DISCUSSION AND REVIEWS

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Does Pearce's challenge refute Stegmüller's thesis? — a discussion note*

Let me consider one problem which has both historical and methodological import. This problem concerns the *logical or conceptual aspect* of Kuhnian incommensurability, i.e. the problem of translating the language or the conceptual framework of one scientific theory into the language of another, rival theory of framework.

David Pearce has on several occasions challenged the validity of a thesis which I shall here call *Stegmüller's thesis* (see for example Pearce, 1987). The following passage (Stegmüller, 1975: 96) expresses the content of Stegmüller's thesis:

> A 'proper scientific revolution' exhibiting scientific progress consists in a theory T_1 being supplanted by a theory T_2 whereby (1) T_1 and T_2 are incommensurable and (2) T_1 is reducible to T_2 but not vice versa.

Pearce (1987: 19) believes that it can be shown that no pair of theories T_1 and T_2 exists such that clauses (1) and (2) of Stegmüller's thesis are jointly satisfied. The reason is that if a theory T_1 is reducible in the structuralist sense to a theory T_2 then, under plausible assumptions, it can be shown that the language of T_1 is translatable into the language of T_2. And this, Pearce claims, ought to be quite sufficient to ensure their commensurability.

Somewhat simplified, Pearce's argument can be restated as follows (for the original formulations and points, see Pearce, 1987: 15—40):

(1) If theory T_1 is reducible in the structuralist sense to theory T_2 then theory T_1 is translatable into theory T_2.
(2) If theory T_1 is translatable into theory T_2 then theories T_1 and T_2 are commensurable.
(C) If theory T_1 is reducible in the structuralist sense to theory T_2 then theories T_1 and T_2 are commensurable.

I claim that Pearce's argument contains a false premise. Consider premise (2) in the form

(2') If theories T_1 and T_2 are incommensurable then theory T_1 is not translatable into theory T_2.

In fact Pearce uses premise (2') throughout his book, although in the more general form "if two theories are incommensurable then they are not translatable". But this premise is simply false if the concepts of commensurability and incommensurability are used in the *Kuhnian sense.* In his sense, translatability does not imply commersurability. Hence, intranslatability *is not a necessary condition* for incommensurability contrary to premise (2) or (2'). Note that at least since the second enlarged edition of "the Structure of Scientific Revolutions" Kuhn has not denied the possibility of translating one theory into another. Rather he argues that translatability is not sufficient for commensurability. Recently Kuhn (1983) has differentiated explicitly *local incommensurability, translation and interpretation* from one another. Here Kuhn argues that interpretation does not imply translation; translation and interpretation are very different processes. I won't consider here the differences of Kuhn's views of interpretation and translation. Instead I shall consider in the following the relationship between translation and (in)commensurability.

Note first that Kuhn (1970a: 202—204) discusses explicitly the possibility of translating one theory into antoher. But there is only a rather short fragment which is directly relevant for the problem of translation and incommensurability. It is the following (Kuhn, 1970a: 203—204): ". . . As translation proceeds, furthermore, some members of each community may also begin vicariously to understand how a statement previously opaque could seem an explanation to members of the opposing group. The availability of techniques like these does not, of course, guarantee persuasion. For most people translation is a threatening process, and it is entirely foreign to normal science. *Counter-arguments are, in any case, always available, and no rules prescribe how the balance must be struck.* Nevertheless, as argument piles on argument and as challenge after challenge is successfully met, only blind stubbornness can at the end account for continued resistance" . . . (italics added).

I interpret this passage as indicating the primary source of incommensurability in the context of translation. The source of incommensurability *is not* intranslatability, buth *the vagueness* and *ambiguity* of translation. In order to show that my interpretation is not arbitrary, consider next the following text fragments from Kuhn in which he considers in particular the problem of translatability and incommensurability. Kuhn (1970: 267—268) writes: ". . . Our choice of the term 'incommensurable' has bothered a number of readers. Though it does not mean 'incomparable' in the field from which it was borrowed, critics have regularly insisted that we cannot mean it literally since men who hold different theories do communicate and sometimes change each other's views. . . . . . Though one must know two languages in order to translate at all, and though translation can then always be managed up to a point, it can present grave difficulties to even the most adept bilingual. *He must find the best available compromises between incompatible objectives.* Nuances must be preserved but not at the price of sentences so long that communication breaks down. Literalness is desirable but not if it demands introducing too many foreign words which must be separately discussed in a glossary or appendix. People deeply committed both to accuracy and to felicity of expression find translation painful, and some cannot do it at all. . . . . . *Translation, in short, always involves compromises which alter communication. The translator must decide what alterations are acceptable.* . . . . . One need not go that far to recognize that *reference to translation only isolated but does not resolve the problems which have led Feyerabend and me to talk of incommensurability. . .*" (italics added).

Kuhn (1976: 191), discussing the adequacy of structuralism for reconstruing hid ideas and views of philosophy of science, repreats the problems of vagueness and ambiguity in translation. Consider the following passage: ". . . Translation always and necessarily involves imperfection and compromise; the best compromise for one purpose my not be the best for another; the able translator, moving through a single text, does not proceed fully systematically, but must repeatedly shift his choice of word and phrase, depending on which aspect of the original it seems most important to preserve. *The translation of one theory into the language of another depends,* I believe, *upon compromises of the same sort, whence incommensurability.* Comparing theories, however, demands only the identification of reference, a problem made more difficult, but not in principle impossible, by the *intrinsic imperfections of translations...*" (italics added).

Finally, consider Kuhn's (1983: 670—671) views of translation and incommensurability. Kuhn writes: "... Remember briefly where the term 'incommensurability' came from. The hypotenuse of an isosceles right tirangle 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. Applied to the conceptual vocabulary deployed in and around a scientific theory, the term 'incommensurability' functions metaphorically. The phrase 'no common measure' becomes 'no common language'. The claim that two theories are incommensurable is then the claim that there is no language, neutral or otherwise, into which both theories, conceived as sets of sentences, can be translated *without residue or loss...*" (italics added).

The collection of excerpts above clearly shows that insofar as the terms 'commensurability' and 'incommensurability' are used in Kuhn's sense it is false to propose that "translation is sufficient for commensurability" or equivalently that "intranslatability is necessary for incommensurability". There clearly may exist translatable, but incommensurable theories in Kuhn's sense. In particular, *at least two different sources* of incommensurability in the context of translation can be differentiated; they are the *non-uniqueness* (the vagueness and ambiguity) of translation and *the partiality* of translation (translations with some residue or loss).

Consider now translations in Pearce's sense. Roughly speaking a translation consists of a truth-preserving mapping of the sentences of one theory into the sentence of the other theory such that definite conditions on the models of the related sentences are satisfied. For details of a translation, consult Pearce (1987). To refute Pearce's challenge it is sufficient to show that his translations are ambiguous and vague. I think that there are three (perhaps more) *logical* senses in which Pearce's translations may be ambiguous and vague: first, a translation may be *partial* in the sense that it *does not offer a translation between the concepts of theories.* Second, a translation may be partial, in that it *does not cover all expressions of the translated language or theory.* Third, a translation may be ambiguous and vague if *it cannot be proved that the translation is unique,* i.e. if there are two or more different translations.

Let me first refer to Peter Schröder-Heister's and Frank Schäfer's (1989) important logical considerations. The authors show first that reductions of theories in the structuralist sense give rise to so-called "representations" of theories in the statement sense and vice versa where representations are understood as functions that map *sentences* of one theory into another theory. This result is completely parallel with Pearce's (1987) results. Second, the authors argue that commensurability between theories should be based on functions on *open* formulas and *open* terms for capturing more adequately the Kuhnian idea of (in)commensurability at the *conceptual* level. But, a function mapping *the sentences* of one theory into another theory is *not necessarily* extendable to a function which maps *open* formulas and *open* terms of one theory into another theory. So, the structuralist reduction does not necessarily imply commensurability contrary to Pearce's claim. In other words, partial translations do not guarantee commensurability.

As an example of partial translations in the second sense, consider one of Pearce's (1987: 188—199) examples of commensurability, Gaifman's theory of conceptual framworks. Here Newton's (absolute) space-time ontology is represented by means of several different but equivalent frameworks. For each of them there is a precisely defined counterpart framework representing Leibniz's (relational) space-time ontology. In each case, there is a translation from the Leibnizian framework into the Newtonian one. This translation can be chosen as an identity mapping on *some* sentences. The translation is therefore partial, but literal.

However, because no full translation is

available in the sense that all sentences of the Leibnizian framework are translatable into the Newtonian one, the framworks are clearly incommensurable in the Kuhnian sense. Thus, Gaifman's theory of conceptual frameworks is not an example of commensurability in the Kuhnian sense.

To show that Pearce's translation may generate incommensurability in the third sense of ambiguity and vagueness, i.e. there is no guarantee that the translation is unique, it is sufficient to quote Pearce himself. Pearce (1987: 34) writes: ". . . In general, ρ [the structuralist reduction relation] cannot be taken to determine the translation Γ *uniquely*, nor is there any guarantee that this syntactic correlation is an effective or recursively definable mapping. . .". Thus, some contextual and pragmatic restrictions are needed to narrow down the class of translations. But such contextual and pragmatic restrictions clearly lead to incommensurability problems of the types described in the Kuhn-excerpts above.

In his reply to Schröder-Heister and Schäfer, Pearce (1989) presents four points:

(i) Schröder-Heister and Schäfer have essentially weakened the concept of reduction;
(ii) Having acknowledged projective definability as a plausible condition for associating representations with reductions, they should similarly admit that there might be equally plausible conditions under which stronger forms of translation, including their commensurability functions, are also correlated with reductions;
(iii) Schröder-Heister's and Schäfer's distinction between sentence-to-sentence and formula-to-formula translations does not seem to distinguish between commensurability and incommensurability;
(iv) Schröder-Heister's and Schäfer's advocated "theory of meaning", i.e., that theorems are the prime indicators of meaning, is untenable for empirical theories.

In what follows I shall comment on these points in the reversed order. One of Pearce's idiosyncrasies seems to be that he ignores the relevant discussion context. Schröder-Heister and Schäfer (1989) point out several times that their discussion is intended to capture and explicate definite *Kuhnian* problems. However, Pearce (1989), arguing against Schröder-Heister's and Schäfer's commensurability concept and their "theory of meaning", *nowhere* in his reply relativizes his arguments to the Kuhnian discussion context. Consequently, Pearce's claims (iii) and (iv) above are to a large extent beside the point.

Points (i) and (ii) are to some extent justified. However, liberalizing the concept of reduction *is not* ad hoc from the point of view of Schröder-Heister and Schäfer, as Pearce (1989: 159) leads his readers to think. Instead of the original structuralist condition

$$\forall x' \in D_1(R)\ \forall y,z \in Mp\colon x'Ry \wedge x'Ry \Rightarrow y = z$$

they use condition

$$\forall x' \in D_1(R)\ \forall y,z \in Mp\colon x'Ry \wedge x'Ry \Rightarrow y \equiv z.$$

Thus, reduction relation R is only unique on the right *modulo* elementary equivalence, '≡', intead of being unique on the right (see, Schröder-Heister and Schäfer, 1989: 143; for the structuralist reduction relations, consult for example Balzer, Moulines and Sneed, 1987 or Stegmüller, 1986). Note, however, that Schröder-Heister and Schäfer assume throughout their paper the framework of first-order logic (see Schröder-Heister and Schäfer, 1989: 150). In first-order logic it holds that all isomorphic models are elementarily equivalent, and the converse holds for finite models. Although elementarily equivalent models are not in general isomorphic, their differences cannot be expressed in the language for which they are models. Thus Schröder-Heister's and Schäfer's liberalization is justified from their point of view, but of course not from the point of view of structuralism, nor from the point of view of Pearce.

However, even if Pearce's points (i) and (ii) are accepted they do not suffice to establish his premise (2). The main reason is that there is no apriori guarantee that translations correlated with structuralist reductions are adequate in Kuhn's sense. Note first that Pearce has nowhere showed literally that structuralist reduction implies translatability. In fact his simplified concept of structuralist reduction (see Pearce 1987: 23) does not satisfy even the four minimal adequacy conditions of structuralist reduction relation (for these conditions, see Stegmüller, 1986: 129—130). Pearce (1987) pays no attention to the important adequacy condition which demands that the intended applications of theories should be adequately correlated by a reduction relation. Neither does he consider the additional conditions imposed on the related partial possible models and constraints. The same is true in the case of Schröder-Heister and Schäfer (1989). In the following I try to show that neglecting these additional conditions of reduction might have important consequences for translatability and commensurability.

Consider an other one of Pearce's examples of commensurability, the translation of classical mechanics *(CM)* into special relativity theory *(RM)* (see Pearce 1987: 184—190 and 199—206). The basic idea stems from Veikko Rantala (1979). Rantala used nonstandard analysis, or the theory of infinitesimals, to explicate in a logically precise manner how certain models of *RM* are 'close' to counterpart models of *CM*, in fact infinitesimally close. (For additional references to technical details, see Pearce, 1987.)

As is well known Newton's second law is obtainable from the Minkowski force law in the limit when $c \to \infty$ (the speed of light converges infinity), or when $ds/c \to 0$ (particles under consideration are stationary). However, the latter case with zero particle velocities is trivial and physically uninteresting whilst the former is counterfactual or inconsistent with the empirical claim that *c* is a fixed finite number.

Now the crucial step is to replace the subjunctive, counterfactual conditional $c \to \infty$ by condition $ds/c \approx 0$ which reads as "*ds/c* is infinitesimally close to 0". This is the essential condition in giving a translation of *CM* into *RM*. Pearce then goes on to construe a semantic correlation between the models of *CM* and *RM* and, a translation of *CM* into *RM*.

Consider now the condition $ds/c \approx 0$. Pearce (1987: 201—202) claims that this condition is an *indicative, truth-functional* condition. However, it seems that it is not. It is true if *ds* is infinitesimally close to zero (which is the physically trivial and uninteresting case) or *c* is infinitesimally close to infinity. But the latter condition is clearly counterfactual in the light of *RM*. I think that these observations are sufficient to show that Pearce's translation of *CM* into *RM* fails to make the theories commensurable in the Kuhnian sense. However, to get more evidence for my claim let us go on and consider the semantic correlation between models of *CM* and *RM* and the related translation which Pearce construes.

Pearce (1987: 203) construes an *F*, an operation or construction which yields for each model *m* in its domain K' (the set or class of nonstandard models of *RM)* a unique model *F(m)*. *F(m)* is the standard approximation of *m*. Moreover, *F(m)* belongs to the set or class of models of *CM*. According to Pearce (1987: 203), the range of *F* consists of all 'standard' models of *CM*, but Pearce fails to tell that the domain of *F* consists of *nonstandard* models of *RM*. But this fact has important consequences for the associated translation *I*.

Translation *I* is a recursive and nonliteral translation of all $L(\tau)$-formulas into $L(\tau')$-formulas where τ and τ' stand for types of vocabularies of *CM* and *RM*, respectively, and *L* is a sufficiently strong logic such that the classes of models of *CM* and *RM* are axiomatizable in $L(\tau)$ and $L(\tau')$ by some sentences of $L(\tau)$ and $L(\tau')$. According to Pearce (1987: 204), translation *I* is adequate in the sense that for all $m \in K'$ and all $\varphi \in \mathrm{Sent}_L(\tau)$ condition

$$m \vdash_L I(\varphi) \text{ iff } F(m) \vdash_L \varphi$$

holds. Reading this condition from right to left says roughly the following: any sentence φ which is true classically and thus holds in particular in all standard models of *CM* has a translation $I(\varphi)$ which holds in all *nonstandard* models of *RM*. Moreover, *F*(m) is the *standard approximation* of *m*.

Consider now translation I from the point of view of commensurability. Note first that the translation of φ, $I(\varphi)$ holds in all *nonstandard models* of *RM*. But *no* nonstandard model of *RM* belongs to the class of *intended models* of *RM*. Thus, in this respect translation clearly fails to yield commensurability in the Kuhnian sense. Second, there is a drawback in the semantic correlation between the models of *CM* and *RM*. Operation or construction F yields the *standard approximation* between the models of *CM* and the nonstandard models of *RM*. This fact is clearly another symptom of incommensurability in the Kuhnian sense. To summarize the above points we then have the result that Pearce's translation of *CM* into *RM* does not make these theories commensurable in the Kuhnian sense.

Consider next Pearce's translation of *CM* into *RM* from the point of view of structuralism. Let s denote the spatial position of particles, let m stand for their mass and f for force, and let c be an individual constant denoting the speed of light. Moreover, let A be a purely mathematical, standard or nonstandard model of analysis and let P and T be domains corresponding to sets of particles and time points related to functions s, m and f. Let $Mp = \{< A;P,T,s,m,f >\}$ and $M'p = \{< A;P,T,s,m,f,c >\}$ be the classes of possible models and $M \subseteq Mp$, $M' \subseteq M'p$ be the classes of models of *CM* and *RM*, respectively. Extend function F to function F^* relative to Mp and $M'p$ as follows: $D_I(F^*) \subseteq M'p$, $D_{II}(F^*) = Mp$ and for any $m = < A;P,T,s,m,f > \in Mp$, for any $m' = < A;P,T,s,m,f,c > \in M'p$: if $m \in D_{II}(F)$ and $m' \in D_I(F)$ then $F^*(m') = F(m')$; for the case $m \in Mp - D_{II}(F)$, $m' \in M'p - M'$ we define $F^*(< A;P,T,s,m,f,c >) = < A;P,T,s,m,f >$.

Consider now Pearce's simplified version of the structuralist reduction relation which is a partial function $\rho: M'p \rightarrow Mp$ with the following property: for all $m' \in D_I(\rho)$: if $m' \in M'$ then $\rho(m') \in M$ (see, Pearce 1987: 23). Recalling that $D_{II}(F)$ consists of the 'standard' models of *CM*, and $D_I(F)$ of the nonstandard models of *RM*, we immediately obtain the result that F^* is a *reduction* in the simplified Pearcean sense. Now if the above critical results concerning the adequacy of Pearce's translation of *CM* into *RM* are acceptable we get a counterexample to Pearce's premise (2).

On the other hand, if for example the structuralist condition imposed on the intended applications is taken into account it follows that Pearce's semantic correlation F cannot be extended into a structuralist reduction relation. This is seen as follows. F correlates nonstandard models of *RM* with the models of *CM*. It is quite implausible that nonstandard models of *RM* or any appropriate reducts of them might count as intended applications of *RM*.

Finally, let me point out that Pearce's translation of *CM* into *RM* is in fact defined on atomic formulas and open terms of *CM* and *RM*, and it is then extended to the sentences of *CM* and *RM* (see Pearce, 1987: 203—204), yielding a recursive and non-literal translation of all *CM*-formulas with quantifiers relativised at the appropriate places. Thus, Pearce's translation clearly is is more akin to a formula-to-formula translation than a sentence-to-sentence translation. Consequently his critical point (ii) above seems to loose its force as a counter argument.

I conclude that premise (2) or (2') of Pearce's challenge is not true. Perhaps one might argue that Kuhn's (in)commensurability concept is not acceptable, because it is too strong. I do not want to defend Kuhn's concept of (in)commensurability, but I do think that such an argument is beside the point. Stegmüller's (1975, 1976, 1979, 1980) primary theoretical target is to reconstruct some *Kuhnian* views of the philosophy of science and 'defend' Kuhn from his critics. Pearce expresses this fact explicitly as he discusses the theoretical status of Stegmüller's thesis (see, for example, Pearce 1987: 16 and 19). But note that Stegmüller (1975, 1976, 1979, 1980) *nowhere* characterizes the Kuhnian (in)commensurability *relative to (in)translatability.* This fact is obviously due to his strong commitment to *the earlier radical nonstatement view of* theories, and this fact is recognized also by Pearce: 1987 15—40). Stegmüller's (1975, 1976, 1979, 1980) accounts of the Kuhnian (in)commensurability concept might of course be criticized as inadequate or even fallacious, but note that Stegmüller's interpretation of the Kuhnian (in)commensurability does not prevent us from evaluating Pearce's challenge independently of it. And then the remarks above clearly indicate that premise (2) of Pearce's challenge is false. It is simply false to claim that translatability implies commensurability.

Structuralists have later tried to characterize the concept of (in)commensurability in the context of translation (see, for example, Balzer, 1985 and Stegmüller, 1986: 298—310). The main reason for this move has obviously been Pearce's effective critical comments. Let me just say that I do not view for example Balzer's (1985) explicate of incommensurability as being very successful. (See Pearce's, 1987: 41—69 critical notes.) But note that Stegmüller's thesis retains its validity or invalidity independently of these later considerations, because Pearce's challenge is simply invalid.

*) Mr. Mark Shackleton and Dr. Matti Sintonen have kindly revised my English.

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Terttu Luukkonen: Citations in the rethorical, reward, and communication systems of science, Acta Universitatis Tamperensis, Ser A vol 285, University of Tampere, Tampere 1990

Bibliometry has for some time been criticised for lacking a theoretical base. Although there are some strong statistical regularities to be found in bibliometric data, the laws of Bradford, Lotka, Zipf, Price, etc., there is still a need for understanding the processes that bring about such invariances. In almost any set of documents the distributions of publications and citations are extremely skewed, suggesting that a few percent of the scientific community is highly productive and influential. The average scientist only publishes one or two papers per decade, recieving very few or no citations. It seems reasonable that science is stratified in this manner — we cannot expect but a few scientists to produce significant work.

The problem that Terttu Luukkonen adresses in her thesis is, to what extent the distribution of citations is an adequate reflection of the stratification system. In an exellent review of the ongoing debate on the validity of citation counts she discusses how citations are effected by three subsystems of science; the reward, the rethorical and the communication system. For example, the citation rate of a scientific paper is not only effected by its intrinsic properties, it is also influenced by the status of its authors, its use in the rethorical context of the citing papers, and the visibility of the journal in which it is published. In a number of case studies of Nordic cancer and cardiovascular research she shows, in a most skillful manner, how journal attributes such as language of publication, degree of specialization and regional location, effect the citation scores of articles. Of course, this means that citation counts have to be used with great care when evaluating science, and Luukkonen ends her thesis with an interesting discussion of the pros and cons of using citations for judging the quality of scientific work.

It is obvious that a comparison of citation scores should not be the end point of an evaluation process. There is always a need for explanation, and the evaluation must explain in order to be useful. When differences in citation counts can be attributed to the functioning of the communication system, an alteration of the journal selection behavior might be enough to reduce a large part the citation gaps. But, the fact that citation scores need explanation does not make them less relevant. From the point of view of the individual scientist, lack of recognition is a frustrating fact, especially when it could be explained by his own communication behavior.

Luukkonen's study clearly indicates that the journal market of science is dysfunctional in several respects. It would have been interesting if she had elaborated the question at how the journal system might be improved. What would a publication system look like if it were to remove the barriers of language and regional location of researchers? Should English be declared as the official language of science? Can full text databases, distributed online and on CD-ROMs, replace the journal market of today? What would a referee system look like in the era of electronic publishing?

Citation counting could be looked upon as the first generation of bibliometric analysis. During the last few years there has been a shift of interest among bibliometricians away from the straight counts towards various methods of mapping the subfield structure of scientific research. The position of a scientist's publications within the cognitive and collaborative networks of his own research speciality is another important indicator to pay attention to when evaluating research. Science is both vertically and horizontally structured, and we need information on both dimensions to be able to say anything of interest about any piece of work. The data sets that are used in citation studies are built upon the relations between citing and cited documents. Counting citations is to make use of only one of the marginal frequencies of this matrix, leaving all other information unprocessed. Therefore, much is yet to come in the bibliometric analysis of research.

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