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Incommensurability and the Bonfire of the Meta-Theories: Response to Mizrahi
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What is Taxonomic Incommensurability?

Moti Mizrahi states Kuhn's thesis of taxonomic incommensurability (TI) as follows:

> Periods of scientific change (in particular, revolutionary change) that exhibit TI are scientific developments in which existing concepts are replaced with new concepts that are incompatible with the older concepts. The new concepts are incompatible with the old concepts in the following sense: two competing scientific theories are conceptually incompatible (or incommensurable) just in case they do not share the same "lexical taxonomy." A lexical taxonomy contains the structures and vocabulary that are used to state a theory (2015, 2).

Mizrahi cites Kuhn (2000) as a basis for this definition. There, and elsewhere, Kuhn repeatedly employs the metaphor of incommensurability from Greek geometry:

> The hypotenuse of an isosceles right triangle is incommensurable with its side[,] or the circumference of a circle with its radius[,] in the sense that there is no unit of length contained without residue an integral number of times in each member of the pair. There is thus no common measure. But lack of a common measure does not make comparison impossible. … The claim that two theories are incommensurable [in the context of scientific theories] is then the claim that there is no language… into which both theories… can be translated without residue or loss (Kuhn 2000, 36).

Prima facie, it's a terrible metaphor. We can "compare" the circumference of a circle with its radius, but we can't divide the circumference by the radius and get an integer. One way to read this is that there is no way to say, for instance, "The circumference of a circle divided by its radius is 2y," where y is a unit of measure.

One might object that of course there's a unit of measure y such that the circumference of a circle divided by its radius is 2y, for instance. Define a unit of measure "y" as $\frac{C}{2r}$, half the circumference of a circle divided by its radius. If $y = \frac{C}{2r}$, then $2y = \frac{C}{r}$. However, that move is not possible in ancient Greek geometry, which is synthetic geometry. Analytic geometry defines geometrical magnitudes using algebraic variables and functions. Synthetic geometry constructs geometrical magnitudes using compass and straightedge. You can't construct a length equal to the circumference of a circle by repeatedly drawing a line equal to the radius, or equal to some fraction of the radius. If you draw the radius six times, the line will be shorter than the circumference, while if you draw the radius seven times, the line will be longer: there will be a deficit ($2\pi r-6r$) or a residue ($7r-2\pi r$). If you define the unit of measure as half the radius ($\frac{r}{2}$), and define the magnitude as between

6r and 7r, say, $\frac{13}{2r}$, the deficit or residue will decrease but not vanish. You can "compare" the two magnitudes by dividing the radius as finely as you wish ($\frac{r}{3}$, $\frac{r}{4}$, and so on), and as the fractions get smaller, you can construct magnitudes closer and closer to 2πr. But because π is irrational, you will never be able to construct a magnitude equal to 2πr out of radii or segments of radii. The magnitudes C and r are incommensurable in Greek geometry.

This is why it's a terrible metaphor. What's stated in the previous paragraph is by no means common knowledge, and even once you understand the geometrical case, the comparison is not direct. Also, both examples involve a famous irrational number (π in the circle example, $\sqrt{2}$ in the triangle case), which confuses the issue. Still, it is possible to explain why taxonomic incommensurability is like geometrical incommensurability. In fact, the concept "geometrical incommensurability" itself could be read as a taxonomic incommensurability that arises when moving from synthetic to analytic geometry.[1]

For Kuhn, the business of science is problem solving, which takes place according to a paradigm that sets the rules of the game.[2] In synthetic Greek geometry, the circumference of a circle and its radius are incommensurable. You cannot divide the circumference by the radius, or by some fraction of the radius, when playing by the rules of ancient Greek geometry. But using analytic geometry and real analysis we can remove the "incommensurability" from any ratio involving circles and triangles.

To do that is to leave behind the meaning of the word "incommensurability" in ancient Greek geometry. The word had that meaning only when playing by the rules of synthetic geometry. When moving to analytic geometry, we lose the Greek meaning of incommensurability, and that is a taxonomic deficit of analytic with respect to synthetic geometry. By doing some violence to the semantic framework, we may be able to translate the proposition "A circle's circumference and radius are incommensurable" into the "language" of analytic geometry.[3]

But that is not the point. The proposition is not provable within the system of analytic geometry, and it is provable within the system of synthetic geometry. When the provable statements of synthetic geometry are "divided" by the provable statements of analytic geometry, it leaves a taxonomic residue. The concept of the geometrical incommensurability of certain magnitudes makes sense if you've undertaken the project of constructing one magnitude out of another, but it doesn't make the same kind of sense if you begin by defining geometrical quantities using variables and functions.[4]

On Mizrahi's definition of taxonomic incommensurability, "two competing scientific

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[1] For now, I will bracket the question of whether the change from synthetic to analytic geometry is a paradigm shift.
[2] Richardson (2002) explains the idea of playing by the rules more clearly and in more detail.
[3] As Kuhn remarks, Davidson's claim that incommensurability is a failure of intertranslatability is "literally correct but regularly overinterpreted" (2000, 34).
[4] There could be an analogue in analytic geometry, but not the same concept.

theories are conceptually incompatible (or incommensurable) just in case they do not share the same 'lexical taxonomy'." That "just in case" limits incommensurability to a lexical feature of the theory. But incommensurability is not necessarily a failure of translatability, nor is it merely the change of reference of theoretical terms. These are effects of a cause, which is the residue or deficit left when comparing the propositions that make sense and are derivable within a theory.

Kinds of Arguments

Mizrahi cites the lack of a strong inductive (section 3) or sound deductive (section 2) argument for incommensurability. He concludes from this that there is no argument for taxonomic incommensurability. But many influential arguments from the history of science are neither inductive nor deductive. They are abductive, or are inferences to the best explanation.[5]

Campos (2011) observes that for Peirce "abductive reasoning is inference to an explanatory hypothesis, and this form of reasoning is to be distinguished from induction, in which we have already adopted a hypothesis and are only testing its consequences" (425, see Peirce 1867). Abduction is the process of forming an explanatory hypothesis about a data set. It involves either coming to see that a previously known law applies to the data, or coming up with a law or principle adopted, provisionally, as an explanation of the data.[6] An inference to the best explanation (IBE) reasons from the fact that a given hypothesis is the best (available) explanation of the evidence to the claim that the hypothesis is true.

In Mizrahi's chosen context, philosophical reasoning about the history of science, abductive arguments and IBEs flourish. Laudan (1981) describes convergent realism in philosophy of science as resting on two "elaborate abductive arguments". One of these is,

> 1. If the earlier theories in a 'mature' science are approximately true and if the central terms of those theories genuinely refer, then later more successful theories in the same science will preserve the earlier theories as limiting cases;

> 2. Scientists seek to preserve earlier theories as limiting cases and generally succeed;

> 3. (Probably) Earlier theories in a 'mature' science are approximately true and genuinely referential (Laudan 1981, 21-2).

It's not clear why the arguments Laudan cites would be abductive, as others have observed. Still, the conclusion (3) could be regarded as a law formulated to explain the premises. Or the argument may be a species of IBE: the claim in (3) is described as

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[5] I am grateful to Ben Jantzen for discussion of this.
[6] See Campos 2011, 425-6.

probably the best explanation for the facts in (1) and (2). Either way, the argument is neither inductive nor deductive.

Laudan's confutation of convergent realism often is described (though not initially by Laudan) as a "pessimistic meta-induction." Against the optimistic realist abduction, Laudan argues that many terms employed in earlier, mature scientific theories do not refer (ether, caloric, phlogiston), and that those theories are not approximately true. He concludes, realism cannot, even by its own lights, explain the success of those many theories whose central terms have evidently not referred and whose theoretical laws and mechanisms were not approximately true (47). Laudan's confutation is an argument against a realist inference to the best explanation. It has been construed as a reductio. Whatever it is, it is not a straightforward inductive argument.

Hilary Putnam's no miracles argument is an example of an IBE, as Lipton (2001) explains:

> Suppose that all the many and varied predictions derived from a particular scientific theory are found to be correct: what is the best explanation of this predictive success? According to Putnam, the best explanation is that the theory itself is true. If the theory were true, then the truth of its deductive consequences would follow as a matter of course; but if the hypothesis were false, it would be a "miracle" that all its observed consequences were found to be correct (191).

Laudan's confutation and Putnam's no miracles argument are seminal. Almost every paper on scientific realism begins by citing them. Putnam's no miracles argument is an IBE, and so would not be considered an argument under Mizrahi's *a priori* classification of arguments as inductive, deductive, or not an argument. Laudan's confutation, if it's a *reductio,* might be deductive. But Laudan's conclusion is that realists have not given a good explanation of the history of science, so it would still be a deductive argument employed to refute an inference to the best explanation.

Many of the most influential arguments in the history and philosophy of science are second-order inferences about what we can conclude from the empirical success of science. They are explanatory hypotheses, or inferences to the best explanation, that cannot be construed as first-order inductive or deductive inferences.

So much the worse for these influential arguments, we might conclude. They should be tossed onto the fire with Kuhn's taxonomic incommensurability argument. Mizrahi will get no objections from me, if his goal is to limit grand, speculative, meta-explanatory projects based on meager inductive evidence from the history of science. In fact, I read Laudan's 1981 paper as a plea for such limitation, not as an argument for scientific anti-realism. So perhaps his paper should be saved from the bonfire of the meta-theories.[7]

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[7] I "read" it this way because Laudan says explicitly that that is the point of the paper.

What Kind of Argument is Kuhn's Argument for Taxonomic Incommensurability?

Should Kuhn's argument be tossed onto the bonfire? Mizrahi argues that there is neither a deductive nor an inductive argument proving that Kuhn's taxonomic incommensurability applies to every case of competing scientific theories. The former wouldn't be a plausible reading of Kuhn's stated views in any case. In the latter case, Mizrahi says "I think that it is a mistake to generalize from a few selected examples that competing theories in general are taxonomically incommensurable" (2015, 8).

Kuhn and Feyerabend did not argue that every case of competing theories, or of theory change, requires taxonomic incommensurability involving meaning or reference variance. Kuhn argued that this is only the case with paradigm shifts, and Feyerabend, only with paradigm shifts that affect fundamental ontological categories. The class of competing theories is much larger than the class of competing paradigms.

Kuhn (2012/1962) defines a paradigm shift as a case in which persistent anomalies, discrepancies in the "fit between theory and nature", come to be seen by scientists as obstacles to pursuing research under the existing paradigm (81). The application of the paradigm, perhaps to novel evidence, gives rise to the anomaly or anomalies. For an episode in science to be a revolution, a paradigm shift, it must involve an anomaly that provokes a crisis: a realization among scientists that scientists had been tackling the problem from the wrong angle, an increasing tendency to go outside the rules of the paradigm when solving problems, and even an increasing lack of consensus about what the paradigm is (83).[8] Resolving a crisis involves giving up the old paradigm altogether and adopting a new one, a new set of rules of the game. By definition, doing so changes the conceptual categories, the strategies for puzzle-solving, and the implicit reference of scientific terms, associated with the old paradigm.

Kuhn's invocation of the history of science in his explanatory analysis of the history of science is not an inductive argument. Kuhn does not give examples to demonstrate that all theory changes are paradigm shifts. He defines a paradigm shift, and then uses that definition as a criterion to identify examples. You can argue that a particular theory change is not a paradigm shift, even one that Kuhn uses as an example, but that doesn't affect the validity of the definition as part of an abductive, explanatory framework.

In §3.1, Mizrahi gives an account of the case of anastomoses in the history of medicine, which

> exhibits continuity in terms of the problems practitioners worked on, and supplementation in terms of old concepts that were abandoned for a while (e.g. anastomoses) but then rediscovered and added to the new theory, rather than discontinuity and replacement, as (TI) predicts (2015, 10).

If the problems worked on stayed the same, and if it turned out that an existing concept

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[8] Not every anomaly provokes a crisis.

could solve those problems, including anomalies, then the case of anastomoses is not a paradigm shift. Dealing with the problems did not provoke a crisis, did not cause scientists to question the paradigm itself, and did not cause them to depart from the existing paradigm in problem-solving. Mizrahi's own telling of the history supports the claim that the case of anastomoses is a solution to a puzzle within an existing paradigm. On Kuhn's theory, then, there is no requirement of taxonomic incommensurability in this case.

Structure relies on inductive evidence only in the context of explanatory hypotheses. The reasoning in support of these is abductive and explanatory, not inductive. The idea of a paradigm shift is an explanatory hypothesis. To cite examples of problem solving in science that are continuous, and that supplement concepts with others, does not affect the explanatory framework of paradigm shifts. You could argue against that explanatory framework by arguing that the examples Kuhn does give of paradigm shifts and of incommensurable theories are not valid examples, which some have done. A knock-down argument would require proving that there are no significant, persuasive such examples in the history of science.

That reasoning may seem *ad hoc*. Mizrahi has just given a counterexample, and I have said that it doesn't count as a counterexample because it doesn't fit the definition of a paradigm shift. But that is a legal move on my part: that is how definitions work, in this explanatory context.

Is it ethical? It is if Kuhn limited taxonomic incommensurability to paradigm shifts, that is, to cases in which dealing with persistent anomalies requires changing paradigms. Unfortunately, in his later work, Kuhn does apply the concept beyond the domain in which it is justified as an explanatory hypothesis or inference. Mizrahi cites Kuhn (2000), which includes an essay on possible worlds in the history of science which links taxonomic incommensurability to intensional semantics (58-89). As soon as taxonomic incommensurability is divorced from analysis of the proof-structure within paradigms, it loses force. Considered as a general claim about linguistic usage in distinct contexts, it is unjustified. There, Mizrahi and I may agree.

But Mizrahi links his analysis to Kuhn's account of the history of science, and Mizrahi defines taxonomic incommensurability as arising in cases of revolutionary change (2). In the context of scientific revolutions, Kuhn does not argue from particular examples of scientific theory change to a general claim that every theory change involves incommensurability. Instead, he formulates explanatory hypotheses about those theory changes that are demonstrable cases of incommensurability: paradigm shifts.[9]

I would put Mizrahi's paper into a broader framework, then, of explaining why Kuhn's

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[9] In formulating these abductive explanatory hypotheses, Kuhn constructs historical narratives and interpretations (Kuhn emphasizes this in *The Essential Tension* and in Kuhn (2000, 37-40)). But Kuhn's narrative and interpretive methods are employed within an explanatory framework, formulating and testing abductive hypotheses against abundant evidence from the history of science. Those who followed Kuhn were not always so careful.

concept of taxonomic incommensurability is defensible within the framework of the analysis of paradigm shifts in science, but not necessarily in the broader contexts in which Kuhn and others later used it. Mizrahi blends these two argumentative contexts. I would insist on separating them. The force of Kuhn's argument in *Structure* is that a paradigm that sets the rules of inference and of puzzle solving implicitly limits the inferences that can be drawn, the puzzles that can be solved, and even the propositions that make sense within that paradigm.

Scientists working within a paradigm must play by the rules of the game of that paradigm in solving problems, and that is why incommensurability arises when the rules of the game change. If we deny the thesis of the priority of paradigms, then there is no good argument for the incommensurability of theories and thus for taxonomic incommensurability, because there is no invariant way to determine the set of results provable, puzzles solvable, and propositions cogently formulable under a given paradigm. I suspect Mizrahi and I agree on this conclusion, but disagree about whether the priority thesis is plausible.

I sympathize with Mizrahi's implied point that meta-theorizing about theory change seems to have come unmoored from philosophical argument. If the plan is to consign sweeping, inductive meta-narratives to the flames, I will bring the gasoline and firecrackers. All I ask is that we take care before we incinerate at least the earlier Kuhn, who was pursuing an explanatory, not an inductive project. There are philosophers who construct speculative second-order narratives based on the history of science, making a meal of a few cherry-picked examples before inhaling the nitrous oxide of best explanations. Kuhn is not one of them.[10]

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[10] I would like to thank James Collier and *Social Epistemology Review and Reply Collective* for inviting me to respond.

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