【論文】

Reevaluating the Initial Social Accounts of Scientific Practices Discussions

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Abstracts

This paper proposes that we need to be back to the initial big questions again to revitalize the science studies. In 1980s, there were many fertile and intrinsic questions in the discussions about the social accounts of scientific practices. But in this decade, many science studies have divided into more discipline-specified small questions. The departmentalized and single-filed elaboration abates the chaotic but imaginative inquiring mind. In this paper, I emphasize the necessity of intrinsic philosophical discussions like in the initial debates. This paper focuses on the reexamination of the eliminability of "the social" factor, which is a critical issue in the science studies, mainly through the Bloor-Laudan debates.

Key Words：Science Studies, Social, Strong Program, Worldview, Value, Consensus

1. Introduction

In this paper, I will compare two extreme positions about how to understand the nature of scientific practices. One is the "consensual"（Worrall, 1984）view. The advocates deem that the state of consensus is normal in scientific practices. Thus, their main question is how the normal state can be maintained; in other words, how and why certain scientific disagreements are rationally resolved. L. Laudan（1984）is a radical figure of this view. Another is the "dissensual" view. The subscribers of this view regard the state of dissensus in scientific practices as natural. They also ask how the natural state is caused: that is, their main question is why scientists maintain different positions. A group of extremists participate in the "strong program" or the sociology of scientific knowledge. D. Bloor（1976）is a prominent advocator in the program.

Their positions are the reactions to T. S. Kuhn's denunciation of the traditional "logicism" based on the consensual view. Until 1950s, the logicists dominated the discussions about the resolutions of scientific agreements. For them, the state of which scientists agree is normalcy. As an explanatory model of consensus formation, they propose a "hierarchical" model. It consists of hierarchically ordered the scientists' basic commitments: factual/theoretical, methodological, and axiological ones. The disagreements about certain theoretical claims, although often breaking out, are quickly resolved by the shared certain methodological and axiological claims. They usually assume that there are no significant cleavages in the scientists' axiological claims. For their position, methodology is the key for scientific rational action. The logicists' most important role is to fix the epistemic values of certain normative methodological claims. For this purpose, they have attempted to find the universal standards of rationality from scientific theories as finished products, especially logical claims. The logicists' main problems are that (i) they totally eliminate "actors" from the process of knowledge production, (ii) they ignore the changes of rationality standards along with the development of science, and (iii) they do not have any firm grounds to evaluate the axiological disagreements.

The holistic historicist models of scientific change which Kuhn (1970) and Feyerabend (1975) present have broken the domination of the consensual view. In their model, the scientists' disagreements are ever resolved after all inasmuch as the disagreements are deeply rooted in differences of their worldviews or "paradigms." A theory is "underdetermined" by facts or evidence. There are no universal standards which govern the scientific practices as a neutral and objective connector between an old paradigm and a new one. They think that a scientific theory is a closed and self-developing system of concepts. The meanings of elements are totally determined by the overall structure of theory, "paradigm." Changing one concept always transform the structure as a whole. According to the holistic view of scientific practices, a new structure is incommensurable with the previous one. Their position is based on the "epistemic relativism." Thus Kuhn, as Barnes (1977: 23) valuates, proclaims that "fundamental theoretical transitions in science are not simply rational responses to increased knowledge of reality, predictable in terms of context-independent standards of inference and evaluation." Their actual concern is to "describe" a variety of norms, methodologies, values, and goals, operating in science. However, this account does not fit for most historical changes in science well. Certainly, there have been many disagreements in science, as the model points out; however, the disagreements have not been resolved in irrational or extra-rational fashion.

Bloor and Laudan are two prominent constructors of alternative models to understand the nature of scientific practices. Epistemology as a "normative philosophy of scientific knowledge" has been attacked. Philosophers, such as Feyerabend and the neo-Popperians, principally deny the existence of scientific research rules in scientific practice. Some advocate that epistemology should be analyzed by methods of empirical sciences: "naturalized epistemology." They deny the rationalist elements, "fixed *a priori* truths about logic or conceptual frameworks to which science must conform." The first prominent figure is Quine. For him, we progressively obtain better knowledge of the objective natural world by methods of try and error, correlated with our biological and cultural evolution. Success of knowledge is guaranteed only in the struggle for existence, not in the *a priori* truths.

However, the naturalized epistemology is so individualistic despite the inclusion of biological and cultural evolution in the scope. Bloor and his comrades conclude that the epistemology is insufficient to consider social context of science. Thus, the advocates of the strong program purport to build "socializing epistemology (Hesse, 1988)." They propose that wider social contexts should "always" be considered in analyses about scientific knowledge. They essentially extend the Kuhnian model. On the other hand, Laudan (1984) presents the "reticulated model." He tries to solve the question of irrational or extra-rational scientific changes presented by Kuhn more rationalistic or reasonable way. The basic point of his model is the homeostatic adjustment of three elements of scientific practices: theory, methodology, and aim. He emphasizes, "Axiology, methodology and factual claims are inevitably intertwined in relations of mutual dependency." Based on the homeostatic mutual dependency model, he tries to describe scientific changes in piecemeal fashion, instead of the Kuhnian extra-rational revolutionary changes. In short, his purpose is to denounce the Kuhnian model.

Essentially, the comparison of the two extremists' very different metaphysical positions is inconclusive. Nevertheless, they also propose more pragmatic or methodological stance to study scientific practices. In this ground, the comparison will provide useful framework to approach to scientific practices. One of their common methodological grounds is the above-mentioned "naturalized epistemology." The naturalists share the commitment to the continuity of epistemology and science, rejecting the autonomy of the former. The problem is how to interpret the continuity: contextual, epistemological, methodological, analytic, metaphysical, and axiological continuities (Maffie, 1990). The critical point in the debate between Bloor and Laudan is whether or not social or extra-rational factors should be included in an analytical framework for scientific practices. Their main concern is how accurately scientific practices and progress can be interpreted empirically. Thus, the orientation to normative prescriptions about scientific practices, which the traditional epistemologists have maintained, is not so "strong." Epistemological claims have been regarded as normative prescriptions whereas scientific ones basically as categorical descriptions. Their orientation is the latter.

Their debate is also included other important issues in the science studies, such as the issues of internal/external approach and micro/macro level of analysis. Furthermore, it is strongly related to the social legitimation of an emerged disciplines, sociology of scientific knowledge. The power struggle or tension between philosophers and sociologists stimulate the emergence of new issues and disciplines, for example, cognitive sciences and computational models. Therefore, the comparative analysis between Laudan and Bloor gives us significantly useful resources to understand scientific practices better. In the following section, I will show the characteristics of their models. Then, I will discuss the critical issues, such as relativism and rationality, which separate their positions.

2. Characteristics and Problems in the Social and Rational Accounts of Scientific Practices

2.1. Bloor and the Strong Program

The advocates of the strong program purported to analyze the very content of scientific knowledge. The sociology of knowledge, like K. Mannheim, has focused on the correlations between scientific beliefs or practices and socio-political concerns in the wider society. S. Shapin (1982) emphasizes that the correlations are not conclusions but only starting-points to scrutinize scientific knowledge as a social product. Some sociologists, like P. Forman, have used "intellectual resources associated with other forms of culture" as a cause of scientific belief. Others, such as D. Mackenzie, have used particular views of nature as a discourse strategy of specified social groups.

Intellectually, the strong program is influenced by E. Durkheim and L. Wittgenstein. Bloor (1976: 45) expresses the Durkheimian influence as follows: "When men think about the nature of knowledge, what they are doing is indirectly reflecting on the principles according to which society is organized." Therefore, the aim of the program is to specify the relationship between certain knowledge claims and the social bases. Bloor insists that "it is only by examining the culture of science that we come close to the heart of that activity." The Wittgenstein's influence shows on the interpretation of scientific practices. Many people refuse the idea that scientific practices are merely “social conventions.” That is because they think that the conventions based on people’s consensus are eventually “arbitrary” decisions. Conversely, Bloor (1976: 32-37) insists that the conventions are socially constrained by the social credibility and practical utility; thus, they are arbitrary. Inevitably, these conventions are not self-evident, universal, nor static because of societary nature. Like any other social conventions, scientific theories and procedures should harmonize with other conventions accepted in a social group. This interpretation, for instance, is strongly influenced by Wittgenstein’s concept of “language-game.”

One of the main obstacles to the sociology of scientific knowledge is that “non-social” criteria, such as “impersonal criteria for making observations and performing competent experiments,” solve scientific disputes. Nonetheless, some sociological studies of scientific knowledge, such as H. M. Collins (1975) and A. Pickering (1984) show that scientists’ theoretical claims or judgments are underdetermined by reality, logic, and impersonal criteria of the “experimental method.” The underdetermination opens the ways to the sociology of scientific knowledge. As well as the description of the underdetermination, the sociology purports to show the contingency between knowledge claims and the certain group’s “collective consciousness” or social context. Certain skills and technical competence, obtained through socialization, consist of “a set of vested social inte rests” within a scientific community.

Most traditional sociologists of Science, such as R. K. Merton, have not scrutinized the internal nature of knowledge itself but limited on the institutional framework which externally influences the growth or direction of knowledge. Related to this trend, Bloor (1976: 37) strongly criticizes the imperialistic influence of the analysis of scientific contents by philosophers or psychologists of science. With respect to usage of language of truth and falsity, the strong program is opposed that false beliefs categorized “*a priori*” are explained differently from truth ones. A target is Lakatos’ clear separation of “internal” and “external” histories. By emphasizing the self-sufficient (autonomous) development of scientific knowledge, Lakatos (1971) emphasizes the disciplinary superiority of philosophers’ “rational reconstruction” or “internal history.” In his sense, sociologists can elucidate merely the irrational empirical residue. Bloor (1976: 8) strongly criticizes the tacit “teleological (goal-directed)” assumption in Lakatos’ vision of knowledge and rationality. That is, Lakatos assumes that man as a rational animal naturally orients to true beliefs which need no special comment. This natural tendency towards true beliefs is disturbed by social or extra-rational impediment to scientific progress. Thus, the disturbance should be explained by sociology. Another target is the Baconian empiricists. F. Bacon states that the social influences, which he calls the “Idols of Tribe,” the “Market Place,” and the “Theater,” distort our perception and sensory-motor apparatus to produce true beliefs. The empiricists also use *a priori* division of labor between psychologists and sociologists: the psychologists deal with true knowledge while the sociologists with error one. Nonetheless, criticizing the overconfidence of individual experience, Bloor (1976: 10-13) stresses that the individual experiences are signified by socially-shared (theoretical) knowledge, “collective visions of reality,” as culture. The truthness cannot be determined *a priori*.

Against the containment of the sociology of science in the explanation of error and irrationality, Bloor (1976: 9) presents the strong program, metaphysically based on a causal model. Certainly, he admits that the comparison of the metaphysical differences between teleological and causal models is indeterminable.

However, he emphasizes the methodological predominance of his program. That is, his program, unlike the teleological one, holds the basic requirements of empirical sciences, such as causality and moral neutrality. For Bloor (1976: 3), the most basic aim in any sciences is to determine certain "causal" relationship in the data at hand. Methodologically, the program declares that "the sociologists is a scientist too, and ought to act as scientists do." For the strong program, "social causes are always present; they are the determining factors (Brown: 1984)."

Bloor (1976: 40-47) analogizes the exclusion of the sociology of scientific knowledge from the philosophy of science to Durkheim's distinction between the sacred and the profane. He emphasizes that many philosophers and sociologists' mythtification of science by the dichotomous distinction is only a strategy to defend the ideological position. Analyzing the Popper-Kuhn debate, Bloor (1976: 2-3) shows how epistemological debates are connected to ideological ones. After all, theories of knowledge are reflections of continuously changing social ideologies. Thus, to elucidate the nature of knowledge, one must analyze the fundamental origins of ideological presuppositions. For the sociologists, knowledge does not mean true beliefs but "what men take to be knowledge." The starting-point of research for a sociologist of knowledge is how and why knowledge changes.

According to Shapin (1982), what he calls the "coercive" model to which most philosophers and historians of science have subscribed has the following characteristics:

i) Sociological explanation consists in claims of the sort: "all (or most) individuals in a specified social situation will believe in a specified intellectual position."
ii) It treats the social as if one could derive it by aggregating individuals.
iii) It regards the connection between social situation and belief to be one of "determination."
iv) It equates the social and "irrational."
v) It equates sociological explanation with the invocation of "external" macro-sociological factors.
vi) It sets sociological explanation against the contention that scientific knowledge is empirically grounded in sensory input from natural reality.

The traditional sociology of knowledge has attempted to specify single types of social interest. Shapin stresses that "If one proceeds in a traditional historical manner and concentrates on key individual actors, one can discover interesting differences in their social and political views." It is not an accurate picture of sociological practice. Shapin presents an "instrumental" model: "Knowledge is produced and judged to further particular collectively sustained goals."

The sociology of scientific knowledge "as a scientific discipline," Bloor (1976: 4-5) accentuates, should be maintained the following four methodological tenets.

i) The "causal" relationships bringing about certain knowledge should be elucidated.
ii) The dichotomous explanations with reference to truth or falsity, rationality or irrationality, and success or failure, need "impartial" treatments. Both sides need explanations.

iii) The style of explanation must be "symmetrical"; that is, both sides of the dichotomies must be explained by the same types of causes.
iv) The patterns of explanation ought to be "reflexive" or applicable to sociology itself.

2.2. Laudan and the Reticulated Model

Laudan (1984: 42-66) presents a model of consensus-formation mechanisms when scientists disagree about their basic cognitive aims or goals. Traditionally, philosophers of science, including Kuhn, "postulate a unidirectional justificatory ladder" proceeding from axiological to methodological to factual claims. Aims and goals define methodology, which does factual and theoretical claims. A lower level disagreement is resolved by a higher level of elements. But a disagreement about aims and goals does not have a higher level of resolution for scientific debates, such as conceptual economy, predictive accuracy, and manipulative simplicity. Others, like Popper and Reichenbach, it tends to be regard aims or goals of science as a subjective and emotive matter, not as a reasonably negotiable one. Conversely, exemplifying the Newtonian success of hypothetico-deductive method, Laudan shows that the Newtonian scientists developed a new axiology of science to endorse the unobservable entities against the empiricist tradition dominating their age. Thus, he emphasizes that aims or goals of science should not be eliminated from reasonable criticism and modification.

Laudan (1984: 68-87) endeavors to reform Kuhn's model of scientific revolution. The biggest complaint is that the unit of analysis for scientific change, "paradigm," has so extricable and static character. A paradigm shift is caused only by a simultaneous change of the ingredients, i.e., ontology of nature, methodology, and cognitive values about teleology of natural inquiry (aims of science). The individual changes of ingredients do not affect the shift at all. Therefore, for Kuhn, scientific changes are basically non-rational and non-sequential. However, this explanation does not fit the actual course of science history, as Laudan says. In particular, Kuhn argues that "inter-paradigmatic disagreements …must always be resolved using extra-rational resources." Laudan (1987a) believes that scientists' aims and goals of science are not so different from each other; furthermore, the paradigm choices are rationally explainable. There are often rational grounds to change one's aims and goals. In part, one's cognitive values expressed overtly must consistent with the latent ones in one's theory preferences.

Kuhn regards an individual paradigm as a rigidly closed system. Different paradigms have different contents and standards; inevitably, the inter-paradigmatic comparison of the ingredients in the paradigms is meaningless and inconclusive. Thus, a paradigm shift, as a fundamental closure of scientific debates, must be understood by the factors outside the contents of paradigms, such as demise of some participants and institutional power struggle in a scientific community.

Against Kuhn's relativistic, subjectivistic, and irrationalistic model of scientific progress, Laudan revises Kuhn's model significantly. He strongly denies the inextricability of paradigm ingredients. Even though recognizing some revisions of some components within a paradigm, Kuhn denies any change of the fundamental commitments, "hard core," change which forces to reject the paradigm itself. In fact, scientists themselves, Laudan points out, have not so recognized own paradigms. Rather he states that even an individual element in a paradigm provides a decisive factor to compare paradigms. In addition, he insists that Kuhn's hierarchical view of justification should be substituted with his "reticulated" model. Laudan says that the Kuhnian holistic and irrational view of scientific change is nothing but "tunnel vision, in which a sequence of gradual shifts is telescoped into one abrupt and mighty transformation." Against the Kuhnian telescope interpretation, Laudan emphasizes that "the various components of a worldview are individually negotiable and individually replaceable in a piecemeal fashion."

Laudan criticizes the covariant presumption underlying the "hierarchical" model: a factual or methodological disagreement is ultimately caused by the axiological differences. He emphasizes that "axiological differences can coexist with factual-level and methodological agreement." Moreover, even in the major scientific revolutions, he points out, scientists have resolved the axiological disagreements in a "logical and reasonable" way. To evaluate the viability of proposed cognitive aims rationally, Laudan uses his "reticulated" model of justification. Compared with the unidirectional justificatory ladder in the hierarchical model, the reticulated model stresses the "mutual dependency" of the elements, i.e., theories, methods, and aims. The reticulated model, a model of scientific progress, presupposes that there is no single "right" goal for inquiry. Laudan emphasizes that one needs to use change of axiology as an indicator of scientific progress, as well as changes of factual (or theoretical) claims and methodology. In his model, progress is ultimately measured by to what extent an aim or goal is accomplished.

Laudan points out two general standards to evaluate a proposed cognitive goal or a set of goals. One is the realizability or operationalizability of goal in question. Second is that a goal should harmonize with the implicit values in the practices and judgment commonly held by a group. For Laudan, meta-methodology contains the theory of methodology per se and the theory of axiology as a mixed empirical/conceptual discipline. "One of the decisive constraints on any proposed scientific aim is that we must have grounds for believing that it is realizable."

In this whole program, Laudan (1987c) tries to revitalize methodology. The traditional approach to methodology has strongly been criticized since 1960s. Some people say that there are no methodological rules in science at all; others, although granting the existence of such rules, deny the epistemic warrant or merit of methodology. Laudan says that "methodology is the study of how to conduct inquiry effectively." The inquiry is finished by the attainment of satisfactory answers or problem solutions. Our aims and goals evaluate what are the satisfactory answers. Thus, "a methodology is a theory about how to conduct inquiry so as to maximize the likelihood that the answers and solutions we produce will satisfy our ends (practical and cognitive)."

Laudan (1990b) insists that, whereas regarding epistemology and methodology as co-extensive with the sciences, "normative naturalism" provides methodological advice. Normative naturalism, Laudan says, is a meta-epistemology or meta-methodology which tells "how justification-rules or methodological rules can themselves be justified or warranted." Thus, a methodological rule ought to be "hypothetical imperatives," not categorical descriptions: "If one's goal is *x*, one ought to do *y*." It is not a convention, persisted by Popper and Lakatos, but a norm abstracted from empirical information. He says, "methodological rules …take the form of 'hypothetical imperatives' whose antecedent is a statement about aims or goals, and whose consequent is the elliptical expression of the mandated action." Axiology should also be studied empirically.

Against the historicists, "those who still see a prescriptive role for scientific methodology disagree about how to warrant that methodology," Laudan (1987b) says that the historicists' criticisms against methodology since 1960s have been premature to eliminate it thoroughly. The historicists, like Lakatos, have maintained two fundamental theses:

i) The rationality thesis (RT): most great scientists have made their theory choices rationally.
ii) The meta-methodology thesis (MMT): a methodology of science is to be evaluated in terms of its ability to replicate the choices of past scientists as rational.

However, the historicists' meta-methodology ignores the individual and collective variety of aims and methods. Laudan proposes that, to consider an agent's axiological rationality, we must analyze what actions agents select based on their aims and prior knowledge.

Laudan shows the following shared principle in the major theories of scientific methodologies. "If actions of a particular sort, ***m***, have consistently promoted certain cognitive ends, ***e***, in the past, and rival actions, ***n***, have failed to do so, then assume that future actions following the rule "if your aim is ***e***, you ought to do ***m***" are more likely to promote those ends that actions based on the rule "if your aim is ***e***, you ought to do ***n***." Laudan (1990b) says that "Rules are best seen … as proposed means to the realization of desired ends. To put it crudely, one is justified in following a methodological rule to the extent that one has good reasons to believe that it will promote the ends of inquiry." For Laudan, science has become progressively successful, according to our standards, at producing the epistemic goods as time goes by.

3. Discussions

3.1. Symmetry and Relativism

Hold moderate rationalism.

The older sociological tradition has maintained a hands-off policy about the contents of scientific knowledge. Many sociologists, even Barnes (1974: 180), tend to eliminate the problem of relativism by regarding not as a sociological problem but as an epistemological one. The strong program breaks into the knowledge and beliefs themselves. A major controversial point about the strong program is the very nature of relativism. Barns and Bloor (1982) present their version of relativism as follows. The simple starting-point of relativist doctrine is (i) the observation that beliefs on a certain topic vary, and (ii) the conviction that which of those beliefs is found in a given context depends on, or is relative to, the circumstances of the users. Sayers (1987) positions the strong program as a strong relativism against the absolutists of scientific rationality. The latter believes that there are standards of rationality at the end of our justification chains. In contrast, the relativists claim that the absolutism cannot be established "since it must presuppose its own truth to succeed." Instead, they claim that standards of rationality are only local ones "akin to what Wittgenstein calls language-games or a form of life." Therefore, there are no such standards at all. The controversy is caused by the interpretation of whether or not human conditions are inseparable from the products.

Gregersen and Køppe (1988) criticize that "epistemological relativism makes it impossible to account for the central facts of accumulation and the material core of science.” They classify relativism into three subcategories: epistemological, sociological, and historical relativism. That is, methods, theories, and worldviews, in science are exclusively determined by the primary relation between man and relations, collective consensus, or historical epochs, respectively. The strong program is not regarded as a supplement but an alternative to philosophical accounts. Gregersen and Køppe define that “Science is a complex social, psychological, institutional, linguistic, communicational etc. process.” Science is overdetermined, i.e., “in every element of the scientific process you choose, you will be able to find some relevant theme related to the different levels, from purely methodological questions to the most general worldview or ideological structures.”

I also think that epistemological relativism should be avoided. In contrast, sociological and historical relativism are inevitable in a certain degree. Bloor（1976: 97）attempts that any “internal” history can be reduced to certain social conventions and values at that time. Analogously, Bloor（1976: 85）says that the equator, like a territorial boundary, is nothing but a social convention. As the examples, he points out four “alternative” mathematics able to explain by social causes. As Freudenthal（1979）mentions, these examples are basically questions of definition, which are certainly the object of social consensus. However, he says, “they do not fall（and were never taken to fall）within the realm of mathematical necessity.” Moreover, we must recognize that a mathematical system is in continuous improvement process.

Laudan’s reticulated model, as Doppelt（1986）shows, cannot avoid what he calls “moderate relativism” : “the claim that while here are typically some good reasons for theory change in science, there are often equally good reasons for adhering to the older theory.” Laudan emphasizes that methodology and epistemology of science to assess various rules of inquiry and validation ought to be empirical disciplines rather than normative ones. Methodological norms and rules, for him, should be “conditional imperative.” Worrall（1988）points out the essential problems of the reticulated model as follows. Laudan emphasizes the inevitable emergence of “implicit methodology” which will overcome the dominant, explicit, methodology at certain time. However, if his reticulated model depends on such incommensurable implicit methodologies, it is not different from the Kuhnian model. If only on explicit ones, it does differ from the hierarchical model. Many methodologies, e.g., P. Duhem, H. Poincaré, R. Carnap, C. G. Hempel, and H. Reichenbach, have tried to find the general and unchanging principles in scientific practices. However, the traditional methodologies so not find real, implicit disagreements of methodology which must be resolved in the axiological level. Thus, their main concern is only how methodology governs factual or theoretical disagreements.

Worrall（1988）denounces that since no principles of evaluation stay fixed in Laudan’s reticulated model, we cannot objectively show whether there was progress. Thus, he concludes that “core” methodological principles must remain invariant. In his words, “laying down fixed principles of scientific theory-appraisal is the only alternatives to relativism.” Laudan（1989）counterattacks Worrall’s proposal of fixed methodology. He points out that even in the 20th century, there are many methodological standards implicit, and that the standards have been changed through time. Furthermore, there have been no universal methodological principles even in the so-called mature sciences. Laudan’s favorite example is the transition of methodology from inductivism to hypothetico-deductivism. He focuses on the internal rationality which the latter holds inside the system. But this transition was accomplished in the long-term try and error process by many scientists. We cannot simply say that this transition was caused only by the internal rationality. Therefore, I take Worrall's position in this issue.

Avoid causal symmetry.

Laudan agrees with Bloor that whether or not our theoretical beliefs are true or false is irrelevant to the explanations. However, he (1981) insists that a program for socializing of all forms of knowledge ought not to be committed to the thesis of causal symmetry. What the symmetry thesis is asserting, as Laudan states, is "the causal or explanatory irrelevance of one's knowledge of the truth, rationality or success of a belief in giving a 'naturalistic' account of how an agent came to have the belief." The radical advocates of the strong program emphasize that "whatever causal mechanisms we find useful for explaining beliefs, we should invoke them without reference to the epistemic or the rationality or the pragmatic status of the beliefs we want o explain." However, as Laudan points out, the symmetry tenet "*a priori*" establishes that the generation of beliefs is causally homogeneous. I agree with Laudan that the causal homogeneity about rationality or irrationality and success or failure of scientific beliefs should not be evaluated by the *a priori* criterion but by certain empirical tests. This a priori determination is inconsistent with the orientation to their naturalizing epistemology. I think that the relationship should be solved empirically. As Laudan says, "explanation by rea son" is not equivalent to socio-psychological one. We can reasonably specify why we regard certain beliefs rational or success by the goals and prior beliefs although we do not understand the "real" reasons thoroughly. On the other hand, an irrational or failure belief is the one which cannot be explained by the goals and prior beliefs. Different kinds of causal mechanisms are involved in rationality and irrationality.

Collins (1981) presents the "Normal program" which eliminates two tenets, the impartiality and symmetry principles, from the strong program. The principles are directly related to relativism: standards of truth or falsity, rationality or irrationality, and success or failure, depend on space and time locally. Basically, he agrees with the methodology of the strong program as "the correct method for social studies of science." However, he denies the relativism. The symmetry principle implies that "we must treat the natural world as though it in no way constrains what is believed to be." This is possible only if the dichotomous classifications, such as truth and falsity, regard as the categories which an actor perceives. The normal program uses terms "explained by reference to what is true, rational, successful or progressive (hereafter TRASP)" not as actors' categories. There are two kinds of explanations in the program. The first, "rational actor," explanation regards the TRASP knowledge as being accessible to participants of knowledge production. The second, "hidden hand," explanation treats the TRASPness only as the property of something other than actors.

Collins (1981) says that scientists' judgments of the scientific values of their claims are that a consensus (sometimes only a temporary one) is formed after a period of time in many disputes. In addition, data on the TRASPness, he continues, cannot enter reliably into an explanation of scientific knowledge at the time that the knowledge is being discovered. The consensus is not known while it is being formed. Thus, nothing related to knowledge of the ultimate consensus can have played a part in scientists' decisions about how to act in order to form the consensus (barring precognition). Therefore, data based on that consensus must be irrelevant to rational actor explanation of its formation. Collins concludes that "there is no sound methodology for rational actor explanations whether these are produced contemporaneously or otherwise." With respect to this point, I cannot agree with him. This kind of strategic and intentional action model cannot explain the stability of essential scientific practices. I cannot hold that any social or non-social values are not realized the participants.

Hold the reflexivity tenets.

Nola (1990) criticizes that the reflexivity tenet in the strong program causes an infinite regress for the causes of beliefs. He says, “Sociologists of science can never rest content with any belief unless they uncover its causal conditions; so they forever restlessly entertaining ever new beliefs because of the infinite regress of causes of belief.” The relativism is usually criticized the infinite arbitrariness, “anything goes.” Related to this question, I agree with Wittgenstein’s model (Baker, 1989). Wittgenstein, as Sayers (1987) says, does not believe that “the lack of some ultimate standard of rationality holds no terror for us after all.” Certainly, our beliefs are socially constrained by own “language game.” Moreover, they can communicate with other cultures by some internal revisions since they are not infinite. The problem of reflexivity arises only “if a further assumption is made to the effect that beliefs which are, or can be, explained sociologically are in some way defective − whether false, prejudiced, irrational or whatever.” That is because the outcome of the sociology of scientific error seems to be self-defeating.

3.2. Rationality or Social Determination

A division of labor between philosophers and sociologists of science by a “arationality” principle was destroyed by the emergence of the social studies of science, particularly the strong program. Laudan (1977: 196-222) tries to eliminate “cognitive sociologists” of knowledge from the history of science driven by “intellectual historians.” The sociologists’ essential task is to find social origins or roots of any beliefs: “any cognitive sociological explanation must, at the very least, assert a causal relationship between some belief, ***x***, of a thinker, ***y***, and y’s social situation, ***z***.” he admits that the sociologists have attempted to determine correlation between scientific beliefs and social situations, such as social classes, economic backgrounds, system of kinships, occupational roles, psychological types, and patterns of ethnic affiliation. However, Laudan concludes that they have failed to specify any significant relationships since most scientific beliefs are of no social significance whatever. Thus, “the sociology of knowledge may step in to explain beliefs if and only if those beliefs cannot be explained in terms of their rational merits” : problem-solving effectiveness. On the other hand, Laudan (1977: 132, 214) significantly broadens the concept of scientific rationality. That is, it means “how the ‘intrusion’ of seemingly ‘nonscientific’ factors into scientific decision making is, or can be, an entirely rational process.” He admits neither universally recognized and adhered to analysis of rationality, nor single algorithmic way of making a rational choice. There are a variety of factors that can be taken into consideration in any decision, for instance, what sorts of arguments the actor uses and what factors he takes into consideration are his own choice. Laudan defines the division of labor between philosophers and sociologists as follows: “the philosopher of science studies the logic or rationality of the story that the actor tells, taking into account all of the various factors that the actor considers relevant, while the cognitive sociologists studies how the actor came to construct the particular rational story that he did from the various sources available to him.” However, Jennings (1984) rejects Laudan’s arationality and asymmetry principle because it hampers the fruitful cooperation between philosophy and sociology. Strongly believing that there is a standard and universal account of scientific rationality, Laudan attempts to eliminate the sociology of scientific knowledge totally. His attempt is nothing but “the idea of academic territoriality, or professional boundary maintenance.” In short, Jennings says, “Laudan is engaged in a kind of professional imperialism.” Laudan’s starting-point is to build a standard model of rationality. The degree of rationality is measured by how a rational individual’s aims and goals have accomplished by certain means. Bloor（1981）criticizes the static interpretation of rationality. Moreover, he says, “social factors play a role in shaping the manner in which rationality itself evolves.” Based on M. Hesse’s “network model of classification,” Bloor explains why his “interest model” is adequate to show how and why our reasoning is socially grounded.

i）Since language is a shred practice that can be transmitted to new members of a social group, the decisions involved in concept application must be systematic and to a degree predictable.
ii）The decisions are called conventions. He says, the conventional character of language is what makes the profound involvement of society a pervasive and inescapable feature of knowledge.
iii）The particular form taken by the conventions of a classificatory network are the result of what Hesse calls “coherence conditions.”
iv）The crucial formula is that social interests are coherence conditions imposed on the classificatory network.

Hesse（1988）regards most works of the strong program as actors’ model. They claim the underdetermination of scientific theories by logic and evidence: “a theoretical discontinuity having important metaphysical implications cannot be explained as an outcome of purely scientific reasoning.” They strongly assume that a scientific community is only a subsystem within certain social system, no a closed and independent system. Hesse points out, “although first-order historical work is essential to arguments about the validity of the strong program, yet issues of principle are not going to be conclusively settled by historical evidence.” Thus, the difference between rationalist and strong program is basically to what extent we should consider social factors. Hesse calls the fundamental historical works on the strong program “actors’ models,” which concentrate on views and beliefs overtly expressed or discussed by participants of scientific disputes. Their concern is individuals’ rationalization. Yet they mostly neglect covert meanings in the process of theory-making, i.e., “the latent functionality of institutions.”

Bloor（1988）warns that the development of the sociology of scientific knowledge is significantly hampered by the rationalist history of science. One of the prominent rationalists is Laudan. The situation, he says, is very similar to the suppression of the Tubingen school’s critical movement of theology by the so-called supernaturalism in the 19th century: “the supernaturalists would relate the dogmatically ‘correct’ stance to divine inspiration, and the ‘incorrect’ stance to worldly ambition, ignorance or sin.” Even for the rationalists, Bloor says, the dualism is very difficult to sustain. The rationalist historiographers, e.g., Lakatos, declare that “to be rational was to be impressed if a theory can predict novel facts.” For them, a scientific debate will be closed if scientists present a “progressive” program able to predict novel facts. Just then, degenerating program, depriving predictive success of the novel facts by a rival one, should be abandoned no matter what. Only a socio-psychological factor distorts the normal development of science. Nevertheless, they cannot specify "precisely what point a rational scientist must abandon a degenerating program." Lakatos seems to be able to answer only that "rationality does not reside in what is done, but in how it is done." Bloor concludes that Lakatos's concept of rationality is equivalent to "honesty."

A theoretical doctrine such as science is "a collective work, continually modified, supplemented and interpreted" in different ways from its initial formulation. The contents depend on those who work upon it. Because of inevitable changes of our interests, a theoretical doctrine cannot maintain any superior position. All systems of knowledge, Bloor states, should hold inductive propensities. Sociology is the "science which studies the conventions which always attend and structure the expression of our urges and capacities." Mannheim (1936) says that "The principle thesis of the sociology of knowledge is that there are modes of thought which cannot be adequately understood as long as their social origins are obscured."

I do not totally agree with Bloor's position. That is because he totally eliminates reasons or evidences to explain theoretical beliefs. Brown (1984: 21) says, "Reasons are a kind of cause, and sometimes these reasons-causes are present. They are not always present, but they are not always absent either. Consequently, the symmetry principle, which is an all-or-nothing principle, must be wrong." Certainly science is a social phenomenon. Nonetheless, the claim does not guarantee that the claim that sociology is the best primary tool to understand the phenomena. Indeed, "science is multifaceted process," such as a biological, psychological, linguistic, economic, or political process. Bloor underlines that choices of theoretical beliefs, due to the underdetermination by evidence, must have social causes. This claim also empirical question so that it should not been evaluated by the *a priori* criterion. Moreover, the underdetermination of theory choice does not inevitably induce that only social factor narrow down the choice. Practically, theory choices are the selection of a "very small range of genuine well-articulated alternatives open to the scientists at any given time."

Nola (1990) says that Bloor adopts "a law-covering model of explanation" and "a regularity account of causation." He attempts to specify necessity and sufficient conditions for acts of believing. The strong program neglects the possibility that reasons or evidence can cause theoretical belief. Nola refers that "The point of the critical evaluation of our beliefs is not merely to understand how they arise but to find reasons for them and to change them for better ones." The strong program lacks the critical force of prescription. I also disagree that Bloor removes rationality from the causal explanation, as Schmaus (1985) says. For Bloor, the norms of rationality connecting the reason to the belief in question are not causal principles scientifically analyzable. That is because he strongly insists "moral neutrality," in the sense of being unbiased or objective, of the sociology of scientific knowledge. On the other hand, he assigns value, norms, and reasons an ontological status. Against Bloor's metaphysical separation of reasons from explanation of belief, Schmaus stresses a usefulness of the empirical approach: "whether reasons in any way function differently from other sorts of causes in the explanation of belief." The strong program should positively treat rationality as a description of norms used in a group.

Another problem in the strong program, Turner (1981) points out, is its mode of explanation: that is, "one must describe the way of life in which their wood piling practice is intelligible." "Conflict arises between sociological and ‘rational’ accounts where the one is a ‘pejorative translation’ supplemented by sociological explanations and the other is an adequate rational interpretation which does not need such supplementation.” In particular, Barns and Bloor, as Gregersen and Køppe (1988) mentions, subscribe to the Durkheimian “cultural holism,” i.e., inseparable group coherence of interests, activities, and thoughts. They pay little attention to conflicts within a group.

The other problem is that interests are fixed explanatory resources. Woolgar (1981) points out that “interests can be shown to influence rather than determine knowledge production, or that particular scientific episodes can be better understood in the light of the particular interests of the involved parties, and so on.” Based on a neo-Durkheimian form of causal explanation, the advocates of the strong program regard “interests” as a fixed explanatory resource, not as a continuously changing social resource consisting of conventions or cultural resources. Thus, Woolgar stresses the necessity of an ethnographic approach, as a reflective attention, to the interests: “The construction and use of interest is an aspect of scientific activity which demands treatment as a phenomenon in its own right.” In addition, the advocates neglect that scientists themselves actively manage and attribute the interests. For example, Mackenszie attributes Yule and Pearson’s scientific positions on the debate about statistical association between nominal variables to their social backgrounds and passive internalization to the value.

My position, like McMullin (1984), is between two extremes, Laudan’s and Bloor’. McMullin says, “The influence of non-epistemic factors must not be simply construed as ‘interference.’ Explanation in terms of socio-psychological factors is to be seen as a proper part of historical explanation.” He seeks a middle way between the “presumptions of standard rationality.” Like Laudan, and the “presumption of unrestricted sociality,” exemplifying by the strong program. Based on the “arationality assumption,” the former claims that there is a trans-historical norm of scientific rationality in scientific practice, and that the socio-psychological explanation is needed only when the rationality cannot be embodied in historical events in question. Conversely, the latter emphasizes that scientific activities are nothing but a reflection of socio-historical culture, and that the socio-psychological explanation is compatible, or contains the “rational” one. In the strong program, even epistemological explanations should be reduced to “the social.” Kuhn (1977: 119) emphasizes, “compared with other professional and creative pursuits, the practitioners of a mature science are effectively insulated from the cultural milieu in which they live their extra-professional lives.” McMullin states that the sociological analyses cannot explain how scientists hold certain contents of scientific theories.

To maintain the firm ground between two, we had better consider the following points. At first, Golinski (1990) points out, “Theory, it appears, need not be viewed as existing in the realm of ideas apart from practice; nor need be reduced to its expression in instruments or social relations.” In order to analyze the complex relationships, he insists that we should scrutinize L. Fleck’s concept of “active” and “passive” elements in the production of scientific knowledge. The “constructivists” focus on how effectively scientists use their “active” and strategic elements to build a scientific theory or fact: for example, the aims or interests, the skills or techniques, and the resources. Accordingly, they must permit indefinite interpretative flexibility and negotiability. However, in the history of science, scientific debates have been closed in certain way. Golinski (1990) criticizes that they, unlike Pickering, neglect the “passive” elements beyond individual scientists’ control, for instance, “socially-agreed phenomenal benchmarks as constraint upon instrumental practice.”

Second, we need the reinvestigation of Durkheim. He rejects not only “teleological” explanation but also the participants’ own justifications of beliefs since he seeks to find a deeper level of “regularity” and “reality.” For him, the justifications are nothing but superficial manifestation of “a deeper reality.” In contrast, the strong program positively uses the participants’ descriptions to specify a causal relationship between scientific beliefs and social conditions. Turner (1981) points out that Bloor, although not theoretically equating social causes with “extra-theoretical” factors, mainly uses the extra-theoretically context, specially certain meta-scientific beliefs, as a cause of a scientific belief.

Third, Martin (1989) defends certain epistemic or aesthetic values, e.g., simplicity and elegance to rational activity, on theory selection. Laudan requires the realizability or operationalizability for rationality of axiology. He presents two criteria for evaluating cognitive values. One is consistency of cognitive goals. That is because “rational thought balancing and harmonizing the conflicts that inevitably arise among inconsistent cognitive values.” Second is non-utopianism: a goal state or value is defined as utopian if it does not have any grounds of actualization. However, “if a goal can be explicated, it would seem necessary to validate the goal as well.” Martin proposes that theories are warranted in the context of values, open and tacit, which are distinct from definite goals. He says, “The doctrine that the scientific imagination is ultimately aesthetic values provide an access to reality is not novel.” Also, “inconsistent values are at the core of competent cognition,” most directly expressed by language of metaphor. He says, “epistemic progress is possible because knowers possess a real but inevitably tacit access to reality … it is the aesthetic imagination that both constraints and enables the operation of antagonistic but complementary values in the development of knowledge. Theories are valued insofar as they see as simultaneously satisfying the demands of conflicting epistemic values.”

In conclusion, there are so many fertile and intrinsic questions in the initial discussions about the social accounts of scientific practices. In this paper, I reexamined the eliminability of “the social” factor mainly through the Bloor-Laudan debates. Presently, many social studies of science have divided into more discipline-specified small questions. However, the departmentalized and single-filed elaboration abates the chaotic but imaginative inquiring mind. To revitalize the science studies, we should be back to the initial big questions again.

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