

Maritza Marquina

Exam

**Question 1a**

One of the most interesting ideas that Kuhn proposed in his *Structure of Scientific Revolutions* (hereafter *Structure*) was the idea of paradigms and how they worked and constituted important aspects of the scientific community. But looking at his later works in *The Road Since Structure* we can appreciate the change that the concept of paradigms underwent. But first, it is important to look at the idea as it was in 1977 in *Structure*.

In *Structure* the idea of paradigms was much more extensive than the later idea of a lexicon. In 1977 the Kuhn developed the idea of a paradigm as “a term that relates closely to ‘normal science.’ By choosing it I mean to suggest that some accepted examples of actual scientific practice- examples which include law, theory, application, and instrumentation together- provide models from which spring particular coherent traditions of scientific research” (*Structure*, p.10). The paradigm provides a connection between scientists within that paradigm, so much so that people who research under the same paradigm “are committed to the same rules and standards for scientific practice” (*ibid.*, p.10). To illustrate this we can look at the differences between the Newtonian and Quantum physics. Each of these is constituted by a separate paradigm; for example we can see one of the features of Newtonian physics is that it is highly deterministic, whereas quantum physics is not. These differences can be seen as direct results of the distinct elements of each respective paradigm. We can fairly say, then, that paradigms are the shared (not always explicit) rules, preconceptions, and standards of a scientific community.

But this is not enough to fully capture the meaning or the pivotal importance of the paradigm in the practice of normal science. As the paradigm is not merely an articulation of rules and standards, it is also one of the factors that determine the work of the scientist. Indeed, we can say that, “one of the things a scientific community acquires with a paradigm is a criterion for choosing problems that, while the paradigm is taken for granted, can be assumed to have solutions”(ibid., p.37). The paradigm constitutes its own problems and guarantees a solution within its own framework; it is within the paradigm’s framework that the scientist seeks answers, and so the paradigm itself constitutes the research that happens in the community. It is through the initiation of the student to the scientific community by means of education within the paradigm that we can have “reason to suppose that paradigms guide research by direct modeling as well as through abstracted rules (ibid., p.47).” To exemplify this we can think of the Ptolemaic astronomer, who could not have solved a problem about the shape of the earth’s orbit around the sun, as this problem would have been completely outside the realm of the accepted paradigm.

But deeply tied into this point is one that is an even more pervasive than just a determination of the scientist’s problems. The deeper feature of a paradigm is that it not only determines problems, but it also determines the world that the scientist sees. Different paradigms produce different and incommensurable worlds for scientists, so much so those scientists working under different paradigms are seeing different worlds. One of the reasons why these worlds are incommensurable is that there is no common language. Kuhn points out that even though new paradigms “ordinarily incorporate much of the vocabulary and apparatus, both conceptual and manipulative...they seldom employ

these borrowed elements in quite the traditional way. Within the new paradigm, old terms, concepts, and experiments fall into new relationships one with the other” (ibid., p.149). This does not preclude the possibility of communication from one paradigm to the other, rather it leaves us with a situation where “Communication across the revolutionary divide is inevitably partial” (ibid., p.149). Again, if we recall the Ptolemaic astronomer and introduce him to the Copernican astronomer, they would have serious difficulties communicating, as the paradigms they are each coming from are deeply incommensurable (ibid., p.149).

But on to the more compelling point, different paradigms constitute different worlds. Kuhn says this explicitly when speaking of different paradigms, and identifies as the most fundamental aspect of incommensurability that “the proponents of competing paradigms practice their trades in different worlds”(ibid., p.150). The Copernican astronomer not only sees the world differently than does a Ptolemaic astronomer; the Copernican sees completely different world. Kuhn further expresses this when he points out that “Practicing in different worlds, the two groups of scientists see different things when they look from the same point in the same direction. Again, that is not to say that they can see what they please. Both are looking at the world, and what they look at has not changed. But in some areas they see different things, and they see them in different relations one to the other”(ibid.).” Again, we see the deeply constitutive role of the Kuhnian paradigm, it is a concept that determines what normal science will investigate; but most deeply, it determines the world that the scientist will explore.

But most of this changed by the time that *The Road Since Structure* was published. In Kuhn’s later thought we see that he has replaced the idea of a paradigm

with that of the lexicon. This is a much less inclusive concept; it is no longer the holistic idea that was represented by the paradigm. The idea of a lexicon, rather, is one that is meant to mean something more like a categorical framework (notes 4/15/03). One of the features of the lexicon is that to possess it is to possess “a structured vocabulary, is to have access to the varied set of worlds which that lexicon can be used to describe” (*Road Since Structure*, p.61). This is different from a paradigm as it no longer constitutes the world itself, rather, it gives us access to it. But a deeper shift can be seen when we think that “Different lexicons – those of different cultures or different historical periods, for example – give access to different sets of possible worlds, largely but never entirely overlapping”(ibid., p.61). This is a new change in Kuhn’s view, as before the worlds that were constituted by paradigms were completely incompatible (as I have attempted to demonstrate above); the lexicon is different because it does admit that there are elements which can be common to the worlds that are accessed by different lexicons. This means that there can be overlap between the world that is described by Newtonian and Quantum physics. These worlds, then, are not completely incommensurable; rather, they are only largely incommensurable. But this still does not make communication across the divide perfect, so there is still no perfect translatability. Communication is only possible on the assumption that “anything which can be said in one language can, with imagination and effort, be *understood* by a speaker of another. What is prerequisite to such understanding, however, is not translation but language learning...If ...[the language learner] succeeds...he will become bilingual”(ibid., p.61). This means that the requisites for communication remain the same, though there are now elements which may be common.

Rather, incommensurability becomes “a sort of untranslatability, localized to one or another area in which two lexical taxonomies differ. The differences which produce it are not any old differences, but ones that violate either the no-overlap condition...”(ibid., p.93).

### **Question 2a**

Towards the end of his career Kuhn identified his position as a “sort of Post-Darwinian Kantianism” (*The Road Since Structure*, p.104). This is a position that is best understood if we separate the elements in this statement. The first idea that will be approached will be the analogy to Darwinian evolution which Kuhn applies to scientific development. After this the Kantian element makes more sense in light of the Post-Darwinian element that contributes to it.

In *The Structure of Scientific Revolutions* Kuhn makes an important analogy to Darwinism, he says that the analogy “is very nearly perfect” (p.172). The evolutionary nature of scientific development is clear because “The process described ... as the resolution of revolutions is the selection by conflict within the scientific community of the fittest way to practice future science. The net result of a sequence of such revolutionary selections, separated by periods of normal research is the wonderfully adapted set of instruments we call modern scientific knowledge”(p.172). The process of evolution then applies also to scientific development, as the fittest of the theories is the one to survive; and we can see modern scientific practice as the outcome of this process.

Another element which is an important part of this analogy one which affects our idea of scientific progress. Rather than conceptualizing scientific progress as working

towards something, Kuhn identifies it with the evolutionary tendency to see it as “a process that move[s] steadily *from* primitive beginnings but *toward* no goal”(p.172). That is, scientific progress resembles evolutionary progress rather than the cumulative progress that we try to associate with it. Kuhn fully develops this idea by saying that scientific progress “may have occurred, as we now suppose biological evolution did, without benefit of a set goal, a permanent fixed scientific truth, of which each stage in the development of scientific knowledge is a better exemplar”(p.172-3). Again, this radically changes the idea that is proposed by the standard approach, which proposes that our sciences progress towards truth or efficacy; instead our sciences simply progress from and not towards (p.172).

This analogy was not abandoned by Kuhn, as some of his other ideas were. Rather, this idea was one that Kuhn endorsed more avidly later in his career. As these major points were all endorsed in 1990, the analogy to evolutionary development was kept, and he reiterated that “scientific development must be seen as a process driven from behind, not pulled from ahead – as evolution from, rather than evolution toward”(The Road Since Structure, p.96). But he also developed an idea that he only alluded to in *The Structure of Scientific Revolutions* (p.170), that is the idea of speciation within science. To describe this speciation, Kuhn compares scientific development to an evolutionary tree (*The Road Since...*, p.98) where there is a division from a major branch of science into more specialized branches (ibid., p.97). Kuhn identifies two features of science which are similar to the idea of speciation in evolutionary development. The first is that “revolutions ...produce new divisions between fields in scientific development, are much like episodes of speciation in biological evolution...And the problems presented by

speciation ... are very similar to those presented by revolutionary change and by the emergence and individuation of new scientific specialties” (ibid., p.98). Kuhn points out that the second parallel comes when we look at the unit that undergoes speciation, evolutionarily this is an isolated gene pool; but in science this is “a community of intercommunicating specialists”(ibid.). And this in turn is “a unit whose members share a lexicon that provides the basis for both the conduct and the evaluation of their research and which simultaneously, by barring full communication with those outside the group maintains their isolation from practitioners of other specialties”(ibid.). In keeping with the evolutionary analogy, we can say that this is adaptive because “Very likely it is the specialization consequent on lexical diversity that permits the sciences, viewed collectively, to solve the puzzles posed by a wider range of natural phenomena than a lexically homogenous science could achieve”(ibid., p.99). It is accurate to say, then, that this speciation contributes to scientific progress.

But this Post-Darwinian approach affects Kuhn’s Kantianism, as there are no longer permanent “categories”. This is because Kuhn substitutes Kant’s categories by saying that, “Like the Kantian categories, the lexicon supplies preconditions of possible experience” (ibid., p.104). Here the Post-Darwinian element becomes important because we have seen that science itself moves as evolution does, constantly changing these lexicons. It is because of this that “lexical categories, unlike their Kantian, forbearers, can and do change, both with time and with the passage from one community to another” (ibid.). In this sense we can see the lexicons as serving the purpose that the categories served, though they are not fixed, they do constitute some basic principles which allow us to build up from them. But they are more than this, Kuhn says that if we take

Riechenbach's distinction between Kantian a priori seriously we can endorse the constitutive version of this concept (ibid., p.245). If we understand a priori in this sense we can take lexicons to be similar to Kant's a priori in that they "Both are constitutive of *possible experience* of the world, but neither dictates what that experience must be. Rather they are constitutive of the infinite range of possible experiences that might conceivably occur in the actual world to which they give access" (ibid.).

But another Kantian element in Kuhn's thought is his acceptance of the *Ding an sich*, of which he says "Underlying all these processes of differentiation and change, there must ...be something permanent, fixed, and stable...but it is ineffable, undescrivable, undiscussible" (ibid). This means that none of these lexicons provides us with direct access to the real world, only with sketches, which are not themselves candidates for truth or falsity; as they are appropriate to different purposes (ibid.).

### **Question 3c**

True, but in need of qualification. This is because the meaning of social that is meant by Longino is not the standard meaning. Indeed the meaning of 'social' is not the one meant by the strong program. Longino says that we should not interpret observation and rationality as something that is "so idealized as to be irrelevant to the epistemological analysis of actual scientific practices"(Longino, p.99). Instead Longino has a different proposal to approach these two elements, she says "I propose, contrary both to many philosophers and to many social theorists of science, that when we understand observation and reasoning as social practices, their justificatory role becomes clearer. In particular, I propose to treat both observation and reasoning as dialogical, that is, as

activities involving discursive interactions among different voices” (ibid.). This constitutes for Longino a different concept of social than the one that is commonly thought of, she clarifies this by pointing out that social does not mean that the idea is held collectively or commonly; rather, her idea of social is one that involves an interactive process that occurs within a greater community of scientists (ibid.). But to see this concept at work, it is useful to see how this idea is at work in the concepts of observation and reasoning.

Observation is characterized as social by Longino, because, she argues, it is embedded in a greater social context. This is because “Observation is not simple sense perception... but an organized sensory encounter that registers what is perceived in relation to categories, concepts, and classes that are socially produced” (Longino, p.100). That is, what we see is socially determined by that categories and classes that we are socialized into seeing; an example of this is the example of Hanson’s physicist and his son. Where the physicist sees something different than his son, because he has been indoctrinated in the accepted categories and classes of his field. The social dependence of observation is noted by Longino, she says, “What counts as an observation for community C will depend on how these differences are resolved in C. There is no court outside of C to make this determination”(Longino, p.101). This makes the standards of observation specific to each scientific community, so that what constitutes a legitimate observation in one community may not constitute the same thing in another community.

But another issue which is a requirement of observation relates to experiments and their repeatability. To ensure that only observations of a special class are allowed as valid this criteria is essential, indeed it is necessary for these issues to have validity as

data (ibid.). Indeed we can say that “The requirement of repeatability of experiments is a requirement for the intersubjective accessibility and cross-subjective invariance of data serving as evidence because it is a requirement that anyone similarly placed with similar equipment would see (perceive) the same thing” (ibid.). This ensures that only valid (to that extent that this process can regulate the validity of observations) observations are allowed to be taken seriously. Observation is social in this sense, as it is socially dependent; it is dependent on others and it requires the scientist to keep an open mind to criticism and suggestions (Longino, p.103).

But again, the influence of the social also extends to the concept of reasoning. However, here Longino distinguished between two forms of reasoning: constructive and justificatory reasoning. Constructive reasoning is “the combination of idea or information to produce new ideas”(Longino, p.103). This form of reason is social because “Some of the ideas so combined may be value-laden or value constrained in ways that reflect views of the individual reasoner or the shared assumptions of a community...” (Ibid.). However, the more salient of the two is the justificatory context of reason. This can be understood as “part of a practice of challenge and response: challenge to a claim is met by the offering of reasons to believe it, which reasons can then be challenged on grounds both of truth and of relevance, provoking additional reasoning”(ibid.). Longino argues that in this way, the process of justification acquires its social context, because it deals with the interaction of the individual and the object of cognition; such that the considerations which are considered appropriate are determined by interactive (therefore social) processes (Longino, p.103-4). The main point which can be taken from this idea of justification is that (like constructive reasoning) it “is a social practice and standards of

justification (the appropriate degrees of opacity and evidence of defeators) are determined intersubjectively, that is, socially (Longino, p.106).

### **Question 3d**

True. This is a claim that Kukla would absolutely agree with; indeed, he provides an emphatic argument for this limitation of logical constructivism. To really understand the scope of the argument that Kukla puts forth, it is important first to understand the view that he is criticizing. Then it is important to follow the steps of the argument that he provides against logical constructivism.

Kukla defines logical constructivism as “the view that there are no validly independent rules of logic”(Kukla, p.119). This means that there are rules of logic that are valid without being rendered valid by human activity – it is all contingent on us (ibid.). Kukla points out that “logical constructivists want to distinguish their position not only from rationalism, but also from a blanket *irrationalism* that won’t recognize any epistemic constraint on opinion or discourse...Unlike irrationalists, logical constructivists are willing to say that some inference rules are valid. They just disagree with the rationalist thesis that the validity of logic is independent of human activity” (ibid.). In a sense then, the logical constructivist wants to have his cake and eat it too; that is they want to say that there are valid rule of logic, but they are not objective. This leaves us with a view that seems to retain some modicum of logic, though it is not an independent system.

Despite the apparent reasonableness of this view, Kukla argues (quite convincingly) that it collapses into irrationalism. He points out that one of the limitations of the logical constructivist position is that it precludes scientific change. He establishes

this by arguing that “on the constructivists’ own accounts, orthodoxies relating to socially constructed facts are still *facts*, and to endorse and promulgate their negation is to embrace the *false*. If scientific facts are constructed by broadly consensual processes, then every new scientific proposal that contradicts current views is false” (Kukla, p.120). This leaves the logical constructivist in a very binding position, as they seem to have constructed a position that is very static and very much involved in the defense of the status quo. But Kukla argues that the logical constructivist can argue that this is not the goal of their project, and that “constructed truths don’t provide any normative constraints on our epistemic activity” (ibid.). This view seems to be closer and closer to irrationalism at every step. This is exacerbated by the idea that “there are bound to be some logics where constructivism wins and some where it loses... Why should constructivist capitulate just because they have the bad luck of living under a regime which legislates a logic that renders their favorite thesis false”(ibid.)? This means that logic can be rejected, but ,they argue, it cannot be rejected willy-nilly; there are higher order rules that legislate the permissibility of breaking first-order rules, of course, these are also socially determined (Kukla, p.121). Constructivists argue that this distinguishes them from the irrationalists, that there are higher order rules to how one should break first order rules. This means that even though there is no rule of logic that is absolutely sacred, not just any old view can be considered logical.

But Kukla points out that this view also collapses into irrationalism, since it collapses into an infinite regress. He says, “if all rules of logic are constructed, and if constructed truths needn’t be respected, then there’s no compulsion to follow the higher-order rules that tell us how to break the lower-order rules”(p.121). This leads us into and

infinite regress because all negotiations of these kinds of higher order rules require some previous logic; in the very best case scenario, it only leaves the possibility that the logic required and the rule being negotiated developed at the same time (and this seems iffy at best ) (ibid.). Kukla says there is a different and more serious view in play here, and this is that “the logic that you need is itself negotiable, *so you can break its rules*. For negotiation to be possible, there have to be higher-order rules about how the first-order rules can be broken. But these too are negotiable – so you can break those too, and so on. The result is that there are no constraints on the moves that logical constructivists allow themselves to make. Therefore logical constructivism reduces to irrationalism (p.121-122).

But, Kukla says, the logical constructivist can argue that rationalism is guilty of also abandoning logic when it is not convenient for it. He does engage this point, but argues that “What precipitates the collapse of logical constructivism to irrationalism is the license to change logics *ad libitum*”(p.153). He points out that rationalism does not do this; rather, it stipulates change only in exceptional cases, or it can stipulate its own changes (ibid.). This is one of the distinguishing factors between rationalism and logical constructivism/irrationalism.

#### **Question 4a**

One of the people who Bruno Latour credited with contributing to his own views, as we have mentioned in class, is Thomas Kuhn. Kuhn himself has pointed out that the identification of many sociologists of science with his view is based on a fundamental misunderstanding of his views. I will attempt illustrate why Kuhn is not to be classed

with the thinking of (specifically) Latour, and some of the sources of disagreement between their respective views. To do this I will provide a cursory account of two of Latour's major views, then I will provide what I believe Kuhn would have to say about them.

The first of Latour's views is that of the creation of interest by the scientist. In this sense Latour identifies the scientist as more than just a scientist, but a salesman of sorts. To illustrate this idea Latour uses Pasteur as the paradigm example of someone, who in some sense knows how to play the scientific game. He points out that "Pasteur, from the start of his career, was an expert at fostering interest groups and persuading their members that their interests were inseparable from his own. He usually achieved this fusion of interests ...through the common use of some laboratory practices"(Biagioli, p.263). We see Pasteur then as more than just a scientist, but almost as a lobbyist of sorts, one that tries to convince interest groups, companies and people that can contribute money to invest. This becomes clearer when we take another look at the relationship between Pasteur and agricultural interests. We see that "A hard negotiation ensues between Pasteurians and agricultural interests on the conditions of the experiment...the problem is to find a compromise that extends Pasteur's laboratory far enough – so that the vaccination can be repeated and work – but which is still acceptable to the farming representatives so that it is seen as an extension of lab science outside"(ibid.). This gives us the sense that the scientist makes concessions to monetary and P.R. issues, and that sometimes even the science itself is adapted to these ends.

But Kuhn would never agree with this characterization of science. Indeed, he has responded to this by saying that "Interest, politics, power and authority do play a

significant role in scientific life and its development. But the form taken by the studies of “negotiation” has, as I’ve indicated, made it hard to see what else may play a role as well... ”(The Road Since Structure, p.110). He adds that “Of course power and interest play a role in scientific development, but there’s room for a great deal else besides (ibid., p.115-116). These are both elements that are important in the Latour’s views (though the first, and arguably the second can be said to apply as well to the Strong Program). Kuhn is not denying that interests play a role in science. Rather, he denies the reduction of science to a mere economic and interest enterprise; science itself does not take a back seat to these outside interests. If this were so, there would seem to be no point in engaging in the kind of artifice that Latour and other proponents of sociology of science put forth. Science would simply be one more, cheap, purchased commodity. Kuhn would never accept such a view.

Another idea that is proposed by Latour is that facts are not objective; rather they are simply social constructs. Latour claims that ideas such as “all over France” are social constructions (Biagioli, p.264). He says that “all over France” is a social construct because it was a construction of the “statistic-gathering institutions”(ibid.). But a more extreme view is the idea that “We do not have, on the one hand, a history of contingent human event and, on the other, a science of necessary laws, but a common history of societies and things. Pasteur’s microbes are neither timeless entities discovered by Pasteur, nor the effect of political domination imposed on the laboratory by the social structure of the Second Empire, “nor are they the careful mixture of “purely” social element and “strictly” natural forces. They are a new social link that redefines at once what nature is made of and what society is made of” (Biagioli, p.284). This leaves us

with the idea of *quasi objects* that Latour proposes, this leaves us with a sense that there are not really microbes as discovered by Pasteur, but that they are dependent on us, constructed by us in some sense.

Again Kuhn would never agree with this, he specifically criticizes the limited involvement of nature in this view. He says specifically that the world “is not invented or constructed”(Kuhn, p.101). He continues by saying that theories such as the ones proposed by Latour “acknowledge that the world plays a role in scientific development. But they remain almost totally uninformative about that role – about the way, that is, in which nature enters the negotiation the produces beliefs about it (ibid., p.110). Kuhn criticizes not only that facts are being constructed, but that there is a supposed acknowledgement of the role of nature which is not being elaborated. The connection between nature is not made, it is only mentioned; only to be insulted later with comments such as those proposing the non-existence of microbes independent of ourselves.

