2. Contingent versus Constitutive Views of the Sociality of Scientific Inquiry

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I thank the Homi Bhabha Centre for inviting me to this conference. It is an honor to be able to discuss these questions about the interaction of science and science education with their multi-cultural contexts among such a distinguished group of scholars.

I will be talking about two contrasting ways of thinking about the social nature of scientific knowledge that has emerged in North America in the past two decades. I label these as contingent and constitutive approaches; the contingent approach holding that the sociality of science is a feature of the way the sciences are currently organized, the constitutive holding that the sociality of science is a feature of the very nature of knowledge, towards which science aims.

I begin by reviewing briefly the developments that contributed to this social turn in philosophy of science. Some were external to the academy. A revitalized feminist movement in North America grew out of the anti-war and civil rights movements of the 1960s. Feminists issued scathing critiques of sexist and eurocentric scientific practices, from deliberate practices of exclusion to explanations of sex and gender that naturalized social inequality. Women were discouraged from pursuing scientific careers, were not admitted to graduate education and faced many obstacles establishing careers even if they did complete their education. Women's biology or psychology suited them to the domestic role of nurturer and caretaker, while men's biology suited them to activity in the public world of commerce, the professions, and politics. Feminists challenged all that, and as a search for scientific personpower began to dissolve some gender barriers, feminists found themselves in a position to challenge the prejudicial content of the sciences as well. The intertwining of science and social values was successfully exposed through their interventions. Critical science movements further challenged the ideal of social neutrality of science by highlighting the dangers of science-

based technologies, like nuclear weapons or nuclear generated electric power, by focusing on the effects of industrial pollutants, and, like the feminists, by criticizing the racism in the organization of science and implicit in certain research agendas. Scientists, they claimed, ought to be at the forefront of challenges to socially detrimental science-based technologies.

Inside the academy, sociologists, anthropologists, and historians were developing accounts of the history and contemporary practice of science that challenged the independence of the conduct of science from its social, economic, and political context. And, in philosophy, a move toward naturalism led philosophers to take the empirical study of knowledge seriously. Philosophers were more attracted to psychology than to the social sciences, in part because of a continuing commitment to the individual thinker and actor as the analytic center, and in part because at least some of the social researchers seemed to be challenging the very rationality of the sciences. These academic developments, taking place in the context of the social and cultural developments, provided a field of discourse for the emergence in philosophy of science of work that engaged the interaction of science with its social context in a philosophical way.

Some philosophers have emphasized a continuity with the social engagement of the Vienna Circle philosophers of the 1920s and 1930s, especially Otto Neurath, but also the other members, before being hounded out of Europe by the Nazi Anschluss. It is important to note, however, that the conditions today, anywhere, are different from those that prevailed in early 20th century Europe. North American philosophy of science is still emerging from a stripped down logical positivism, as contrasted with the situation of the logical empiricists who were trying to escape the metaphysical and normative inclinations of pre-1920s philosophy. And, the sciences in the late 20th century and early 21st century have developed in status and achievement only dreamt of in the early decades of the century. The industrialized societies have created and inhabit a science and science-based technology saturated world. And industrializing societies expand the reach of science and science-based technologies. So we philosophers considering the relations of science and its socio-cultural context are operating in a novel rhetorical and material environment.

As I said, it is possible to distinguish at least two kinds of approach to thinking about these relations. As representative of the contingent approach, I take Philip Kitcher's ideal of well-ordered science as developed in his 2001 book, *Science, Truth, and Democracy* (Kitcher, 2001). As representative of the constitutive approach, I take the critical contextual empiricism of my 2002 book, *The Fate of Knowledge* (Longino, 2002). I would like to contrast the philosophical fundamentals of these two responses to the complex situation I have just outlined and to indicate the different directions each offers for articulating relations between the sciences and their sustaining societies.

Kitcher's 'well-ordered science' stakes out a position between those he describes as the constructivist debunkers of science, on the one hand, and the uncritical scientific faithful on the other. Neither of these is satisfactory, but he defends scientific realism against philosophical instrumentalists (whom he reads as giving implicit aid and comfort to the debunkers) and objectivity against those who would claim that science is overrun by social and political interests. This defense is tempered by an important shift from his earlier positions, however. He rejects the idea that there could be a single system of natural kinds, given by the nature of things, instead there are multiple systems of kinds answering to our specific interests in the world, multiple correct ways

of representing reality, multiple languages and classificatory schemes each correct relative to some set of interests. This pluralism, however, is itself tempered by a requirement that these different schemes or classificatory systems be consistent with each other in the sense that truths in one can be translated into truths in another.

I see the sociality here as contingently sociality. Scientific research happens to be pursued by communities on inquirers working competitively and cooperatively. These competitive and cooperative relations can be harnessed by reward structures to maximize scientific productivity. Sociality can speed or hinder knowledge production, but knowledge itself can be understood in conventional ways, preserving our conventional (and individual-centered) conceptions of rationality as the core of science. Nevertheless, the absence of a privileged classificatory scheme, the availability of both epistemic and social criteria of significance, and the plurality of goods we might as societies seek to achieve through the support or pursuit of science, mean that the direction of research is not given by the phenomena, but not only is, but must be subject to democratic oversight.

For Kitcher the central question is 'Can collective research be organized in a way to promote our collective values in the most encompassing sense?' Well-ordered science is the answer. Kitcher (2001) has in mind a kind of enlightened democratic decision-making, not the raw *vox populi*. The fundamental idea is that through an iterative process in which members of a decision-making assembly express their preferences, then consider the set of preferences expressed, defend theirs and attend to the defenses offered for others, consult experts as to the feasibility of the various proposals, eventually come to agreement about the research agenda to be pursued. But, only the research agenda, not the research process, is subject to democratic oversight. Once the agenda has been decided in this democratic way, the execution of the agenda, the actual research is in the hands of the expert, the scientist. The research will be governed by the universal norms of rationality and of objectivity internal to science and democratic process will only interfere with their proper operation.

Are these limits justified? This question must be addressed by considering the defenses of objectivity and rationality. I will consider only the defense of objectivity, as that will suffice to show that the limits are not justified. Kitcher defends the objectivity of science through a discussion of the classical problem of the underdetermination of theories by data. There are two ways of understanding this feature of the logic of the sciences. One represents underdetermination as a matter of the empirical indistinguishability of two (or more) fully developed theories. Kitcher argues that in the cases offered as examples of this kind of undetermination we just have two (or more) expressions of the same formalism and so not two (or more) distinct theories after all. These situations do not provide an opening for contextual social values or interests to play a role and hence pose no challenge to the objectivity of science. But he allows for 'mixed situations', involving theories not fully developed each account for some aspect of the relevant data, but not all of it. Here, where evidential support is incomplete, reasons (or motives) for accepting one or the other can include external, social contextual interests, as well as evidence. Over time, though, as advocates of the different theories develop the theories, accumulate additional evidence, criticize the competing theories, the community will accept the theory with the greatest empirical support. He offers the history of the confrontation of Priestley's qualitative phlogiston theory of combustion by Lavoisier's quantitative chemical theory as an example of the process he outlines and claims

that this shows that objectivity – understood as acceptance determined only by evidence – is a meaningful ideal.

Kitcher's argument here ignores metaphysical differences that shape our perceptions of the empirical world and our interpretations of those perceptions. And, at a less abstract level, differences regarding which observations are relevant can arise from disciplinary differences, social, locational differences, and so on. It's an unargued assumption that in the end the winning theory is supported by all and only the evidence, as in the course of deliberation the shape of the domain to be explained and from which data could be drawn, may have been determined by social contextual factors. Changes in what counts as evidence and why may not be evident to the participants in the debate, so its resolution may be shaped by external factors that can only be excavated after the fact, and after the consensus has been cemented. Simply outlining an ideal without considering what is required to realize it is not enough to 'save' objectivity.

Advocates of contingent sociality like Kitcher fear that endorsing constitutive sociality leaves scientific inquiry ungrounded, arbitrary, and viciously subject to political interference. But in the haste to avoid such a conclusion, they overestimate the effectiveness of pure rationality as they imagine it to achieve total comprehension of the evidence and its relevance. Critical contextual empiricism (CCE) assigns the social interactions of scientists a central role in its conception of scientific inquiry, and it does so, in part, as a way to mitigate the threat underdetermination poses to objectivity. In the CCE view underdetermination is not a matter of empirically undecidable conflicts between two or more theories, but a matter of relations between theories and the evidence available for them. When we consider the content of our theories, models or hypotheses and the content of data descriptions, it becomes clear that there is a significant semantic gap that cannot be crossed by formal means. Hypotheses, for example, about subatomic particle interactions contain language that does not occur and cannot be explicated in the descriptions of data. The data for proposals about neutrinos, muons, and such are tracks in compressed gas or the sequence of ciphers on data tapes. Once sensitized, we can see this kind of underdetermination spread through the sciences. Data alone cannot establish their relevance to hypotheses, nor do hypotheses alone determine the data that will count as evidence for them. Instead data requires supplementation in order to be made evidentially relevant to hypotheses and theories. That supplementation is provided by background assumptions.

Background assumptions form the framework within which inquiry is pursued and structure the domain about which inquiry is pursued. They are the intellectual context that provides a scaffolding for our reasoning between observational data and the theories and hypotheses that are presumed to explain and to be supported by the data. Hence, the contextual part of critical contextual empiricism. How then is scientific inquiry not left to the vagaries of chance and personal preference?

Both the underdetermination problem and the empirical study of scientific practice by sociologists and historians of science change the ground on which philosophical concerns operate. The empirical work suggests we treat the agents of knowledge as located in particular and complex interrelationships in multiple and partially overlapping networks. The underdetermination argument shows we must acknowledge that purely logical constraints cannot compel such agents to accept a particular theory or hypothesis. Together these support a reconfiguring of traditional understandings of justification as not only the testing of hypotheses against observational data, but as also the subjection of hypotheses, data, reasoning and background assumptions to criticism from a variety

of perspectives. On the critical contextual empiricist view, it is this critical dimension that saves the objectivity of science.

The introduction of criticism socializes the epistemic/cognitive aspect of science in a deep way. Once critical interaction is taken as an aspect of scientific methodology, as it must be, if it is understood as a means of coping with the underdetermination problem, norms of critical interaction must also be articulated. A community can tolerate or incorporate criticism. Tolerated criticism that goes unattended is ineffective. A community seeking objectivity, i.e., seeking to avoid being swayed by power relations or idiosyncratic interests, must satisfy certain conditions. I have proposed four such conditions:

- *Venues*: there should be established venues (such as space in journals, conferences, etc.) for critical exchange;
- *Uptake*: the views accepted in the community should change in a way that reflects the critical discourse taking place within it;
- *Public Standards*: the standards regulating assessment of scientific content as well as criticism should be public;
- *Tempered Equality of Intellectual Authority*: at the outset of interaction, every perspective ought to be treated as equally capable of generating productive criticism, though this is a status that can be forfeited by failure to fully engage.

Because assumptions that are shared by all members of a community will be shielded from criticism and, if they persist in the face of apparently effective structures perhaps even strengthened, these conditions require two further conditions:

- *Diversity and inclusion*: the research community should include representatives of all the perspectives in that community, especially those with an interest in the outcome of any particular debated issue;
- *Openness to perspectives other than those of the community*: indeed the community should seek interaction with other communities that may have constructive perspectives not yet represented in the community.

On this view, objectivity is not a matter of settling on the theory that eventually comes to have greater evidential support. What even counts as evidential support with respect to some issue may itself be a matter of debate. Instead, objectivity on the critical contextual empiricist view consists in critical scrutiny of data, reasoning, and assumptions by a scientific community characterized by interaction among multiple perspectives and satisfying the 4 plus 2 norms just outlined.

In addition to this contrast regarding objectivity, there are differences in the kind of pluralism endorsed by the two approaches. Kitcher's approach acknowledges multiple classificatory schemes, but requires that they be mutually consistent. This evinces a monist predilection. On the critical contextual empiricist view, not only are multiple non-congruent and non-reconcilable accounts of a set of phenomena possible and admissible, but they may even be necessary for a comprehensive understanding of those phenomena. My own view is that a theory of knowledge should not presume monism or pluralism. Kitcher's limited pluralism imposes an external constraint of consistency that scientific theorizing may not be able to meet. Whether plurality or singularity holds should be a

case by case matter, made evident in the interaction of our explanatory categories with the empirical world, not a matter legislated by a theory of knowledge.

So far, I have outlined some differences between two views of the sociality of science. According to a contingent view, as a matter of fact scientists are located in communities and accept content as a result of their interactions with each other. According to a constitutive view, a normative social element is part of the very meaning of 'knowledge'. Do these differences matter? In developing an affirmative answer, I would like to refer to three aspects of research where contingent and constitutive sociality approaches recommend quite different attitudes: feminist interventions in the sciences; risk assessment; and the privatization of knowledge.

Since the 1970s, feminists have intervened in the content of sciences in at least three ways. They drew attention to the ways in which research questions privileged a masculine perspective, whether in medical research that used only men and male animal models as subjects or contraceptive research that asked only about benefits, not about risks. They drew attention to the ways in which not just the questions but the answers were shaped by androcentric and masculinist perspectives, even when standard protocols of 'good' science, 'good' methodology, were observed. In physical anthropology, neuroendocrinology and cell biology, feminist scientists, historians and philosophers of science demonstrated the role of androcentric and sexist assumptions in gathering and interpreting data. These efforts have been documented in many volumes (Harding and O'Barr, 1987) including a selection of essays from the journal *Signs*, edited by Jean O'Barr. Feminist interventions had the result of shifting the consensus in the sciences in which they intervened away from blatantly discriminatory theories. The epistemological questions that they made salient were: What counts as evidence? And, how is evidential relevance determined? Methodologically, feminists, like Evelyn Fox Keller (1986), challenged the idea that objectivity is somehow connected to masculinity and its companion idea that the feminine virtues (nurturance, receptivity, softness of feelings) that women ought to exemplify are incompatible with objectivity. They also emphasized a methodological conception of objectivity: objectivity is a matter of how one goes about the conduct of research, the effort to minimize the influence of subjective preferences, rather than a property of the outcome of research. Feminists also engaged in discussion of cognitive values, embracing and defending alternatives to the traditional ones. In this connection, the volume edited by Nelson and Nelson (1996) shows the range of feminist philosophical responses to the scientific interventions. The effect of this intervention is to underscore the point that the traditional virtues (simplicity, breadth of scope, etc.) are not self-evidently truth conducive.

It should be emphasized that there is not one single feminist perspective, but many (or at least several); and there may be gender neutral or gender egalitarian perspectives. The point is that they are sources of constructive criticism that moves the community in productive ways. Recognizing the constitutive sociality of science permits us to see how feminist critical engagement with the sciences is part of the way scientific inquiry, at its best, works.

Or consider, as a second example, health risk research. Philosopher of science, Heather Douglas (2000) has shown how methodological decisions at the very heart of risk research are not determined by the phenomena or by methodological rules. Setting criteria of significance, deciding whether to minimize false positives or false negatives, determining protocols for handling borderline cases are all open decisions that will have effects in the world of work and production when reflected in exposure standards. The contingent sociality approach can acknowledge this, but

has no recommendations that come out of its approach to methodology. Affected communities, workers, factory owners, downstream residents are given no reason to trust the results, when internal technical choices are arbitrarily left open to interests of one side or the other. The constitutive sociality approach can recommend that research teams include as members representatives of the various constituencies that will be affected by the outcome of the research. Those open choice points can then be decided in a negotiated, collaborative, inclusive way that assures all sides that they have been 'at the table'.

Thirdly, consider the fact that, in the United States, at least, much research is now funded by industry, most notoriously the pharmaceutical and medical devices industry, but other industry as well, now that government support for research is shrinking. Much of this industry supported research involves contracts that grant ownership to the knowledge to the particular corporate entity that has provided the funds; researchers are not permitted to present results not sanctioned by the sponsoring entity. And the private (oil company) sponsorship of research has severely muddied the waters over climate change. The contingent sociality approach can condemn these practices as contrary to the traditional openness of science, but does not have a principled reason for such condemnation. The constitutive sociality approach, on the other hand, regards these practices as violations of the critical contextual empiricist norms of objectivity. It offers a principled form of objection to these practices as undermining the epistemic goals of science.

Finally, research by historians of science and philosophy have shown how culture and science are resources one for the other. Their research raises questions such as: How do specific cultural configurations shape scientific ideas and practices? How do scientific ideas and practices shape cultural configurations? If the research process is black-boxed, treated as insulated from its social and cultural context, philosophy of science becomes powerless to fully explore these and other questions about the reliability and trustworthiness of scientific knowledge in different institutional forms. Contingent sociality treats social and cultural matters as external to the properly scientific process. For the advocate of constitutive sociality a philosophy of science that is genuinely social must be accountable not just to the scientific practitioner but to the recipient of knowledge and bearer of knowledge's benefits and burdens. Philosophy of science should seek not to seal off a zone of immunity where values do not penetrate, but rather to understand the complex relations of science, society, and culture. We must open up the black box of research rather than insulate it from social and cultural examination.

Thank you for your attention. I look forward to our discussion.

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DISCUSSION

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Q1: It's a brilliant exposition. I really liked the underlying idea, because in linguistics also some of us are thinking along these lines. I could see shades of various thinkers in this line of thought that you presented, for e.g. in linguistics also we have the so called known evolutionary theory of evolution of language. There are Kuhnian perspectives and Chomsky himself subscribes to the idea where this multiple exercise of different forms is very much a part of the reality. I could also see the sociologist Habermas' underlying idea that various things are social facts which are not in the grasp of the individual, that it is a social fact. Saussure took up this idea and said that language is a social fact as well and is not individuated in an individual, but it is more than an individual. I was just wondering, from another point of view, where you talked about critical interaction, I was thinking about the Habermassian idea of social interaction. Because he also subscribes to the idea that it is the premeditated or precommunicated action of interaction with another person. This idea is what we are trying to take in linguistics. As we cannot take the whole society to enter into formal device or formal science, we are trying to at least take the speaker, the listener into view. That is the minimal interaction one can have in terms of language. I was thinking how much of that kind of Habermassian idea can be read into your talk as well, specially the communicative reason part.

HL: Yes, there are certainly Habermasian overtones, and undertones and sidetones and so forth in this view. There is one fundamental difference with what I understand Habermas would have said and this has to do with Habermas' acceptance of the kind of view of truth of Charles Sanders Pierce, namely that the outcome of this unconstrained communicative discourse is true. So there is a kind of idea in the Habermasian view that we have to have a definition of truth and there is something called truth which is singular which will be the outcome of this discourse. And that's where our paths have problems. I don't think there is a guarantee, there is never a guarantee of truth. We aspire to truth and as a pluralist, I am open to there being in the end, whatever that it is, multiple non congruent truths.

Q2: The last point brings us to the most contentious issue which is also very centered to science education. If there are multiple and non-congruent truths, then how do we bring in, and when we bring the appreciation of constitutive sociality of science in science education, then how do we avoid slipping into relativism? Because if in science education, relativism comes in its extreme form, then there are obvious dangers to it. So if we take a pluralist stance, as you are taking, and that is necessary because that is one way to understand and appreciate sociality of science. But then the central question becomes how do we avoid slipping into relativism?

HL: I am not sure that I can give an answer that is fully satisfactory. But I think pluralism is the end result of the argument and not the beginning point of an argument. So one way to

avoid slipping into relativism, especially in science education, is to emphasize the role of experiment and observation. I was struck by what has been said about integrating theoretical work with hands-on laboratory work. I think that's when students understand the ways in which the natural world resists our efforts to describe it and understand it. And the natural world, even though it doesn't place complete constraints, nevertheless places very strong constraints on what we can say. Within one's understanding/framework there are correct and incorrect statements to be made about the world. I think that's what is important to understand. That is my way of addressing your question remaining within an overall pluralist framework.

Q3: In your account, a thing is objective to the degree of its satisfaction of social norms as suggested by CCE account. You also give room for a kind of a realism, in the sense that theories are in some sense conforming to the world. So my question is, whether the social norms are sufficient conditions for the objectivity of the theories in science or whether the world in some sense of correspondence has involved to define objectivity?

HL: I think the way I understand that question is in a way it's a question of relationship between what we might call a methodological conception of objectivity and a substantive conception of objectivity. Sometimes when we say that science is objective, what we mean is that science actually achieves a correspondence with the world that it is attempting to describe. That's what I call substantive objectivity. Methodological objectivity has to do with the methods that we employ to try to achieve substantive objectivity. That is the method that we employ to try to achieve what we correctly attribute to the conformation of our theories with the part of the world that they explain, describe or understand. So, we have to seek objectivity in our methods because we want to achieve objectivity, substantive objectivity of our descriptions. So pursuing a methodology that can be characterized as objective is our best way of achieving substantive objectivity. It is not a guarantee because there are so many things that stand in the way of actually getting a correct account if we follow the instrumentalists. You are quite right, I'm a realist in the sense that I think that there is an actual world out there and I think that what we are trying to do in science is to describe that world, not just develop instruments to our predictions. But there are also theories that somehow capture some aspect of the processes that produce the experiences of the world that we have. But I think that pluralists hold that the representations of that world are in some cases going to be partial and not complete. So I think we actually make connections, we do achieve objectivity but that objectivity is limited by the partiality of our views.

Q4: I just wanted to draw your attention to the distinction between the individual and the social, and that knowledge is constituted. That there is a constitutive element of sociality to knowledge which is the argument you are making. Interestingly there is an analog in education as well between what's called the individual psychological constructivism and social constructivism in the classroom where children learn. Its quite interesting because if you say that learning takes place in children psychologically there is a psychological change. You need to access that in some way through assessments of psychological tests and knowledge and so on and you need to assess the social dimension through other kinds of data. Now there are many who argue that these are actually complementary aspects. What I understand to mean is

that it is impossible to make sense of one description without the complementarity of the other description. When you come to philosophy of science and characterization of scientific knowledge you see that philosophers don't really bother about how one accesses this individual psychological dimension of knowledge. I think I'm not sure, they seem to operate with this idealized notion of belief system which can somehow be assumed to be individual. So would you think that this is a real difference, or would it be valid to say that even in the case of scientific knowledge it would be impossible to understand one dimension without the complementarity of the other.

HL: Yes, these must be understood. We can't understand the individual apart from the social (and vice-versa). The individual and social in a way constitute each other because the social world requires individuals to operate as social. So I quite agree with you, and also yes, philosophy has tended to operate in an idealized context. This naturalism that I was referring to is an effort to take the work of psychologists and sociologists seriously and bring that empirical work into our thinking about knowledge, as well as the normative concerns of philosophers. David Hull and Alvin Goldman are among the foremost exponents of this but there are others, like Nancy Nersessian incorporating lessons from cognitive science. So naturalism is a way to bring philosophy back down to the ground. But you are right, for the most part, even when that happens it stays in a very idealized level, not the ground level of the individual who is actually trying to understand the world and engaging in a psychological process (or the level of the community seeking to amalgamate the multiple and often conflicting results of inquiry into a coherent view).