COMMENTARIES

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Chapter 7

Reflections on Conceptual Change

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When discussed among philosophers of science, the issue of conceptual change brought in its tow incommensurability. Kuhn and Feyerabend both suggested that in the transition from the old to the new paradigm (or theory, or conceptual scheme) there is a deep conceptual asymmetry. Hoddeson (this volume) narrates elegantly the shock that the community felt and explains the ways that several members of it dealt with the post-Kuhnian trauma.

In *Structure*, Kuhn made notoriously extravagant claims about the proponents of competing paradigms practicing their trades in different worlds, or being transported to different worlds etc. When the dust settled, Kuhn drew a distinction between global and local incommensurability and defended a version of the latter. Machamer (this volume) discusses this issue in his piece and makes what I take it to be the exactly right point about global incommensurability: it entails relativistic solipsism (which, I add, a moment's reflection shows that it is either incoherent or absurd); hence, any theory that entails global incommensurability is reduced to absurdity.

What then of local incommensurability? In his mature thought, Kuhn argued that it occurs when the competing theories have locally different taxonomies of natural kinds (what Kuhn called *lexical structures*). It amounts to a claim of local untranslatability, due to a mismatch between the lexical taxonomies associated with the two theories. Kuhn, actually, was very careful in characterising the situations in which incommensurability arises. The obvious objection to his claim is that mismatches in the lexical structures may well be there and yet, the lexica might be sufficiently similar to enable setting up at least rough-and-ready correspondences among their nodes. To this he replied that not any "old difference" yields incommensurability; rather, there must be differences that test the compatibility of nodes in the lexical structures. He talks, for instance, about the no-overlap principle which captures the old Lockean thought that there are gaps or chasms in nature: the members of two natural kinds that are not related to each other as genus and species are disjoint. Incommensurability, then, arises when lexical structure A classifies entity x under kind K and structure B classifies entity x under the disjoint kind L; or when lexical structure A classifies entity x under kind K and structure B classifies entity y — but not x — under the kind K.

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1   Machamer (this volume) thinks Kuhn is basically right about local incommensurability.
2 But I do not see why this should give us pause. As stated, the thesis of local incommensu-
3 rability merely recapitulates the claim that there is conceptual change. To talk of concep-
4 tual change as opposed to conceptual replacement presupposes that something remains
5 unaltered while something else changes. Conceptual replacement does occur but gives no
6 rise to incommensurability. Conceptual change, on the other hand, reassigns referents
7 (extensions) to new kinds or reshuffles the referents (extensions) of old kinds. But this
8 would be a problem only if concepts were identified extensionally. Since they are not,
9 there is no problem.
10   The real issue, I take it, concerns the similarities and differences in the conceptual roles
11 assigned to them. These can offer criteria for conceptual similarity and difference. To clar-
12 ify this point I will appeal to Sellars (1973). He defended a kind of nominalism about con-
13 cepts. He denied that concepts are abstract entities (types or universals) to which thought
14 is somehow related. Concepts are not the kind of thing one is related to by having certain
15 thoughts — e.g., the concept DOG is not the kind of thing one is related to when one has
16 dog-thoughts. He took the line that concepts are (or are related to) dispositions and abili-
17 ties (hence, concepts are not things of any sort), viz., dispositions to think thoughts of cer-
18 tain things — e.g., to have the concept DOG is to have the ability to think thoughts about
19 dogs, and in particular to think thoughts to the effect that such and such thing is a dog.
20   Sellars conjoined this dispositional account of concepts with a functionalist account of
21 thought: thoughts have the content they do because they function in a certain way and two
22 thoughts with the same function have the same content. If that is what is important about
23 thoughts (as Sellars says, apart from their functional role, thoughts are *neurophysiological*
24 processes), concepts — being constituents of thoughts — can be identified functionally as
25 well. Identical are those concepts that function in exactly the same way. This move opens
26 up the space of comparing concepts according to their functional *similarity*. How a con-
27 cept functions will depend on its connection to other concepts (that is, to its conceptual
28 role) as well as the rules (formal or material, as Sellars would have it) that determine its
29 contribution to a conceptual framework. Similarity of function is then the means to build
30 bridges among concepts — even if these concepts are not identical (despite the fact that
31 they might have the same names.) For instance, one could say that the function of a pitcher
32 in a baseball game is similar to the function of a bowler in a game of cricket. (So: the con-
33 cepts PITCHER and BOWLER are similar — this is illuminating, I take it, since we can
34 now focus on where they differ. Pitching is primarily defensive — it aims to prevent the
35 other team from scoring runs — while bowling is offensive — it aims to remove the bats-
36 man). Similarly, (*mutatis mutandis*) one could say that the function of relativistic mass in
37 special relativity is similar to the function of classical mass in classical physics.
38   All this presupposes that the functional role of a concept is *not* determined in a holistic
39 and undifferentiated way by the framework in which it belongs. Arabatzis (this volume)
40 disagrees. He defends conceptual holism (that is, the view that a concept is identified holis-
41 tically by means of all properties possessed by, and all law-like connections that charac-
42 terise, the entities to which it applies). But conceptual holism cannot explain the robustness
43 (or invariance) that many concepts possess, viz., that they do not change very easily, and
44 certainly they do not change when small alterations take place in their conditions of appli-
45 cation. Besides, conceptual holism cannot explain what the possession of a concept consists

1 in since it *cannot* consist in the possession of a full theory. The sensible thing to say is that
2 not everything (in a conceptual scheme) is constitutive of its concepts and not all belief
3 changes constitute concept changes. That a line should be drawn seems imperative.
4 Arabatzis seems to be saying this: concepts are individuated holistically, but there can be
5 conceptual stability provided there are independent ways to identify what the concept
6 stands for as the same again. These independent ways are tied to experimental procedures
7 in which a stable referent for a concept is identified. Therefore, Arabatzis makes experi-
8 ments the locus of conceptual stability.
9   But there seems to be a problem with this. Let us take theories to be (or to fix the)
10 modes of presentations (senses) of concepts and let us take the experimental procedures to
11 fix the reference of a concept (the type of entity for which the concept stands for). How
12 are these related to each other? It seems that, on Arabatzis' story, these two semantic vehi-
13 cles move parallel to each other and it is a fortunate *coincidence* that they, if at all, cross
14 each other's path. There is no reason to think that the referent identified in a series of sta-
15 ble experiments is the referent of a concept whose content is identified holistically by a
16 theory unless the theory informs the experiments and the experiments expand the theory.
17 Here again, if the *whole* of the theory informs the experiment, when the theory changes,
18 the experiment changes too and the referent that has been identified experimentally
19 changes too. I do not doubt that experiments offer useful ways to identify entities; the issue
20 is that they have to identify an entity as the referent of a *concept*. If this concept is indi-
21 viduated (or is introduced) in the process of the experiment, well and good. If this concept
22 is individuated by a theory, we need an extra assumption that it is *this* concept's referent
23 that is identified by the experiment.
24   With this in mind, let us move back to Sellars (1973). I take him to have argued that it
25 is precisely because some concepts *are* similar, that is, they play a similar functional role
26 in their respective frameworks, and that not all parts of the framework are equally respon-
27 sible for determining the content of concepts. But how can we classify concepts as being
28 similar? Sellars' deepest thought in this connection was that it is a mistake to think that
29 there is just one single concept of *X*. Conceptual change (not replacement) occurs when
30 we move from one concept of *X* to another concept of *X* and this, of course, is compatible
31 with the thought that these concepts of *X* are relevantly similar to each other (hence, there
32 is conceptual continuity of the sort that allows for conceptual change).
33   To illustrate this, Sellars considers the following cases:

34
35   (A)   ISOSCELES TRIANGLE vs. SCALENE TRIANGLE
36   (B)   EUCLIDEAN TRIANGLE vs. RIEMANNIAN TRIANGLE

37   In case (A), the two concepts operate within a certain (Euclidean) framework and are
38 species of a genus. Using type-hierarchies, we could say that EUCLIDEAN TRIANGLE
39 is a supertype, having ISOSCELES TRIANGLE and SCALENE TRIANGLE as subtypes.
40 These two subtypes are similar in specific respects (since they inherit the properties of
41 their common supertype) and different in others (in virtue of which they are classified in
42 different subtypes). In case (B), the two concepts operate within different frameworks.
43 Using type-hierarchies, we could say that both EUCLIDEAN TRIANGLE and RIE-
44 MANNIAN TRIANGLE are subtypes of a supertype TRIANGLE. What differentiates the
45 subtypes is the fact that EUCLIDEAN TRIANGLE is governed by the axioms of

1   Euclidean geometry, while RIEMANNIAN TRIANGLE is governed by the axioms of
2   Riemannian geometry. It is obvious that ISOSCELES TRIANGLE and SCALENE TRI-
3   ANGLE are more similar to each other than are EUCLIDEAN TRIANGLE and
4   RIEMANNIAN TRIANGLE. But there is no reason to think that both EUCLIDEAN
5   TRIANGLE and RIEMANNIAN TRIANGLE are not TRIANGLE concepts. There is, as
6   Sellars (1973, p. 90) put it, a generic functioning of TRIANGLE "which abstracts from the
7   specific differences between Euclidean and Riemannian geometries". Sellars' point is that
8   it is wrong to think of these two concepts as single concepts. Rather, the expressions
9   "Euclidean" and "Riemannian" qualify the concept TRIANGLE. (So, strictly speaking, we
10 should write: *Euclidean* TRIANGLE and *Riemannian* TRIANGLE.) The Euclidean
11 TRIANGLE and the Riemannian TRIANGLE are both varieties of TRIANGLE; they
12 function in a similar way in their respective frameworks and they function in a way that
13 TRIANGLE (more abstractly specified) functions.
14   In *On Certainty*, Wittgenstein (1969) claimed that some propositions play the role of
15 hinges that hold together a language-game (or a conceptual scheme). He did not think,
16 however, that hinges share something deep in common in virtue of which they function as
17 hinges. They are not analytic truths, nor *a priori* truths. They are rather heterogeneous col-
18 lections of propositions (including arithmetical truths, straight empirical propositions and
19 others). What hinges share in common is that they *function* in a certain way, viz., within
20 certain language games, they cannot be doubted without revealing some conceptual short-
21 coming (cf. p. 137). Their functioning this way, according to Wittgenstein, is the result of
22 an *act* (or a *deed*, as he put it): the act that has to do with that
23
24   "we just *can't* investigate everything, and for that reason we are forced to
25   rest content with assumption". (p. 343)
26
27   Baltas (this volume) finds in this Wittgensteinian story a way to understand what
28 happens during a revolution — and in particular a way to understand a conceptual change.
29 What he calls *background "assumptions"* are hinges, with the difference that they are hid-
30 den — they lie in the background of concepts and determine their conditions of their appli-
31 cation. According to Baltas, background "assumptions" determine the logical *grammar* of
32 a concept. His idea is that a revolution (a paradigm change) consists in a leap into the
33 ungrammatical (from the point of view of the superseded paradigm) while it also consists
34 in bringing to the foreground and challenging the background assumptions of the concepts
35 of the old paradigm (from the point of view of the new paradigm). This is an interesting
36 asymmetrical relation: the new paradigm opens up a new grammatical (hence conceptual)
37 space that was not possibly available from within the old paradigm.
38   The emergence of a new paradigm, according to Baltas, turns a background assumption
39 into an ordinary proposition, and in particular, one that can be subjected to doubt, empiri-
40 cal examination, revision or rejection. By challenging this assumption, the new paradigm
41 redefines an old concept by removing a presupposition for its application.
42   How are the new and the old concepts related to each other? As Baltas himself notes,
43 there *must* be some continuity (similarity) between the two concepts; otherwise, it will be
44 unclear that they relate to the same phenomena and in particular, that the conceptual
45 change was prompted (at least partly) as a response to an empirical anomaly that the old

1 paradigm faced. Now, Baltas seems to be saying three things in response to the question
2 above. *First*, there is continuity because the non-grammatical character of the new concept
3 *vis-à-vis* the old one is repressed in memory — scientists, that is, forget or choose to for-
4 get that there was a leap into the ungrammatical. *Second*, in the transition from the old to
5 the new paradigm, some worldly item (whatever constituted the anomaly that brought the
6 old paradigm to crisis) remains invariant. *Third*, the retention of the same *name* for the old
7 and the new concepts constitutes "the necessary grammatical reminder of this [conceptual]
8 continuity".
9 　　However, even if we were to grant these three points, they would locate the continuity
10 at the wrong place. The locus of conceptual continuity should be similarities among the
11 concepts, i.e., among their *content* (their functions, or their conceptual texture, as Sellars
12 (1973) would put it). Anything else, desirable though it may be, is an extrinsic character-
13 istic. Baltas' first point makes conceptual continuity a matter of (fortunate) psychological
14 contrivance; his second point makes it a matter of referential stability; and his third point,
15 makes conceptual continuity a matter of naming. It is worth noting that Sellars' account
16 sketched above would be a better candidate for capturing Baltas' insight. If we think of not
17 just one but of many concepts of *X* (e.g., *classical* WAVE; *non-classical* WAVE etc.),
18 Baltas' idea of changes in the background assumptions can be part of the story that
19 explains how these concepts are similar to, and different from, each other. What more
20 should we expect from a theory of conceptual change?

Acknowledgements

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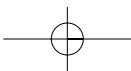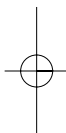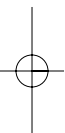
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