

***A Reply to James Marcum’s “What’s the Support for Kuhn’s Incommensurability Thesis?”***

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Both Patton (2015) and Marcum (2015) think that there is compelling evidence for Kuhn’s incommensurability thesis, specifically, taxonomic incommensurability (TI). They disagree, however, about how the argument for TI is supposed to run. Patton (2015) claims that there is an Inference to the Best Explanation (IBE) to be made for TI. In my response to Patton (2015), I argue that this is easier said than done (Mizrahi 2015b). Marcum (2015, 51), on the other hand, claims that the historian’s personal or psychological experience of accessing a revolutionary change in science—as illustrated in Kuhn’s own experience of laboring to understand the Aristotelian idea of motion while assuming a Newtonian idea of motion—represents a compelling type of support for TI.

In other words, Marcum (2015, 58) claims that “support for [TI] should also include its discovery.” Like ‘knows that  $p$ ’, however, ‘discovers that  $p$ ’ is factive. That is to say, if one knows that  $p$ , then  $p$  is true. Likewise, if one discovers that  $p$ , then  $p$  is true. After all, just as one cannot know what isn’t the case, one cannot discover what isn’t the case. For instance, to say “I discovered that Kuhn was a smoker” is to say that ‘Kuhn was a smoker’ is true. Similarly, to say “Kuhn discovered that Aristotelian physics and Newtonian physics are taxonomically incommensurable” is to say that ‘Aristotelian physics and Newtonian physics are taxonomically incommensurable’ is true. But that begs the question against anyone who doesn’t already accept that TI is true. And even if it is granted that Aristotelian and Newtonian physics are taxonomically incommensurable, no general conclusions about revolutionary change in science follow from that (Mizrahi 2015, 368-372).

Marcum (2015, 60) talks about the “discovery experience” as “a conversion, like a religious one.” Like a religious experience, however, a “discovery experience” may simply be “in one’s head,” as when René Blondlot thought he had “discovered” N-rays or when Leon Lederman thought he had “discovered” Upsilon. It would hardly be convincing if Blondlot were to cite his “discovery experience” as evidence for the existence of N-rays or if Lederman were to cite his “discovery experience” as evidence for the existence of a subatomic particle now known as “Oops-Leon.”

For these reasons, the argument Marcum (2015, 57) sketches on Patton’s behalf, namely,

1. If terms of competing theories are non-overlapping taxonomically and do not refer to the same thing, then the competing theories are TI.
2. Terms of competing theories are non-overlapping taxonomically and do not refer to the same things.

Therefore:

3. Competing theories are taxonomically incommensurable (Marcum 2015, 57).

(which, contrary to what Marcum seems to suggest, is a deductive argument, not an IBE; cf. Mizrahi 2015b) is both question-begging and unsound. This argument is question-begging because to assume (2) as a premise in an argument for TI is to assume that TI is true, since, as Marcum (2015, 56) himself writes “terms and ideas between the two competing paradigms [namely, Galen’s and Harvey’s] are *non-overlapping and thereby taxonomically incommensurable*” (emphasis added). In other words, what makes competing theories,  $T_1$  and  $T_2$ , taxonomically incommensurable is the fact that their terms and ideas are non-overlapping. Accordingly, to assume as a premise, i.e., premise (2), that terms of competing theories are non-overlapping taxonomically is simply to assume the truth of TI.

This argument is unsound because reference change does not imply TI (Mizrahi 2015a, 364-365) and reference discontinuity does not imply TI (Mizrahi 2015a, 365-366). Since neither reference change nor reference discontinuity implies TI, premise (1) of Marcum’s argument, which states that reference change implies TI, is false.

It is interesting to note that, although they both seek to defend TI, Patton and Marcum disagree about the Anastomoses case. Patton (2015, 56) thinks that “the case of anastomoses is not a paradigm shift,” whereas Marcum (2015, 56) thinks that it marks “a shift from the Galenic paradigm to what would eventually be Harveian paradigm of blood circulation.” To support this claim, Marcum points out the following differences between Galen’s theory and Harvey’s theory:

Harvey posits a single system that consists of the heart, arteries, and veins—in contrast to Galen’s two systems (liver/veins and heart/arteries)—and in so doing he combines the two functions of blood—nutrition (venous blood) and animation (arterial blood) of the body—of the two systems into one system. Moreover, he shifts the “pores” from the interventricular septum between the right and left ventricles to the body’s tissues, and he eliminates the vein-artery anastomoses in the tissues and the associated recessive flow of blood, to justify a circular, unidirectional flow of blood (Marcum 2015, 56).

Even if Marcum is right about all of this, it still does not follow that “Harvey’s work on blood circulation is not normal but revolutionary science [...] in that terms and ideas between the two competing paradigms are non-overlapping and thereby taxonomically incommensurable” (Marcum 2015, 56). This is because the aforementioned changes from Galen’s to Harvey’s theory are not necessarily *reference changes*. And even if they were, reference change does not necessarily imply TI (Mizrahi 2015a, 374). To see this, take for example the terms ‘arteries’ and ‘veins’. Let us grant that Harvey “*redefines* arteries and veins in contrast to Galen,” as Marcum (2015, 65) claims. From the fact that Harvey *redefines* these terms, however, it doesn’t necessarily follow that he is talking about something other than what Galen was talking about. To think otherwise is to assume that reference is fixed by descriptions. But that is an assumption that few philosophers would accept (Reimer and Michaelson 2014).

Similarly, as I am careful to point out in my (2015a), from the fact that Galen and Harvey used the terms ‘arteries’ and ‘veins’, it doesn’t necessarily follow that they were talking about the same thing. For just as the sense/reference distinction undercuts the supposed entailment from semantic change to TI (Scheffler 1982, 59-60), the semantic/speaker’s reference distinction undercuts the supposed entailment from reference change to TI (Mizrahi 2015a, 374).

Indeed, when he writes about the case of Thrombin, which is supposed to illustrate the point that “the discovery experience or process [can serve as] support for TI” (Marcum 2015, 59), Marcum admits that “thrombin refers to a clotting factor in both competing theories but how it functions differs drastically” (Marcum 2015, 59, footnote 8). But if “thrombin refers to a clotting factor in both competing theories,” how do we know that “the two clotting theories [are taxonomically] incommensurable” (Marcum 2015, 59)? Marcum seems to be inferring that the two competing theories are taxonomically incommensurable from the fact that they offer different *descriptions* for the mechanism of clot formation. But again, few would accept the claim that reference is fixed by descriptions (Reimer and Michaelson 2014).

Marcum (2015, 61) may be right that “judging whether a historical case study is a ‘rebutting defeater’ of TI depends on the reconstruction and interpretation of the case study.” Perhaps some might interpret the Anastomoses case as evidence for TI (see, e.g., Quin 1997 and Marcum 2015), whereas others might interpret it as evidence against TI (see, e.g., Mowry 1985 and Mizrahi 2015a). If a particular case history can be interpreted as evidence for TI or as a defeater against TI, then that case history is indeterminate and we need independent evidence for TI. I take that to be further support for the claim that the Anastomoses case was meant to illustrate in the original paper, namely, “that one can reach different conclusions about the alleged TI of competing theories depending on the examples one (cherry-) picks” (Mizrahi 2015a, 368). Of course, this means that there is no strong inductive support (in the form of inductive generalizations from case histories) for TI.

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