Field Theory as a Vehicular Theory of Science: Pierre Bourdieu as a Sociologist of Scientific Validity*

Kyung-Man Kim
Sogang University

The huge literature in philosophy and sociology of science during the last four decades amply attests that no conclusive proof or disproof of a scientific theory is possible and science is therefore subject to the social and political influences as any other cultural practices. Pierre Bourdieu has been no exception and argues that scientific practice, if it is to be understood properly, must be subject to a thoroughgoing sociological analysis. In this article, I shall be concerned with showing that Bourdieu's sociology of science chimes with the post-positivist philosophy of science propagated largely through the writings of Kuhn, Hesse, Toulmin and others in that it stresses the "embodied" rather than disembodied and transcendental nature of scientific knowledge. By clarifying the ways in which scientific knowledge is affected by the social characteristics of the vehicle in which it is embodied, I shall show how Bourdieu makes a dialectical synthesis of the relativist and the positivist view of scientific practice through his field theory of science. To support the above arguments, I shall make use of a historical-sociological study of scientific change that best illustrates Bourdieu's approach to scientific practice.

Keywords: post-positivist philosophy of science, sociology of scientific knowledge, vehicular theory of knowledge, Bourdieu's theory of scientific field, scientific validity

INTRODUCTION

Ever since Kuhn's *Structure of Scientific Revolutions* (1962) and the copious works of the sociologists of scientific knowledge appeared, the social constructivist/relativist interpretation

* I am grateful to the three anonymous reviewers of KJS for their helpful criticisms for the improvement of this article. The research for this article has been supported by a research grant from Sogang University. Direct all correspondence to Kyung-Man Kim, Department of Sociology, Sogang University, C.O.P Box 1142, Seoul 100-611, Korea (Phone: 82-2-705-8365; E-mail: Kmkim@sogang.ac.kr).

of scientific practice has been accepted as a truism (see Shapin 1995). The huge literature in philosophy and sociology of science during the last four decades amply attests that no conclusive proof or disproof of a scientific theory is possible and science is therefore subject to the social and political influences as any other cultural practices. Pierre Bourdieu has been no exception and argues that scientific practice, if it is to be understood properly, must be subject to a thoroughgoing sociological analysis. Bourdieu however distinguishes his field analytical approach to scientific practice from relativistic approach by suggesting a rapprochement between the relativist and the positivist approach to scientific practice. Although Bourdieu acknowledges that social and political factors are constitutive of scientific practice, he nonetheless argues that science can produce "valid" knowledge about the external world.

In this article, I shall be concerned with showing that Bourdieu's sociology of science chimes with the post-positivist philosophy of science propagated largely through the writings of Kuhn (1962), Hesse (1974; 1980), Toulmin (1977) and others in that it stresses the "embodied" rather than disembodied and transcendental nature of scientific knowledge. By clarifying the ways in which scientific knowledge is affected by the social characteristics of the vehicle in which it is embodied, I shall show how Bourdieu makes a dialectical synthesis of the relativist and the positivist view of scientific practice through the introduction of his field theory of science. To support the theoretical arguments made in the previous two sections, in the final section of this article, I shall make use of a historical-sociological study of scientific change that best illustrates Bourdieu's approach to scientific practice.

EMBODIED KNOWING: VEHICULAR THEORY OF SCIENCE AND THE POST-POSITIVIST PHILOSOPHY OF SCIENCE

When Bourdieu's last lecture at the Collège de France was published as a book (2004), sociologists of science published a wide variety of critical evaluations as to Bourdieu's contribution to the field of sociology of science (Camic 2006; Gieryn 2006; Mialet 2003; Inglis 2005; Sismondo 2005). All of these critical evaluations of Bourdieu's contributions to the sociology of science however show an astounding failure to understand Bourdieu's epistemology of science, for these critics neglect the close connection that exists between Bourdieu's epistemology of science and the post-positivist philosophy of science.[1] Both Bourdieu and the post-positivist philosophers reject the hard-and-fast distinction between fact and theory and the verification of theory of meaning which are the hallmarks of what Bourdieu

[1] These critics' misunderstanding of Bourdieu's position can be found in their attempt to reduce Bourdieu's field theory of science to the ideal speech situation of Habermas or to Merton's normative structure of science. I have dealt with this issue briefly in section 3 below. For a much more detailed and lengthy discussion of this problem, see Kim (2009).

(1991) calls the "absolutist" theory of knowledge.

Although Bourdieu does not offer an explicit philosophy of science that specifies the relation between theory and the external reality, he stresses that, like literary expression, scientific expression "relies on conventional codes, socially grounded presuppositions, [and] historically constituted classificatory schemas" (1996: 335). In arguing so, Bourdieu endorses the post-positivist philosophy of science characterized by the following features:

1. Scientific theories are not objective representations of the external world; they are sometimes closely connected with the particular ideologies of the scientists.
2. Popperian falsifications of theories are impossible because of a host of auxiliary hypotheses that are used to rescue them.
3. No naked observations are available. Observations are meaningful only within a certain theoretical or metaphysical framework in which such observations are embedded (see Hesse 1974; 1980 for more details).

In company with the post-positivist philosophers and the relativist sociologists of science who stress the "interested" nature of science, Bourdieu argues that the scientific field is replete with struggles for power, capital, and dominance.

For Bourdieu, the natural scientific community has—to use Donald Campbell's (1979) apt phrase—a set of "tribal requirements" for the continual existence of the scientific community as a viable organization. For both Campbell and Bourdieu, knowledge—is not a set of transcendental and disembodied statements about the external world. Rather, it is *embodied* in a certain vehicle or a carrier that has its own "structure-maintenance" (or organizational) requirements that must be met before it functions as a vehicle or carrier of knowledge. Campbell (1979) nicely illustrates what such vehicle structure requirements—or requirements for tribal solidarity—are like by using an example: consider a mosaic mural designed to picture a street scene. While it embodies knowledge of the street, people, and buildings depicted, the mosaic mural nevertheless cannot accurately represent the details of the street scene because the size of the stones, the thickness and colour of the cement, the range of natural colours available, and the restriction to a two-dimensional surface all contribute to the *diminution* of the mosaic mural's validity of mapping the street scene. Thus the knowledge embodied in the mosaic mural, at its realized best, is some compromise between the vehicular characteristics and the attributes of the referents. Although we can increase the accuracy of representation by using smaller and smaller pieces of stone and hence minimize the vehicular contribution, we can never completely eliminate vehicular restrictions and bias for the embodied knowledge. Thus:

> [T]he vehicular substance that carries knowledge is unavoidably alien to the referents of

> knowledge—it is a different substance with different structural characteristics. Complete flexibility in depiction, reflection, transmission, or recording, is precluded by the structural requirements of the vehicle. If the vehicle is completely flexible it lacks the rigidity to hold together the picture [knowledge] it carries. These vehicle-structure requirements produce not only restrictions on fineness of detail, but also bias and limitations of aspect. (Campbell 1979: 184)

Campbell uses this analogy to analyze the self-perpetuating social systems that are the vehicles for scientific knowledge. In a social system carrying scientific knowledge, the social glue that holds its elements together acts as a "social selector," sacrificing more truthful representations of the external referents in favour of those that facilitate the maintenance and continuity of the social vehicle that carries knowledge. Such structural requirements are therefore inimical to the selection of valid beliefs. To maintain such tribal solidarity, journals must be published to give young scholars the outlets for publication, jobs must be found for them, dogmatic adherence to the validity of the currently accepted beliefs are encouraged to deflect radical challenges. For Campbell, not only individual scientists have their own biases and preferences that tend to distort the valid mapping of the physical world, but also a group of scientists has its organizational requirements that dictate the "trade-off" between keeping the vehicle intact and validly mapping the physical world. Thus the requirements of achieving such a tribal continuity come first, even though they are in conflict with the cognitive task of validly mapping the physical environment.

THE SPACE OF POSSIBLES AS A SITE FOR STRUGGLE

The concept of *illusio*—or the belief in the legitimacy of scientific game—is central to the proper understanding of Bourdieu's field theory of science, for the production of the "practical" (or doxic) experience of the scientists is closely related to *illusio*. Bourdieu argues that, for anyone engaged in the struggle for symbolic capital in the fields of cultural production—be it scientific field, philosophical field, or literary field—it is essential that he/she should have an *inclination* to honor and abide by the rules of the game, that is, he/she must accept the often implicit presuppositions and practical wisdom that everyone else in the field takes for granted and shares with him/her; for example, what to honor, what to cherish, what to avoid, what to desire and so on. It also refers to the *ability* of the participants of the game to cope with various situations and contingencies that arise from the everyday scientific practice.

While constitutive of the game, however, these rules of the game and the *illusio* associated with it, when observed from a detached point of view, are a cultural 'arbitrary' imposed on the

rest by those who accumulate the largest amount of symbolic capital through previous struggles. Not being able to recognize that even what counts as scientific issues and facts is itself subject to and determined by the scientific struggle for capital and dominance (Bourdieu 1975: 24), those scientists participating in the game of science believe in the legitimacy of the dominant values and methods currently accepted in the field, and endorse these arbitrary values and methods imposed on the community by the most powerful. It is for this reason that Bourdieu argues that

> [S]cientific thought has no foundation other than the collective belief in its foundations that the very functioning of the scientific field produces and presupposes (1991:8).

Without such doxic belief, the very basis upon which scientists' beliefs are erected will collapse. Thus the scientific *illusio* cannot be sustained without the "complicity" of the scientists involved in the game. The misrecognition of the scientists is a 'collective deception' without a particular deceiver, for it is a misrecognition embodied in the *habitus* of scientists. Indeed, the collective belief in science reinforces itself when the scientists immerse themselves in the scientific game.

Bourdieu now argues that in a given field of science characterized by a specific *habitus* which is obtained through the embodiment of the dominant definition of science, the possibility of a scientist to get ahead in his research field is determined by the two factors: first the universe of possibilities or "the space of possibles," which is defined in terms of the currently available theories, methods, instruments, data, and metaphysics, and second, the resources that he/she can mobilize to "actualize" some of these possibilities. In contrast to the scientists' belief that they are "pure creators, who invent in a vacuum, *ex nihilio*," Bourdieu argues that scientists are mere actualizers of the potentialities that are *socially* instituted; and these socially instituted potentialities—or 'a space of possibles'—are perceived as such only by those who are endowed with certain socially constituted dispositions, that is, *habitus* (Bourdieu 1991: 10-1). Like Campbell and Kuhn, Bourdieu here argues that scientific knowledge is not a body of transcendental, objective, and 'disembodied' knowledge. Rather, knowledge is *embodied in a "social space or a vehicle"* in which a wide variety of forces—social, political and ideological forces—compete against each other for the imposition of a particular definition of science as the universal one.

Success in such an attempt to actualize the potential inherent in the given field or space in turn contributes to the increase of the amount of symbolic capital held by a scientist and thereby enables him/her to wield power over other scientists in the field. Bourdieu argues that, by imposing a definition of science that is in accord with the theories and methods with which he/she practices science:

> [T]he dominant consecrate certain objects by devoting their investments to them, and … through the very object of their investments, they tend to act upon the structure of the chances for profit and thereby upon the profits yielded by different investments (1991: 13).

The "structure of the chances for profit" not only restricts the range of strategies scientists can deploy to increase symbolic capital but also enables them to change the *topography* of the social space of possibles by challenging the legitimacy of the theories, instruments and methods currently available within the space of possibles.

Translated into Bourdieu's terminology, the vehicular requirements correspond to the efforts made by those scientists with the largest amount of symbolic capital to keep other scientists in line with the epistemological doxa they manage to set up, so that they follow the succession strategies rather than the subversion strategies in the struggle for the hegemony of the field. According to Bourdieu, currently accepted classification of the natural world, the explanatory theories associated with it, and a host of other taken-for-granted assumptions that comprise the *habitus* of the scientists are, in fact, social arbitrary imposed on the scientists by those who accumulated the largest amount of symbolic capital in the history of previous struggles. Pre-reflexively accepted by the scientists in a given field, the *habitus*, or the embodied disposition of scientists gives scientists the specific sense as to what kind of research would most likely bring her the results that would be regarded by her colleagues as important and worth pursuing. The complicity of the social agents engaged in the game of science makes all such manoeuvring possible. On the other hand, however, there is always a tension between the dominant and hence the necessarily conservative classificatory schema and the innovative and hence subversive theories that attempt to "redraw" the boundaries of the space of possibles.

RATIONAL DIALECTIC WITHIN A SPACE OF POSSIBLES: BOURDIEU AS A SOCIOLOGIST OF SCIENTIFIC VALIDITY

As I have discussed above, all of the "vehicle maintenance requirements"—trust in the sense of dogmatic adherence to the previous consensus, clique-formations, tribal leadership, adherence to the dominant positions beyond competence, ideologies that bind members together, the pursuit of power and capital—seem to work *against* the enhancement of the validity in science.

However, theorists of science who still believe in science's capacity to improve its collective representation of reality—let me call them sociologists of scientific validity—argue that, despite such liabilities, the social system of science can improve the mapping of the physical world precisely because of the special features, or assets, that the successful natural

sciences have developed over the past three hundred years (Campbell 1986; Rudwick 1985; Kim 1994; 2009; Hull 1978).

As I shall show below, Bourdieu is one of those sociologists of scientific validity who believe in the interested (or political) nature of science, but who nonetheless also believe that, because of the specific features built into the scientific field, science can improve its collective belief. Bourdieu locates that specific feature in the *autonomy* of scientific field that grants scientists the exclusive power to distinguish truth from falsehood. Such power, Bourdieu argues, stems from the symbolic capital that those experts working in the field accumulate over a long period of time. The cultural capital incorporated within the scientific field in turn enables the scientists to build a barrier by increasing the cost of entry into the specific research field. In such an autonomous field, the intellectual products of the field are consumed and evaluated only by the members of the field. This helps them mutually monitor and cross-validate each other's solution without being interfered by the non-experts outside of the field.

In arguing so, Bourdieu endorse the view that is widely shared by many theorists of science (Ben-David 1984; Campbell 1986; Kuhn 1962; Turner and Kim 1999). Kuhn, for example, argues that, being exclusively addressed to and evaluated by other members of the profession, the creative work of a scientist needs not be concerned with lay approbation. Since a scientist can take a single set of standards for granted, "he needs not worry about what some other group or school will think and can therefore dispose of one problem and get on to the next more quickly than those who work for a more heterodox group" (1962:163).

Bourdieu has exactly the same view on the relationship between the degree of autonomy (or insulation) and the genuine growth of scientific knowledge. For Bourdieu, the *arbitrariness* of beliefs varies inversely with the degree of autonomy achieved in a given field. Thus depending on the degree of the autonomy of the field, a field of cultural production can be positioned somewhere between the religious (or literary) field where social arbitrariness of belief is maximized through the imposition of official truth and the scientific field where the social arbitrariness of belief is kept at the minimum level through the social mechanism of cross-control and mutual criticism (1975: 34-5; see also Bourdieu 1990; 2004). Bourdieu argues, however, that the banishment of the social arbitrariness of belief in the scientific field is possible not because the scientific field is an exception to the fundamental laws of all fields, particularly the law of interest, but because the struggle between those who dominate the field and those who try to subvert the received interpretation of the world brings about an "unintended consequence," that is, the elimination of the arbitrariness of belief. For Bourdieu, such a struggle leads to "a *systematic diversion of ends* whereby the pursuit of private scientific interests … continuously operates to the advantage of the progress of science" (1975: 32 [Italics added]). In contrast to Merton (1973) who argues that it is the "distinctive pattern of institutional control" rather than the selfish motives of scientists that characterizes the behavior of scientists, Bourdieu thus argues that the manifest goal of scientific institution, i.e., the

growth of empirical knowledge, is quite compatible with scientist's pursuit of selfish motives, that is, their pursuit of symbolic capital, power and domination within the field.

> The transmutation of the anarchic antagonism of particular interest into a scientific dialectic becomes more and more complete as the interest that each producer of symbolic goods has in producing products that … are "not only interesting to himself but also important to others," hence likely to win recognition of their importance and of the importance of their author, comes up against competitors more capable of applying the same means in the service of the same intentions—which with simultaneous discoveries, leads more and more frequently to one or both producers' interests being sacrificed; or to put it another way, the transmutation becomes more complete as each individual agents' private interest in fighting and dominating his competitors in order to win their recognition comes to be equipped with a whole set of tools which endow his polemical intention with maximum efficacy by giving it the universal scope of methodical control. The antagonism which is the basis of the structure of and transformation of any field tends to become more and more radical and more and more fruitful because of the *forced agreement* in which reason is generated leaves less and less room for the unthought assumptions of doxa (1975: 33).

Now, if every scientist tries to overthrow other's ideas and instead get his ideas accepted in its place, what would be the consequence of this selfish behavior? The answer is evidently the growth of knowledge, the manifest goal of the scientific institution. Science works so well because the pursuit of the selfish goals by individual scientists happens to coincide with the manifest goal of science as an institution. Such "rational dialectic" within the field, as Bourdieu calls it, contributes to the "alchemical transformation" of the private pursuit of interest into scientific knowledge.

In his last lecture at the Collège de France, Bourdieu paraphrased this argument as follows:

> If sociological analysis of the functioning of the scientific field in no way condemns one to a radical relativism, if one can and must acknowledge that science is a thoroughly historical social fact without concluding that its productions are relative to the historical and social conditions of their emergence, this is because the 'subject' of science is not an integrated collective (as Durkheim and the Mertonian tradition supposed), but a field, and a very particular one, in which the power relations and relations of struggle among the agents and institutions are subject to the specific laws (of dialogue and argument) flowing from the two closely interrelated fundamental properties which I set out earlier: closure (or competition among peers) and the arbitration of the real (2004: 70).

Bourdieu argues that the two essential elements for the rational dialectic that transforms the private interest of scientists into the progress of science are the "closure" (or competition *within* the autonomous field) and what he calls the "arbitration of the real." Bourdieu argues that, no matter how elusive the relationship between theory and the external reality is from the philosophers' point of view, scientists believe that research and experiments will arbitrate different views and settle the controversies. And these beliefs in the existence of the objective reality and its power to arbitrate different views make scientists engage in a critical dialogue that is conducted "in the name of reference to the real" (2004: 69).

In arguing so, Bourdieu participates in the tradition of Popper (1945), Campbell (1986) and Hull (1978; 1988) who argue that scientific objectivity comes not from the individual scientists' effort to be objective and impartial, but from the intersubjective criticism directed to correct one another's intellectual work (Bourdieu 2004: 82-3). Central to such a conception of scientific validity is the notion of the 'scientific city' (Bourdieu 1991)—or what he calls in the above quote 'closure'—in which scientists with different theoretical, and metaphysical orientation stay together in a focused controversy, attending to and monitoring each other's arguments and demonstrations, and keeping each other honest. And, by virtue of this social character of scientific objectivity, can we eliminate the taken-for-granted prejudice, or in Bourdieu's words, social arbitrary or unthought assumptions.

Can we however find a study that can empirically substantiate Bourdieu's argument sketched above? In what follows, I shall briefly discuss a historical-sociological study of scientific change that exemplifies Bourdieu's sociological approach to scientific validity. In Martin Rudwick's (1985) historical-sociological narrative of the resolution of the Devonian controversy in geology, struggles for power, dominance, and symbolic capital are shown to be constitutive of the historical evolution of the geological research field between 1820 and 1840. However, Rudwick also shows how such struggles were *regulated* by the ideal of the arbitration of the real within the autonomous scientific field and, as a result, how a new a consensus emerged through the mutual monitoring, disputations, and cross-examination of data among the geologists. The subject of the controversy that Rudwick studied was the proper interpretation of the character of the transitional stratum then known as Greywacke found at North Devon by a British geologist, Henry De la Beche. De la Beche's particular interpretation of this newly found stratum however was severely criticized by another eminent British geologist, Roderick Murchison who immediately noticed the damaging implication of De la Beche's particular interpretation of the Devon Culm for his own research program. The controversy between these two main protagonists had a wide repercussion in the geological community, and a number of geologists such as Charles Lyell, Adam Sedgwick, George Greenough, William Buckland, and John Phillips—and a host of other less eminent geologists of the time—soon participated in the controversy that lasted almost for ten years.

Around the 1820s, most geologists were concerned with solving the problem of correlation,

i.e., matching of a specific sequence of geological formation found in one area with that in another area. Underlying such a preoccupation with the problem of correlation was the assumption that "there was indeed such a universal sequence to be found" (Rudwick 1985:53). While, at that time, there was a rough consensus among geologists about the sequence of the various strata in the Tertiary and the Secondary rock formation, a specific category of rocks intercalated between the Secondary and Primary rock type known as "Greywacke" (or transition rocks) could not be easily reduced to the kind of orderly sequence that was clearly established in the Secondary strata (since these were found to be folded and faulted and not generally differentiated into distinctive formations). Murchison's aim was to provide an interpretation of the transitional rocks that was consistent with the well-established fossil-based type of stratigraphical geology that had been successfully applied to the elucidation of the orderly sequence of the various strata found in the Secondary rocks. According to such a fossil-based type of stratigraphy, fossil plants should not be found in the Greywacke but only in the relatively younger stratum known as "Coal Measures" which belonged to the so-called Carboniferous group. However, De la Beche claimed that the transitional stratum (i.e., Greywacke) that he found at Devonshire apparently contained a great deal of fossil plants. This was indeed an anomaly for Murchison because, if true, it was fatal to his fossil-based type of stratigraphy. The controversy broke out when Murchison leapt to attack De la Beche's particular interpretation of Greywacke.

Through his detailed examination of the 'temporal evolution' of the controversy, Rudwick shows how a new consensus emerged through the argumentative process of the participants. In doing so, Rudwick considers a host of factors other than scientific evidence that impinge on the eventual resolution of the controversy including the social status of the main protagonists in the social hierarchy of the 19th century England, their social and political interests, and their pursuit of power in the academic as well as in the bureaucratic world. For instance, while Murchison who dominated the geological society of London was the most powerful in terms of intellectual achievement and research money, his main antagonist, De la Beche, occupied a rather lower position within the social hierarchy of England and also in the academia. Rudwick also takes into account the effects of social as well as political positions of the other participants of the controversy on the "shaping" of the evolution of the controversy. As is expected from Bourdieu's theory of scientific field, Rudwick's narrative account of the evolution of the controversy vividly demonstrates that the temporal evolution of the controversy cannot be understood without taking into account all those social, political and economic factors that impinge on the trajectories of the intellectual evolution of the main protagonists involved in the controversy. Together with the intellectual and scientific factors, these social and political factors contribute to the "construction" of the theories each of these protagonists advocates. Scientific theories, Rudwick concurs with the post-positivists like Bourdieu and Campbell, are *socially constructed* within an evolving social space in which a

series of new interpretations are being constructed against the backdrop of the interpretive resources already available within that space.

> When examining an exposure of a rock on the seashore or in a road cutting … What the geologist perceived were the *already interpreted features* of strata with measurable orientation, containing identifiable fossils, and capable of being integrated on the spot … into an imaginative picture of vastly larger structures and sequences. A feat of *interpretive construction* lay behind even the bare and terse notes of a Sedgwick and still more obviously behind the sketched field sections and more discursive reflections of a Murchison (431 [Italics added]).

Such "interpretive constructions" about the nature of the newly found stratum, as Rudwick freely acknowledges, were made available through the modifications of the theories already available in the "space of possibles" that has been collectively constructed through the evolution of the geological research tradition. And, in that sense, they were *not* the results of the direct perception of the nature of geological reality. However, Rudwick also showed quite convincingly that the geological community's decision as to which of these social constructions is the most plausible representation of the natural world was made possible through what Bourdieu calls the "rational dialectic" of the field, i.e. through the process of mutual criticisms and persuasion based on visual demonstrations rather than through the purely political and verbal maneuvering as the relativists argue. Such rational dialectic within the autonomous field made Murchison, the most powerful opponent of De la Beche's discovery, concede his error and accept the latter's interpretation of the newly found stratum instead of his.

> Now Murchison was being forced by the logic of his own interpretation to incorporate his opponent's [De la Beche's] suggestion, and thereby to abandon a central component of the rhetoric that had distinguished their positions hitherto (278).

Without acknowledging the validity of De la Beche's argument, Murchison could not indeed resolve the "anomalies" that made his theory incongruent with the data obtained from his own fieldwork (278). Throughout his narrative about the evolution and the final resolution of the Devonian controversy, Rudwick used the term "shaping" (454-5) rather than "discovering" to indicate the nature of consensus formation in science. For Rudwick as well as for Bourdieu, scientific knowledge is social through and through and the struggles to impose the dominant definition of reality by the participants of the field are central to the scientific knowledge production as in other fields of cultural production. For both Bourdieu and Rudwick, scientific facts are certainly "malleable" and socially constructed, but scientists can also make some of

these malleable constructions "refractory" through the argumentative process within the scientific field.[2]

CONCLUSION

Bourdieu's field theory of scientific practice argues that scientific truth is not a disembodied or a transcendental body of knowledge but a collective thing embodied in a historically evolved social space called scientific field. Conceptualized in this way, scientific knowledge gets stabilized and attains the status of certified knowledge through the "collective winnowing" process that selects those knowledge claims that are supposed to represent the natural world in the most plausible way. Being a carrier or vehicle that contains scientific knowledge, scientific field imposes on itself a set of requirements—or to borrow Bourdieu's phrase, "social conditions of the possibility of knowledge"—that are needed to maintain science as a social institution. Such organizational requirements are surely inimical to the selection of the most plausible construction of reality. But as Bourdieu and other sociologists of scientific validity argue, such social conditions of the possibility of knowledge are being constantly challenged and overcome through the argumentation among scientists as to which of the competing social constructions can be counted as the most plausible representation of reality. Scientific truth thus produced can never attain the status of final truth but it still can retain the correspondence meaning of truth precisely because it is obtained in a specific way that constantly challenges the very social conditions of which it is a product.

---

[2] Another reviewer of this article argues that since I fail to demonstrate that Bourdieu successfully reconcile the positivist and the relativist position, he/she has no reason to change his/her view that Bourdieu is closer to the relativist position. The falsity of this reviewer's criticism however can be found in his/her argument that the Rudwick example demonstrates that social/political factors rather than epistemological factors determine the course of the controversy. Such argument stems from his/her misunderstanding of Rudwick's "shaping" argument. Bourdieu, like Rudwick, argues that data and evidence do matter in the 'shaping' of the eventual resolution of the scientific controversy. Those data and evidence, however, are at the same time the result of 'social construction' of the scientific field that constructs scientific hypotheses on the basis of the 'interpretive schema' already available within the field. Although scientists winnow implausible theories through the argumentation process—or to use Popper's word, their attempts at "falsification"—the hypotheses survived in the end do not have the status of 'final truth.' It is only tentatively corroborated. If the reviewer believes that Popper is a relativist, then both Bourdieu and Rudwick can be surely counted as relativists. But such relativism is clearly different from the relativism Bourdieu criticizes.

REFERENCES

Ben-David, J. 1984. *The Scientist's Role in Society*. Chicago: University of Chicago Press.

Bourdieu, P. 1975. "The specificity of scientific field and the social conditions of the progress of reason." *Social science information sur les Sciences Sociales* 14: 19-47.

_____. 1990. "Animadversiones in Mertonem." pp. 297-301 in J. Clark, C. Modgil & S. Modgil (eds.) *Robert K. Merton: Consensus and controversy*. London: Falmer Press.

_____. 1991. "The peculiar history of scientific reason." *Sociological forum* 5: 3-26.

_____. 1996. *The Rules of Art*. London: Routledge and Kegan Paul.

_____. 2004. *Science of Science and Reflexivity*. trans. by Richard Nice. Cambridge: Polity.

Camic, C. 2006. "Review of *Science of science and reflexivity*." *American Journal of Sociology* 111: 1569-71.

Campbell, D. T. 1979. "A Tribal Model of the Social System Vehicle Carrying Scientific Knowledge." *Knowledge: Creation, Diffusion, Utilization* 1: 181-201.

_____. 1986. "Science's social system of validity-enhancing collective belief change and the problems of the social sciences", pp. 108-35 in D. Fiske and R. Shweder (eds.) *Metatheory in Social Sciences: Pluralism and Subjectivities*. Chicago: University of Chicago Press.

Gieryn, T. F. 2006. "Review of *Science of science and reflexivity*." *Contemporary Sociology* 35: 185-7.

Hesse, M. B. 1974. *The Structure of Scientific Inference*. Berkeley: University of California Press.

_____. 1980. "Duhem, Quine and New Empiricism." pp. 208-28 in H. Morick (ed.) *Challenges to Empiricism*. Indianapolis, IN: Hackett.

Hull, D. 1978. "Altruism in science: A Sociobiolgoical Model of the Cooperative Behavior among Scientists." *Animal Behavior* 26: 685-97.

_____. 1988. *Science as a Process*. Chicago: University of Chicago Press.

Inglis, D. 2005. "Review of Science of science and reflexivity." *European Journal of Social Theory* 8: 375-82.

Kim, K. M. 1994. *Explaining Scientific Consensus: The Case of Mendelian Genetics*. New York: Guilford Press.

_____. 2009. "What Would a Bourdieuan Sociology of Scientific Truth Look Like?" *Social Science Information sur les Sciences Sociales* 48: 57-79.

Kuhn, T. S. 1962. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.

Merton, R. K. 1973. *The Sociology of Science: Theoretical and Empirical Investigations*. Chicago: University of Chicago Press.

Mialet, H. 2003. "The righteous wrath of Pierre Bourdieu." *Social Studies of Science* 33: 613-22.

Popper, K. R. 1945. *The Open Society and Its Enemies*. London: Routledge and Kegan Paul.

Rudwick, M. 1985. *The Great Devonian Ccontroversy*. Chicago: University of Chicago Press.

Shapin, S. 1995. "Here and Everywhere: Sociology of Scientific Knowledge." *Annual Review of Sociology* 21: 289-321.

Sismondo, S. 2005. "Review of Science of Science and Reflexivity." *Canadian Journal of Sociology Online*. March-April, 1-2.

Toulmin, S. 1977. "From form to function: philosophy and history of science in the 1950s and now."

Daedalus 106(3): 143-62.
Turner, J. and K. M. Kim. 1999. "The Disintegration of Tribal Solidarity Among American Sociologists: Implications for Knowledge Accumulation." *The American Sociologist* 30: 5-20.

[Submitted March 29, 2009; Accepted June 12, 2009]