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Re-formulating The Generalized Correspondence Principle: Problems and Prospects

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Abstract. The generalized correspondence principle is the assertion of something like the following methodological norm: successor theories ought to incorporate precursor theories as special cases. However, the actual core connotation of this principle seems to be that when we are *constructing* new theories in some domain of application we ought to retain as much of prior but refuted theories as is possible while eliminating inconsistency with the data. As a result, it is argued here that the correspondence principle has not been correctly formulated. Also, it is argued here that there is no compelling extant justification of this proposed methodological norm.

1. Introduction

The generalized correspondence principle is the assertion of something like the following methodological norm: successor theories ought to incorporate precursor theories as special cases. In accordance with this idea, it has often been interpreted as a relatively simple principle concerning syntactic inter-theoretic relations, and so interpreted, it is placed squarely in the context of justification. However, the actual core connotation of this principle seems to be that when we are *constructing* new theories in some domain of application we ought to retain as much of prior but refuted theories as is possible while eliminating inconsistency with the data. As a result, the simplicity of the generalized correspondence principle is only apparent and it seems more properly to be a rather complicated principle concerning the context of discovery that is intended to govern how science is supposed to function over time (i.e. diachronically).

So while the generalized correspondence principle might well have some significance for the issue of inter-theoretic explanatory and justificatory relations this is at best only a secondary aspect of that principle. When it is unpacked, the generalized correspondence principle then seems to incorporate the following more basic principles:

P1 [*continuity*]—every series of temporally ordered theories in a given domain exhibits continuous evolution (i.e. the historical progression of theories in any given domain has no gaps).

P2 [*conservatism*]—for any two theories related as precursor and successor, the successor theory is the most conservative revision of the precursor theory that eliminates the precursor theory's empirical inadequacy (i.e. newer, more sophisticated theories, are minimally revised, corrected, versions of older theories).

It is also often interpreted so as to include the following, perhaps more controversial, principle:

P3 [*convergence*]—the process of continuous and conservative revision will ultimately result in the production of a unique theory.[1]

Notice however that P1-P3 lack any normative content and so if we correctly understand the generalized correspondence principle as a methodological *norm* we must re-formulate P1-P3 as follows in order to take this into account:

P1' [*normative continuity*]—every series of temporally ordered theories in a given domain *should* exhibit continuous evolution (i.e. the historical progression of theories in any given domain should have no gaps).

P2' [*normative conservatism*]—for any two theories related as precursor and successor, the successor theory *should* be the most conservative revision of the precursor theory that eliminates the precursor theory's empirical inadequacy (i.e. more sophisticated theories, should be minimally revised, corrected, versions of less sophisticated theories).

P3' [*normative convergence*]—the process of continuous and conservative revision *should* ultimately result in the production of a unique theory.

The real issues to be addressed here then concern the following two points. First, how are we to formally capture P1'-P3' in terms of a relatively precise methodological principle? Notice that in answering this question it is clear that P1'-P3' will serve as adequacy constraints on any such proposed interpretation. Second, we may consider whether the generalized correspondence principle is epistemically justified with respect to the conduct of science, whatever formal presentation it is given.

[1] Note, however, that this should not be confused with realism. Ultimate theoretical progress in a domain might satisfy P1-P3 without our having to accept that the resulting theory is strictly true. That is a different matter altogether.

1.1 Problems with the Generalized Correspondence Principle

The first problem concerning the generalized correspondence principle to be addressed here is that there has been considerable difficulty in producing an adequate formulation of that principle. This is worrisome because the generalized correspondence principle—or something quite like it—plays a central role in a number of methodological programs in the philosophy of science and so it is worthy of close scrutiny.[2] In any case, the historical origin of the generalized correspondence principle is often traced back at least to the work of Henri Poincaré and Niels Bohr. A number of philosophers of science working in fairly traditional terms have offered renderings of this principle that are alleged to capture the fundamental insight of Bohr in particular.[3] But, as it turns out, neither Poincaré's nor Bohr's work is especially helpful for this admittedly more philosophical task.

More importantly, the logical empiricists' dismissal of the context of discovery as philosophically irrelevant—that many of these philosophers have inherited from that earlier tradition—has apparently led to serious confusion about how to approach the issue of offering a precise rendering of the generalized correspondence principle. Moreover, syntactic approaches have dominated the attempts to render the generalized correspondence principle explicit. As a result, the generalized correspondence principle has almost universally been mistakenly conflated with the concept of inter-theoretic reduction in the tradition of Nagel.[4]

The second major worry that arises with respect to the generalized correspondence principle is that the *bona fides* of this principle as a methodological imperative have been almost totally ignored. It seems that this is likely the case *because* the correspondence relation has been misunderstood to be nothing more than the synchronic relation of inter-theoretic reduction. With respect to the epistemic evaluation of the generalized correspondence principle, what critics have tended to focus their attention on is the historical accuracy of the descriptive claim that the history of science takes the form of successive applications of this principle in the non-normative sense expressed by P1-P3.[5] In the latter

[2] Notable defenders include Karl Popper (1963), Leszek Nowak (1980), Władysław Krajewski (1976, 1977), Michael Friedman (2001) and Eli Zahar (1983, 2001).

[3] See Krajewski, 1977 and Zahar, 2001.

[4] See Nagel, 1961.

[5] Most crucially, see Laudan, 1981.

sections of this paper the challenge of questioning the *bona fides* of a more adequate normative version of the generalized correspondence principle from a methodological perspective will be taken up.

In doing so it will be argued first that—as traditionally formulated—the generalized correspondence principle makes little if any methodological sense. This, of course, is not an entirely new criticism. It is also true that some more contemporary treatments of the correspondence relation also face serious problems largely because they also misinterpret the generalized correspondence principle as a synchronic inter-theoretic relation; i.e. as a form of reduction or emergence as it applies to pairs of theories in the context of justification.[6] Ultimately, it will be argued here that when suitably interpreted as a *diachronic norm* in the context of discovery, there is no extant adequate justification of the generalized correspondence principle.

2. The Generalized Correspondence Principle.

The generalized correspondence principle is typically presented as a synchronic and (broadly) syntactic inter-theoretic relation as follows:

(CP) $(\forall T_{i+1})(\forall T_i)(\exists p_j)[(T_{i+1} \,\&\, (p_j = c)) \supset T_i]$.[7]

This principle, in this more or less traditional formulation, establishes a sort of reductive logical connection between two theories, T_{i+1} and T_i in some specified domain, under the assumption that were some relevant factor p_j in T_{i+1} set to some special value or range of values c, we could logically derive T_i as a special case of T_{i+1}. Notice in particular that in effect CP is supposed to inter-theoretically relate *precursor* and *successor* theories by introducing the indices involving i. However, on closer inspection it turns out that CP cannot possibly do so in any interesting diachronic sense.[8] In fact, it cannot really be a diachronic principle at all because the conditional in CP is assumed to be a material conditional and hence the successor theory conjoined with the idealizing assumption entails the precursor theory in the purely deductive and hence eternal sense, whether we know this at some point in time or not. As a result, it should be clear that CP fails to capture the temporal content of adequacy conditions P1'-P3'.

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[6] See Pearce & Rantala, 1985 and Radder, 1991 for criticisms of the traditional formulation of the correspondence principle. See Pearce & Rantala, 1985 and Batterman, 2002 for two recent attempts to interpret the correspondence principle synchronically.

[7] This formulation is essentially that presented by Krajewski (1976, 1977).

[8] See Krajewski, 1977 for an extended discussion of the correspondence principle.

Nevertheless, in accord with P2' the iterated application of CP is then supposed to allow for the derivation of older theories by setting more such factors to specific values and so a set of theories can be inter-theoretically linked via syntactic relations into a chain of such related theories.[9] CP is then presented as a claim to the effect that the synchronic logical relationship between successor and precursor theories is material implication from the former theory conjoined with a claim that some relevant factor has a restricted influence to the latter theory. This is also importantly intended to represent how science should advance over time, but notice that CP also contains no normative content. As a result, it fails to account for the important content that differentiates factual adequacy conditions P1-P3 from normative adequacy conditions P1'-P3'. In any case, ignoring the worry about normativity for the moment CP is supposed to guarantee that practitioners using the successor theory can retain precursor theories as special cases holding only in restricted conditions and this is supposed to be both because the successor theory developed out of the precursor theory and because it is capable of accounting for new data that could not be explained by the precursor theory. In turn, the fact that the successor theory developed out of the precursor theory—which must have been partially successful—in this manner explains why the successes of the precursor theory can be retained. They can be retained because the precursor theory is a deductive consequence of the successor theory. The retention of prior theoretical successes is assured because we can derive the precursor theories as special cases holding for a restricted sub-domain of the successor theories.

What CP then simply and descriptively tells us is that more simple theories are kinds of reductive cases of more complex theories. So it should then be clear that this has little or nothing to do with how *theoretical progress* is supposed to be achieved over time. The correspondence principle, so rendered, simply deals with the *de facto* synchronic and syntactic relations between *more complex* and *less complex* theories in the context of justification and so it then surely fails to meet the adequacy conditions P1'-P3' for at least two reasons. As a result, it is hard to see how CP can possibly function as the kind of methodological norm that is supposed to govern the evolution of science that the generalized correspondence principle is intended to be. All that CP asserts is that some theories reduce to other theories given appropriate idealizing conditions, and assessing whether such a relation holds between

[9] This principle plays a prominent role in Bohr's and Poincaré's philosophies, and it has received considerable attention in Krajewski, 1976 and 1977, in Post, 1971, and in Zahar, 1983 and 2001.

any two theories is neither a temporal activity nor is it normative in any sense.

There is, however, another problem that must be addressed here and it concerns the issue of conceptual change over time in science. If CP makes any sense at all, then it must be the case that T_i and T_{i+1} are expressed in the same language so that terms common to both theories refer to the same things in terms of the same concepts. If this were not the case, then what appear to be terms common to the successor and precursor theories could have different meanings, they might effectively be expressed in different theoretical dialects. If this were the case, then CP would literally be meaningless as it would not be a well-formed sentence of a particular language. So in order to make CP meaningful the successor and precursor theories must be interpreted in terms of the same concepts. This, however, is clearly not true of the history of science, and if we require that the precursor theory must be translated into the dialect of the successor, then the actual content of the precursor will often be lost. The result of adopting this approach would be to essentially cede the view that CP is an *inter*-theoretical relation. What has typically been done in responding to this sort of worry is to restrict P1' and to interpret the generalized correspondence principle as the claim that the history of science should exhibit only numerical and formal continuity.[10] In other words, by restricting P1' in this way the kind of conservatism endorsed by P2' is then supposed to require that over time (as theoretical progress occurs) only the well-confirmed equations and constant values of prior theories must be preserved, but not the semantic content of those structures. This yields a much weakened interpretation of CP, but it does allow those who defend CP to retain that principle in light of the potential problem of the incommensurability of old and new theories.

From a formal perspective however, CP is also inadequate and so it is not merely the case that CP can be amended in order to avoid the three charges just leveled against it. The formal inadequacy of CP can be seen most easily by looking at its contrapostivie:

$$(\text{CP})\ (\forall T_{i+1})(\forall T_i)(\exists p_j)[(\neg T_i \supset \neg(T_{i+1}\ \&\ (p_j = c)))]$$

What one finds is that when CP is so rendered as its equivalent contrapositive, it implies that if a precursor theory T_i is false, then either the successor theory T_{i+1} of that precursor theory must be false or the idealizing assumption $p_j = c$ must be false—or both might be false. The problem here is that T_i might well be false while both T_{i+1} and $p_j = c$ are true. This sort of case can be most easily seen in cases of causal laws where the functional relation between parameters in T_i may be blocked or

[10] See Radder, 1991.

altered by some causal factor other than p_j, thus rendering T_i false even though both T_{i+1} and $p_j = c$ are true.[11] So, we are left at this point with the observation that the traditional formal rendering of the generalized correspondence principle is inadequate, and similar worries plague more recent attempts to salvage the synchronic sense of the generalized correspondence principle.

3. Re-interpreting the Generalized Correspondence Principle in Light of the History of Scientific Progress

As noted earlier, the generalized correspondence principle, when properly interpreted as a diachronic methodological norm, is by no means a new principle and it appears to be based on the idea that science progresses as a series of more sophisticated and realistic theories that somehow capture their less refined predecessors by treating them as special cases of the newer theory *and* from which the newer theory evolved. This is supposed to allow for the retention of confirming instances, theoretical equations, constants and explanatory power in the face of theoretical progress and it guides subsequent further progress by asserting that new theories ought to be the most conservative expansive revisions of their predecessors that eliminate certain unrealistic (i.e. idealizing) assumptions. Given this more accurate understanding of the generalized correspondence principle as a diachronic methodological norm, it is supposed to guarantee that practitioners using the successor theory can retain precursor theories as special cases holding only in restricted conditions, *but* this is supposed to be the case because the successor theory was dynamically developed out of the precursor as a case of expansive theory revision of the precursor theory.[12] So a successor theory is capable of accounting for new data that

[11] One might be tempted to introduce some sort of completeness requirement similar to that defended by Hempel (1965) in the context of his theories of explanation. However, this sort of approach is itself a controversial matter. For example, we may simply not be aware of all the causal factors relevant to some current theory and such completeness assumptions may not even be correct in the case of the true theory in some sequence of corresponding theories as the logic of causality appears to be non-monotonic.

[12] We must be clear here that there are two kinds of revision involved in this account of dynamic theoretical relations. The first kind are what we might call progressive theoretical revisions and these occur when a theory is superseded by a new theory and the revision involves taking new data into account and modifying the precursor theory appropriately. The second kind of revision is regressive and involves revising the successor theory by introducing and idealizing condition, making the appropriate revisions and thus recovering the precursor theory as a special case.

could not be explained by the precursor theory and this fact then in turn explains why the successes of the precursor theory can be retained. The retention of those older theoretical successes is assured because we can recover the precursor theory as a revision of the successor theory. This sort of "contracting" revision then yields the precursor theory as a special case of the successor theory holding for a restricted set of cases of the domain of the successor theory. So the relation between successor and precursor theories involves theory revision both ways. First, we have progressive revisions from precursor theory to successor theory which is meant to be conservative and to secure theoretical progress. Second, we have regressive revisions from successor theories to precursor theories designed to maintain continuity and to both retain and explain past successes.

Adopting this stance towards the generalized correspondence principle yields the desired dynamic reading, but then it is clear that the intended content of the generalized correspondence principle can be adequately captured neither by CP nor by the available alternative formulations because they all fail to capture the temporal and normative content of that principle. Moreover, in the post-Kuhn, Feyerabend, and Hanson era of philosophy of science it is hardly worth mentioning that from a descriptive perspective it is historically inaccurate to claim that the history of science is constituted by a series of theories that satisfy the descriptive content of P1-P3.[13]

Nevertheless, the methodological force of the dynamic generalized correspondence principle as captured by P1'-P3' remains untouched by such empirical observations and it would still seem to be possible to defend a suitably interpreted dynamic generalized correspondence principle as an *a priori* normative principle that governs science, or on the basis of some reasoned argument for that principle. This is the case because it is clearly possible that the normative principles may hold even though they may often be violated. However, in what follows it will be suggested that when subjected to critical scrutiny the claim that the progress of science should obey this sort of dynamic generalized correspondence principle has *not* been provided with sufficient warrant. As a result, while it may well be true that science ought to obey a suitably rendered version of the generalized correspondence principle, there is no extant adequate reason to believe that this is true.

[13] Although with respect to the correspondence principle, Post (1971) is a notable holdout on this count.

4. The Dynamic Generalized Correspondence Principle as Methodological Norm

The first problem that then needs to be addressed at this point concerns how the dynamic generalized correspondence principle should be formally rendered. Given the lessons from the previous sections, there are three important desiderata that must be observed in doing so. First, the theory relation that the dynamic generalized correspondence principle concerns is not a deductive relation, and so an adequate formulation must cash out the relation between successor and precursor theories in some other manner. Second, the generalized correspondence principle is a normative principle. Finally, the formulation must capture the specific content of P1'-P3'.

Where '*O*x' is the deontic operator representing the idea that it ought to be the case that x and '*REV*<x, y> = k' is some suitable theory revision operator representing the operation of revising x by y to yield k, we can understand the dynamic generalized correspondence principle as the following assertion:

(DGCP) $O(\forall T_{i+1})(\forall T_i)(\exists p_j)[(REV\langle T_{i+1}, (p_j = c)\rangle = T_i]$.

The main task in making sense of this—admittedly generic—principle is then to see how it fares with respect to the three desiderata just mentioned. To begin, the second desideratum appears to have been dealt with via the introduction of the deontic operator, but there are many difficult issues about how to properly interpret operators in deontic logic and so things are not quite so easy with respect to this issue. But, this issue can be ignored here, as there are other more troubling issues to be addressed. What is more worrisome yet is whether the first and third desiderata can be adequately addressed with respect to the revision operator that is the essential element in DGCP. Specifically, providing a suitably formal and conceptually satisfying account of the revision operator *REV*<x, y> = k that captures the content of both P1' and P2' is no easy task.

The most obvious place to turn to here is to the well-known AGM account of the belief revision, but that theory is itself the subject of considerable controversy and providing a formal and conceptually adequate account of the revision operator has proved to be much more difficult than it was first thought to be.[14] The main problem here concerns how to define what constitutes a minimal revision and a number of proposals have been made concerning how this should be done. Nevertheless, the general sense one gets from the exchanges concerning the logic of theory revision is that there is no unique account of what

[14] See Alchourrón, Gärdenfors, & Makinson, 1985 and Hansson, 2000.

constitutes a minimal revision, and this situation is reminiscent of the earlier, less than happy, results concerning the logic of induction arrived at by Carnap and developed by Hintikka.[15]

In any case, given even this cursory and general analysis of the generalized correspondence principle it is clear that there is much work that would need to be done to make the DGCP both more precise and more conceptually satisfactory. However, DGCP is a step in the right direction with respect to offering a more adequate formal rendering of the generalized correspondence principle, even if it is only a rather minor step. What having DGCP in hand does allow us to do, whatever its inadequacies are qua specific details, is to turn our attention to the more interesting question concerning the justification of the generalized correspondence principle.

4.1 Justifying the Dynamic Generalized Correspondence Principle as a Methodological Norm.

When we both put aside the historical aspects of the dynamic generalized correspondence principle by jettisoning P1-P3 and shifting our attention to P1'-P3', and when we conveniently ignore the apparently formidable technical problems in rendering that principle precisely, things become both more interesting and more difficult. When read as a methodological imperative, the dynamic generalized correspondence principle implies that successor theories like T_{i+1} ought to be the most conservative revisions of more idealized predecessors like T_i and so successor theories should both fit the data and retain as much of the precursor theory as is possible. But what exactly is the motivation for accepting the claim that we ought to obey this imperative in progressive theory development/construction?

Unless we accept the dynamic generalized correspondence principle on the basis of some merely *a priori* intuition, we ought to be able to offer some substantial (i.e. not purely pragmatic) justification in support of the principle, especially in light of its manifest historical inaccuracy.[16] What sort of justification might be offered for such a principle? The two most plausible extant options seem to be as follows. First, the dynamic generalized correspondence principle might be justified on the basis of the Quinean claim that science just happens to be so constituted. Second it might be justified on the basis of an appeal to the principle that information is fundamentally valuable and the closely related principle that theory revision should be maximally conservative.

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[15] See Carnap, 1952, Hintikka, 1966 and Hilpinen, 1968.

[16] See Laudan, 1981 for a host of historical examples that violate the CP.

4.2 QUINEAN SCIENCE AND THE CORRESPONDENCE PRINCIPLE

The first potential solution to the problem of justifying the dynamic generalized correspondence principle, as just mentioned, derives from the work of Quine. In his 1960 Quine and in Quine and Ullian 1970 it is famously claimed that the norms that govern science are simply constitutive of that particular form of inquiry. As such, there is no more justification for them than noting that their violation entails that one is not doing science. Of course Quine also held that all elements of the web of belief are revisable and that essentialism is simply mistaken, and so Quine could not have held that those principles that are constitutive of science are essential to it.

Michael Friedman (2001), however, has recently argued essentially the same thing about the correspondence principle in particular. He claims that the history of science generally exhibits continuity, conservatism and convergence (i.e. P1-P3) and so science generally exhibits a retrospective sense of rationality, even though these very methodological principles themselves are not essential to science. As such, science is simply just a conventional method of inquiry that is itself dynamic and subject to revision over time. However, at the same time Friedman argues that the assumption of the generalized correspondence principle qua P1'-P3' is necessary for science to exhibit a prospective form of rationality, a sense of rationality that would capture the temporal content of P1'-P3'.

Friedman (2001) consequently argues that if science did not operate under the aegis of the generalized correspondence principle, then there would be no protection against the Kuhnian charge that theories are incommensurable. Accepting P1'-P3' as the governing principles with respect to science guarantees both that there is sufficient continuity in the progress of science to permit meaningful inter-theoretic communication, and that new theories evolve out of their precursors. However, when addressing the issue of the justification of P1'-P3', Friedman (2001) claims that such principles cannot be empirically justified. Moreover, while he accepts that the mathematical elements of scientific theories are justified *a priori* relative to that theory, there is nothing relative to which philosophical principles like the correspondence principle could be similarly justified. Finally, it is nothing more than question-begging against the Kuhnian to argue that such a principle *must* govern science because it is simply stipulated that theories are commensurable. As a result, Friedman's imposition of the dynamic generalized correspondence principle as a norm that governs science is simply arbitrary and so it is not so different from the outright Quinean claim (seemingly inspired by

Wittgentstein) that such norms are mere conventions based on our idiosyncratic conception of how science is to be conducted.[17]

The problems with this generally Quinean approach to justifying the dynamic generalized correspondence principle are then twofold. First and most importantly, in treating such governing norms as mere conventions, these approaches fail to provide any *epistemic* justification for the dynamic generalized correspondence principle and so they are impotent as a defense of the epistemic *bona fides* of DGCP, however it is ultimately unpacked. Secondly, it is simply false that the history of science universally exhibits this character (i.e. conforms to P1-P3) and so it is dubious that science has actually ever been actually so constituted. Given this latter worry, it is hard to see how one can seriously entertain Quine's and Friedman's claims that these principles are constitutive of science itself and so to violate them is just to do something other than science.

4.3 Conservatism and Correspondence

The second extant solution to the problem of justifying the dynamic generalized correspondence principle involves a more or less direct appeal to a fundamental principle of epistemic conservatism, and initially this appears to be a more promising approach. This appears to be so because at least here one might find some deeper epistemic reason to ground our accepting P1'-P3' as the governing principles of properly conducted science. Zahar, for example, explicitly notes that the function of the correspondence principle is "…to preserve all empirically ascertained knowledge (1983, p. 248)." Similarly, Post claims of the method that employs the correspondence concept that "it is also essentially inductive in retaining the old theory in a certain sense: it is conservative (1971, p. 218)." So, it is at least superficially plausible to suppose that there is some epistemic purchase here for justifying the DGCP.

Again, it is useful here to turn to the work done in defense of the AGM theory of belief revision as this kind of conservatism is an integral part of that theory. This is doubly appropriate because the AGM theory is the best theory about how to cash out the revision operation in DGCP. In any case, in defense of the core conservative assumption of AGM, Peter Gärdenfors essentially argues that information is an intrinsically valuable epistemic resource that should not be ceded lightly even in the face of undermining evidence, and this captures the core insight that the DGCP can be justified

[17] See Shaffer (forthcoming) for a more thorough criticism of Friedman's view.

by an appeal to epistemic conservatism.[18] Gärdenfors explicitly tells us that,

> When we change our beliefs, we want to retain as much as possible of our old beliefs; information is in general not gratuitous, and unnecessary losses of information are therefore to be avoided (1992, p. 381).

Similarly we are told that

> When changing beliefs in response to new evidence, you should continue to believe as many of the old beliefs as possible (Gärdenfors, 1992, p. 381).

So Gärdenfors' core insight in this respect is in basic agreement with the comments of Zahar, Post, et al. on the reason grounding the generalized correspondence principle.

Essentially what they all agree on is that from an epistemic perspective it is wrong to jettison empirically well-confirmed elements of theories because empirically well-confirmed beliefs are epistemically valuable. However, it should be obvious that this is a matter that admits of degrees with respect to the importance of information. What this in turn suggests in terms of our earlier restriction of P1' discussed in section 2 is that what is most desirable in the course of scientific progress so understood is the retention of the well-confirmed core theoretical equations and values of constants, i.e. those equations and constants that describe the basic relations between and properties of the core theoretical entities described by any such theory. This also then reveals to us why the dynamic generalized correspondence principle cannot be realistically interpreted as implying that as science progresses conceptual interpretations of those equations must be treated conservatively. While the history science often exhibits such conservative continuity with respect to both core equations and constants, it clearly does not and should not exhibit such conservative continuity with respect to the concepts by which those mathematical structures are made meaningful. It would be simply silly to suggest that science should be conceptually conservative in this more robust sense.[19] Much of the "meat" of scientific progress is to be found in discovering adequate concepts in terms of which we can understand the world via applications of the formal elements of theories to real systems and so the progress of science is in an important sense the task of seeking new

[18] See Gärdenfors 1984, 1988, and 1992 and Gärdenfors & Makinson, 1984. Harman, 1986, Sklar, 1975 and Lycan, 1985 also concern aspects of epistemic conservatism.

[19] See Radder, 1991 for a similar point. There he argues that the correspondence principle can only be seriously entertained as a principle that concerns numerical and formal correspondence.

concepts that help us to understand the world in better and more accurate ways.[20]

However, once this is admitted, the whole purpose of appealing to the fundamental principle of epistemic conservatism as justification for the DGCP is undermined. If the defenders of the DGCP retreat to the position that we should practice science in the sense of P1'-P3' because we should retain the well-confirmed elements of past theories and that we should do so because well-confirmed information is essentially valuable in the epistemic sense, then they stumble painfully into what appears to be a critically fatal contradiction when we realize that P1' must be restricted to purely formal and numerical structures.

The problems here are twofold. First, and most crucially, it is well understood that the logic of the confirmation of empirical sentences is not itself purely formal as it involves substantial empirical assumptions. This raises the worry that what count as well-confirmed empirical statements can change radically in cases of theoretical progress that involve conceptual change and so the idea that we should always retain the well-confirmed elements of prior theories is wrongly predicated on the idea that there is some objectively legitimate and purely formal logic of confirmation that itself remains untouched over the course of theoretical progress.

Second, once this first point is admitted and the restricted version of P1' introduced in section 2 is adopted, we find that it is simply nonsense to suppose that in the progress of science we should retain the empirically well-confirmed elements of prior theories. What are supposed to be retained across concept changing cases of scientific progress are not then properly empirical statements at all, but rather they are only the purely formal structures and numerical constants of the precursor theory stripped of any conceptual interpretation. If this is really what is supposed to be continuous and conserved as science progresses, then a serious problem arises. The problem is that these oddly hollow theory elements are not even capable of empirical confirmation; to strip the precursor theory's equations of any interpretation is to both render them unconfirmable and to strip them of any degree of confirmation they might have had in the context of the precursor theory, and so it simply makes no sense to suppose that we ought to retain the well-confirmed parts of prior theories as science progresses. It is thus hard to see how appealing to conservatism in order to justify the DGCP is even coherent.

[20] See Brown, 2001.

4.4 *SHOULD* SCIENCE OBEY THE DYNAMIC GENERALIZED CORRESPONDENCE PRINCIPLE?

Even if we ignore the historical inaccuracy of the dynamic generalized correspondence principle, the difficulties of providing that principle with a precise formulation and the failure of the most obvious attempts to provide it with a satisfactory epistemic justification, the main crucial question remains. Should science proceed in a manner that satisfies the dynamic generalized correspondence principle?

As we have seen, typical discussions of the generalized correspondence principle say little or nothing about its justification other than alluding to the *a priori* insight that we ought to try to retain as much of a previously confirmed theory as is possible when we propose a more sophisticated successor theory.[21] But, if we are good naturalists, then we ought to be wary of any attempt to ground the dynamic generalized correspondence principle as an *a priori* justified normative principle. So what we need to do is to continue to examine what kind of empirical justification might be provided for this important methodological norm, or at least something like it.[22] However, as we have seen this task is fraught with problems and so we are left with the conclusion that as things stand there is no adequate reason to suppose that the progress of science should satisfy the generalized correspondence principle even if it is true that it should do so.

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[21] See, for example, Post, 1971 for an explicit defense of this sort of ultra-conservatism.

[22] An interesting possibility is suggested in MacIntyre, 1977. MacIntyre's discussion suggests that what is needed is not something like a new interpretation of the general correspondence principle, but rather something more like a principle by which we can understand how we came to hold precursor theories and why we were misled by them. This would allow for a sort of understanding of both precursor theories and their successors in a manner in which whatever continuity there is between theories would make sense without necessarily incorporating any sort of conservativism of the sort discussed here. Of course, this suggestion is in need of serious technical elaboration, especially concerning how it would meet objections concerning incommensurability.

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