organ of the Taiwan Science, Technology and Society Association in 2008. The other journal, which published its first issue in 2007, is *East Asian Science, Technology and Society: An International Journal*, sponsored by Taiwan's National Science Council.

The social movements of the 1980s and 1990s also left their mark on STS studies. Following the democratization of Taiwan, many citizens—among them university students and professors—worked to improve the rights of laborers, farmers, women, and other marginalized groups. Some of these radicals went on to careers in cultural studies and STS studies. Naturally, such scholars showed a strong concern for social practices and public affairs. The hope that the fruits of their research can be immediately applied to actual problems had led them to investigate many local cases. Therefore, practicability and utility of academic research are regarded as important values that STS scholars and other academic researchers should pursue.

Both academically and socially, STS studies present a challenge to the development of philosophy of science, a discipline marked by increasingly metaphysical concerns with little obvious connection to social practices. Moreover, there has always been a degree of skepticism among some STS scholars about just how *constructive* philosophical debates are for the junction between science and society.[8] Since the history of philosophy of science consists of a long chain of philosophical debates on the nature of science, the skepticism over the constructive function of philosophical debates implies skepticism about the relevance of philosophy of science in general: Is it so speculative and theoretical that it is no longer needed in Taiwan? Might a young and energetic STS be strong enough to replace it entirely? As the challenge is actual and emergent, how can the philosophers of science respond?

4 Do Philosophical Debates Help?

In 2002, as STS studies were expanding, the important and influential academic journal *Taiwan: A Radical Quarterly in Social Studies* published a special issue on technology and society. In both an editorial comment and an article, Lei Hsiang-Lin, a leading STS scholar, cast doubts on the value of philosophical debates about realism and constructivism. In "Techno-science, Democracy and Society in Transformation: Challenges for STS," Lei urged scholars to address the complexity of science, technology, and society. He made three interrelated arguments: (1) the relationship between technoscience and society has been going through a profound transformation in the recent decades; (2) to cope with this recent development, scholars need to develop a new theoretical framework; (3) the time has come for a democratic science. In the conclusion, he suggests a set of experimental conceptions and practices to three relevant groups: the scientific community, the civil-technical group, and the STS scholarly community.

In discussing how to cope with this new situation, Lei claimed:

> STS studies should also enter a new stage. The key task should no longer be to demonstrate "the social shaping of techno-science" (while we should, however, pursue the question of which "social contexts and values" should be integrated into the process of knowledge production) or to engage in never-ending philosophical debates over the grand narratives of "constructivism" or "cultural relativism." (Lei 2002: 138, Lei's translation)

What were their so-called philosophical debates? Lei explained:

> Until today, on many occasions when scholars of science studies have interacted with physical scientists, debates have continued to focus on grand narratives such as realism, social constructivism, and cultural relativism. As a result, these debates have often and repeatedly fallen into predictable, inflexible, and extreme philosophical positions, and therefore have been unable to produce any constructive dialogue. (ibid.)

In a footnote, Lei clarified further:

[8] The use of the term *constructive* is to follow Lei's use of "constructive dialogue," a English translation of 建設性對話. I think that the term fits well with my argumentative point: philosophical debates help constructing new theory versions.

> Here, I must emphasize again that I have absolutely nothing against philosophical debates. What I discourage is un-reflectively committing oneself to debates over those grand narratives. As the contending parties often fall into the predictable, inflexible and extreme absolute philosophical positions, they fail to generate any constructive dialogue. (Lei 2002: n. 70)

The special issue includes another related article, "Superstitions, Imposture, Misunderstandings and Debates in the Science Wars," written by Chen Hsin-Hsing 陳信行. The article discusses the Chinese translations of Paul Gross and Norman Levitt's *Higher Superstition* and Alan Sokal and Jean Bricmont's *Fashionable Nonsense*, arguing that the science wars raised meaningful debates on issues such as the democratization of science, the strategies for the Left in the United States, and postcolonial critiques of science (H.-H. Chen 2002).

In the editorial of the special issue of *Taiwan*, the editor commented on Lei's and Chen's articles:

> Working from quite different premises, both reject the priority of the science wars as an academic dispute. They think that it is worthless to continue the outdated debate that pits realism against constructivism. They also point out that academic debates tend to be far from the reality [of science, technology and society]. (Anon. 2002: vii–viii)

Are Lei and the editor right? Should philosophical debates such as those about realism versus constructivism be sidelined? Several questions occur to me: How do we distinguish between useful and useless debates? Do debates about realism and constructivism really push partisans into extreme positions? Has Lei been stipulating methodological norms for STS studies?

It is not easy to distinguish between constructive and destructive debates. So long as every approach in STS presupposes special ontological, epistemological, and methodological positions, any critique must be grounded in a different position. This can only lead to further disputes regarding the new foundational principles. If debates between the existing and opposed positions are destructive, then how is a dispute based on a fresh position somehow constructive? To support such a judgment requires setting up a Popperian criterion or a methodological prescription. Can this kind of criterion or prescription be justified? Finding a reliable answer demands carefully philosophical reflection, examination, and disputation. In other words, partisans of STS studies must engage in more philosophical debates rather than fewer.

Lei's implicit distinction between constructive and destructive debates is reminiscent of the debates philosophers held in the 1970s that contrasted progressive scientific theories with degenerate ones. As the science wars were a philosophical debate about realism and constructivism or relativism,[9] Lei seemed to have viewed that contentious period as a waste of time. Was it really? Did the participants take

[9] "The science wars are thus a very strange conflict... Although the debate is ostensibly about science, the content of the debate is philosophical. Contrary to the typical way the science wars are portrayed, the conflict is a metaphysical battle fought by conscripts who have limited training in the martial art of philosophy" (Lynch 2001a: 53). Also, "The science wars are open to all comers. It helps to be a professional scientist (preferably a famous one) if you want to represent the science side, and it helps to have conducted empirical case studies of scientific practice if you want to speak up for the sociologists. Training in philosophy is a

two steps backward at the end of all that discord? I think not. Even a brief synopsis of the science wars is impossible here.[10] I will, however, identify what I see as their *constructive achievements*. They prodded many scientists into reassessing the philosophical presuppositions of scientific knowledge and practices. They revealed the scientists' image of science (namely, the self-image of science), leading many, including those who study the interpretation of science and society, to take seriously the relationship between the image and actual scientific practices (Sokal and Bricmont 2001a, 2001b; Labinger and Collins 2001). They led sociologists to look at scientists' working philosophies, which inevitably direct their professional efforts.[11] Labels such as realism, constructivism, and relativism had been carefully analyzed and distinguished. Relativism, for example, was analyzed into methodological, epistemological, and metaphysical types and discussed seriously one by one (Brown 2001). The science wars amounted to a grand *negotiation* between scientists and humanists (including STS scholars) and finally produced a consensus (Labinger and Collins 2001; R.-L. Chen 2005). In my view, it can be better expressed by Chen Hsin Hsing's comment: "Is science a social construction involving a variety of social conflicts or a reliable instrument for knowing the objective reality? Most of the participants in the science wars came to the conclusion that science is both" (H. H. Chen 2002: 200).

Lei exposed his attempt to set up methodological prescriptions for STS studies when he worded his statements in terms of "should" and "should not." This echoes the commitment to a normative methodology of science, affirmed by the rationalist philosophers of science—principally Popper, Lakatos, and Laudan. Certainly I have no objection to the commitment to a normative methodology of science and STS studies, but there is no question that they invited debates. When scholars who identify with STS studies attempt to evaluate a variety of particular technoscientific practices, they should also reflexively evaluate a variety of approaches to their own discipline. Debates between realists and constructivists evoke the most key issue confronted by science, technology, and society: the value of truth in society. Is it a good idea to abandon truth as the cardinal value as we work toward the best possible technoscientific society? Will deconstructing or reinterpreting the traditional meaning of truth assist the project? As scientists, philosophers, and social scientists follow the evolution of technoscientific society and the development of new sciences and techniques, I believe that these sorts of debates will recur and produce new content. History shows that people never cease to dispute about the priority of a value to others. Would it not be futile to exclude philosophical debates from STS studies?

There seems to be a Kuhnian overtone to Lei's attempt to stipulate a methodological prescription for STS studies. If this is so, he would imagine that once STS studies establish a *paradigm*, they enter into a period of *normal science*. Philosophical debates often occur, according to Kuhn, in the prescientific or crisis stages during which the

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bonus, but it isn't really necessary for playing the game. The entry requirements are low, and it appears that the whole world is joining in" (Lynch 2001a: 54).

10 A vast literature on the science wars was produced from 1996 to 2001. A few outstanding examples are Jardine and Frasca-Spada 1997; Brown 2001; Labinger and Collins 2001.

11 The Nobel physicist Steven Weinberg (2001: 240) suggests the concept of working philosophy, getting an echo from Michael Lynch (2001b: 271–74).

accumulation of knowledge is rather haphazard. Kuhnian theory of scientific development may be the basis for Lei's skepticism about philosophical debates. Can Kuhn's "factual" description of scientific developments serve as a normative guide? This is the subject of the following section.

5 How Can Philosophy Help the Normal Development of STS? Kuhn versus Feyerabend

In *The Structure of Scientific Revolutions*, Kuhn proposed a three-stage model for the development of the sciences: the normal science, the crisis, and the scientific revolution. He pointed out that philosophical debates on metaphysical and methodological topics usually become the central concern of scientists either before the stage of normal science or during the crisis stage. An obvious feature of the two stages is the proliferation of theories, because scientists would frequently challenge their rivals about the fundamentally ontological assumptions or the legitimacy of methods and methodologies. Once a theory becomes a new paradigm by defeating its rivals, coming to set constraints on normal researches, philosophical debates gradually fade out. As scientists have a stake in a new paradigm, they dislike being disturbed by their rivals' philosophical questions. If any of the normal scientists cast doubts on the very paradigm, they risk exclusion from the newly legitimized puzzle-solving activities.

Two possible readings of Kuhn's dynamics of scientific development have been proposed. Popper, Lakatos, and Feyerabend read Kuhn's theory as providing a "law" of the development of sciences. Later philosophers prefer to read Kuhn's theory by a "model-based approach."[12] The former group interpreted that Kuhn attempted to derive a development "rule" for new disciplines from the "law," as Kuhn suggested that scientists *should not* advance philosophical issues or engage in philosophical debates after a paradigm is firmly established: such talk only disturbs the normal advancement of a discipline. Although Popper, Lakatos, and Feyerabend held different philosophical positions, they stood at the same side as criticizing Kuhn's concept of normal science. They argued that this concept either was refuted by historical facts or normatively unacceptable because of its dogmatism and monopolistic stance (Lakatos and Musgrave 1970). Let me focus on Feyerabend's objection.

Feyerabend (1970) lodged a series of criticisms against Kuhn's model of normal science. He argued that, de facto, no single, monopolistic paradigm could be found in any period. He expressed grave doubts about the very existence of normal science, and he indicated that at least three mutually incompatible "paradigms" existed during the mid-nineteenth century: (1) the mechanical point of view in astronomy, in kinetic theory, and in the various mechanical models for electrodynamics as well as in the biological sciences, especially in medicine; (2) the phenomenological theory of heat, which was inconsistent with mechanics; (3) the electrodynamics of Faraday, Maxwell,

[12] This reading emphasizes that the central concern of *The Structure of Scientific Revolutions* is the puzzle-solving in normal science that is done by means of using model and analogous reasoning instead of deducing empirical applications from general principles. Readers who accept this reading do not view the three-stage structure as a rigid historical pattern (Nickles 2003). I thank an anonymous reviewer for urging me to consider this reading.

and Hertz (Feyerabend 1970: 207–8). In addition to this fact, Feyerabend also argued that Kuhn failed to demonstrate that the stage of normal science is a necessary and *normative* condition to produce scientific revolutions. According to Feyerabend, Kuhn defended the belief that revolutions could only take place after a certain amount of normal science had been carried out; solving "tiny puzzles" improved the fit between theory and reality, yielding progress.

Feyerabend did not believe the desirability of revolutions can be gained by Kuhn's model. Rather, a scientific revolution resulted, he insisted, from the intercalated implementation of the *principle of tenacity* and the *principle of proliferation*. The former principle advises that one should select from a number of theories the one that promises to lead to the most fruitful results, sticking to it even in the face of severe difficulties. The scientist who adopts tenacity cannot rely on recalcitrant facts to rebut a theory. If a revolution is to occur, scientists must turn to another theory that accentuates the problems that crop up with the established theory while at the same time promising to resolve them. Feyerabend calls the advice to pursue alternatives to the established theory the principle of proliferation (Feyerabend 1970: 203–6). Without a doubt, the philosophical debates are the best means to proliferate scientific theories; Kuhn (1970a) demonstrated that in his description of the prenormal and crisis stages, and Feyerabend (1978) made this point from different premises.

So opposed are the arguments of Feyerabend and Kuhn that if we embrace proliferation and the debates it entails, we must reject the notion of normal science. If we prefer to preserve normal science, we must turn our back on proliferation.

As an attempt to escape this dilemma, I want to invoke my own conception of *theory version*.

> A theory version is a formulation of a theory by one scientist in a manner different from that of other scientists. But a theory version within a family is not revolutionary compared to other versions within the same family. In contrast to theory versions, a revolutionary theory always establishes a new family. Of course, it is possible for a theory version that was originally part of one family to provide the basis for the development of a new family. In this case, the theory version may be a relatively remote one compared to other central versions, or part of the theory may be applied to an entirely new field, yielding a new family distinct from but related to the original family. (R.-L. Chen 2000: 452)

All theory versions within a family have more or less similar *conceptual structures*. Conceptual structures are organized according to their categories and taxonomies, as may be seen in Newton's and Hertz's theory versions of mechanics (R.-L. Chen 2000). The subject of Feyerabend's work, mid-nineteenth-century physics, lends itself quite well to a reconstruction in terms of theory version. That is, we can view the mechanical point of view, the phenomenal theory of heat, and the electrodynamics of Faraday, Maxwell, and Hertz as three families of theory versions. The family of electrodynamics or electromagnetism had developed from Faraday's conceptual structure of lines of force to Hertz's manipulation of electromagnetic wave via Maxwell's version of electromagnetic field.

A theory version is a slight—but significant—departure from the original theory. It is necessarily a modification of the original theory rather than a mere application of that theory, for it has a different categorization and taxonomy; the original theory is in turn regarded as the paradigmatic version in that family. In other words, I have managed to narrow Kuhn's paradigm and redefine it.

For the sake of consistency, I will continue to use terms which relate to the family. If a new version is constructed under the inspiration of one or several old ones, then the new is a *daughter version* and the old ones are the *parent versions*. Thus, the paradigmatic version from which other versions originate is the *primary ancestor* of the family of versions. Obviously it would take some time for the establishment of such a family. Kuhn's concept of normal science under the guidance of a paradigm can, then, be reinterpreted as the building of a family of theory versions. That is, the process in which a paradigmatic theory generates its descendant versions is just the stage of its normal science.

Furthermore, I will adapt Feyerabend's principle of proliferation to the expansion of theory versions, avoiding a more purely Feyerabendian view of the proliferation of alternative theories that can accentuate difficulties against the paradigmatic theory. In other words, by reinterpreting the principle of proliferation as a methodological prescription, we should modify the paradigmatic theory rather than *eradicating* it in favor of alternative theories. The development of a family means constructing new conceptual frames that are similar to the paradigmatic theory rather than solving "tiny puzzles" provided by the paradigm. Philosophical debates come to play a *necessary role* in the growth of a family of theory versions: they result in the proliferation of theory versions. Still, mere philosophical considerations are not sufficient to construct new theory versions. But the generation of a new version relies on methodological considerations and a metaphysical reclassification, both of which can modify the conceptual structure of the paradigmatic theory.

To sum up, you cannot construct a new theory or theory version by philosophy alone, but such projects do require dealing with philosophical problems and setting up a new conceptual structure. A great deal of scientific research can be accomplished without involving any philosophical problems, but such work does not in itself construct a new theory version. The history of STS theories illustrates this developmental pattern of theory versions.

6 An Illustration: Current STS Theories That Have Grown like Families

Looking back at Kuhn's model of revolution cycle to David Bloor's strong program, Barry Barnes's sociology of scientific knowledge, and Bruno Latour's actor network theory, the development of European STS studies assumes the shape of a branching track from a theory version to another version accompanied by philosophical reflections and debates.

As the beginning of it all, Kuhn presented, in an embryonic form, sociological explanation for scientific development. His analogy between political and scientific change was pinned to the dominance of a central ruling theory or paradigm. He anticipated the three methodological tenets of Bloor's strong program in his three-stage model: the principle of causal explanation, the principle of impartiality, and the principle of reflexivity. Kuhn applied concepts such as scientific community and incommensurability to his explanation and reflexively used his model to interpret the difference between Popper and himself in his "Logic of Discovery or Psychology of Research?" (Kuhn 1970b). It is not clear whether the principle of symmetry was implied in Kuhn's methodology; I tend to think not. I regard the principle of symmetry as Bloor's contribution due to his metaphysical insistence that the society is the cause of the truth claims in science. However, Kuhn did not rely exclusively on sociology to explain the dynamic process of science, and he probably would reject the principle of symmetry. This may be why he later expressed objections to the strong program.[13]

The strong program and the sociology of scientific knowledge established the new philosophical position, social constructivism, and led the way forward in a new discipline: science studies. The kernel claim of the strong program is that scientific, technological and technoscientific knowledge, goals, practices, instruments, objects, facts, and so on are socially constructed. This means that scientific "truths," "efficient" techniques, and stable technological systems and objects are produced by ongoing social negotiations and conventions. Shared interests and special forms of social life in a community determine what is accepted and popularized. There is no essential difference between technoscientific activities and political actions. Society is the driving force behind technoscience.

Following the establishment of the strong program, three pioneering studies appeared: Bruno Latour and Steve Woolgar's *Laboratory Life: The Social Construction of Scientific Facts* (1979), Andrew Pikering's *Constructing Quark: A Sociological History of Practice Physics* (1984), and Steven Shapin and Simon Schaffer's *Leviathan and Air Pump: Hobbes, Boyle and the Experimental Life* (1985). Although later Latour helped build another family that opposed social constructivism, it is clear from the subtitle of *Laboratory Life* that he had been ever influenced by social constructivism. After a period of methodological and ontological reflection, Latour confidently dropped the word *social* from his title for the 1986 edition. He and Woolgar wrote a postscript to proclaim the demise of "the social," arguing that society is not prior to natural facts. Latour had begun to formulate a new and generalized *principle of symmetry* that claims that the scholars of STS, as anthropologists, should stand at a middle position between nature and society, subjects and objects, and values and facts.[14]

Had Latour ever been a social constructivist? I tend to believe the affirmative answer. One can object to the belief by citing the following paragraph: "the distinction between 'social' and 'technical' factors is a resource drawn upon routinely by working scientists. Our intention is to understand how this distinction features in the activities of scientists, rather than to demonstrate that emphasis on one or the other side of the

[13] Kuhn wrote, "It is needed, that is, to defend notions like truth and knowledge from, for example, the excesses of postmodernist movements like the strong program" (Kuhn 2000: 91). Daiwie Fu (2008) commented that Kuhn's hostility to the strong program remained a mystery to him: as one of the Taiwan translators of Kuhn's classic, *The Structures of Scientific Revolutions*, Fu found it quite easy and even straightforward to travel from Kuhn to the next generation of social constructivism. Hacking gave an interestingly psychological explanation for Kuhn's hostility to the strong program in his response to Fu (Hacking 2008: 268–70). But Hacking scores Kuhn as a thorough social constructivist, according to his three sticking points: contingency, nominalism, and explanation of stability (Hacking 1999: 99).

[14] In 1987, Latour formulated seven rules of method in his *Science in Action* (1994). They can be read as an expansion of the generalized principle of symmetry and a methodology for the research program of ANT. Latour attempted, I believe, to replace the four tenets of the strong program with his seven rules.

duality is more appropriate for our understanding of science" (Latour and Woolgar 1986: 27). Latour seemed to reject that he was a social constructivist by explaining his misuse: "Given our explicit disavowal of 'social factors' in the first chapter, it is clear that our continued use of the term was ironic" (Latour and Woolgar 1986: 281). Did he in fact *explicitly* disavow "social factors"? Is the disavowal not a certification with hindsight? Some message seems to be revealed in their confession: "it was not clear until now that we could simply ditch the term: our new subtitle now denotes our interest in 'the construction of scientific facts'" (Latour and Woolgar 1986: 281). This seemed to mean that Latour needed at least seven years to get rid of the philosophical imprinting of social constructivism. For in spite of fretting about the distinction between "social" and "technical," Latour and Woolgar still retained statements such as "It was not simply that TRF was conditioned by social forces, rather it was constructed by and constituted through microsocial phenomena" (Latour and Woolgar 1986: 236). Even microsocial phenomena are social.

Latour's rebellion against the social constructivism sparked a retort from Harry Collins and Steven Yearley (1992) and from Bloor (1999). Latour responded to their criticism by standing at the position of actor network theory, which pushed him far from social constructivism (Callon and Latour 1992; Latour 1999). Obviously those philosophical debates within STS studies marked the proliferation of theory versions. One can see that the establishment of the new family, actor network theory, was recognized by the new methodological and ontological assumptions. Actor network theory is an academic brand French in origin and style; partisans of that theory insist that sciences and technologies are not socially constructed. Rather, since societies, communities, and interests stand on one side, with facts, artifacts, and objects on the other, everything is at the same time constructed in/as a network that is a mix of human and nonhuman actors/actants. Every actor's function, role and interest is continuously modified, broken, recombined, and translated in the process of the construction and reconstruction of actor networks.

Another debate centers on Bloor and Barnes's insistence that Collins's philosophical position amounts to methodological idealism, while their own approach is based on methodological materialism (Barnes, Bloor, and Henry 1996: 13–17). Collins himself prefers to refer his philosophical position to methodological relativism. This debate contributes to the idea of proliferation of theory versions within the social constructivism family. One may wonder why Collins created a new version rather than a completely new family. In his recent grand book, *Gravity's Shadow*, he reviewed the difference between Bloor and himself: "David Bloor has criticized methodological relativism even though it seems to me to be a corollary of the principle of symmetry in the so-called strong program in the sociology of knowledge. Bloor, a philosopher, considers the idea too 'idealist.' . . . Nevertheless, in face-to-face discussion Bloor and I have established that, though we may disagree on philosophy, the consequences for the practice of studies of science of our respective positions are almost identical" (Collins 2004: 797n11). This philosophical disagreement and practical agreement distinguishes different theory versions within an identical family, as the foregoing quotation shows.

I am not suggesting that philosophical debates are the sufficient causes of the development of STS studies. Of course, STS studies could develop without any philosophical considerations—such is a purely positivistic and empirical research! But philosophical debates are necessary for theory-branching in STS studies. There is a normative implication to my argument: members of the field should pursue normal development by constructing a new version or a new family rather than by merely applying established theories to empirical cases. This does not mean that we should pursue philosophical speculations to the detriment of empirical investigations: careful research ought to be integrated with philosophical thinking.

Let us return to my questions. Can philosophy contribute to the (normal) development of STS studies in Taiwan? Yes—in fact, the branching process typically found in European STS theories fits well with my model of theory versions. What is the connection between STS studies and philosophy of science? The philosophical debates that take place over the development of STS studies, I should note, are peculiar to the philosophy of science, both because STS studies are a science and because the debates inevitably involve the nature of science. It is the philosophy of science, technology, and society as a complexity; it combines the philosophy of natural science, the philosophy of technology, and the philosophy of social science. In a nutshell, philosophers of science in Taiwan should extend their vision to the philosophy of STS studies.

So far I have only tried to refute the skepticism surrounding the benefits philosophy of science offers to STS studies. However, this is only a negative response. Now I shall ask what philosophers can learn from STS studies.

7 Positive Strategies for the Extension of Philosophy of Science

Much of philosophy is academic, speculative, and metareflective—it does not seem to have much to do with social practices. Empirical STS approaches would appear to be more appropriate for understanding social practices, for they produce relatively concrete, contextual, and local knowledge and strategies rather than abstract principles, theories, and metaphysics. This does not imply that philosophy of science is no longer necessary. Empirical studies are still academic activities that need philosophical reflections; on the contrary, philosophical activities are also social. Thus philosophy of science and STS studies permeate each other. In order to engage in effective practices in this technoscientific society, philosophy of science is needed to provide general knowledge and a general image of science as a whole for the public. It is helpful as we set out to develop practical strategies. Therefore, I suggest that the appropriate place for philosophy of science is at a meta level, where it can encourage, catalyze and reflect a variety of scientific, humanistic, and sociological researches in Taiwan.

Several strategies for philosophy of science shall be suggested. These strategies, I hope, will not only urge young people in philosophy of science to think of their direction, but also attract the STS people to engage with studies in the philosophy of science, technology, and society. A new hybrid with the “blood” of both disciplines may emerge from this thinking and engagement.

(1) *Constructing a new and general image of science*. Most of the studies carried out by STS researchers are quite limited in scope. Only a few have attempted to construct a broader and general image of science as a whole. Moreover, constructing such an image will, I predict, become harder when STS studies develops into a “normal science.” It is likely that most of the scholars in the

field will reject a single, general scientific image, as they are inclined, by and large, to see diversity rather then generality. However, to search for an image about science as a whole is always the goal of philosophy of science, and the mission provide a complementary, macro speculation to a large amount of micro STS case studies. The new scientific image, of course, should be constructed by facing STS studies.

(2) *Constructing plural scientific images linked to a general image*. In all likelihood there are many diversified sciences, and no single image can stand for all of them. Still, usage suggests that a single and general image of science can help the public cope with the complexity of the universe of sciences. In addition, we need to construct a nonpositivistic image of science to compete with the positivistic one that dominates the popular mind in Taiwan. But does a single, general image *in conception* conflict with plural sciences *in fact*? Let me appeal to the concept of family resemblance. On one hand, a single scientific image presents an image of the family of sciences rather than an image of a united science with a reductionism order; on the other hand, images of every member of the family of sciences should be constructed so as to echo the family's general image.

(3) *Extending the scope of philosophy of science*. How can philosophers ensure that the results of their labors are helpful in many STS case studies? They need to extend their concerns to technology, medicine, and society and expand their scope to include philosophy of natural, social, technological, and medical sciences. Otherwise, philosophers of science have no ability to provide a general image and plural images. This does not imply that they should take a position that pleases every STS researcher or that all of their claims should be compatible with STS studies. But their traditional focus on physical, chemical, and biological knowledge is really too narrow for the new role I propose. Moreover, STS studies have issued a challenge to philosophy of science by treating science, technology, and society as a complex whole. What will the reply to this challenge be?

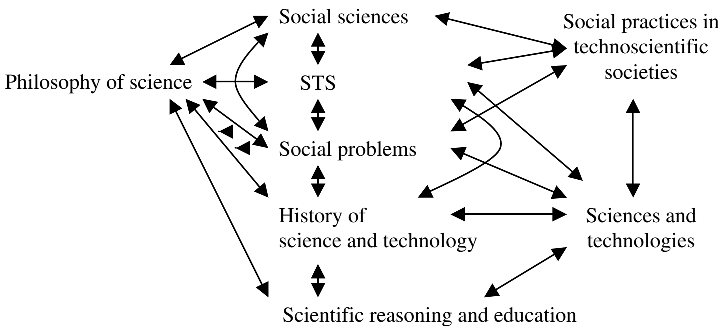
(4) *Showing the social aspects of the sciences*. In the long tradition of philosophy of science, thinkers have seldom given much thought to the social aspects of the sciences. In order to respond to the STS challenge, philosophers will have to present their views on the relation between science and society. This does not imply that they must accept social constructivism. However, they do need to speak out on the subject rather than keep silent. Even a realistic view is also a response to the issue. The dominant realists in philosophy of science have to answer what role society plays in the development of science. After all, many scientific disciplines—medical science, public health, ecology, nursing, informatics, etc.—involve both of the natural and the social.

(5) *Formulating a philosophy specific to STS studies*. In an academic institution, STS studies amount to a humanistic and social science about science, technology, and society. History, sociology, and other disciplines that inspired STS studies each have their own philosophical commitments—so should STS studies. According to Quine, Kuhn, Feyerabend, and Laudan, all sciences inevitably involve philosophy. STS studies are no exception. In fact, many philosophers of science have identified the philosophy of STS studies as a

strong form of social constructivism or relativism—then refuted it. But this effort is in vain. Not all theories of STS studies share a common philosophy. This means that we ought to dodge the traditional attitude of most philosophers of science, who see STS studies as a rival. On the contrary, philosophers should appraise new theories, explain its history and success, discern the discipline's different philosophical commitments, and try to guide its development. To sum up, the philosophy of STS should treat STS studies as a science.

(6) *Creating a practical philosophy for a technoscientific society*. The technoscientific societies have brought forth endless problems of modern life, from environment preservation and social changes to globalization and the extinction of traditional cultures, which may be addressed under the heading of ethics, social philosophy, and practical philosophy. They tend to be excluded from philosophy of science, whose traditional concerns are methodology, epistemology, and metaphysics. But philosophy of science should be extended to practical philosophy of science, which addresses the concepts and elements peculiar to technoscientific society, the genuine environment of our lives. (A technoscientific environment is different from the traditional natural and cultural environment both in concept and in substance.) This technoscientific environment itself is changing rapidly, owing to a large amount of novel scientific concepts and technological objects. So philosophers need to make revisions to the technoscientific image over time. Technoscientific society will be better able to understand itself thanks to this sort of work, and philosophers should contrive practical strategies for assisting the public to cope with the technoscientific environment. This is the practical philosophy of technoscientific society.[15]

Let me sum up these strategies into a conceptual frame or network.



I want to highlight three points. Philosophers of science should study and converse with not only natural sciences and technologies but also STS studies, history of science

[15] An anonymous reviewer suggested that there are useful models in Western philosophy of science to support my emphasis on a more policy- and case-oriented contribution, for example, Kitcher's work on science, truth, and democracy, Cartwright's work on causality, and on evidence for use in policy analysis, Sherrilyn Roush's work on evidence, and Wendy Palmer's and Elisabeth Lloyd's work on climate modeling and its application. Yes, I agree. Of course, East Asian philosophers of science can also develop policy-oriented reflections from East Asian STS case studies.

and technology, the social sciences, and so on. Second, philosophers of science should connect to social practices via the social sciences, STS studies, and the problems of modern technoscientific life. Third, there should be a close interaction between all pairs of neighboring items connected by a two-head row. The frame provides a map of related disciplines and defines the social place of philosophy of science in Taiwan.

8 Why Not Philosophy of Technology?

In June 2009, I presented a draft of this article at the third *East Asian Science, Technology and Society Journal* conference. Members of the audience questioned me about the specific role I imagined for philosophy. The anthropologist Francesca Bray asked, "Why not philosophy of technology?" Obviously, I regard philosophy as crucial for the normal development of various sciences, including STS. I will further confess that I prefer philosophy of science to philosophy of technology, for two reasons.[16] Science has provided us a variety of perspectives on the world since the seventeenth century. The pictures we form of the world need to be continuously examined and revised to cope with the changing world. To do this, we need a general image of science as well as plural partial images of particular sciences—this can be provided by philosophy of science. To my knowledge, philosophy of technology does not really concern worldviews, although it can provide a single overall image or several discrete images of technology, skills, and human abilities. The second reason I prefer philosophy of science is that STS studies are themselves a kind of science, and philosophical inquiries into that discipline are thus a kind of philosophy of science. The philosophy of STS studies can explain their relation to other sciences and relax the tension between STS studies and other natural sciences.

I admit that STS studies could be seen as a technology—a variety of techniques to change, transform, or translate the relation between science, technology, and society. Worldviews could be seen as no more than the unintended results of the intervention of technology in the world, much as Martin Heidegger argued that hammers were used before they were understood (Heidegger 1986). According to this view, philosophy of technology precedes philosophy of science. Yet philosophers of science can also argue that the ways in which technology intervene in the world are always influenced by an existing picture of world—a thesis based on the idea that skills must arise out of beliefs, just as observations are shaped by existing mentalities. We appear to have run into a dead end, an interminable chicken-and-egg enigma. To solve the impasse, let us jump to the meta level and reflexively ask whether philosophy of technology is a science or a technology. I cannot conceive of the philosophy of technology as a technology: this is my final argument for the key status of philosophy of science. This is not to say philosophy of science is somehow superior to philosophy of technology. Simply put, philosophy of science cannot be replaced by philosophy of technology.

[16] An anonymous reviewer suggests that one way to emphasize my claim for the continuing relevance of philosophy of science compared to philosophy of technology is to ask whether there is, as yet, such a thing as philosophy of technology. Yes, I agree. Philosophy of technology appears much diversified and disunited as technology does. Maybe the STS scholars will regard this feature as being advantageous.

Still, the philosophy of technology can play an integrative and incentive role in the normal development of STS. This is not incompatible with the key status of philosophy of science, as both philosophies need the other. But what sort of philosophy of technology can contribute to the development of STS studies?

At the foregoing conference, Taiwanese sociologist Huang Ho-Ming 黃厚銘 insisted that Heidegger's philosophy of technology was also a kind of STS studies, arguing that it could guide the development of STS studies. I doubt this, since Heidegger's speculation was done without the analysis of real techniques and the history of technology. Since I cannot provide a detailed explication on Heidegger's philosophy, let me address any philosophical theory whose contents lack discussions on the history of science or technology. Can any such a philosophical theory guide the development of STS studies? The historical lesson is clear on this point: logical positivism was discarded due to its separation from the history of science. If we consider the lesson seriously, a philosophical theory of technology without the history of technology should not be adopted as a useful guidance. Of course, the issue needs more analyses rather than mere assertions. Let me leave it for the future.

9 Conclusion

Inevitably, the Taiwanese scholars who specialize in philosophy of science and STS studies owe a tremendous debt to their Western predecessors, yet one can speak of peculiarly Taiwanese developments in both disciplines. As Western philosophers of science have encountered STS studies in recent years, they have turned away from what could be a very fruitful dialogue. The Taiwanese philosophers of science should not go down the same road, although they are facing the challenge posed by STS studies. They should treat the challenge as a moment to conduct a collaborative interaction with the scholars of STS studies. But a moderate Oedipus and reverse Oedipus complex between philosophy of science and STS studies may be healthy. I hope that in the years to come there will be more philosophy in STS studies and more acknowledgment of the importance of science, technology, and society in philosophy. The two traditions could remain distinct in their everyday operations, with continuous intersections for renewal and reflection. Such a future is exceedingly promising.

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