Realism and the need for a Phonological Semantics

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1. Introduction

One of the main arguments of my thesis[1], from which this paper is an extract, is that phonological theory should aim to reflect the reality of the phonology-phonetics interface, and I shall endeavour here to provide some justification for this argument. Of all the motivations for the biological-naturalistic metatheory I propose[2], realism is perhaps the most challenging to state succinctly without getting lost in labyrinthine philosophical questions and qualifications. But its basic tenet should be made clear from the outset, namely that I assume that the statements of any plausible phonological theory purport to be true, and purport to be true about things in the natural, biological, spatiotemporal world. In other words, I assume that phonological symbols - that is graphical representations - are used to stand for purported truths about certain spatiotemporal events, whether these be individual actions, mind/brain phases, mental representations or specific people at specific times. The most convincing justification for the adoption of such a position is to be found in the work of Philip Carr, in particular his (1990) monograph *Linguistic Realities*, which makes a seminal contribution towards the elucidation of the nature of theorising in linguistics by providing an accessible digest of the relevant parts of the extensive literature on the philosophy of science. Following Carr, I advocate the model of realism posited by Sir Karl Popper (1959, 1963, 1972, 1982/3), and shall indicate in this section its particular relevance to the future development of a biological-naturalistic metatheory as one of the main foundations of phonological science.

[1] Durgauhee (submitted)
[2] See e.g. Durgauhee (1997a)

2. Realism

Popper's realism has three main characteristics. Firstly, Popper (1959) postulated a demarcation criterion based on the notion of *falsifiability*, intended to divide scientific from non-scientific hypothesis. In order to classify a particular theory as scientific, it must be able to demonstrate that there are certain states of affairs which are logically incompatible with it, such that if it were possible to show that these states of affairs were actually true of the world, said theory would thus be falsified. According to this view, the scientific content of a particular theory is a function of its falsifiability: if there exists a theory from which no particular states of affairs are deducible, then it is clear that it cannot demonstrate anything *not* to be true, that it excludes nothing, and is therefore lacking in scientific-theoretical content. Thus, according to Popper, science proceeds not by the accretion of a body of proven, entirely unfalsifiable facts about the world, but instead by means of a hypothetico-deductive process in which certain statements about certain states of affairs are deduced from initial conjecture, and are tested by making attempts to *refute* these (i.e. show them to be false). A successful scientific theory is consequently one which is in essence falsifiable, but which it has been consistently impossible to falsify. Such a theory is assumed to be largely *corroborated*, but not necessarily true - it is merely a present best guess as to the real ontology of the object of inquiry, but its status is still essentially that of a conjecture which is entirely open to refutation.

The second aspect is intimately related to the first, namely that whilst it is impossible to speak of a theory which is indubitably true, there is still the notion of a convergence upon that which is true. For Popper, theories have greater or lesser degrees of *verisimilitude*, that is closeness to the truth. By means of the concept of falsifiability and its concomitant processes of corroboration and refutation, we are able to develop theories which come closer and closer to a truthful description of the object of inquiry, but we are never able to attain truth since what we started off with was merely conjecture. As Carr (1990:8) points out, whilst Popper's view of science is dependent upon a notion of truth, it is a concept which is much weaker than in - say - a verificationist view of science.

In order to come to terms with the third tenet of Popper's approach to the philosophy of science, it is essential to note that meaningfulness in one particular sense is not that important a criterion as far as the division of scientific from non-scientific hypothesis is concerned, especially with respect to the status of metaphysics. Rather than being anxious to make a strict distinction between the two - with the former, as opposed to the latter, being firmly based on observation, as in positivism - Popper instead attempted to provide some demonstration of the relation between them (i) by claiming that all observation statements are theory-laden, and (ii) by emphasising the importance of metatheory and so-called 'metaphysical research programmes', that is the broad foundation of *Weltanschauungen*, metaphors, myths and conjectures upon which any scientific theory is initially built. Some of the kinds of phenomenon in (ii) are those which I discuss extensively in Chapter 3 of my thesis in the terms of the varying sociological and scientific contexts from which various theories of phonology have emerged during the century.

By way of an example of (i), it is appropriate here to review at some length an important but as yet largely ignored argument presented by Bromberger and Halle (1997) which is of direct relevance to the proposal that the adoption of Popperian realism in phonological inquiry is timely. Their argument is simply that phonological symbols do not have the same semantic content across different phonological theories (i.e. they are theory-laden observation statements), given certain assumptions. Their assumptions are as follows. First of all, they assume in Popperian fashion that phonological symbols stand for purported truths about (i.e. predicates which purport to be true of) the spatiotemporal world, and that phonology as a scientific discipline is about things in that world. (Indeed, it is worth noting at this point that the first of these assumptions is entirely incompatible with the 'semantic' view of the phonology-phonetics relation as encountered in Chapter 2 of my thesis, which holds that phonological symbols stand for *individual constants* which denote/name individual spatiotemporal objects or sets of spatiotemporal objects (Bird 1990, 1995; Bird and Klein 1990; Broe 1993; Local 1992; Pierrehumbert 1990; Scobbie 1991; Scobbie *et al.* 1996), and with one possible interpretation of Burton-Roberts' Representational Conjecture which would hold that phonological symbols do not denote abstract, Platonic objects (Burton-Roberts 1994; Burton-Roberts and Carr 1997). These are important points, and I return to them in my discussions of biological naturalism elsewhere[3].) More importantly, however, they also make the assumptions that (a) only one of the many extant theories of phonology can be the closest approximation to the truth (I shall call this a *non-overdetermination thesis*), and (b) that phonological symbols are used unambiguously within any theory, but not necessarily across them. This is because part of the meaning of these symbols is determined by the theoretical framework within which it is postulated. Thus, any phonological statement which is made within a particular phonological theory is constrained by and defined in terms of the theory within which that statement is made.

To illustrate their point, Bromberger and Halle compare the semantic contents of phonological signs used in derivational- and optimality-theoretic analyses using λ-calculus. Below I give their example of a heavily simplified derivation for the utterance 'Canadians live in houses', with (1) being the systematic phonemic representation (Chomsky 1964) and (1a) being the systematic phonetic representation.

[3] Durgauhee (1997a, 1997b)

(1) {[kænæd-i-æn], Noun...}+{[z],Pl...}+{[lIv], Verb...}+{[In], Prep...}+ {[haYs], Noun...}+ {[z], Pl...}

....

....

(1a) k℘néydiy℘nzlIvInháwz℘z

The semantic content of the first symbol in (1a) is analysed in terms of the predicate in (2):

(2) λx [kx] $=_{\mathrm{df}}$ λx [dorsal x & -cont x & -voice x & -nasal x]

$=_{\mathrm{df}}$ λx [dorsal x] & λx [-cont x] & λx [-voice x] & λx [-nasal x]

If (A) is taken to refer to the utterer of (1a) at the time of the utterance of the first segment, then (3) is true:

(3) λx [dorsal x] (A) & λx [-cont x] (A) & λx [-voice x] (A) & λx [-nasal x] (A)

In answer to the question as to how A met the *satisfaction conditions* of the predicate defined in (2) so as to make (3) true, Bromberger and Halle provide the answer that A was in the intentional mind/brain state that preceded the gymnastics. According to this, (3) is true because A *had the intention to perform* the articulatory gymnastic. This is altogether more satisfactory than saying (3) is true because A actually performed the gymnastic, since the former answer is much more in accordance with current phonological practice, allowing for subvocal speech, silent writing, silent reading, and for impediments in the mouth or in the environment.

Similarly, '℘' in (1a) stands for a predicate defined as:

(4) λx [℘x] $=_{\mathrm{df}}$ λx [-round x & -high x & -low x & +back x & +ATRx]

And (5) is true because A *had the articulatory intentions* predicated by the conjuncts in (5):

(5) λx [-round x] (A) & λx[-high x] (A) & λx[-low x] (A) & λx[+back x] (A) & λx[+ATRx] (A)

However, it is not such a simple matter to define the meaning of the systematic phonemic symbols in (1). For instance, whilst λx [kx] (A) could plausibly be true of the first of the phonological symbols, the same cannot be said for the first 'æ', since it surfaces as '℘'. λx[æx](A) is false because A produced '℘' sounds instead of 'æ' sounds *automatically*, in a way driven by the implicitly cognised rules of his or her acquired phonology. Therefore the solution is to hold that A had the intention to perform the gymnastics for 'æ' unless precluded by some rule or rules, a qualification which is easily summarised by means of 'upsr' (unless precluded by some rule) clauses.

(6) λx [$\underline{\text{æ}}x$] $=_{\mathrm{df}}$ λx [upsr -back x] & λx [upsr -high x] & λx [upsr +low x] & λx [upsr -round x]

Thus λx [$\underline{\text{æ}}x$] (A) is true. However, the account needs to be reconciled with three other facts essential to derivational theory, namely (a) the fact that each phonemic symbol does not occur in isolation, but is ordered with other symbols in whole lines, (b) the fact that each of these lines occurs not in isolation, but is ordered with other lines, and (c) the fact that the derivation is relevant not only to the particular utterance produced by A, but to indefinitely many other actual and conceivable utterances.

With regard to (a), Bromberger and Halle would postulate that our line (1a) as a whole stands for a more complex predicate, defined as:

(7) λx [$\underline{\mathrm{O}}x$] $=_{\mathrm{df}}$ λx[$(\exists r)$ $(\exists s)$ $(\exists t)$ ... $(\exists u)$ $(\exists v)$ { $r > s$ & $s > t$ & ...& $u > v$ & λy [$\underline{\mathrm{k}y}$](r) & λy [$\underline{℘y}$] (s) & λy [$\underline{\mathrm{n}y}$] (t) &... &λy [$\underline{℘y}$] (u) & λy [$\underline{\mathrm{z}y}$] (v) & S { $r, s, t, ... ,u ,v$} $= x$}]

(7) asserts that a stage of A was made up of subsidiary stages, that these stages occurred chronologically and that the first of these stages was a k, the second an ℘, etc. Similarly, line (1) - the systematic phonemic representation - also stands for a complex predicate defined as:

(8) λx [Gx] $=_{df}$ λx[($\exists l$) ($\exists m$) ... ($\exists n$) ($\exists o$) { $l > m$ ... & ... $n > o$ & λy [{[kænæd-i-æn], Noun...}y] (l) & λy [{z , Pl...} y] (m) &... & λy [{[hIs], Noun...} y] (n) & λy [{z , Pl...} y] (o) & S { l, m, n, o} = x}]

(8) asserts that a stage of A was made up of a number of subsidiary stages, that these stages were ordered such that l was prior to m etc., and that the first of these stages ('l') was "the intention of uttering the noun pronounced [kænæd-i-æn] unless a rule or rules require modification.

With regard to (b), the fact that the first line of the derivation precedes the second, the second the third, and so on, signifies two things, namely (i) that A went through a series of stages: first a stage which met the satisfaction conditions of 'λx [Gx]', then one that met those of 'λx[Dx]'and so on, and finally a stage which met those of 'λx [Ox]', and (ii) that each of these stages brought about its successor through a process modulated by the phonological rules internalised by A. Thus what the derivation as a whole expresses about As utterance is also expressed by the conjunction:

(9) λx [Gx] (A_1) & λx[Dx] (A_2) & λxy [motivate xy] (A_1 A_2) & ...λx [Lx] (A_m) & λx [Ox] (A_n) & λxy [motivate xy] (A_m A_n) & ¬($\exists x$) { λxy [motivate xy] (A_n x)}

In (9), 'lxy [motivate xy]' is a diadic predicate whose satisfaction conditions are met by a pair of speaker stages iff (if and only if) the first member of that pair brings about the second member, and does this through a causal process modulated by internalised phonological rules.

With regard to (c), the account must be reconciled with the fact that the derivation in (1, 1a) is pertinent to indefinitely many actual and conceivable utterances beyond the one produced by A. Bromberger and Halle suggest this can be done by interpreting the derivation as not referring explicitly or implicitly to any individual stages at all, but as signifying a *law* or *nomological generalisation* that happens to have been instantiated by A at a particular point in time, but need never have been instantiated to be true.

(10) Δ($\forall z$) { λx [Gx] (z) → ($\exists u$) { λx [Dx] (u) & λxy [motivate xy] (zu)} &... ($\exists v$) { λx [Lx] (v) & ($\exists w$) { λx [Ox] (w) & λxy [motivate xy] (vw)} & ¬($\exists t$) { λxy [motivate xy] (wt)}}}

(10) states that to any stage satisfying the predicate of the first line, there must correspond a stage satisfying the predicate of the second line and motivated by the former, and so on, and a stage satisfying the predicate of the last line that is motivated by a stage satisfying the predicate of the penultimate line, but motivates no further stage. (10) does not refer to any particular entity or entities. The law has a domain, but it can be true even if no entity in that domain meets its antecedent conditions.

Having thus defined the semantic contents of the phonological sign in traditional serial-derivational theory, it is simple to show that the meaning of similar symbols is entirely different in an optimality-theoretic approach. As before, the initial 'k' of the optimal output candidate is taken to stand for a predicate true of a particular stage of A (at a particular place and point in time), and true of that stage because it was a stage of *intending* to move the articulators in certain specific ways. But the predicate for which 'k' is a stand-in in the tableau cannot have exactly the same satisfaction conditions as the 'k' in the derivation, since the definition of the latter presupposes the possible intervention of rules optimality theory has no rules in the serial-derivational sense. For the same reason that (11) is inappropriate for the 'k' in the derivational theory derivation, it is also inappropriate for optimality theory:

(11) $\lambda x\ [\mathrm{k}x] =_{\mathrm{df}} \lambda x\ [\mathrm{dorsal}\ x]\ \&\ \lambda x\ [\text{-cont}\ x]\ \&\ \lambda x\ [\text{-voice}\ x]\ \&\ \lambda x\ [\text{-nasal}\ x]$

But the way out of this problem is similar to that used for derivational theory, but with a different unless clause, the meaning of this one instead being 'unless not optimal'.

(12) $\mathrm{l}x\ [\mathbf{k}x] =_{\mathrm{df}} \mathrm{l}x\ [\mathrm{uno\ dorsal}\ x]\ \&\ \mathrm{l}x\ [\text{uno -cont}\ x]\ \&\ \mathrm{l}x\ [\text{uno -voice}\ x]\ \&\ \mathrm{l}x\ [\text{uno -nasal}\ x]$

The 'k' in the input to GEN records that at some initial stage A intended to perform-a-certain-gymnastic-unless-this-would-not-be-optimal etc. The same letter in the winning output candidate records that at a final stage A had the same intention. However, in a similar way to the derivational theory account, the optimality theory account still needs to be reconciled with the fact that (a) phonological symbols do not occur in isolation, but occur in an ordered sequence with other symbols in whole lines, (b) each of these lines occurs in a tableau and (c) the tableau as a whole is relevant not only to the particular utterance produced by A, but also to indefinitely many other actual and conceivable utterances.

As far as (a) is concerned, the solution is the same as for derivational theory, but the corresponding predicates are defined using "uno" clauses instead of "upsr" clauses:

(13) $\lambda x\ [\underline{\mathbf{O}}x] =_{\mathrm{df}} \lambda x[\ (\exists r)\ (\exists s)(\exists t)\ \ldots\ (\exists u)\ (\exists v)\ \{\ r > s\ \&\ s > t\ \&\ \ldots \&\ u > v\ \&\ \lambda y\ [\underline{\mathbf{k}}y]\ (r)\ \&\ \lambda y\ [\underline{\wp}y]\ (s)\ \&\ \lambda y\ [\underline{\mathbf{n}}y]\ (t)\ \&\ldots\ \&\ \lambda y\ [\underline{\wp}y]\ (u)\ \&\ \lambda y\ [\underline{\mathbf{z}}y]\ (v)\ \&\ \mathrm{S}\ \{\ r, s, t, \ldots, u, v\} = x\}]$

(13) asserts that a stage of A was made up of subsidiary stages, that these stages occurred chronologically, and that the first of these stages was a **k**, the second was a $\underline{\wp}$, etc. Input to GEN as a whole stands for a predicate similar to (8), but again with the derivational theory phonological constituents replaced by the corresponding phonological predicates of optimality theory, that is predicates which are defined with "uno" clauses instead of "upsr" ones. Whilst the forms of (8) and (13) imply that they can be true together, the non-overdetermination thesis dictates that this cannot be.

With regard to (b), the relation between the input and the winning output of GEN is reminiscent of the relation between the first and the last line of the derivation. But, unlike for derivational theory, this relationship cannot be expressed by the diadic predicate '<u>motivate</u>', since this was defined as a transitive relation whose occurrence is modulated by phonological rules. Instead a new diadic predicate is required:

(14) $\lambda xy\ [\underline{\mathbf{motivate}}\ xy]\ (A_1'\ A_2')$

So the tableau asserts:

(15) $\lambda x[\underline{\mathbf{G}}x]\ (A_1')\ \&\ \lambda x[\underline{\mathbf{O}}x]\ (A_2')\ \&\ \lambda xy\ [\underline{\mathbf{motivate}}\ xy]\ (A_1'\ A_2')$

Similarly for (c), the fact that the tableau is relevant beyond A's particular speech event can be expressed as a *law* or *nomological generalisation*, that happened to be instantiated on that occasion, that may have been instantiated on many other occasions, but that might never have been instantiated whilst still being true:

(16) $\Delta(\forall z)\ \{\ \mathrm{l}x\ [\underline{\mathrm{G}}x]\ (z) \rightarrow (\exists w)\ \{\ \mathrm{l}x\ [\underline{\mathrm{O}}x]\ (w)\ \&\ \mathrm{l}xy\ [\underline{\mathbf{motivate}}\ xy]\ (zw)\ \}\}$

(16) states that to any stage satisfying the predicate of the input to GEN, there must be a stage satisfying the predicate of the winning output, and motivated by the former.

In summary, I assume on the basis of the discussion in the first two chapters of my thesis that the respective advocates of Optimality Theory, traditional derivational Generative Phonology and other prominent phonological research programmes such as Declarative Phonology (e.g. Bird 1991) and Government Phonology (e.g. Kaye *et al* 1985) have seemingly always taken it for granted that the phonological symbols on which each of their theories relies have identical meanings. However, if Bromberger and Halle's (1997) argument is sound - and I believe it is - much of the body of work in comparative phonological theory (e.g. Roca 1997) has been distinctly oversimplistic in terms of its definition of the semantic content of the symbols used, and would appear to have largely ignored the Popperian notion of theory-laden observation. This seems to constitute a well-reasoned motivation for a realist, biological-naturalistic foundation for phonological theory based in part on the four assumptions that (i) phonological symbols stand for predicates, (ii) phonological symbols are used unambiguously within any theory, though not necessarily across them, (iii) phonology is a natural science, and about things in this, the natural world, and (iv) the phonetic characteristics of utterances are not overdetermined. Furthermore, it is evident from analyses such as that described above that there is tremendous scope for the future development of an explicitly formalised Phonological Semantics which, as well as putting biological naturalism on a firm formal footing, will indicate once and for all that phonological symbols emphatically do not have identical semantic contents across frameworks which are radically different. If all of this holds, then there of course exists the distinct possibility that the clutch of extant phonological theories are incommensurable (Feyerabend 1975), and if they are, each theory can be seen to be absolutely exclusive claims about the logical, philosophical, and neurological status of phonological representation. But incommensurability is a problematic notion for the realist, as we shall see below.

Returning to our discussion of Popper's realism, it will be instructive to consider and deal with a couple of possible objections to - or rather, misunderstandings of - his position. The first of these, as pointed out by Carr (1990:14), is that Popper's emphasis on the notion of theory-dependent observation could be said to entail his denial of the existence of a theory-external natural world which is to be described by our theories. However, to come to such a conclusion is to deny the importance of metaphysical speculation to scientific inquiry: Popper (e.g. 1959:274) makes the explicit assertion that, in his espousal of a realist stance, he makes the *assumption* that there is a theory-external world, and that adoption of metaphysical positions such as this is inevitable in the construction and evaluation of scientific theories. Of course, the claim to the contrary (i.e. that there is no theory-external world) would be no more or less aprioristic, but it is nonetheless clear that the notion of theory-laden observation does not in and of itself necessitate anti-realism. Theory-laden observation is an inevitable consequence of the intimate relationship between science and metaphysics, and does nothing to prevent the adoption of a biological-naturalist position in linguistic metatheory.

Our understanding of the incoherence of this first possible objection will be made clearer by considering a second. "If ", one might argue, "we accept the idea of the provisional nature of theory construction and refutation, surely this to deny the possibility that there is some kind of semantic continuity between earlier and later conceptions of the same object of inquiry as dealt with in different theories? Surely it must be the case that there are certain core semantic notions which link these conceptions, or even remain essentially unchanged?". According to this objection, the fundamental meaning of the symbol /b/ as defined and used in the SPE serial-derivational theory of phonology would be assumed to retain its essential properties when used in an optimality or government-theoretic approach, apparently *contra* the views of Bromberger and Halle (1997). In other words, the advocate of this position would hold that the fundamental meaning of /b/ has simply been refined in optimality-generative phonology, but that its essential properties, having been defined in an older but nonetheless scientific theory, have remained predominantly unchanged. An extreme version of this position, namely essentialism (discussed in Popper 1963: 97-120), would hold that to define an entity once in a scientific fashion is to isolate its essential properties once and for all. Of course, an inevitable consequence of my adoption of Popperian realism, with its concomitant notion of the provisional nature of theory construction, is the acknowledgement that we cannot expect to incontrovertibly establish what the properties of the external world actually are: we can merely expect that as our theories develop there will be some convergence upon the truth, and that our characterisation of the natural world will become more and more accurate. Indeed, this embodies the further assumption that single theoretical terms do not alter their meaning independently of the broader conceptual framework with which they are located: when an entire network of theoretical assumptions changes, as was the case when optimality-theoretic generative phonology superseded its serial-derivational precursor, so do the meanings of the individual terms located within them. Moreover, since it is impossible to predict just how and in what respects our theories will change in the future, it is similarly impossible to posit which components of the meaning of a particular term should be regarded as prototypical and unchangeable. And furthermore, this kind of argument would appear to have some bearing on the notion of falsificationism if we consider the so-called Duhem-Quine thesis (Duhem 1969; Quine 1953, 1976), which argues that single hypotheses alone are not susceptible to disconfirmation, since a given hypothesis acquires its meaning from an entire network of theoretical assumptions. These are serious objections, and it is unfortunately beyond the scope of this thesis to deal with them in great detail. But the essence of my argument is as follows: whilst one of the main tenets of Popperian realism is the claim that our definitions of /p/ and /b/, and indeed all aspects of our theoretical apparatus concerning the ontology of the natural world, are subject to revision, all this necessitates is the abandonment of the entirely *post hoc* assumption that much of the definition of a particular piece of terminology will remain the same once it is used within a new theoretical framework. We need not deny the possibility that there is some kind of semantic continuity between earlier and later conceptions of the same object of inquiry as dealt with in different theories: all that needs to be eschewed is the idea that similar scientific terms do not need to be redefined - and their new or modified meaning made explicit - when used within a new theoretical framework. Indeed, it is quite conceivable that similar terms within different frameworks which offer descriptions of the same type of entity will exhibit some kind of 'semantic continuity'. For example, in Bromberger and Halle's (1997) analysis, the only main difference between the semantic content of a phonological sign at the systematic phonemic and GEN-input levels is the presence of either a 'upsr' or 'uno' clause respectively, in the face of many similarities. All that the advocate of this particular interpretation of Popperian realism must ask is that we make no *a priori* assumptions that whole or partial semantic continuity will be the case, bearing in mind that all of our scientific observations are, by necessity, theory-laden. Of course, all of this entails that biological naturalism must distance itself from the strong notion of incommensurability as put forward by Feyerabend (1975) and Kuhn (1962, 1970, 1976), which states that the terms contained within different scientific theories have entirely different sets of referents. Once again, there is no need for the biological-naturalist to deny the possibility of cross-theoretical semantic continuity: all they must reject is the assumption that they can isolate in advance what that semantic continuity will be.

It is still apparent that there is much here to which a non-realist could level objections. For instance, given the falsificationist nature of the realism put forward here, he or she could conceivably claim that, by emphasising the fact that falsification is heavily theory-dependent, I am almost forgoing my right to claim that scientific theories are potential descriptions of reality, and am an instrumentalist rather than a realist. Whilst I am convinced that the argument in this section does much to discredit this kind of objection, I shall now turn my attention to the disadvantages of the main possible alternative to realism: instrumentalism.

3. The instrumentalist alternative

By direct contrast with Popperian realism, the variety of approaches to the philosophy of science which might generally be characterised as 'instrumentalist' deny that all of the statements of a particular theory purport to be true of the world. In other words, a particular phonlogical theory need not be 'of-phonology'; the most important thing is that it is 'of-use-to-the-phonologist'. For the instrumentalist, Government, Declarative or Optimality approaches to phonological theorising merely provide alternative symbol-manipulating recipes which are capable of computing certain outputs from certain inputs, but which do not claim to contain realistic information beyond that contained in outputs, and sometimes the inputs. Tableaux and derivations are taken to have an *instrumental* value, and it is not considered to be a matter of any great importance that the intermediate representations which exist within either theory should claim to reflect any kind of psychological, biological or linguistic reality. In short, instrumentalists ask whether a particular description is *warranted*, rating economy, efficiency and effectiveness of description in terms of its instrumental as opposed to its realistic value. As I have implied in the last section, a significant proportion of the objections to realist interpretations of scientific theories have centred on the relationship between observation and theory, particularly the relation between certain theoretical constructs and the observable facts. For the instrumentalist, science does in some serious sense consist in the steady accretion of known facts, and since it is the facts which count, theoretical constructs are therefore best regarded as nothing more than a means of achieving this end. One of the key points upon which instrumentalists and realists differ, then, is that the former explicitly reject any attempt to elevate theoretical constructs beyond their status as mere postulates so as to claim that they refer to some extra-theoretical reality over and above the observable facts. In other words, any metaphysical content in scientific theories is to be avoided. The works of Mach (1966) and Duhem (1953, 1969) *inter alia*, are replete with these kinds of argument.

In order to demonstrate how Popperian realism is able to withstand and overcome arguments for an instrumentalist view of science, it will be fruitful examine a number of views which may be broadly grouped under the heading 'transcendence of data by theory' (Worrall 1982; Carr 1990). The meaning of this phrase will become clearer if we consider a hypothetical argument for instrumentalism, which might run as follows:

"It is not unreasonable to assume that, in any area of scientific inquiry, there will be more than one theory capable of providing a plausible account of the facts. Surely this would suggest, then, that we ought to be rather wary about the kinds of claim we make as to the ontological status of certain constructs in a given theory, and that we should be especially wary of any interpretation of these constructs which claims that they are descriptions of some underlying reality?".

In other words, our hypothetical instrumentalist concurs with Duhem that since theories transcend or are underdetermined by data, this should be taken as a good reason to make only fairly modest claims as to the status of our theoretical constructs. Our hypothetical instrumentalist would doubtless also agree with Quine (1953:19), according to whom all theoretical constructs, including those which refer to physically real objects, should be taken to be 'myths':

'...viewed from within the phenomenalist conceptual scheme, the ontologies of physical objects and mathematical objects are myths. The quality of the myth, however, is relative; relative, in this case, to the epistemological point of view. This point of view is one among various, corresponding to one among our various interests and purposes'.

Quine (1953:19)

This kind of view is very closely connected to the pragmatist strand in the thought of many prominent instrumentalists (see Quine 1953:79) which holds that theories are best evaluated according to the purposes of the scientist, rather than in terms of their correspondence, or degree of correspondence, to some assumed extra-theoretical reality. Indeed, he frequently links the two notions, as in this particular broadside on physicalist realism:

'...the myth of physical objects is epistemologically superior to most in that it has proved more efficacious than other myths as a device for working a manageable structure into the flux of experience.'

Quine (1953:44)

Of course, a complete unpacking of the term 'instrumentalism' could constitute a thesis in itself, so I shall limit the discussion here to the core issues pertaining to the instrumentalist/realist debate. From the arguments considered so far, we are now able to come up with general definitions of the two theses which broadly sum up the instrumentalist approach to the philosophy of science:

(17) The Underdetermination Thesis

For any given theory, it is possible to construct an alternative, logically distinct theory which entails the same body of data.

(18) The Duhem-Quine Thesis

By means of adjustments to some part (or parts) of the theory, or to the background knowledge within which it is located, any theory can be protected from falsification when faced with contradictory evidence.

Having given a general characterisation of instrumentalism, the question must now be addressed as to how the realist can justify his or her position in the face of such arguments. I shall discuss this in four parts, with regard to (i) the interpretation of theories as myths, (ii) the pragmatic method of theory-assessment, (iii) the underdetermination thesis and (iv) the Duhem-Quine thesis.

With regard to the interpretation of theories as myths, this kind of argument should cause no great concern for the Popperian realist. Since the version of realism I adopt admits of the fact that our scientific theories must by necessity start out as myths, hypotheses or conjectures, this in itself does not constitute an argument against realism. Whilst it is crucial to maintain a distinction between scientific and non-scientific hypothesis, with only the former being testable and potentially falsifiable, this particular brand of realist thought is entirely compatible with the acknowledgement that scientific theories have their roots in 'metaphysical research programmes'. Of course, given the criterion of falsifiability, even Quine would have to admit that scientific myths are epistemically superior to non-scientific ones, but he would not have to concede that they thus shed any light on the as-yet-unexperienced extra-theoretical world. The main point to emphasise here is that the whilst the realist makes the *a priori* decision that there is more likely to be an extra-theoretical world in existence than none at all, the instrumentalist does not. For Quine, all we are warranted in claiming is that scientific myths are epistemically superior. Against this, the realist could argue that it is hardly likely to be mere chance that certain constructs or sets of constructs appear to be more fertile, exhibiting a greater explanatory power than others; they must, in some sense, be closer to the truth. However, instrumentalists do not permit themselves to regard things in this manner, but at the root of this forced reticence is a serious incoherence in their position: since the instrumentalist does not admit of the possibility that scientific theories can be descriptors of the real world, he or she therefore has no yardstick with which to measure the epistemic superiority of one theoretical construct over another. By contrast, the realist is able to describe the success of a particular construct with regard to the extent to which it is a fair approximation of the organisation of part of the assumed extra-theoretical world.

Similarly, it can also be argued that pragmatism as a means of theory-assessment is not incompatible with realism. The argument that changes in a theoretical framework should be evaluated from a pragmatic point of view can easily be seen to have a somewhat tenuous anti- or non-realist basis, especially once it is appreciated that the covert goal of most varieties of pragmatism is to reveal something about the structure of the world. Moreover, to claim that most varieties of scientific pragmatism do not have their roots in realism is to underestimate the importance of heuristic factors in scientific activity: if a given metatheoretical approach consistently gives rise to theoretical constructs which are corroborated by the observed facts, this would naturally suggest that some modicum of verisimilitude is being achieved. Indeed, any instrumentalist attempt to weaken to weaken the superior heuristic value of a realist approach to linguistic research can be countered with the assertion that, in the absence of perceived success in scientific discovery as the criterion by which to evaluate a particular metatheoretical approach, the instrumentalist is left with precious little foundation for his or her standpoint.

As far my discussion of the underdetermination thesis is concerned, I shall follow Newton-Smith (1978) and Carr (1990) and distinguish between weak and strong interpretations. The strong version, as advocated by Quine, holds that all theories are underdetermined by the data of which they attempt to make sense, with the consequence that for each theory there will always be an alternative and logically distinct theory which will provide an equally plausible account of the same data. By contrast, the weak version acknowledges only the possibility that some, but not necessarily all, theories can be underdetermined. In addition, it is especially important to draw a distinction between the concept of absolute underdetermination as employed in abstract discussions of the philosophy of science, and the kinds of situation which often arise in the practical scientific world - such as that discussed in Chapter 2 - where there seems to be an almost insurmountable difficulty in choosing between alternative accounts of the same set of phenomena. As both Worrall (1982) and Carr point out, the sense in which theories are underdetermined by data can simply be regarded as a trivial consequence of the transcendent nature of the theories which we postulate. Indeed, underdetermination can even be taken to be a feature of scientific theorising which supports realism, since it would seem to weaken the instrumentalist's inductivist claim that observation is methodologically privileged with respect to theory. Furthermore, it is also apparent that there is a serious problem with the notion that two theories can be perfectly aligned with respect to the same set of data, as implied by the strong version of the underdetermination thesis. Since two competing theories are never identical, it is clear that *a priori* criteria such as simplicity, unity or internal coherence will inevitably guide the scientist towards deciding which of the theories is better warranted by the data; a situation which would only serve to underline the indispensable importance of metaphysical considerations to scientific activity, as well as the superior heuristic fertility of a realist approach.

None of the points considered so far would seem to pose any serious problems for a realist approach to linguistic-phonological metatheory, but this is not the case with respect to the Duhem-Quine thesis, or at least not at first blush. For the instrumentalist, as will be clear by now, theories are neither true nor false, only more or less warranted by the data in question. By contrast, Popperian realists cannot claim (in verificationist fashion) that any theory is indubitably true, but only that all theories are in principle falsifiable, and that only a theory which has been refuted can be said to be false. However, if, as the Duhem-Quine thesis appears to posit, falsification is not possible, the position occupied by this particular ilk of realist would no be longer tenable since we would have to accept that our scientific theories can neither be verified nor falsified. In other words, if falsification is not possible, then realism must be abandoned. Of course, such a claim amounts to a complete misunderstanding of the Duhem-Quine thesis. First of all, it is essential to point out that to 'adjust' some part or parts of a particular theory is to effectively postulate an entirely new - albeit similar - theoretical framework. Therefore, to pursue such a practice is not to protect a particular theory T from falsification, but instead to acknowledge that the theory in its present state is open to falsification, and that certain alterations are necessary in order for the revised theory T_R to be corroborated by the available evidence. In other words, the fact that $T \neq T_R$ would appear to head off the threat to realism posed by the Duhem-Quine thesis.

With regard to the main issues concerned with the transcendence of data by theory, then, it would appear that realism is able to hold its own in the face of some quite strong arguments for an instrumentalist approach.

4. Conclusion

This paper has made the case for a realist metatheoretical approach to phonological theorisng, and has, using the argument of Bromberger and Halle (1997), illustrated how this approach might be put on a firm formal footing using an explicit Phonological Semantics.

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