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Essay review

# On correspondence

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S. French and H. Kamminga (Eds.), Correspondence, Invariance and Heuristics. Essays in Honour of Heinz Post. Kluwer Academic Publishers, Dordrecht, 1993, xiii + 356 pp; US \$251.00, UK £163.25, hardback, ISBN 0-7923-2085-9.

Fifteen essays are contained in this collection, all relating to Heinz Post's article 'Correspondence, Invariance and Heuristics' (Post, 1971), also reprinted. In this article, written in the heyday of the post-positivist movement, Post aims to convince his fellow philosophers of science to bring the issue of heuristics back to the philosophical stage. Examining a wealth of theories and models from the physics and chemistry of the last 300 years, Post extracts several strategies of theory construction of which he considers the *General Correspondence Principle* to be the most important. According to this principle, any acceptable new theory should explain the well-confirmed part of its predecessor. Later Post states the General Correspondence Principle more precisely and uses it with what he considers its de facto validity to argue against incommensurability, Kuhn-losses,[1] and relativism. Post himself seems to support (but does not explicitly advocate) a kind of convergent realism which is most notably expressed in his credo that science progresses linearly.

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[1] In his authoritative and comprehensive exposition of Kuhn's philosophy of science, Hoyningen-Huene (1993) gives the following explication of Kuhn-losses:

> Kuhn often emphasizes the fact that along with revolution — and the associated gain in problem-solving capacity — generally come certain losses. Among these are losses in the ability to explain certain phenomena whose authenticity continues to be recognized, losses of scientific problems of the narrowing of the field of research, and, relatedly, increased specialization and increased difficulty in communicating with outsiders. And so, for Kuhn, the progress which comes with a revolution appears to have been bought at the price of a certain recession, albeit one quickly forgotten along with the articles and textbooks in which the conquered theory, in its historical form, is contained. (p. 260; two footnotes with references to Kuhn have been deleted.)


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The commenting essays, written by former Ph.D. students and a host of distinguished visitors of Post's Department at Chelsea College (a former campus of the University of London whose unfortunate history during the Thatcher Government is told in the editors' introduction), take up Post's ideas, test them against the historical record, or develop them further. The essays fall roughly into two groups. The first contains detailed case studies and focuses especially on the status and the role of the General Correspondence Principle. This group includes papers which carefully investigate the historical or contemporary-science record (Brown, Chalmers, Cushing, Franklin, Kamminga, Redhead and Scerri) and some which analyse episodes from science and its history by relying on normative background assumptions or other scientific theories (Crawford and Hon). The second group contains more general philosophical reflections (da Costa and French, Koertge and Saunders) as well as foundational investigations of topics like quantum mechanics and the special theory of relativity (Kilmister and Tonkinson and Fine). I cannot do justice to all of these articles, but will concentrate on those which are related to the issue which Post himself considered to be the most important of his insightful article: the General Correspondence Principle (GCP). This will be introduced in the next section.

The papers are well written and original. Furthermore, they are closely related to the practice of science, living up to the programmatic opening statement of Post's essay: 'Philosophers of science should be concerned with Science; that is, with the activity of scientists, whether the concern be descriptive, prescriptive, or both' (p. 3).[2] It is worth mentioning that the book does not exclusively examine episodes from physics and its history, but also contains quite detailed case studies from special sciences such as biology (paper by Kamminga) and chemistry (paper by Scerri). The editors and authors of this *Festschrift* made a considerable effort to make it a coherent whole. All contributions refer to Post's original article, although this sometimes seems a little forced. The case studies will especially help readers interested in the topic of theory change and the relation between successive theories to back up their views by appealing to the scientific record. These case studies are the strong part of the book; they can be used as a point of departure for the discussion of a variety of issues in the philosophy of science. However, a discussion is missing of the question of which new or modified view of the relation between theories could emerge from the collaborative work of all authors. In Section 2, I shall develop and evaluate such a view on the basis of the articles of the current volume. I will then go on and investigate in Section 3 how one can make sense philosophically of this new picture.

## 1. Post's general correspondence principle

The model for Post's GCP is the quantum-mechanical correspondence principle. This principle played a crucial role for Bohr and others in the process of constructing

[2] Page citations always refer to the present volume. When quoting Post's reprinted contribution, the page number of the original publication can be obtained by adding 212.

the new quantum mechanics in the 1920s. It was expected that quantum mechanics would account, within certain limits, for the well-confirmed phenomena of classical physics. The quantum-mechanical correspondence principle is, however, somewhat more complicated, as Radder (1991) has shown. The latter consists of various interrelated parts which I will, however, not discuss here. In a first attempt, Post gives the following characterization of 'his' GCP:

> Roughly speaking, this is the requirement that any acceptable new theory $L$ should account for its predecessor $S$ by 'degenerating' into that theory under those conditions under which $S$ has been well-confirmed by tests. (p. 16)

The GCP is claimed to be valid even across scientific revolutions. It presupposes that $S$ and $L$ 'refer (in their statements) to at least some events or facts which are identifiably the same' (p. 8) or, to use a contemporary phrasing, that $S$ and $L$ share a common set of phenomena. The domain of $L$ is assumed to be larger than the domain of $S$ and the account given by $L$ will usually be more precise (or at least not less precise) than the account of the phenomena given by $S$. A typical example is the relation between classical mechanics and the special theory of relativity; the latter theory also correctly describes particles that have very high velocities and provides a more accurate account at low velocities than the former.

Post goes on to discuss several possible relations between $S$ and $L$ that range from a complete reduction (which seems hardly ever to occur in science) to approximate or inconsistent correspondence, but without explanatory losses (such as the relation just mentioned between classical mechanics and the special theory of relativity). Other possible relations between $S$ and $L$ that exhibit losses would count as evidence against the GCP; Post holds that these relations have never occurred in the history of science in the last 300 years — apart from one exception which will be discussed below.

One of Post's favourite examples to support the GCP is the periodic system which survived the quantum-mechanical revolution. Post explains:

> The periodic system is the basis of inorganic chemistry. This pattern was not changed when the whole of chemistry was reduced to physics, nor do scientists ever expect to see an explanation in the realm of chemistry which destroys this pattern. The chemical atom is no longer strictly an atom, yet whatever revolutions may occur in fundamental physics, the ordering of chemical atoms will remain. (p. 25)

Post generalizes this example and maintains that the low-level structure of theories is particularly stable, while higher and less-confirmed levels are subject to change in the process of scientific theorizing. The pattern of the atoms remains, although quantum mechanics replaced the former framework theory. Da Costa and French call this the 'Principle of the Absolute Nature of Pragmatic Truth': '[O]nce a theory has been shown to be pragmatically true in a certain domain, it remains pragmatically true, within that domain, for all time' (p. 146). This principle seems, at first sight, to be quite plausible; but is it correct? Doubts arise once one recalls that Post himself confesses that the successful part of $S$ may be smaller from the perspective of the new theory $L$ than from the perspective of $S$ (p. 20). Given this, it is not clear how there can be a 'resistant kernel' in the long run that 'remains pragmatically true ... for all time'.

Later Post refines his proposal to also account for theories $S$ and $L$ with a different vocabulary. These vocabularies have to be translated into each other and this translation $\mathcal{T}$ may turn out to be more difficult than a mere one-to-one mapping. Also, a condition $Q$ on $L$ has to be specified such that the truncated $L$ and $S$ have (perhaps only approximately) the same domain. If the well-confirmed part of $S$ is denoted by $S^*$ (the extent of which is only a conjecture at a given time[3]), the GCP can be conveniently expressed as $S^* = \mathcal{T}(L|Q)$ — i.e., the well-confirmed part of $S$ is identical to the suitably translated part of $L$ which fulfils the condition $Q$. If $L^*$ is the well-confirmed part of $L$ and $S^{**}$ is the intersection of $S^*$ and $L^*$, then the thesis of zero Kuhn-losses is that $S^*$ is identical to $S^{**}$. Post claims that the historical record supports this thesis.[4]

It should be noted, however, that Post's analysis does not take into account what Hoyningen-Huene (1993) aptly called the 'loser's perspective'. From this perspective, there are indeed successes of the old theory which the new theory cannot account for.[5] Besides, even from the 'winner's perspective' the thesis of zero Kuhn-losses may be too strong, as Saunders concedes in his contribution. Saunders writes that 'Laudan (1981) is right to insist that one can always find some theorem, deduction, conjecture, or explanation that has no precise correlation in the successor theory (what Post calls 'Kuhn-losses')' (p. 296). He then goes on, though, to distinguish between significant and insignificant Kuhn-losses; only the insignificant ones are, of course, 'allowed'. I will come back to this issue below. Radder (1991) has pointed out another problem for Post's approach. Not *all* equations of $L$ might 'degenerate' into equations of $S$. As an example, consider the famous formula $E = m_0c^2$ for the energy of a particle with rest mass $m_0$. This equation makes sense only in the special theory of relativity. In the well-known limit of low velocities ($\beta := v/c \rightarrow 0$) it remains unaltered; it does not, however, correspond to an equation of classical mechanics.

According to Post, the GCP is both a descriptive and a normative thesis. It is considered to be a post hoc elimination criterion, and theories that do not fulfil it should be, as Post boldly advises, consigned to the 'wastepaper basket' (p. 23). Examining cases from the history of science, Post only spotted one obvious 'counterexample' to the GCP. Ironically, it is the best theory we have today: Quantum Mechanics. This theory cannot be strictly reduced to classical mechanics (p. 21), and this is a crucial failure which Post blames on the supposed incompleteness of quantum mechanics (pp. 22, 34). Quantum mechanics therefore does not, for Post, count as a case against the GCP; instead, the fact that quantum mechanics does not fulfil the GCP shows that this theory should not be accepted or at least that it should not be considered to be the successor of classical mechanics. It

[3] Cf. Koertge (1973), 172f.

[4] For a comparison of Post's GCP with other correspondence principles, such as the ones suggested by Fadner, Krajewski, Radder, and Zahar, see Radder (1991).

[5] Cf. Hoyningen-Huene (1993), pp. 260–262, and the references to the work of Kuhn cited therein.

belongs, perhaps, in the wastepaper basket. Other proponents of a GCP, such as Radder, do not go as far and emphasize the correspondence relations which hold between quantum mechanics and classical mechanics — and so do Cushing, Fine, and Saunders in their contributions, though in different ways. Their arguments will be examined in the next section.

Before doing so, another issue needs to be mentioned. So far, the following three theses are in conflict: (1) Post's GCP is descriptively correct, (2) belief in the truth of quantum mechanics is justified,[6] and (3) quantum mechanics and classical mechanics share a common set of phenomena. Rather than rejecting thesis (1) or (2) one might doubt — following Cartwright's lead — thesis (3). Cartwright (1999) argues that we have good reasons to believe that there are two disjoint classes of phenomena; some can be modelled by using the toolbox of quantum mechanics, others by relying on classical mechanics. There is consequently no quantum-mechanical model of classical phenomena. Contrary to Cartwright, however, Post and (I believe), most physicists hold the view that quantum mechanics and classical mechanics do share a common set of phenomena. They assume that quantum mechanics accounts for the phenomena of classical mechanics *in principle*; it is merely a matter of computational complexity to demonstrate this. This might, as Cartwright supposes, be a metaphysical dream.

Consider the following case: theory $S$ accounts for a set of phenomena. Now, new phenomena occur, which are similar (in a certain sense) to the phenomena accounted for by $S$, but these new phenomena do not belong to the domain of $S$. What can be done? One option, which is in accordance with the spirit of the GCP, is to develop a theory $L$ that accounts for both, i.e., the phenomena already described by $S$ and the new and so far unexplained phenomena. The other option is to start from scratch and develop a theory for the new phenomena only. Here is an example from current physics. For almost 15 years, theoretical condensed matter physicists have aimed at understanding high-temperature superconductivity. This is an extraordinarily difficult task and no consensus has been reached so far even about the 'global' strategy of research. Some theorists suggest following the lead of the theory of conventional superconductors (the so-called BCS theory) as closely as possible. This would eventually enable a unified treatment of conventional and non-conventional superconductors. Others propose more revolutionary models that do not relate to the established BCS theory in a straightforward manner. To be more precise, these new theories do not 'degenerate' into the BCS theory in some limit.[7] The point I wish to make is that it is not always obvious to which theories the GCP can be applied. Should the revolutionary theories of high-temperature superconductivity be

[6] I here follow the useful distinction between acceptance and belief proposed by da Costa and French in their contribution:

> Acceptance differs from factual belief in that the former involves a voluntary act of commitment, whereas the latter does not. It is, however, tied to a representational belief in the partial truth of what is accepted and the commitment is to the use of the representation or model concerned. Both inconsistent and strictly false theories may be regarded as partially true and accepted in this sense. (p. 155)

[7] For details see Hartmann (1999b; 2001, in preparation).

abandoned? The task to decide on this issue is a matter of scientific research, and it is often not clear at the outset what the best strategy is.

It is instructive to discuss Franklin's contribution in this context. Franklin presents a good account of the history of alternative gravitational theories that explain the experimentally established violation of CP symmetry in certain quantum systems. The new suggested force, called the Fifth Force, turns out to be a modification of the Newtonian law of gravitation which is, in a specific limit, obtained by a 'degeneration' process from the Fifth Force. But why should disparate phenomena such as the attraction of the Sun and the Earth, and symmetry violations at the quantum level, be treated by one and the same theory? Why is this a case that supports the validity of the GCP? This appears, at first sight, highly mysterious, but the story Franklin tells makes the detailed arguments of scientists in favour of such a conjecture.

What is the outcome of the discussion so far? First of all, it is not clear when the GCP is applicable. This is demonstrated by my example of quantum mechanics and the case of high-temperature superconductivity. Secondly, when the GCP is applied, it often does not hold strictly, as Radder's example shows. Besides, there are losses from the loser's perspective and maybe also from the winner's. Thirdly, as a consequence of all this, there is a tension between the practice of actual science and a normative reading of the GCP. But still: Post rightly remarked that there is a lot of continuity in scientific theorizing, even across scientific revolutions. The relations between various theories in the history of science are, however, much more complicated than the GCP would lead us to believe. Perhaps there is no single and non-trivial principle that captures the rich structure and variety of developing scientific theories. This can only be established empirically. What is needed is a careful examination of a lot of episodes from contemporary science and the history of science on which a meta-induction can be based. As a first step, it is helpful to highlight various relations that hold between successive scientific theories. The contributions to the present volume motivate a list which will be presented in the next section.

## 2. A plurality of correspondence relations

The contributors to the present volume were invited to test Post's GCP against the historical and scientific record. At the end of the day, a considerable number of them boldly conclude that 'Post [...] was right, and Kuhn was wrong' (Saunders, p. 321). Things are, however, not as simple as I made it out in the previous section. In the development of scientific theories, continuities as well as discontinuities appear. Hence, the interesting question to be addressed is: which elements of $S$ and $L$ correspond to each other, and which elements do not? Are there general rules that guide practising scientists in those difficult decision situations? As a prolegomenon to such a task, it is reasonable to examine more closely how specific scientific theories are related to each other. Which elements are taken over, what are the motives for doing so, and how are the elements of the old theory made to fit the new theory?

Based on the case studies of the current volume, I will address these questions and provide a preliminary and presumably incomplete list of correspondence relations which *may* hold between successive theories. Some theories exhibit more than one of these relations, and some correspondences appear at different stages of the development of a theory.

A useful first distinction is between *ontological* and *epistemological correspondence relations*. An ontological correspondence relation holds between $S$ and $L$ if some or all of the entities of $S$ are also entities of $L$. As Saunders convincingly argues in his contribution, a host of intriguing problems (such as reference, etc.) emerge here; I will therefore follow Saunders' implicit advice and consider only epistemological correspondence relations,[8] of which the following types can be distinguished.

1. *Term Correspondence*. Here, certain terms from $S$ are taken over into $L$. This is a standard strategy in the development of scientific theories. In *The Structure of Scientific Revolutions*, Thomas S. Kuhn writes that '[s]ince new paradigms are born from old ones, they ordinarily incorporate much of the vocabulary and apparatus, both conceptual and manipulative, that the traditional paradigm had previously employed' (Kuhn, 1996, p. 149). Now it is well-known that Kuhn also argues in the very same book that this continuity goes along with meaning variance and problems of reference. A standard example is the meaning shift from 'mass' in classical mechanics to 'mass' in the special theory of relativity. A disclaimer or two is in order here. Term Correspondence does not imply that *all* terms of a theory correspond to terms in the successor theory. Often, only a few key terms are carried over, while others are left aside and new terms are coined in addition. Also, a correspondence relation between two theories can be established by a suitable translation of the respective terms, as Post has also pointed out. Term Correspondence is a rather minimal requirement; it is presupposed by all other corrrespondence relations to be discussed below.
2. *Numerical Correspondence*. Here $S$ and $L$ agree on the numerical values of some quantities (cf. Radder, 1991, pp. 203–204). Numerical Correspondence therefore presupposes Term Correspondence. An example discussed by da Costa and French and also by Scerri in their contributions is the spectrum of hydrogen in the Bohr model and in quantum mechanics. Although the assumptions that were made to calculate the spectrum differ considerably in both theories, they nevertheless lead to the same numerical values. Again, this is a rather weak kind of correspondence relation which is moreover usually realized only approximately (as in the example just discussed). Its heuristic value is low since the principle can only be applied post hoc. Obviously, Numerical Correspondence is only interesting in the mathematical sciences; in large parts of biology and archaeology, for example, the requirement of Numerical Correspondence does not apply.
3. *Observational Correspondence*. This kind of correspondence relation is introduced in Fine's contribution in the context of his interesting resolution of the quantum

[8] Saunders suggests omitting reference to Fregean sense (pp. 303, 307) and urges that we study only the relation between the mathematical structure of the theories in question.

mechanical measurement problem. Fine, like Einstein whom he quotes approvingly, does not accept Cushing's claim that Bohm's version of quantum mechanics should have been chosen according to Post's GCP (p. 262), because the Bohm theory 'did not enable one to retrieve the classical and well-confirmed account of a ball rebounding elastically between two walls' (p. 280). It therefore does not fulfil Post's (and Einstein's) correspondence principle. Bohm's theory does, however, fulfil a weaker form of a correspondence principle. Fine writes: '[W]here the classical account itself is well-confirmed, the Bohm theory 'degenerates' into the classical account of *what we are expected to observe* under well-defined conditions of observation' (p. 280). Unfortunately, the standard Copenhagen version of quantum mechanics does not fulfil the principle of Observational Correspondence, and Fine therefore presents his solution of the measurement problem in order to restore this. Abstracting from quantum mechanics, Observational Correspondence means that $L$ 'degenerates' into *what we are expected to observe* according to $S^*$ under well-defined conditions of observation. Observational Correspondence, like Numerical Correspondence, presupposes Term Correspondence, but differs from Numerical Correspondence which may also apply when the quantities in question cannot be observed. Besides, Observational Correspondence relations can also hold in sciences which do not represent their content numerically. Observational Correspondence emphasizes the role of the conditions of observation which are especially important in the context of quantum mechanics. A heuristic principle based on the demand of Observational Correspondence is again only a post hoc selection criterion. It is of no help in the actual process of constructing new theories. Observational Correspondence alone also does not suffice to provide an explanation for the success of the old theory. It is therefore weaker than Post's GCP.

4. *Initial or Boundary Condition Correspondence*. According to the syntactic view of scientific theories, which most authors of the present volume adopt (a notable exception is da Costa and French), a theory is a set of axioms (or laws) plus suitable initial or boundary conditions. Kamminga complains in her contribution that the philosophical focus (including Post's) is too much on the axioms (or laws), leaving initial and boundary conditions aside. This is unfortunate, since especially in the non-formal sciences, Kamminga claims, these conditions play an important role which is relevant to the issue of inter-theoretic relations. It turns out that there are theories which incorporate consequences of their predecessor as an initial or boundary condition. Kamminga, whose examinations of various consecutive theories of the origin of life are illuminating, sums up her methodological points as follows: '[I]n the attempt to integrate the original theory $T$ with another theory outside its domain, some consequence of the latter is incorporated into $T$ as an antecedent condition, which then places strong constraints on the selection of laws that have explanatory relevance in the modified theory $T'$' (p. 77). This procedure, therefore, provides a link between the two theories. This way of connecting two theories is, however, a very loose one. It has some heuristic value, as Kamminga herself claims, but it should be noted that

the assumptions taken over from the predecessor theory remain unexplained in the successor theory.

5. *Law Correspondence*. Laws from $S$ also appear in $L$. This kind of correspondence relation often holds only approximately. An example is the kinetic energy in classical mechanics and in the special theory of relativity. For low velocities, $T_{\mathrm{CM}} = 1/2\ mv^2$ and $T_{\mathrm{SRT}} = (m - m_0)\ c^2 = 1/2\ mv^2 \cdot (1 + 3/4\beta^2 + \mathcal{O}(\beta^4))$ are approximately the same. Therefore, the special theory of relativity reproduces and explains the successful part of classical mechanics. It is probably this kind of correspondence relation that Post had in mind when he suggested his GCP. Law Correspondence implies Numerical Correspondence and presupposes Term Correspondence, the difficulties of which (such as meaning variance, etc.) therefore occur again. Despite all this, it is required that the terms in question have the same operational meaning in $S$ and $L$ (cf. Fadner, 1985, p. 832). In many cases, Law Correspondence is only a post hoc selection criterion of theory choice. As Radder's above-mentioned example demonstrates, it may only hold for *some* of the laws of the theories in question.
6. *Model Correspondence*. This type of a correspondence relation comes in two variants. (1) A model which belongs to $S$ survives theory change and reoccurs in $L$. A typical example is the harmonic oscillator which is widely used in classical mechanics, but is also applied in quantum mechanics and in quantum field theory. It should be noted that models, such as the harmonic oscillator, are not only taken over by the theory which succeeds the original theory, but also by quite unrelated theories. This is best seen by pointing to all other theories of physics which employ the harmonic oscillator; in fact, it is difficult to find a theory which does not employ this model! Model Correspondence of this first kind has a considerable heuristic potential; it is, however, not guaranteed that the new theory explains the success of the old theory, because the model in question may be embedded in a completely new framework theory which also affects the overall correspondence relation between $S$ and $L$. (2) Post mentions another strategy of theory construction which takes models seriously: 'In this case we adopt a model already available which may initially have been offered as an arbitrary articulation of the formalism only. […] It is a case of borrowing a model of the $S$-theory which contained features not *essential* for the modelling of the $S$-theory ('neutral analogy'), and assigning physical significance to such extra features' (p. 29). An example is crystallographic models which were used already a century before physicists identified the units of the regular lattices with physical atoms. Sometimes, Post concludes, scientists built 'better than they knew' (p. 30). This example also shows that Model Correspondence of this second kind may indeed lead to an explanation of the success of the predecessor theory.[9] However, the criterion is highly fallible, as Post himself grants.
7. *Structure Correspondence*. Here, the structures of $S$ and $L$ correspond. But what is a structure, and what does it mean that two structures correspond? In his contribution, Saunders suggests using the term 'structure' only in its precise

[9] More on the relation between models and theories can be found in Hartmann (1999a).

mathematical meaning (groups, etc.). This unfortunately restricts the application of the correspondence principle to a specific part of physics which Saunders calls 'dynamics'. Saunders' own definition of 'dynamics' is somewhat unorthodox and not very precise: 'By 'dynamics' I mean to include statics and kinematics, as well as mechanics and field theory' (p. 295). Given this mathematical understanding of 'structure', it is obvious how to flesh out the idea of a correspondence relation between two structures; here mathematical concepts such as sub-groups and group contractions are applied. Indeed, many 'dynamical' theories can be linked to each other in this way, as Saunders shows in detail. A typical example is the relation between the inhomogeneous Lorentz group and the inhomogeneous Galilei group which 'correspond' in a precise mathematical sense. In examples like this, Structure Correspondence works best. Another interesting case, also discussed by Saunders, is the relation between the theories of Ptolemy and Copernicus. Saunders shows that '[a]n astronomy based only on epicycles […] corresponds to an expansion of the form $\sum_i c_i \exp(i\omega_i t)$ (with the earth chosen as origin)' (p. 299). The mathematical structure of both theories is (perhaps only approximately) the same. There is therefore no reason to worry — with Feyerabend, Kuhn, Laudan and the likes — about the abandonment of the Aristotelian world-view or a wholesale change of paradigm (p. 298).

Saunders' large-scale fight against relativism (for Saunders, 'relativism' is a collective name for social constructivism, historicist epistemology, linguistic holism, and anti-realism; cf. p. 295f) appears somewhat cheap; parts of theories where problems for the idea of correspondence show up are deemed 'insignificant' (such as the ontology of a theory,[10] but also laws, etc.), while only the mathematical structure of a theory remains, in some sense, stable. But even here things are not that easy. With respect to the role of gravity, Saunders concedes that he does 'not suggest that these things can be completely codified', but goes on to confess that this strategy 'is, and […] has always been, the essence of the enterprise of dynamics' (p. 306). Confessions like this are not enough to make one accept the editors' judgement that Saunders provides a 'vigorous defence of the cumulative, progressive view of the history of physics' (p. xxiii). Saunders showed, however, that mathematical structures of consecutive theories may and often do correspond in a strict mathematical sense.

It should be noted that it is also possible to talk of structures outside the realm of (what Saunders calls) dynamics. In their contribution, da Costa and French provide a flexible framework that allows the comparison of scientific theories with different structures. Their central idea is to add partial structures to the model-theoretical account of scientific theories which also include inconsistent theories the role of which in the dynamics of theories da Costa and French rightly emphasize (p. 142).

Structural Correspondence does not imply Numerical Correspondence. Often, the structure is 'too far away' from the empirical basis of a theory to guarantee

[10] Cf. Saunders' discussion of the ether, p. 299.

continuity at that level (especially in the cases Saunders has in mind). It is therefore not at all trivial to reproduce the empirical success of the precursor theory once one has decided to take over parts of the structure of the old theory. Despite this, Structure Correspondence has a very high heuristic value, especially in the kind of physics that Redhead discusses in his contribution, viz. the quest for an ultimate theory. Because of the huge gap between these theories and the world to which we have empirical access, abstract reasoning, such as symmetry considerations, is often the only tool which enables scientists to proceed.

Three conclusions can be drawn from the above analysis. First, successive theories can be related in many ways. Sometimes only Numerical Correspondence holds (approximately), at other times entire mathematical structures correspond. Hence I suggest that philosophical issues, such as meaning variance and incommensurability, should first be discussed 'locally', i.e. on the basis of concrete case studies that exemplify specific types of relations between scientific theories (p. 262). Second, there are continuities and discontinuities in scientific theorizing, although it is not clear a priori which elements of a theory will survive theory change, and which ones will have to go. An additional difficulty for correspondence theorists is the notorious problem of underdetermination which Cushing discusses in his contribution (p. 262). Maybe there is no unique best choice of which elements of successive theories should correspond and which elements should not correspond with each other. Third, the philosophical project of a methodology is best described by the picture of a *toolbox*. According to this view, methodologists extract — on the basis of a wealth of case studies — a set of methods and techniques which can tentatively be applied by practicing scientists in a particular situation. What is in the toolbox may, however, depend on time: methods, as well as scientific theories and goals may change over time (cf. Cushing, 1998, p. 368). Good scientists know, of course, already a lot of tricks and methods, and they also know how to use them flexibly and appropriately. This view of the status of methodology is a middle ground between two extreme positions. Zahar (1989, pp. 258ff) defends a rather strong form of a rational heuristics which leaves little room to chance and other influences, while Popper's (1972, Chapter 7) evolutionary picture supports the opposite view: there is no rational heuristics, it is the job of the scientists to make bold conjectures which then have to 'survive' empirical tests and rational criticism (cf. Radder, 1991, pp. 201f). My conclusion seems, after all, to be similar to Post's own view on the role of heuristics which he illustrates with an apt analogy: 'The study of the structure of existing houses may help us in constructing new houses' (p. 5).

## 3. Rationality, realism and coherence

How can one interpret philosophically the prevailing continuity in scientific theorizing? Even if there is no single principle, such as Post's GCP, which governs the dynamics of theory construction, the existence of various correspondence relations between successive scientific theories cannot be doubted. In 'The Social Construction of What?', Hacking (1999) claims that from now on,

> Future large-scale instability seems quite unlikely. We will witness radical developments at present unforseen. But what we have may persist, modified and built upon. The old idea that sciences are cumulative may reign once more. Between 1962 (when Kuhn published *Structure*) and the late 1980s, the problem for philosophers of science was to understand revolution.[11] Now the problem is to understand stability. (p. 85)

Stability, as Hacking uses the term, is closely related to da Costa and French's 'Principle of the Absolute Nature of Pragmatic Truth'. The Second Law and Maxwell's Equations, for example, 'are not going to go away' (Hacking, 1999). And neither will many of the theories and laws discussed in the volume under discussion go away. Hacking's stability thesis (which he owes to Weinberg) is, however, considerably weaker than Post's GCP. Stability in Hacking's sense does not imply progress in theory construction. Cartwright's dappled world, for example, is stable in Hacking's sense, but does not fit Post's account. As a result of my previous sections, it is clear that Post's GCP is too strong and does not hold empirically in a strict sense. A weaker, though testable and philosophically justifiable version of a universal correspondence principle, based on the typology and the toolbox view outlined above, is difficult to formulate. That is why, in the remainder of this review, I only address the question of how the existence of various correspondence relations between successive scientific theories can be interpreted. My discussion is partly inspired by the arguments for the stability thesis Hacking discusses. These arguments shall be addressed first.

To begin with, it should be noted that Hacking's main aim is somewhat different from mine here. He is concerned with a clarification of the positions put forward in the so-called science wars. In this controversy, two opposing views can be identified. Roughly speaking, there are realists (such as Weinberg) and constructionists (such as Pickering). The realists subscribe to the thesis that the progress of science can be explained by pointing to factors internal to science only, while constructionists emphasize the impact of factors *external* to science (i.e., social and cultural factors). I will now investigate whether the existence of correspondence relations between successive scientific theories can be justified or explained by means of each of these contrary positions. It turns out that both accounts meet serious difficulties. I will then suggest a middle ground between the accounts that makes use of the epistemological concept of coherence.

The *first* option, realism, comes in different variants. One of them is *convergent realism* which holds that successive theories of 'mature science' approximate the

[11] This assessment is also Post's opinion: 'From the point of view of present-day [i.e. 1971, S.H.] philosophy, the fact that there is continuous progress in science is a problem, while the fact that there are occasional revolutions is not' (p. 25).

truth (i.e., the ultimate or final theory) better and better.[12] This presupposes the existence of a measure of the distance of a given theory from the truth (or at least an ordering relation), which is a controversial topic despite all the worthwhile work on verisimilitude and truthlikeness, and conflicts with the many discontinuities that have emerged in the development of 'mature' scientific theories, as Laudan (1981) has convincingly demonstrated. But perhaps there is no ultimate theory, as Redhead speculates in his contribution. It is possible that the process of constructing ever better theories never ends, because 'there are infinitely many levels of structure that can be unpacked, like an infinitely descending sequence of Chinese boxes, or to use the more colloquial expression: it is boojums all the way down' (p. 331). Obviously, Laudan's critique is relevant here as well. A weaker variant of convergent realism — which seems to be able to handle the problems raised by Laudan and others — is *structural realism*. According to this position, defended implicitly by Saunders in his contribution, at least the high-level mathematical structures of scientific theories converge. Continuity on the level of ontology and perhaps even on the level of one or another law is, however, not required. This might appear to be the result of an immunization strategy, to use Hans Albert's apt term, because Saunders calls only those elements of theories significant of which we have good reasons to assume that they do in fact correspond. Be this as it may, my discussion in the previous section showed that there does not seem to be enough empirical evidence for structural realism. Besides, there are more plausible ways to explain the persistence of certain mathematical structures; I will come back to this below.[13]

The *second* option, constructionism, also comes in different variants. All of them emphasize the role of factors external to science. In his section on stability, Hacking quotes the historian of science Norton Wise who argues that culture and science are inseparably connected with each other. Cultural influences go into the discovery of scientific theories and leave an indelible footprint there. Weinberg, whom Hacking quotes approvingly, maintains, however, that these influences 'have been refined away' (Hacking, 1999, p. 86). Koertge, in her contribution, makes a similar point (in a decision-theoretical context) with respect to the influence of ideologies on scientific theories. What about the remarkable stability of scientific theories? Is there a viable constructionist explanation for this? Following roughly Kuhnian lines of thought, one could state that scientists grow up and are trained within a certain research tradition, they learn all the theories and techniques of this tradition and, of course, they want to promote their career; these scientists are well advised to demonstrate their affiliation to the tradition in question by following the research program of that tradition; all junior scientists who are too radical are not made protegés and their careers may take a turn for the worse. The scientific community does not reward disloyal behaviour. Another, and perhaps somewhat more plausible, variant to explain the continuity in scientific theorizing by external factors is this: it is simply

[12] Post also seems to support this view: 'Contrary to Kuhn, I believe that scientific theory *converges* towards a unique truth' (p. 28).

[13] Another realist way put forward to account for the stability of scientific theories is Radder's (1988, 1991) moderate realism.

too costly to start from scratch when confronted with a new problem. Scientists who follow this strategy will not be able to produce a sufficient number of papers in renowned journals that are necessary to survive in academia. This also explains why the mathematical structure of theories is extremely stable: since so much depends on it, a revision would be very costly indeed. Although there might be some truth in these stories, I think that there is more to be said.

The *third* and final option relies on the concept of coherence and takes, in a way, the best of both worlds. It is weaker than realism (although coherence is compatible with realism) and leaves enough space for external factors. Here, the success of correspondence considerations in scientific theorizing is explained by showing that this way of conducting research leads to more coherent belief sets. How can this be achieved? First of all, the notion of 'coherence' must be clarified. BonJour (1985) explains:

> What then is coherence? Intuitively, coherence is a matter of how well a body of belief 'hangs together': how well its component beliefs fit together, agree or dovetail with each other, so as to produce an organized, tightly structured system of beliefs, rather than either a helter-skelter collection or a set of conflicting subsystems. It is reasonably clear that this 'hanging together' depends on the various sorts of inferential, evidential, and explanatory relations which obtain among the various members of a system of belief, and especially on the more holistic and systematic of these. (p. 93)

This explication still needs to be made more precise in order for us to be able to compare the coherence of two different belief sets — say, before and after a scientific revolution. To assess the coherence of a belief set, a colleague and I have constructed a probabilistic model that yields a coherence measure (cf. Bovens & Hartmann, 2000). This measure is a function of the joint probability of the propositions in the belief set, as well as of the conditional dependencies among these propositions. Stating the joint probability is a rough and ready way to take into account factors external to science. Factors that are (mostly) internal to science, such as the 'the various sorts of inferential, evidential, and explanatory relations which obtain among the various members of a system of belief', discussed by BonJour, are modelled by means of conditional probability distributions. Our model accounts for both factors. Additional assumptions are needed in the model to show that the coherence is higher if the new theory is linked to the old theory through correspondence relations than if this condition is not fulfilled. Although we have not yet modelled this claim within our framework, this does seem to be a plausible hypothesis. Further support comes from Koertge who states that 'the fact that a new theory stands in a correspondence relation to a refuted but largely successful older theory may in some circumstances confer a degree of prior *plausibility* on that new theory' (p. 134). This prior plausibility enters our coherence measure. All this can, of course, only be shown in detailed models. It should be clear, however, that the account just sketched allows for a rational reconstruction of radical breaks in the history of science: radical breaks turn out to be necessary in order to render possible more coherent belief sets (cf. Salmon, 1990). This is an advantage of the coherence account over the convergent realism account, but there is also a disadvantage which Kosso emphasizes in his textbook: 'Coherence among theories will secure a cozy network of cooperation and consistent beliefs, but there is no obvious reason to think it will secure any anchor to reality' (Kosso, 1992, p. 136). Maybe we cannot achieve more than an account of nature that is as coherent as possible.[14]

Science and the dynamics of its theories is much more complicated than Post's GCP suggests. His principle was only a first approximation, and so were his reasons for adopting and defending the principle. A real understanding of how theories are related can only be obtained by carefully analysing many detailed case studies. The contributions in this volume provide a good starting point for this. The book will therefore be of great help to all philosophers of science who want to get new vistas on the old (and perhaps somewhat old-fashioned) problems of theory choice and the relations between scientific theories. In sum, this is a welcome contribution to the current debate and should be consulted by everyone who wishes to back up her position by means of examples from actual scientific practice.

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## References

BonJour, L. (1985). *The structure of empirical knowledge*. Cambridge, MA: Harvard University Press.

Bovens, L., & Hartmann, S. (2000). Coherence, belief expansion and Bayesian networks. In C. Baral & M. Truszczynski (Eds.), *Proceedings of the 8th international workshop on non-monotonic reasoning, NMR 2000*. http://www.cs.engr.uky.edu/nmr2000/proceedings.html.

Cartwright, N. (1999). *The dappled world: A study of the boundaries of science*. Cambridge: Cambridge University Press.

Cushing, J. (1998). *Philosophical concepts in physics: The historical relation between philosophy and scientific theories*. Cambridge: Cambridge University Press.

Fadner, W. (1985). Theoretical support for the generalized correspondence principle. *American Journal of Physics*, *53*(9), 829–838.

Hacking, I. (1999). *The social construction of what?* Cambridge, MA: Harvard University Press.

Hartmann, S. (1999a). Models and stories in hadron physics. In M. Morgan & M. Morrison (Eds.), *Models as mediators* (pp. 326–346). Cambridge: Cambridge University Press.

Hartmann, S. (1999b). Uber die heuristische Funktion des Korrespondenzprinzips. In J. Mittelstraß (Ed.), *Die Zukunft des Wissens* (pp. 500–506). Konstanz: Universitätsverlag Konstanz. (For an English abstract of this paper, see Modelling high-temperature superconductors: Correspondence at bay? In J. Cachro & K. Kijania-Placek (Eds.), *Volume of abstracts*, 11*th International Congress of Logic*,

[14] Although with slightly different emphasis, Post (p. 17), Kamminga (p. 80), and Saunders (p. 307) seem to hold a similar view. For an alternative account which explains the various relations which hold between successive scientific theories see Scheibe (1997, 1999). Scheibe (1999) contains a wealth of detailed case studies from relativity and quantum mechanics.

*Methodology and the Philosophy of Science* (p. 169). Cracow: The Faculty of Philosophy, Jagiellonian University.)

Hartmann, S. (2001). (in preparation). Correspondence, coherence, and high-temperature superconductivity.

Hoyningen-Huene, P. (1993). *Reconstructing scientific revolutions: Thomas S. Kuhn's philosophy of science*. Chicago: The University of Chicago Press.

Koertge, N. (1973). Theory change in science. In G. Pearce & P. Maynard (Eds.), *Conceptual change* (pp. 167–198). Dordrecht: Reidel.

Kosso, P. (1992). *Reading the book of nature. An introduction to the philosophy of science*. Cambridge: Cambridge University Press.

Kuhn, T. S. (1996). *The structure of scientific revolutions*. Chicago: The University of Chicago Press.

Laudan, L. (1981). A confutation of convergent realism. *Philosophy of Science*, *48*, 19–49.

Popper, K. (1972). *Objective knowledge. An evolutionary approach*. Oxford: Clarendon Press.

Post, H. (1971). Correspondence, invariance and heuristics. *Studies in History and Philosphy of Science*, *2*, 213–255.

Radder, H. (1988). *The material realization of science*. Assen: Van Gorcum.

Radder, H. (1991). Heuristics and the generalized correspondence principle. *British Journal for the Philosophy of Science*, *42*, 195–226.

Salmon, W. (1990). The appraisal of theories: Kuhn meets Bayes. In A. Fine, M. Forbes, & L. Wessels (Eds.), *PSA 1990: Proceedings of the 1990 biennial meeting of the Philosophy of Science Association*, Vol. 2 (pp. 325–332). East Lansing: Philosophy of Science Association.

Scheibe, E. (1997). *Die Reduktion physikalischer Theorien: Ein Beitrag zur Einheit der Physik. Teil I: Grundlagen und elementare Theorie*. Berlin: Springer.

Scheibe, E. (1999). *Die Reduktion physikalischer Theorien: Ein Beitrag zur Einheit der Physik. Teil II: Inkommensurabilität und Grenzfallreduktion*. Berlin: Springer.

Zahar, E. (1989). *Einstein's revolution: A study in heuristic*. La Salle, IL: Open Court.