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Metatheoretical problems in phonology with the common criteria simplicity (Occam's Razor) and non-arbitrariness (non-*ad-hoc*-ness)

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Introduction

I would like to claim in this paper that certain popular but misunderstood and, more importantly, untestable versions of the supposedly science-creating criteria simplicity and non-arbitrariness (and, consequently, some versions of elegance) are the main reasons for the high degree of unscientificness observable over the last decades in the linguistic discipline phonology. Another aim of this paper is to show how by the use of Karl Popper's criterion testability this inadequacy can be avoided and how scientific versions of simplicity and non-arbitrariness are derivable as theorems and thus become unnecessary as assumptions. It will be part of my findings that the so-called strict CV approach, the hypothesis that phonology is motivated phonetically, Lexical Phonology and Optimality Theory are pseudoscientific consequences of misunderstood versions of simplicity and non-arbitrariness, and that John Goldsmith's approach to scientific research as summed up in Goldsmith (1998) can only be used for the investigation of the sociology, history and psychology of science but not for the establishment of objective knowledge.

1 What is wrong with simplicity, non-arbitrariness and elegance?

To understand what is wrong with simplicity, non-arbitrariness and elegance, it is helpful to have a look at what we usually mean when we refer to these criteria. As will become apparent, elegance, if it is not meant as a purely aesthetic criterion, is a mixture of flawed versions of simplicity and non-arbitrariness:

(1) Common but *unscientific* version of the supposedly science-creating criteria simplicity, non-arbitrariness and elegance

Simplicity: also called "economy" or "density"; the theory containing fewer assumptions is simpler and, because of that, better, scientifically speaking; often (a misunderstood version of) Occam's Razor is referred to support (this kind of) simplicity or economy.

Non-arbitrariness: in this case one believes that *ad hoc* is bad and that therefore assumptions from which observable facts can be derived are scientifically supported by the very existence of these facts, because one assumes that a logical derivation separates assumption from derived facts via abstraction and that thus such an assumption cannot be called *ad hoc*. In other words, the more abstract an assumption and the more observations it accounts for the less *ad hoc* and the more scientific it is, so one thinks.

Elegance: this unscientific criterion is a mixture of the unscientific versions of simplicity and non-arbitrariness as described above (if it is not understood as a purely aesthetic criterion): the fewer assumptions one has to make (simplicity) and the more abstract the assumptions are (non-arbitrariness), i.e., the more observations they account for, the more elegant a theory is. Also, in opposition to testability, elegance as an aesthetic criterion is not objective; testability, on the other hand, a criterion that is defined by a logical relation between an assumption and the class of possible and impossible basic statements predicted by it, is by that very logical definition not subjective. If elegance is understood as an aesthetic criterion, it is subjective and then, like all things subjective, not argueable.[1]

Let me be quite clear about this: I have nothing against simplicity or non-arbitrariness (other than that I consider these criteria unnecessary, as I will show below). However, the versions as described in (1) are as unscientific as they are popular. As examples of the popularity of simplicity and non-arbitrariness I will discuss the so-called strict CV approach as prosodic subtheory of standard Government Phonology (for misunderstood simplicity), and the Phonetic Hypothesis, Lexical Phonology and Optimality Theory (for misunderstood non-arbitrariness). I will finish with a discussion of a set of "key aspects" of science mentioned in Goldsmith (1998). As I will show, none of them has any scientific value, and two (or three) are Goldsmith's adaptations of the flawed versions of simplicity, non-arbitrariness and elegance presented in (1).

2 Simplicity

2.1 The dilemma

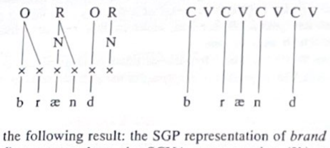
In the following, I will investigate the prosodic subtheories of two closely related phonological theories which differ precisely in relation to the assumptions which they make about prosody: standard Government Phonology ("SGP", cf. Kaye, Lowenstamm & Vergnaud 1990) and the strict CV approach ("SCVA", cf. Lowenstamm 1996). I should point out that I provide this comparison merely as an example of how the simplicity criterion can be misused, even though it of course involves a deconstruction of the SCVA.

To this end, let us compare the syllable[2] structures of the English lexeme *brand* as they are proposed by SGP (2a) and the SCVA (2b):

[1] Cf. Popper (1972, 1973) for a critique of subjectivism.

[2] Neither in SGP nor in the SCVA, there exist syllables *qua* constituents. "Syllabic structure" is thus, as far as these theories are concerned, a synonym for "constituent structure".

(2) a. *brand* (SGP) b. *brand* (SCVA)



We note the following result: the SGP representation of *brand* (2a) contains one (domain-final) empty nucleus, the SCVA representation (2b) contains two additional (domain-internal), i.e., all in all, three empty nuclei.[3]

Supporters of the SCVA like to point to the *greater degree of simplicity* that their deviation from SGP supposedly entails.[4] They do not have to propose a constituent rhyme after all! So they have less *theoretical machinery* because they make fewer assumptions, thus their approach is better for metatheoretical reasons, they believe.

In addition, SCVA enthusiasts think that in a version of Government Phonology ("GP") which has not been modified in an SCVA kind of way, e.g., in the standard version (SGP), both constituent structures given in (2) are possible for English *brand*—the form in (2b) adjusted to SGP such that "Cs" and "Vs" are substituted for "Os" and "Ns" (each dominated by "R") and that the missing skeletal points are added—so that one always has too decide, as a linguist investigating English or a child acquiring it, whether one wants to or has to propose a structure with branching, as in (2a), or one without, as in (2b).

What a dilemma!

If we do not even allow both possibilities we gain the advantage that we can avoid this dilemma (if you want to believe SCVA supporters), and, since the SCVA makes fewer assumptions (no rhyme constituent and no branching), it is the *simpler* and therefore better theory. After all, Occam's Razor also tells us that the simpler theory is the better one. There we have it!

Before we deal with the simplicity criterion, we should become aware about the fact that also in SGP there is something that is simpler than in the SCVA: a structure with branching contains fewer empty nuclei than one without branching.

So we ask ourselves: what is simpler now (has more simplicity)? How can we weigh the absence of rhymes and branching against the absence of certain empty nuclei?

To solve this problem it is useful to investigate from a metatheoretical perspective Occam's Razor ("OR"), on the one hand, and the notion branching, on the other. This will make it obvious (a) that the dilemma of deciding between (2a) and (2b) as described above does not even arise if one applies OR correctly, and (b) that by allowing branching SRP does not sanction any metatheoretical complication that the SCVA does not have to sanction too.

[3] The SCVA calls nuclei "vowels" because its supporters do usually not assume the skeleton. Even though a more detailed discussion of the evidence for the skeleton goes beyond the scope of this paper, I would like to point out that the skeleton is not automatically made unnecessary via an elimination of the rhyme constituent. For example, the differentiation between positional and positionless onsets/consonants (i.e., onsets/consonants with or without a skeletal position) could also be made in the SCVA even though this is usually neither the case nor is it discussed.

[4] For example, Lowenstamm 1997; Krysztina Polgárdi, p.c., Leiden, 1999; John R. Rennison, p.c., Johannesburg, 2000; Grażyna Rowicka, p.c., Leiden, 2000.

2.2 The solution of the problem, or why there is no dilemma of decision

In the following I will support Popper's proposal of testability as criterion of demarcation between science and pseudoscience and, based on this, explain which version of OR is scientific and which ones are not.

Testability means that a scientific hypothesis must make predictions which, if actually achieved, falsify this hypothesis.[5] Popper (1994: 88) formulated this in the following way:

(3) Testability (Popper)

"…every scientist who claims that his theory is supported by experiment or observation should be prepared to ask himself the following question: Can I describe any possible results of observation or experiment which, if actually reached, would refute my theory? If not, then my theory is clearly not an empirical theory. For if all conceivable observations agree with my theory, then I cannot be entitled to claim of any particular observation that it gives empirical support to my theory."

Since, when dealing with the simplicity criterion, one often refers to OR (which is also called "law of parsimony" or "law of economy"), let us have a brief look at what OR actually says and where it comes from. OR is named after William of Ockham (1285–1347/49), with "Occam" as latinised version of "Ockham"), who used it mainly for the discussion of metaphysical questions.[6]

(4) Occam's Razor (my translations)

Pluralitas non est ponenda sine neccessitate	Plurality/multiplication must not be set up without necessity
Frustra fit per plura quod potest fieri per pauciora	Without reason/base happens through more what can happen through less
Entia non sunt multiplicanda praeter necessitatem	Entities must not be multiplied beyond necessity

All three quotes denote more or less the same thing. The problem with the simplicity criterion is made apparent in that it is matters what precisely one means by "entities" (to use the language of OR).

My claim is this: Had Lowenstamm employed a more scientific, i.e. testable, version of entity, his dilemma of choosing between (2a) and (2b) would not even have arisen. In other words, Lowenstamm made a mistake as regards how he understood

[5] The subjection of research to falsifiability discussed and supported here does not entail that it must be conclusively shown for a theory that, *summa summarum*, it is false. That such a demand would not be sensible follows from the fact that it is logically impossible to conclusively show for a theory that it is false, because each conclusive falsification of this kind can be avoided by the use of *ad hoc* hypotheses. Popper's call for falsifiability merely refers to a logical relation between a theory and the class of basic statements following from it and, in this way, the class of potential falsifiers of this theory. In the following will investigate this logical relation in more detail. A well-written discussion of this problem can be found in Popper [1956] (1983: xix–xxv, especially xxii).

[6] For more details, cf. Hyman & Walsh (1973) and Thorburn (1918).

simplicity in that in his approach the wrong thing is simple and that which should be simpler is more complicated. In order to understand this, let us look at two versions of simplicity which can be formulated by referring to two versions of entity:[7]

(5) Two kinds of Occam entities—two kinds of simplicity

a. Entity as *universal assumption*:	*Assumed universal* entities must not be multiplied beyond necessity
b. Entity as *existential prediction*:	*Predicted existential* entities must not be multiplied beyond necessity

That simplicity in relation to predicted existential entities (5b) is scientifically sound while simplicity in relation to assumed universal entities (5a) is not, can be motivated by looking more closely at how the structure of a scientific argument is set up and on the basis of what an assumption within an argument becomes testable:

Each argument contains a generalisation, i.e., a universal statement, the truth of which is assumed (e.g., *Water boils at 100 degrees centigrade*), an initial condition/situation,[8] i.e., an existential statement, the truth of which is also assumed (e.g., *The water in the pot standing in front of me has a temperature of 100 degrees centigrade*), and a consequence/theorem, i.e., an existential statement, that can be logically predicted via logical derivation from the assumed universal and existential statements (e.g., *The water in the pot standing in front of me boils*).

The generalisation is testable in that each affirmative universal statement corresponds to a negative existential statement. For example, *Water boils at 100 degrees centigrade* corresponds to *There is no situation such that water does not boil at 100 degrees centigrade*. If it is possible to observe a case in which the corresponding affirmative existential statement is true (*There is a situation such that water does not boil at 100 degrees centigrade*), one has discovered a logical contradiction ($A \wedge \neg A$), which means that the affirmative existential and the negative existential statement cannot both be true in the same world. In other words: the truth of the affirmative existential statement proves wrong the negative existential statement *and thus the affirmative universal statement*. In this way, the existence of certain entities can logically disprove the truth of certain universals. (For more details, cf. Popper 1934 or

[7] I should point out that even though OR is meant to prevent theoretical redundance it does not entail that there is no redundance in nature. That is to say, no version of simplicity or OR predicts universal non-redundance. It is important to mention this because Hale & Reiss (2000a,b) do not clarify whether they understand this point. Hale & Reiss argue that, because of OR, phonology should not encode any phonetic details. On the other hand, since redundance in nature is not uncommon and can thus not be excluded *a priori*, and since the phonology of natural languages is a natural phenomenon, I would have expected that Hale & Reiss' paper to be mainly concerned with the question whether there is evidence for natural redundance in the relationship between the phonetics and the phonology of natural languages. This expectation however is hardly met in Hale & Reiss' essay. Neglecting this, it is also disappointing that Hale & Reiss (2000b) only contains references to Jonathan Kaye's and Stefan Ploch's research because Ploch had complained to the editor-in-chief and the editor of the squib section of the journal *Linguistic Inquiry* about the apparent lack of such references, and that these references had been erased again from the so-called "extended version", i.e., (2000a), which was published by Oxford University Press. The precise sequence of events and Ploch's dispute with Oxford University Press (regarding 2000a) will be made public on Ploch's university webpage (languages.wits.ac.za/~stefan) or at some other place yet to be announced on said webpage.

[8] It is metatheoretically irrelevant whether, terminologically, an assumed universal statement is viewed as belonging to the initial condition/situation.

cally disprove the truth of certain universals. (For more details, cf. Popper 1934 or 1959).

We note: an assumption is testable not by or in itself but only via the *existential statements it predicts*. Therefore it is possible to recognise a pseudoscientific simplification as such when it increases the number of predicted untestable entities, i.e., existential statements for which no contradictory existential statements could even be predicted or specified in theory.

Let us now look at the simplification proposed by Lowenstamm: is the increasing number of predicted empty nuclei in Lowenstamm's approach tantamount to an increasing number of untestable entities, i.e., entities corresponding to untestable existential statements? The answer is "yes": while the test for those empty nuclei which are predicted by both SGP and the SCVA consists in the demonstration of syncope/epenthesis, there is no test for the very empty nuclei which are additionally predicted by the SCVA.[9] Wee see that the higher degree of simplicity as regards assumed universals (no rhyme constituent, no branching) is achieved by an increase of untestable predicted entities (more untestable empty nuclei). Lowenstamm's dilemma is therefore imaginary, i.e., it does not arise. The structures in (2a) and (2b) are after all not metatheoretically identical: (2b) contains *ceteris paribus* more non-science and need and should thus *ceteris paribus* not be taken into account.

We learn that each theory can be simplified by decreasing its scientific content. So one could for example account for every phenomenon by assuming God and eliminating all other assumptions and in this way create a (pseudoscientific) theory of unmatched simplicity. However, if we subscribe to testability as criterion we also achieve a(nother) form of simplicity, i.e., simplicity in relation to predicted untestable entities, without having to additionally refer to the simplicity criterion. On the other hand, if we do not subscribe to testability, the simplicity criterion quickly becomes a tool of pseudoscience.

2.3 The metatheoretical "complication" of allowing branching structures

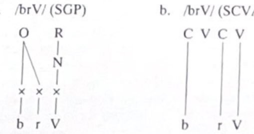
I have been told by supporters of the SCVA on numerous occasions (cf. note 4) that the elimination of branching constitutes a simplification "as such", i.e., branching is seen by them as a theoretical mechanism that complicates a theory, because of which the SCVA, which can do without branching, is supposedly the simpler and therefore better theory. Neglecting that these supporters never provide any details as regards what they mean by simplicity and that we have already seen in what way the version of simplicity they refer to elsewhere, i.e., in relation to assumed universal and not predicted existential statements, is metatheoretically flawed, I want to show in this section why the view that the allowing of branching complicates a theory is mistaken

[9] In my opinion, even the empty nuclei that are proposed by both SGP and the SCVA, i.e., those motivated by epenthesis or syncope, and thus, in the end, all empty nuclei, are not testable, even though those empty nuclei that are motivated by vowel-zero alternations are not as untestable as the ones discussed in this section. So note that there exist degrees of falsifiability. However, the validity of my argumentation against the empty nuclei that are assumed by the SCVA additionally to the ones that also occur in a SGP analysis is similar to but not dependent on my discussion of those empty nuclei that correspond to vowel-zero alternations. Let me put it like this, while there is at least some kind of test for the empty nuclei that correspond to vowel-zero alternations, a test whose scientific value I do not rate highly, there is not even this test or any other test whatsoever for those empty nuclei that never manifest their presence via realised vowels. A more detailed discussion of "alternating" empty nuclei is unfortunately beyond the scope of this paper.

and why branching only implies theoretical machinery that the SCVA has to allow for too. I will also explain in what way this inadequate interpretation on the part of SCVA supporters of the metatheoretical status of branching is an essentialist mistake.

To start our discussion of the SCVA variety of essentialism, compare the following two structures, which are the domain-initial details of the structures in (2):

(6) a. /brV/ (SGP) b. /brV/ (SCVA)



Our argumentation above has already made it clear that there is a domain-internal empty nucleus in (6b) that does not occur in the structure in (6a) and that is not testable via reference to vowel-zero alternations. (Again, I neglect here that there are problems with the testability of empty nuclei corresponding to phonetically realised vowels too, problems that I will neglect here; cf. note 9.) Other than that, we also see the use of a concept, i.e., branching, that the SCVA cannot refer to. One's first impression could be that, even if we grant that the increase of untestable domain-internal empty nuclei apparent in the SCVA is unscientific, the assumption of branching as a concept in SGP makes the SGP more complicated (in this respect) and the SCVA simpler and therefore, *ceteris paribus*, preferable (in this respect). After all, SGP needs a whole mechanism that the SCVA can do without!

This view is mistaken simply because the SCVA uses branching too, as weird as this may sound; or let me put it the other way round: SGP does not apply any theoretical machinery by the usage of branching that the SCVA does not employ too. The reason for this has to do with branching being a somewhat imprecise term for a relationship that SGP assumes, and it is this relationship that we can also find in the SCVA. The relationship I am referring to is of course licensing (with its subtypes constituent government and transconstituent government, amongst others, cf. Kaye, Lowenstamm & Vergnaud 1990). As SGP views so-called branching constituents as a phenomenon involving two skeletal points that are in some kind of asymmetrical relationship (which in both SGP and the SCVA is called licensing), the SCVA also explains the phonotactic restrictions made evident within or between so-called branching constituents in terms of licensing relationships (not necessarily between skeletal points though, but cf. note 3).[10]

Since I consider this type of mistaken human "logic" a common source of pseudoscience, let me briefly explain what the problem is and how it can be avoided. I will start by giving it a name: *conceptualism*. (To my knowledge, this is my term for this kind of logical mistake.) Importantly, it is in my view a subtype of what Popper

[10] Some supporters of the SCVA approach, namely Neubarth & Rennison (in preparation), only assume head-final licensing relationships and account for head-initial relationships that would otherwise have to be proposed (e.g., those that are called constituent government in SGP) via phonological units whose phonetic manifestation includes contouring, i.e., a sequence of sounds. This difference has however no bearing on the present discussion.

kind of logical mistake.) Importantly, it is in my view a subtype of what Popper referred to as *essentialism*.[11]

To understand this, let me now explain what essentialism is, then in what way conceptualism is a type thereof. It should suffice for our purposes to point out that "essentialism upholds the view that science must seek ultimate explanations in terms of essences: if we can explain the behaviour of a things in terms of its essence—its essential properties—then no further question can be raised". An essence is a type of ultimate explanation that Popper (e.g., 1983: 135–136) rejects because it can never be shown for any explanation that it is ultimate. In other words, proposing an essence is a question-stopper, while science consists in finding new questions. Another reason Popper provides for rejecting essentialism is based on his opposition to *what is* questions, say, *what is branching?* The reasons for this are explained in more detail in Popper [1963] (1972: chapter 3); however, we can say that the main problem with a question like *what is branching* is that it entails a definitional or conceptual, not a propositional approach to science.

And here we come to what above I have called conceptualism: First, if we use a definition, we need terms for that definition, which in turn needs terms that need to be defined, and so on; in other words, definitions lead to an infinite regress and are not useful. This problem does not arise in the case of propositions (universal and existential statements as discussed in section 2.2): the empirical content of an explanation is dependent on the details that the propositions employed for this explanation contain; an explanation is always as precise as it is but does not lead to an infinite regress. Of course we can always try to find more detailed explanations (possibly infinitely?), but each explanation is valid with as much details as it has; due to the fact that *what is* questions entail an infinite regress, they are not valid in this way and never lead to anything other than more definitions, which in opposition to propositions, have no explanatory value whatsoever.

In addition to that, two different concepts, say, branching (with all its graphical and metaphorical links/connotations) and a licensing relationship (with its partly different graphical and metaphorical links/connotations) can be conceptually, definitionally different but correspond to the same class of true and false basic statements, i.e., be, scientifically speaking, identical. In other words, concepts and definitions (and, in this way, *what is* questions) are misleading in science, while only propositions matter.

To provide a common example, a SCVA supporter pointing to the fact that they do not refer to branching and that this fact is supposedly an advantage makes a mistake (which is not merely terminological, and let me say it clearly, it actually is amongst other things terminological). They will argue this way: "I am not saying that branching does not also entail a licensing relationship, but let us face it: branching and licensing *are* different!" (Note the intonation and stress on *are*.) That is, in their *essence*, they are different! And they would of course be right: branching and licensing *are* different—conceptually speaking, where the conceptual difference between the concepts branching and licensing relationship contains the terminological difference but also others, e.g., a graphical and thus optical difference. And what point of view is not driven home much more effectively by optical means? Is not almost all music, all politics, everything cool and square accompanied today by optical input? There is of course a good reason for this: that which is optically cool, different or pleasing is also stored and thus remembered as such. The linguist who considers branching and a li-

[11] See under "essentialism" in the indices of any of the works by Popper in the bibliography or, more specifically, Popper (1961: 26ff., 1972: 18ff., 103ff., chapter 3 in general, 1983: 134–140).

censing relationship to be conceptually different because they look different is very much like the average boy band fan or the average voter.

Apart from the optical difference between branching and a licensing relationship, branching also entails, conceptually, a division of one into many (or merely into two in SGP) and a hierarchical order with the two participants (skeletal points or segments, etc.) being at the bottom and the constituent at the top, while a licensing relationship focuses conceptually on the asymmetry between the head of the relationship and the recessive member, with one of participants (the head) at the (conceptual but not necessarily optical) top, so to speak. Of course, both a branching constituent and a licensing relationship can, with all their conceptual differences, be propositionally identical and make the same predictions, i.e., are potentially proven wrong and thus tested by an identical set of existential statements.

This means that the claim by (some) SCVA supporters that SGP uses branching while the SCVA does not and that therefore the SCVA is simpler and better is, even if we neglect what they mean by simpler, based on an essentialist, and to be more precise, a conceptualist mistake whose terminological side is just one facet of this problem. Note also that I have now already provided a solution too (which is Popper's solution): do not care so much about definitions, concepts and graphical illustrations but rather about propositions, their predictions and the class of potential falsifiers of a universal statement and thus of an explanation.

Let us ask ourselves now: where does this conceptualist mistake come from, or, why does it come most naturally to humans? What is it about human reasoning that the brain understands reality in terms of scientifically useless concepts, not useful propositions? To my knowledge, Popper has not looked into let alone found an answer to this question, but still, I will try to sketch out an explanation: In Ploch (in preparation 2), a view of human cognition is proposed according to which reasoning consists of the setting up and accessing of cognitive links between cognitive units, i.e., units that the brain can manipulate/reason with; these units ultimately consist in turn of a restricted universal set of cognitive units, say, phonological elements/features, semantic units, etc. Ploch's approach finds its motivation in the search for a testable solution to the question how non-monotonic logic in human reasoning, and particularly in phonology, can be dealt with, i.e., this motivation is independent of the point at hand, i.e., essentialism. In addition, Ploch proposes a notion of *cognitive prominence*: "How cognitively prominent a form is, is [amongst other factors] a function of ... the frequency with which the links it is made of have been used, i.e., there is also a notion of *link strength* which is a function of link frequency."

For our purposes, this means that the brain stores and understands a new concept, say, "branching" or "licensing relationship", by establishing new links to units it already knows, i.e., to sets of already existing links between universal ultimate units or to universal ultimate units themselves, or by strengthening existing mostly idiolect-specific links. In other words, to the storing and addressing system of the human brain, i.e., the phonology and the semantics and their relationship to the lexicon, branching and licensing relationships *are* different, just as supporters of the SCVA maintain. And this is so simply because branching and licensing are, conceptually and thus mentally, different, i.e., non-identical sets of links. This however does not change that testable claims about branching, licensing relationships or any other notion can only be evaluated in terms of their propositional content, not their definitions or conceptual/mental/cognitive make-up. It is precisely where the propositional content of a notion and its mental conceptualisation are non-identical where researchers who are not aware of the definitional/conceptual bias of the human brain make mistakes; off-line, we humans can of course, dependent on our off-line resources, use testability independently of how easy, or rather, how difficult it comes to us.

Important to mention is also that every time we fall into the conceptual trap we make an essentialist mistake, which means that, whether we are aware of it or not, use definitions in our science instead of propositions and, in this way, may see complexity or simplicity where there is none (e.g., when we view the notion branching as complexity-increasing theoretical machinery because it is an additional concept) without being able to prevent ourselves from doing so as long as we go for concepts/definitions, and, in addition, use an also otherwise metatheoretically unsound approach by (explicitly or implicitly) getting ourselves into an infinite regress of definitions, whether we intend this or not, which, in itself, is of no scientific value at any point, no matter how detailed the definitions may become in our search. As I have already pointed out, there is a solution at hand though: proposing testable propositions in combination with constantly deconstructing and comparing various (testable) claims.

In terms of our discussion of simplicity, we can say that conceptualism like any other form of essentialism is a metatheoretically flawed approach to theoretical simplicity.

To finish this section, I would like to point out that what Goldsmith (1998) says about essentialism in an email sent to the cogling discussion group demonstrates that his views on essentialism are mistaken. This is interesting here because, as I will show in section 4, his whole anti-falsificationist stance is part of an approach that avoids science and not only condones but actually creates pseudoscience. For the purposes of this section it suffices however to show that Goldsmith's views on essentialism are flawed too:

(7) Anti-essentialism (Goldsmith 1998)

> "While everyone has read Feyerabend[12] and doubtless been influenced by him [Goldsmith does certainly not speak for the author of this paper as far as the "being influenced" is concerned], the bigger impact on how science is viewed in recent years has come from those who argue that understanding how science works is NOT equivalent to understanding the structure of scientific concepts (and how those concepts relate to observations). They argue that such factors as: relations between science and technology; relations between people in scientific organizations, especially relations of power; history of instrumentation; history and development of the graphical representation of theoretical concepts; and others—that all these are crucial to understanding science, and that when viewed in these ways, science shares many things with non-science, and is distinguished from non-science in many ways. In many circles, this is known as anti-essentialism, meaning that asking what the essence of Science (or anything else) is bound to get you tied up in logical and historical tangles, and is not the right way to ask the question; and we are very much in a period of anti-Essentialism."

In this quote, Goldsmith sets up a dichotomy between the view that science is equivalent to understanding the structure of scientific concepts and how these concepts relate to observations, on the one hand, and the view according to which science is defined

[12] By this, Goldsmith refers to Feyerabend (1975), but cf. also (1987).

in what we could call social, i.e., socio-political and/or socio-cultural, terms (the relation between science and technology, between academics, etc.). It is the latter approach, the social one, that he refers to as anti-essentialism, while the first one, the logical/conceptual one, which is supposedly interested in the essence of science (or Science), is the essentialist approach. Let me point out briefly (but this will become more apparent in section 4) that even in this quote it is not so much important to Goldsmith how scientific research can be justified in terms of logic (which would give us some kind of objectivity) but rather where "the bigger impact" comes from and in what kind of period we are living, i.e., one in which falsificationism[13] or that which Goldsmith absurdly calls essentialism, does not matter. (How Goldsmith supports this social non-logical and thus subjective and antiscientific view, I will show in section 4.) Be this as it may, it should be clear by now that Goldsmith gets it wrong even terminologically: even though he is right in that what he labels essentialist approach (i.e., Popper's approach) is interested in how scientific "concepts" relate to observations (which is supposedly to be avoided because it may result in some "logical tangle"), and let us neglect for the moment that Popper is at great pains in his writings to distance himself from the scientific usage of (definitional) concepts and argues for propositions, Popper's view or the view that science is about objective knowledge based on logic is certainly not essentialist, as I have explained in this section in some detail.

To sum up, on the basis of Popper's non-essentialist (and thus non-definitional) testability criterion, I have shown that by the usage of branching SGP is in no scientifically relevant way more complicated than the SCVA, and that an interpretation of branching as added complexity is an essentialist (i.e., definitional and conceptual) mistake.

3 Non-arbitrariness

I will demonstrate in this section how a metatheoretically flawed version of non-arbitrariness (cf. (1)), which is meant to avoid scientifically useless arbitrariness, typically results in untestable claims.

Ad-hoc-ness/arbitrariness in scientific research arises when untestable assumptions[14] that prevent the falsification of another assumption are employed in order to *immunise* this other assumption *against refutation* ("immunisation against refutation", Popper's phrase, cf., e.g., 1934, 1959). This point should become clearer by looking at the following two examples:[15]

[13] "Falsificationism" is not a term Popper used for his approach, by the way, which is something Popper explicitly states in Popper (1983: xxxi).

[14] As I have already discussed in the section on simplicity, an assumption is testable only via its predictions, not by itself, i.e., not without such predictions.

[15] Cf. "ad hoc hypothesis", "biorhythms", "astrology" in R.T. Carroll's *Skeptic's Dictionary*. The examples/ quotes in (8) are taken from there too.

(8) Examples of unscientificness via arbitrariness (*ad-hoc*-ness)

a. Bio-rhythms:[16] "... there are many people who do not fit the predicted patterns of biorhythm theory. Rather than accept this fact as refuting evidence of the theory, a new category of people is created: the arhythmic."

b. Astrology: "Astrologers are often fond of using statistical data and analysis to impress us with the scientific nature of astrology. Of course, a scientific analysis of the statistical data does not always pan out for the astrologer. In those cases, the astrologer can make the data fit the astrological paradigm by the ad hoc hypothesis that those who do not fit the mold have other, *unknown influences* [my emphasis] that counteract the influence of the dominant planets."

c. Generally: "In short, whenever the theory does not seem to work, the contrary evidence is systematically discounted" (Carroll, "ad hoc hypothesis").

Note that an *ad hoc*-hypothesis consists typically of a reference to unknown influences. It goes without saying that this terminologisation (labelling without scientific/testable predictions) of such influences does not make them one iota more known, testable or repeatable. The common mistake that I want to refer to here occurs when researchers try to avoid arbitrariness as exhibited in (8) and end up getting the flawed version of non-arbitrariness that I presented in (1) into their theories: This is to say that they try to countermand *ad-hoc*-ness by deriving observable facts from assumptions/explanations which they propose because they mistakenly believe that abstraction is the opposite of *ad-hoc*-ness and that therefore the derivation of observable data provides evidence for the generalisation the derivation is based on. As a result, their method consists in pointing to data accounted for by the generalisations proposed by them, while they obviously believe that explanations can be confirmed or verified by some data. This is a justificationist[17] approach to science, which unfortunately cannot work for logical reasons.

It is important in this context to demonstrate to those who are not aware of the problems the notion "confirming evidence" entails why observations cannot confirm assumptions, i.e., why assumptions cannot be provided evidence for by pointing to data they account for, and why an ever increasing amount of such data cannot confirm or probabilify any assumptions. So for example, no matter how many phonological phenomena the PH accounts for, none of them and no amount of them provides evidence for the PH; in addition, no matter how many more pieces of data that the PH can account for will be found in the next few decades as a result of the never-ending

[16] The precise details as regards the claims made by biorhythm theory are not even relevant here. What matters is that falsifying data is systematically discounted in this pseudotheory. However, those who are interested in this theory may want to have a look at Hines (1998) and the references therein. To provide one detail, biorhythm theory claims that life is subject to cycles, each type of cycle with its own specific duration (e.g., the 23-day physical cycle and the 28-day emotional cycle), and that, based on where one's life is situated at the moment in terms of these cycles, every day corresponds to a specific likelihood for each person to do better or worse in specific areas of one's life, which these cycles cater for (one's physical, emotional life, etc., are such areas).

[17] Regarding justificationism, cf. the references in the index of Popper (1983), p. 414.

quest on the part of the supporters of the PH, not one piece of data will make the PH one iota more likely to be true. This means of course that much what is called phonological research, particularly when it is of the confirming-phonetic-evidence kind (e.g., as part of laboratory phonology), is a pseudoscientific undertaking.

Now, on what do I base my claim, which goes against mostly anything that is taught at our free democratic universities, where the PH keeps being part of the phonological syllabus and is considered so established that it simply does not have to be questioned? On logic and mathematics, is the short answer. The more detailed version of this answer looks like this:

Consider the equation in (9):

(9) Two intuitively sound equations

a. $$\frac{x}{10\,000\,000} < \frac{x + 3\,000\,000}{10\,000\,000}$$

b. $$\frac{x}{+\infty} < \frac{x + 3\,000\,000}{+\infty}$$

We all "know", in the intuitive sense, that if we add a number greater than zero, e.g., 3 000 000 to any other number x, then the result of x divided by some other number y, say 10 000 000, is smaller than what we get if we first add that other number greater than zero (3 000 000) to x, and then divide the result of this addition by y. So one quarter is smaller than two quarters is smaller than three quarters. Analogously, if we have observed the sun rise three times and compare this to a situation in which we have observed it rise 10 000 times, we think intuitively that in the second scenario it is more likely that it will rise again. In other words, the more often we observe something the more likely we feel it is to happen again. When we think like that, what we are doing is to induce a universal statement (*The sun rises every morning*) from one or a number of existential statements (e.g., *On 5 November 1998, the sun rose in the morning*). (I presume here that it is clear what we mean by "morning".) Since a universal statement encompasses an infinite number of existential statements (based on an infinite number of observations), i.e., since *The sun rises every morning* is a statement that is generically true and is thus valid for an infinite number of mornings, the observation of three mornings on which the sun rose in comparison to the observation that it rose on 10 000 mornings is a comparison between three or 10 000 mornings *out of an infinite number* of mornings. In other words, when we look for confirming evidence for any assumption (say, *The sun rises every morning* or *The phonology is phonetically grounded*) by finding more and more examples (i.e., observations resulting in existential statements) where our resulting existential statements are existential versions/instantiations of the (universal) assumption that we are trying to prove/confirm, then we very much believe that the equation in (9b) is valid. Given this validity, we also think that the more existential "instantiations" we find the more likely the assumption we are trying to confirm is to be true.

This induction is, however, not valid, as Hume had already found out.[18] It is neither the case that a universal statement can be logically inferred from any number

[18] Cf. Hume ([1739] 1888, [1777] 1966). Hume could not solve the problem connected to the invalidity of inductive reasoning, i.e., how it is possible, then, to count on, say, the sun rising again, as we all do. For Popper's solution to Hume's problem, cf. Popper (1934, 1959), or, for an even more detailed discussion, Popper (1983: chapter 1).

of existential statements nor can the truth of an assumption (universal statement) be made more likely by an ever increasing number of "confirming" evidence (which can, for logical reasons, not be confirming). The reason for this can be made apparent by the equation in (10), which in opposition to (9b) *is* valid:

(10) The definition of the two symbols $+\infty$ and $-\infty$ (excerpt from Apostol 1974:14, section 1.20, definition 1.24)

By the extended real number system **R*** *we shall mean the set of real numbers* **R** *together with two symbols* $+\infty$ and $-\infty$ *which satisfy the following properties:*

a) *If* $x \in \mathbf{R}$, *then we have*

⋮

$$\frac{x}{+\infty} = \frac{x}{-\infty} = 0$$

So any real number divided by (positive or negative) infinity equals zero. Consequently, no piece of data (existential statement) confirms any explanation/assumption (universal statement), and in a comparison of two situations in the first one of which one has observed something happen 5 000 times and in the second one of which, say, 10^{1000} times, it is *not* the case that, in the second scenario, one has provided more evidence for the assumption in question or at least made it more likely to be true. As will become clearer in the following, the fact that this bit of knowledge is news to most phonologists has been the main reason for misunderstood non-arbitrariness: After all, the flawed assumption that an assumption is not *ad hoc* if it can account for large amounts of data stems from the believe that data can confirm assumptions.

What I would like to show in the following is that, in order not to be falsified but to appear non-*ad hoc*, three of the most successful phonological paradigms of the last few decades, i.e., the Phonetic Hypothesis, Lexical Phonology and Optimality Theory, make use of the very tactic that is the hallmark of astrology and so many pseudosciences: the employment of arbitrariness in tandem with a metatheoretically flawed version of non-arbitrariness.

3.1 Pseudoscience, example 1: the Phonetic Hypothesis

In Ploch (1997), the Phonetic Hypothesis ("PH") was described and labelled for the first time, but it was investigated earlier in Kaye (1989) and is discussed, in the most detailed manner, in Ploch (1999b, in preparation 1):

(11) The Phonetic Hypothesis (Ploch 1997; quoted from Ploch 1999b, 1st page of chapter 1)

"... the mainstream view that phonological phenomena ... are motivated by the properties of a phonetically characterised system, e.g. the articulatory or auditory system."

As Kaye has already shown, a falsifiable version of the PH would predict the phonetic and phonological convergence of all natural languages, a phenomenon which cannot

be observed. Alternatively, let us look at this problem this way: if we claim that assimilation with respect to the place of articulation is motivated articulatorily, i.e., by the frequently employed notion *ease of articulation*: how often or in what language types or under which independently established circumstances (i.e., circumstances proposed as such in a non-*ad hoc* manner) can whatever type of assimilation not take place? If Latin *kt* (*doctor*) changes to *tt* (*dottore*) in Italian due to articulatory reasons, why then is it possible for Classical Arabic *kat* (*kataba* 'he wrote') to turn into Moroccan Arabic *kt* (*ktib*, Kaye's examples)? Which force has what kind of influence in what kind of circumstances? Where are the testable statements here?

It is quite clear at his point of our discussion (and shall become clearer in section 4) that it is totally irrelevant as regards whether a proposed argument is a scientific one, what scientists actually do when they do that which they call "science". It is the logical relationships between propositions and its potential falsifiers that matter for our decision whether there is a scientific point to the articulatory (or any other) version of the PH, not whether it is generally recognised by those who consider themselves linguists. The fact that virtually no-one is concerned with checking whether the PH is testable and if so, whether it is already falsified, and that almost everyone keeps looking for more and more cases where the PH works but ignores that in the same way astrology, biorhythms, the prayer-works hypothesis and any imaginable spiritual assumption is "verifiable", is called the strategy of *denial* in Ploch (1997, 1999b).

If one points out to supporters of the PH (i.e., almost all phonologists) such metatheoretical problems, they will make use of an *ad hoc*-hypothesis, i.e., an additional arbitrary (not independently established) assumption which accounts for precisely those cases (*ad hoc* means 'for this') which would otherwise falsify the PH. Interestingly, attempts at saving the PH then often refer to unknown other influences, e.g., one could claim that the articulatorily motivated tendency for assimilation is opposed by an acoustically motivated tendency for discrimination. The problem with this kind of attempt is that it is not testable as long as the supporters of such views do not clarify what kind of acoustically motivated tendency and what quantity thereof balances out or opposes what kind of articulatorily motivated tendency and what quantity thereof. In other words, this kind of pointing to opposing tendencies is not testable. I would like to add that there are no testable versions of such views known to me. It goes without saying that opposing acoustically motivated forces that one cannot qualify, quantify or both, are unknown influences too, which have been labelled as such by supporters of the PH just so that they can continue to pretend that they are involved in scientific research here. The strategy of immunisation of the PH against refutation consists thus in the *ad hoc* non-application of the PH if it would otherwise we disproved. In Ploch (1997, 1999b), this strategy is called *flexibility of applicability*. I should add that there is no scientifically relevant difference between not applying a generalisation and applying it in a way such that it is violated (cf. section 3.3; Ploch, in preparation 2).

Let me provide a comparative example in which I will save the prayer-works hypothesis and the PH by not applying them in an *ad hoc* manner, i.e., whenever they would otherwise be disproved. Prayer works! I have already prayed a great many times, and then it happened! (There are many phonological phenomena that are articulatorily natural.) But what about the cases when it did not happen what you prayed for? (What about the cases where an articulatorily grounded phenomenon does not occur in the phonology?) Well, then God did not want it to happen, or he had more important things to do, i.e., his tendency to answer prayers was opposed by these more important things or by his conscious decision not to answer your prayer because he loves you so much and he knew that answering your prayer would have harmed your soul! (In such a case, the articulatory tendency was opposed by another influence, most likely the strong acoustically motivated tendency to maintain a certain degree of discriminability, in other words, the violable DISCRIMINATE constraint outranked the violable BEGROUNDED constraint.) And why precisely then? (Ditto.) How important does an important thing have to be? (What kind of degree of discriminability must be maintained?) Important enough! (Enough discriminability.) And what is important enough? (And what constitutes enough discriminability?) Always precisely that which ensures that prayer does not give the wanted results (circular argument). (That which ensures that the articulatorily motivated grounding of phonology, a phenomenon that plays a great role in phonology, cannot be detected, in other words, that which ensures that BEGROUNDED is outranked, also a circular argument.) Thus: prayer works! (We see what have seen time and time again: there is evidence for the phonetic motivation of phonology and for constraint ranking!) In this manner, each pseudoscience can be defended, also the PH or the Optimality pseudotheory (also cf. section 3.3). In any case, science this is not.[19]

We realise that the PH is *per se* testable, but that it has been falsified a long time ago (Kaye 1989) and that it is maintained by its supporters by the use of pseudoscientific means (denial and flexibility of applicability).

Now compare the flexibility of applicability tactic with the following quote about the pseudoscientific biorhythm theory:

(12) The unscientificness of biorhythm theory (from Carroll, "biorhythms")

> "An examination of some 134 biorhythm studies found that the theory is not valid (Hines, 1998). It is empirically testable and has been shown to be false. Terence Hines believes that this fact implies that biorhythm theory 'can not properly be termed a pseudoscientific theory.' However, when the advocates of an empirically testable theory refuse to give up the theory in the face of overwhelming evidence against it, it seems reasonable to call the theory pseudoscientific. For, in fact, the adherents to such a theory have declared by their behavior that there is nothing that could falsify it, yet they continue to claim the theory is scientific."

One and the same strategy is employed by the supporters of the PH, as I have shown. The flawed usage of non-arbitrariness here is based on the belief that the PH is not *ad hoc* simply because observable data can be derived from it. Above, I have already explained in what way this view is logically invalid.

Andrew Dolbey once countered[20] that all phonological phenomena are phonetically natural and that therefore the PH is, after all, testable: for if there were phonetically unnatural phonological phenomena, then the PH would be disproved. To this I

[19] For more details, cf. Ploch (1997, 1999) for the PH; for Optimality "Theory", cf. Ploch (manuscript, 2001), which was censored by *Glot International* for its apparently unconstructive negative, harsh and unpleasant views on Rachel Walker's pseudoscientific account of nasal harmony within the Optimality formalism.

[20] This occurred after Ploch's talk at HILP 4 whose written-up version will be published as Ploch (in preparation 1).

would like to reply that Dolbey's counter-argument contains a logical mistake:[21] Let us assume that it is true that all phonological phenomena are phonetically natural. (In Ploch 1999b, there is a list of "unnatural" phenomena which is, admittedly, small.) Even then this would only mean that there is a statistical correlation between certain phonological and phonetic phenomena but not that this correlation between *A* and *B* provides evidence for *A* being motivated by *B*. Such a correlation could also be caused by *B* being motivated by *A* (certain phonetic phenomena by the phonology) or by *A* and *B* being in a relation via some third phenomenon *C*. So Dolbey mistakes evidence for a correlation between *A* and *B* as evidence for *B* motivates *A* without providing testable evidence for this assumption.

All in all, it can be said that supporters of the PH believe that simply because they have derived phonological data from certain phonetic details the PH is not *ad hoc* and thus non-arbitrary. In this belief, they are mistaken, and so they can only save their otherwise testable and falsified hypothesis, the PH, by immunising it in an *ad hoc* manner. We see there that an assumption does not become non-*ad hoc* and in this way acceptable by deriving observable facts from it. What matters is whether such an assumption is saved from being falsified by *ad hoc*-hypothesis. Importantly, if we adopt Popper's criterion testability, we kill two birds with one stone: on the one hand, we can eliminate *ad hoc* hypotheses (i.e., arbitrariness), which are the hallmark of all pseudosciences (astrology, biorhythm theory, phonetically motivated phonology); on the other hand, testability gives us both the right kind of non-arbitrariness and the right kind of simplicity in our theories (cf. section 2).

To relate the contents of this chapter to what I have said in section 3 (introduction) about the problem of induction, let me make this quite clear: This means that it simply does not matter how many more "confirming" cases the supporters of the PH find, i.e., examples in which some phonological phenomenon can be described in phonetic terms and thus be accounted for by the PH: and if the supporters of the PH provide another million phonetically natural examples every day of the next 20 000 years spent on laboratory work—none of these examples can or will provide even the tiniest shred of evidence, simply because this would be logically impossible.

If they really want to provide evidence for the PH (as opposed to pseudoscientifically maintaining this myth that no-one has ever provided any evidence for), they would have to establish what counts as a phonological phenomenon *phonologically*, i.e., independently of anything else, so also independently of phonetics (cf. the better versions of GP), they would have to establish what counts as a phonetic phenomenon *phonetically*, i.e., independently of anything else, so also independently of phonology, *then* see whether there is a statistically significant correlation between phonetics-independent phonology and phonology-independent phonetics, and *then* not assume *a priori* any specific direction as regards existing causal links but rather find testable assumptions on the basis of which they could find out whether the phonetics involved motivates the phonology involved, or whether it is rather the other way around, or whether phonetics and phonology are in some causal link relationship via a third set of phenomena. Anything else would be, just like most of last few decades of phonology, circular, statistically unsound and metatheoretically flawed (also cf. Ploch 1999b, in preparation 1, manuscript).

[21] A logical mistake for which I would like to express my gratitude to Andrew Dolbey, because this mistake, even though it baffled me at first, then helped me to gain the insight expressed in the following by making me understand its logical nature.

Note also that in my opinion my deconstruction of the PH is a highly constructive approach to the PH since only the cutting off of untestable and unnecessary claims makes evident what evidence for a claim there really is, and what is just make-belief.

This is also why this modern urge (cf. laboratory phonology and the UCLA thinking of late) to unite phonology and phonetics and to make the phonology account for an ever increasing amount of phonetic detail is metatheoretically quite inadequate: if these researchers want to find out (rather than decide upon *a priori*) how much phonetic detail the phonology should encode, then no single piece of phonetic detail can be viewed as phonologically relevant without providing evidence for such a claim. But for this, such researchers would of course have to commit themselves to a phonetics-independent phonological enterprise—and give up their paradigm. Apart from that, they would have to abandon a number of other pseudoscientific undertakings, e.g., Lexical Phonology, Optimality Theory, Feature Geometry, and Underspecification Theory.[22]

3.2 Pseudoscience, example 2: Lexical Phonology[23]

One of the main characteristics of Lexical Phonology ("LP", cf., e.g., Kiparsky 1982) is the assumption that phonological processes operate on certain strata/levels which are ordered in sequence, and that affixes must typically be assigned (in an *ad hoc* manner) to specific levels. As an example, let us look at velar softening in English (/k/ → [s] / _ /i/).[24]

Important for our purposes is that, even though velar softening takes place before the suffix *–ity*, it does not do so before, say, *-ing*: e.g., *electri*[k], *electri*[s]*ity*, **electri*[k]*ity*; but *kick* [kʰɪk], *ki*[k]*ing*, **ki*[s]*ing* (J. Kaye's examples, p.c., SOAS, 1992). In order to get this result, the relevant levels would have to be ordered in the following way:

(13) Velar softening à la Lexical Phonology

			electric	*kick*
Stratum *x*	*-ity*	velar softening	*electri*[s]*ity*	—
Stratum *x* + 1	*-ing*		—	*ki*[k]*ing*
Result			*electri*[s]*ity*	*ki*[k]*ing*

In this theory, *-ing* is affixed on a stratum which is ordered after the stratum on which velar softening takes place and *–ity* is affixed. This ordering ensures that the phonological process velar softening is not applied on all later levels, e.g., the one on which *–ing* is affixed.

So where is the problem? The problem is that the assumption that phonological processes are restricted to specific levels is not testable since the ordering of levels is arranged in a completely *ad hoc* manner. This means, there is no independent evidence for any proposed ordering of levels. And why is this problem accepted—or ig-

[22] For Lexical Phonology and Optimality Theory, cf. below. For Feature Geometry and Underspecification Theory, cf. the above discussion against the PH, of course, and Ploch (1997, 1999, in preparation 1).

[23] The discussion of Lexical Phonology in this paper owes much to Kaye's argumentation in Kaye (1992).

[24] It has no bearing on the following discussion whether one assumes /k/ → [s] / _ /i/ or /k/ → [s] / _ [i].

nored (neglecting that arbitrariness is by many not viewed as a problem in the first place)? Because one believes that a derivation is less "redundant", i.e., it is supposedly "simpler", than the assumption of a human brain which has to remember each form ending in *–ity* as it is. In other words, one tries to derive whatever one can derive, and one does this because, for some reason, one is convinced that storage in the human brain is most limited (cf. Bromberger & Halle 1989: 56, to whom it is "at a premium"; or Chomsky 1995: 235, to whom the lexicon is a "list of exceptions"), at least limited enough in order to justify that one must derive and therefore not store whatever/wherever one can.

This phobia about assuming mental storage of relatively unlimited size is unfortunately, metatheoretically speaking, unscientific:[25] How small the storage capacity of the brain is in relation to the needs of human language cannot be decided upon *a priori*, and if the only way to avoid a mnemonic (lexicalisation-based) explanation consists of assuming untestable strata (only so that something appears as derived or is described in terms of derivation), then one has the better deal when one assumes that levels and processes which would otherwise have to be considered to be *ad hoc* are not derived and are understood, in this way, as underived, completely lexicalised forms (which does *not* exclude the possibility that humans can recognise patterns/regularities within the stored data). The new insight which was brought to us by the advent of LP, i.e., that phonological processes and suffixes are limited to specific strata, is imaginary and is not based on any evidence. Let me also add that it does not even matter whether the supporters of LP view what they describe in terms of derivation (by reference to ordered strata) as derivation in the usual sense or as some "lexical" and thus underived phenomenon.[26]

Importantly, the methodological tool on the basis of which the fallacy of ordered levels is achieved within LP is the arbitrarily established *ad hoc* arrangement of levels and the *ad hoc* assignation of affixes and processes to these levels. The only reason why supporters of LP think that their theory is a scientific one is based on their mistaken view of non-arbitrariness: They believe that because they derive some data from some assumptions, these assumptions are not *ad hoc* to the extent to which they are abstract, i.e., to the extent to which data are derivable from them, and the more levels they assume and the more complex the set-up of strata is the more abstract and thus non-*ad hoc* they believe their assumptions to be. It should have become clear in this section that no degree of abstraction of this sort and no amount of data accounted for can make LP analyses less *ad hoc*. Only independent evidence for each of the assumed levels could, and this is precisely where we find LP's weak point.

[25] Cf. Jensen 2000, especially chapter 1, for a more detailed discussion of this point.

[26] The last statement is also important in relation to our discussion, in section 3.3, of Optimality Theory, a theory which takes great pains to appear non-derivational but imitates with its combination of inputs and outputs, its subscription to grammaticality, its violable constraints, its untestable approach to non-monotonicity in human reasoning and, for some, its sympathy (cf. McCarthy 1997, 1999) a derivational framework very well. The important point to remember here is of course that it is completely irrelevant in what way the *definitions/concepts* "derivation" *versus* "non-derivation" differ, only *propositions* matter, but this is a distinction that would entail the kind of taking seriously testability that would be quite unusual within Lexical Phonology or Optimality Theory. For more details on the untestable way in which Optimality Theory deals with non-monotonicity, cf. Ploch (in preparation 2). For a similar view on the imitative character of Optimality Theory as regards derivation in opposition to its claim that it is a non-derivational approach, cf. Mohanan (2000).

3.3 Pseudoscience, example 3: Optimality Theory

This paper does not provide enough space to discuss the unscientificness of Optimality Theory ("OT") in detail. A treatment of this topic that may not be complete but is certainly more detailed can be found in Ploch's review of Rachel Walker's pseudoscientific analysis of nasals and nasal harmony (Ploch, manuscript).[27]

The case of unscientificness to which I would now like to direct your attention is the untestable nature of each OT constraint and thus, in some fundamental manner, of each OT analysis. Let us look at the following *tableau* (from Kenstowicz, Abu-Mansour & Törkenczy, in preparation, approximately 8th page):

(14) Final devoicing in (a.) Yiddish and (b.) Polish

a.	*klu*/b/	Id-[voice]	*[voice]
☞	[b]		*
	[p]	*!	

b.	*klu*/b/	*[voice]	Id-[voice]
	[b]	*!	
☞	[p]		*

The OT account of final devoicing in (14) work like this: In both languages (or actually, all languages! consider OR for a moment!) there is a constraint that expresses the assumed markedness of voicing, i.e., *[voice], and one that makes explicit the assumed markedness of a change in voicing specification from input to output, i.e., identical voicing specification in input and output (Id-[voice]) is seen as less marked. The two possible ranking arrangements of these two constraints results in two possible language types, one in which Id-[voice] outranks *[voice] and where thus there is not final devoicing (a., Yiddish), and one in which *[voice] outranks Id-[voice] resulting in devoicing taking precedence over the (input-output) faithfulness constraint (b., Polish).

Note that OT supporters would like to present to you as an important and insightful innovation that in OT there are no derivations but that, instead, all constraints operate simultaneously and generate in this way the relevant actual output candidate. One can easily recognise that the non-arbitrariness problem, as we have seen it work above in relation to the derivations and levels of LP (but as it is not inherent in every derivational approach), is not avoided: In LP, derivational processes which are portrayed as phenomena expressing regularities, are made untestable by ordering all those cases that would otherwise falsify the assumption of a process on a *later* stratum. In OT, the same effect is achieved by outranking an otherwise falsified constraint by another constraint, i.e., by preventing the effects of an otherwise falsified constraint by another higher ranking constraint. In this way, one only searches for confirmations of a constraint but not for facts that would falsify it, which

[27] *Glot International*, who had initially accepted Ploch's review, refused months later to publish said review because it was in their opinion too negative. Apparently, one has to like OT and/or Walker's OT-analysis at least a little, one always has to participate a little, always be in-between a little. I keep asking myself how one is supposed to argue against OT if not via detailed deconstructions of OT analyses. If, however, one cannot get these published because they are supposedly too negative or have the wrong tone or whatever, then editors like Rint Sybesma and Lisa Cheng from *Glot International* have become censors and ideological wardens. Who is negative here? The one with this or that opinion (or even this or that tone) or the one who refuses to publish something? The censored manuscript will in the near future be made available at Ploch's university webpage (languages.wits.ac.za/~stefan) or at some other place yet to be announced on said webpage.

confirmations of a constraint but not for facts that would falsify it, which would also not be a sensible undertaking in OT since falsifications of generalisations/universal statements are made impossible in this theory by the ranking system and the assumed violability of constraints.[28] It is precisely this violability of constraints, one of the praised innovations of OT, which places it in the realm of pseudosciences.

3.4 Low truth content *versus* untestability, and how does one avoid pseudoscientific non-arbitrariness?

I have no quarrels with the opinion that *ad hoc*-hypotheses are, metatheoretically speaking, problematic. We should, however, keep in mind when we talk about this problem that it is the untestability of hypotheses which have been immunised against refutation by the means of *ad hoc*-hypotheses because of which *ad hoc*-hypotheses are unscientific. Let us assume, for argument's sake, that, by proposing the Domain-final-obstruents-in-German-are-voiceless Hypothesis ("DOH"), we account for the observation that domain-final obstruents in German are voiceless. In such a case, the DOH may be *ad hoc*, but it is testable. What bugs us about it is that, with it, we are only saying the most trivial, since its truth content, i.e., all true statements that follow from it is quite low.

This problem can be avoided though by listing all hypotheses which one needs in order to account for certain data (and which would, one should think, of course have to be testable), and by organising these hypotheses, like in a matrix arranged from left to right, in a way such that the more to the left a hypothesis is placed the more independent evidence there is for it, i.e., the higher its truth content, and that the more to the right a hypothesis is placed the less independent evidence there is, i.e., the lower its truth content and the more trivial. Our DOH would be located at the right margin of a matrix of this kind. This method would make it very hard for us to hide from ourselves how much trivial statements we need in our theories, and so we could also test different theories, comparatively, for their triviality content. It could then be our aim either to move hypotheses which are relatively far to the right/trivial side more to the left/truth-content bearing side by discovering more independent evidence for them, or to replace them via invoking new hypotheses that are "more left" from the start. Importantly, only to the extent to which the hypotheses occurring in such a matrix are testable they can be compared in a scientifically relevant manner.

I should also point out that while untestable *arbitrary* hypotheses cannot be used for discovering objective knowledge, *ad hoc* hypotheses lacking truth content can, as described above, be employed for such a purpose. By this, I do of course not want to claim that such *ad hoc*-hypotheses that exhibit a low truth content are usually utilised for scientific purposes. It is well possible to build one's carrier at our metatheoretically naïve and testability-phobic universities on the basis of providing one trivial hypothesis after the other. The trick in this consists in the fact that such hypotheses are, because they are testable, explanations but explanations of zero or little content.

Clearly, the way to avoid pseudoscientific non-arbitrariness is, first, not to confuse a low truth content with untestability, and secondly, to use testability as criterion of demarcation between science and pseudoscience: the scientific kind of simplicity

[28] Let me finally also point to the unfalsifiable Grammaticality Hypothesis which is supported by OT, like by most linguistic theories (cf. Jensen, in preparation; Ploch, in preparation 2).

(or of Occam's Razor) and of non-arbitrariness (non-*ad-hoc*-ness) are then mere consequences/theorems.

4 John Goldsmith's recipe for pseudoscience

It is a rare thing that a linguist reveals his views on metatheory. John Goldsmith (1998), however, has summed up how science is constituted in his opinion, and since the views he presents are not scientific ones, I would like to have a look at them in this section. This will also be a convenient means to compare the Popper-based approach to scientific discovery presented above with Goldsmith's more pseudoscientific one.

To begin our discussion, let me remind the reader of the following statement by Goldsmith (taken from (7)):

GOLDSMITH'S VIEW: "They [i.e., those who argue that understanding how science works is *not* equivalent to understanding the structure of scientific concepts and how those concepts relate to observations] argue that such factors as: relations between science and technology; relations between people in scientific organizations, especially relations of power; history of instrumentation; history and development of the graphical representation of theoretical concepts; and others—that all these are crucial to understanding science."

MY VIEW: Goldsmith confuses the social and psychological setting in which scientific (or pseudoscientific) ideas are invented with what constitutes objective knowledge, whether people regarded as scientists are interested in that or not. Remember that testability is a criterion based on logic, i.e., on a logical relationship between an assumption and its potential falsifiers. For example, the universal statement *All swans are white* is proved wrong by the existential statement *There is a non-white swan in my soup*. Importantly, this logical relationship is independent of the relation between science and technology, between people in scientific organisations and of anything Goldsmith mentions. To put it the other way round: just because certain phonologists maintain their power in relation to other phonologists by turning the PH into a dogma and by not testing it, its truth and falsity content does not change. As I have shown above, the graphical representation of theoretical concepts certainly has an influence on how people view their science and even on what assumptions they do and do not come up with; I have even provided an explanation of why this is so (concepts are sets of cognitive links). However, how scientific any view is, is precisely not based on its graphical representation, but is a function of its truth and falsity content, and it does not even matter whether anyone ever sees it that way.[29]

If we follow Goldsmith's socio-political view then it cannot even be established whether what people call science or do as science is science, because that which they do has an influence on what is considered to be science. Similarly, whether and if so, in what way and to what extent the relation between academics has an influence on the establishment of objective knowledge cannot be investigated scientifically if the existing relationships between academics, on the one hand, and the question what constitutes sound science, on the other, cannot be researched independently of each

[29] For arguments against subjectivist views, i.e., views according to which reality is a function of what is perceived, cf. most of Popper's works, e.g., (1973). Note: subjectivist views are always some kind of sophistry and in this way tedious without end. ("But maybe we are all dreaming this." "Does a forest exist that no-one perceives.") The important point here is that subjectivist views are not arguable. Anything goes in a subjectivist world, just like Feyerabend thought, and pigs may even fly.

other. However, with an objective criterion like testability (and I know of no other arguable objective criterion), it can be argued for independently of what people do or what relationships they are in whether what they do and call science actually is science. So note: the sociological and cognitive influences on what is called science that Goldsmith refers to are admittedly crucial to understanding what people do and why they do it. It is completely irrelevant, and must be so, to the answer to the question whether what they do (or do to each other) is science.

Let us now continue this discussion of Goldsmith's views by looking at those parts of his statement that I have not presented yet:

GOLDSMITH'S VIEW: "... falsifiability is a small part of the consitution [sic] of science".

MY VIEW: I agree totally (if we call what so-called scientists do "science"); this is why people support the PH, LP or OT in the first place. Again we see that Goldsmith has a hard time distinguishing science from what people call science or believe to be science or from the sociological environment.

GOLDSMITH'S VIEW: "You don't want a long answer, just a brief statement on how science is constituted logically, epistemologically, socially, and historically? But of course. Well, let's back up a moment, and ask specifically about falsification: where did it come in? Historically, discussions of falsification came not from science, but from philosophy of science, and in particular from Karl Popper's effort to distinguish science, on the one hand, from Freudianism and Marxism, on the other—these latter two being areas that he felt were wrongly held up as scientific approaches to problems. Popper held that the problem of "demarcation"—separating science from non-science—was solved by the identification of science as an enterprise in which each entrant paid his dues by saying what would in principle constitute falsification of his particular claim".

MY VIEW: Again, where falsificationism came from historically is completely irrelevant as far as its scientific worth is concerned; there is simply no logical relation between history or the history of science or philosophy of science, on the one hand, and what constitutes a scientific argument, on the other. I agree that it is interesting to study in what historical period what claims were put forward, e.g., why Popper's testability came up when it came up. This may help us to come up with testable assumptions about history, the history of science (or linguistics), sociology, psychology, etc. This however has no bearing on the scientific, i.e., objective and arguable, value of testability, which is not about the socio- or historico-cultural environment or the psychological make-up of academics but about a logical relationship between an assumption and its potential falsifiers, and so the propositional content of this (or any other) claim does not change dependent on when it was put forward.

GOLDSMITH'S VIEW: "Popper's move was important at the time in that it appeared to save the rationality of science from other challenges, challenges which had already eaten away at the basis of certainty that had seemed to serve as the basis of modernism. The two major philosophers who sought to create a modern conception of certainty were Descartes and Kant —and science was seen, by and large and up till Popper, as seeking (and offering) certainty—who sought to find the basis of epistemological certainty in the mind. Descartes' famous syllogism starts with the cogito, and moves forward to God and eventually scientific knowledge; Kant, far more sophisticated, sought certainty in the categories that made thought possible in the first place. Kant's argument proved too much, though, as was becoming clear by the beginning of the 20th century. Both geometry and even logic were being challenged (in math and in physics) as being empirically testable, and challengeable—and, in fact, challenged. (There are long stories to be told here about non-euclidean geometries, and about the logic of quantum mechanics—just 2 stories to tell when others would serve as well.)".

MY VIEW: I have no problem with Goldsmith's views on Descartes and Kant as far as this paper is concerned. However, Goldsmith's view that "Popper's move was important at the time" is misleading within the context it appears in. Whether testability is an arguable logical criterion has, as already pointed out above, nothing to do with when it was proposed and in what historical context, even though when and why it was proposed may well have something to do with the historical context.

GOLDSMITH'S VIEW: "Two themes collide at this point: the first one that I mentioned was Popper's concern to distinguish science from Freud and Marx; the second, a far greater concern in every sense, was the problem of establishing an epistemological base for science. Popper's demarcation solution (= falsifiability) only served the first function directly."

MY VIEW: Goldsmith claims (without any evidence) that falsifiability as demarcation criterion does not establish an epistemological base for science, at least not directly. I completely disagree: Testability is the best epistemological basis we have. Not only that; Popper has written numerous articles and books after the monograph in which he proposed falsifiability, i.e., after Popper (1934). In these later works he elaborated upon his views and did little other than discuss in what way testability can be used as an epistemological basis; cf. *The Poverty of Historicism* (1957), *Conjectures and Refutations. The Growth of Scientific Knowledge* (1972), *Objective Knowledge. An Evolutionary Approach* (1973), and the first two postscripts to *The Logic of Scientific Discovery* (1934, 1959), i.e., Popper (1982, 1983), and the collection of Popper papers in Notturno (1994).

I understand that the quote discussed here is taken from one of Goldsmith's emails sent to the cogling discussion group and not from one of Goldsmith's publications; however, it is still fair enough that I point out that Goldsmith's view that Popper's testability criterion does not provide an epistemological basis (or not a direct one, whatever that means) appears nonsensical to me. But of course, if Goldsmith was again talking about what plays a role in science and not about how we establish objective knowledge, then his statement may simply mean that scientists in general did or do not base their research on Popper's methodology. Even though I do not think that this is generally true but is actually quite accurate for linguistics and mostly anything called "social science" or "art", disciplines which precisely for this reason are to me still somewhere in the dark ages of religiosity in form of its (post)modern versions subjectivism, essentialism, existentialism, phenomenalism and relativism, no degree of unscientific methodology on the part of academics argues for or against Popper's testability criterion.

Be this as it may, in the works cited above, Popper discusses everything from epistemology without knowing subject (from 1973), i.e., he puts forward an objective/rational epistemology, to metaphysics, corroboration and the propensity interpretation of probability (1983); from the determinism *versus* indeterminism problem (1982), the reasons why the social sciences and natural sciences should not be subject to different scientific criteria (Popper 1957; Notturno 1994: chapter 8), or his objection to scientific relativism (Notturno: chapter 2), to his "pluralist" views on the philosophy of history (Notturno: chapter 7), to name but a few. Generally, *Objective Knowledge* and *Conjectures and Refutations* are about little else other than his evolutionary approach to epistemology and how the testability criterion can be used to this

end. Goldsmith may have been somewhat brief in his email, but he is obviously misinformed as far as Popper's views on epistemology are concerned.

GOLDSMITH'S VIEW: "The problem of demarcation has taken it on the chin from many directions over the last couple of decades, most famously from the late Paul Feyerabend, an apostate ex-follower of Sir Karl (Popper, that is), who argued in Against Method and later books that real scientists used whatever rhetorical tools were necessary to convince their colleagues: in science, Anything Goes, when it comes to spreading conviction in your scientific hypothesis.

MY VIEW: Again, I disagree. The problem of demarcation has taken nothing on the chin, particularly not from Feyerabend. Feyerabend makes the same mistake that Goldsmith makes: he does not realise that there is a difference between what constitutes a scientifically sound argument and what so-called scientists usually call "science" or how they spread their power. Let us agree for the moment that Feyerabend is correct in his claim that real scientist used whatever rhetorical tools were necessary to convince their colleagues: so what? What has this to do with the testability criterion. This simply means that many "real scientists" were or are not particularly scientific about their "science", and if we want to determine whether they were/are or not, we can read Feyerabend to the end of our days and will not find out because he talks only about what he thinks that people *did* and what convinces people. This is also why his slogan "anything goes" is pseudoscience. People can be convinced of all sorts of things, even psychoanalysis or OT. Let me put it this way: "anything goes", but not any story will fly. How pseudoscientific astrology or the PH are is in no obligatory relationship with who could or can convince whom that these views are (or are no) major advances and insights. It is quite tiresome when Goldsmith or Feyerabend cannot talk about objective knowledge without referring to what people end(ed) up doing. Just imagine the world to be different for a moment! Just imagine that Stirner (1845, 1907) is right when he says:

(15) The world of fools according to Stirner [1991: 46] (1907: 55–56)

> "Denke nicht, daß Ich scherze oder bildlich rede, wenn Ich die am Höheren hängenden Menschen, und weil die ungeheure Mehrzahl hierher gehört, fast die ganze Menschenwelt für veritable Narren, Narren im Tollhause ansehe. Ist nicht alles dumme Geschwätz ... das Geplapper von Narren, die ... nur frei herumzugehen scheinen, weil das Narrenhaus, worin sie wandeln, einen so weiten Raum einnimmt? ... Man muß die Tagesblätter dieser Periode lesen, und muß den Philister sprechen hören, um die gräßliche Überzeugung zu gewinnen, daß man mit Narren in ein Haus gesperrt ist."
>
> [Do not think that I am jesting or speaking figuratively when I regard those persons who cling to the Higher, and (because the vast majority belongs under this head) almost the whole world of men, as veritable fools, fools in a madhouse. Is not all the stupid chatter ... the babble of fools who ... only seem to go about free because the madhouse in which they walk takes in so broad a space? ... One must read the journals of this period, and must hear the Philistines talk, to get the horrible conviction // that one is shut up in a house with fools.]

In other words: imagine, most people are fools. As a consequence of this, one could not any longer assume that "anything goes", or that what people intend to be science or what Goldsmith or anyone considers to "play a role" will result in or is science. More importantly, since neither Feyerabend nor Goldsmith (nor anyone) can know *a priori* that what people called scientists intend to be science is science or to what extent this should be so, i.e., since they cannot exclude *a priori* to what extent Stirner is wrong, their assumption that it is in any way relevant to know what people do in order to decide whether some approach is scientific, or that just about anything goes, is not scientific. In Feyerabend's case, who is a self-confessed humanitarian (cf. 1975), the pseudoscience is of a humanitarian nature. He could apparently not face a world in which most academics are fools, and so he proclaimed that anything goes, and in this way no-one was a fool, and he could continue to love everyone or at least "the human *being* in all of us" (cf. Stirner 1845, 1907 for a well-written deconstruction of beings, the human and anything spiritual; also cf. Ploch 1999a). Again, let me stress that I agree that large parts of what is *regarded as* science is spread in all sorts of unscientific ways (cf. this article, above, and Ploch 1999a).

GOLDSMITH'S VIEW: "I'll leave it as a homework assignment for the motivated to decide for themselves whether the search for more falsifiable theories has led to scientific advances. While I can't say that hasn't happened, I can't think of any cases of it, and I'd expect it to be extremely infrequent".

MY VIEW: Popper (1983: xxvi–xxx) mentions 20 examples where in the history of science a great scientific theory was instigated by empirical refutation.[30] But even that is really beside the point: even if there was not a single example where researchers based their approach on Popper's views, then this would not provide any argument against (or for) Popper's opinion on what constitutes a sound scientific argument. Science is not democracy or sociology, and again, there is no evidence for an obligatory relationship between what people intend to be or call science and the logics of truth.

GOLDSMITH'S VIEW: "... I think that the four key aspects of modern science that we should never lose track of are these: the expanding base of what is covered by science; the effort to provide a tighter, denser, more compact and elegant account of reality; the process of integration of the sciences into a coherent organization; and the increasing instrumental (i.e., practical, technological) power brought about by science—all of these being goals, not places yet arrived at. (That last one—increasing practical power—can be paraphrased as saying that science can be turned back into technology.)"

MY VIEW: In this quote, Goldsmith's pseudoscientific views become quite obvious. The first key aspect he mentions is "the expanding base of what is covered by science". Readers of this paper who have got so far know by now that the amount of data a theory covers means nothing without testability (cf. the unscientific versions of simplicity and non-arbitrariness, as discussed above). His second key aspect is "the effort to provide a tighter, denser, more compact and elegant account of reality". Neglecting elegance, this is a reformulation of the unscientific simplicity criterion presented in (1) and elaborated upon in section 2, as long as this key aspect is not based on testability, which would, by the way, make all of Goldsmith's *key* aspects *unnecessary*. Elegance, as I have shown above, is some kind of mixture of flawed versions of simplicity and non-arbitrariness, if it is not simply an aesthetic and thus subjective

[30] Another "chin" that Goldsmith may refer to here is the view that Thomas Kuhn [1962] (1970) has "shown clearly" that Popper's views are refuted by the facts, i.e., by the history of science. Popper (1983: xxxi–xxxv) explains in some detail why this is not so and why Kuhn did not even attempt to do anything like that.

criterion. In Goldsmith's case, we appear to be dealing mostly with a synonym for flawed simplicity.

His third key aspect is "the process of integration of the sciences into a coherent organization". Here it depends on what Goldsmith means by "organisation". If he is referring to the organisation of subtheories within larger theories, then, since testability apparently plays a small role in all of this, we are again dealing with flawed arbitrariness, i.e., the organisation of subtheories within more abstract theories, which makes these theories denser, less arbitrary and more elegant, I presume. If he refers to social organisations, then there is nothing I can say other than that there is no evidence I know of on the basis of which it could be claimed that what constitutes science is in any relation to what people do or how they relate to each other.

Goldsmith's fourth and final key aspect is "the increasing instrumental (i.e., practical, technological) power brought about by science". Unfortunately, this again is not a scientific criterion. Even though I agree that good science may well result in good technology, what is considered to be good technology cannot be used as criterion of demarcation between science and non-science or even as science-creating ingredient. First, there is no obligatory logically valid relationship between what is considered good technology and science. Secondly, if certain relatively scientific theories, like some versions of GP, are not part of the mainstream of some discipline, say, phonology, and note that if this is so then this may well be due to completely unscientific socio-cultural and academico-political reasons (cf. Ploch 1999a), then there is a good chance that such theories will not only not be represented in Goldsmith's collections of papers (1995, 1999), which if they sell well play a role but may or may not be sound science, but also not in technological projects. Scientifically unsound but widespread PH-based phonological pseudotheories, like OT, which correspond to large parts of what many will consider essential reading, may in such a world well be used for technology, which, due to lack of good competition, will be regarded as the state of the art by virtually everyone, particularly by those who play a role and whose work is put into technology, so to speak. And even if a wide range of theories (including marginalised as well as more mainstream approaches) were to be investigated technologically, then there would still be no conclusive relationship from technological failure or success to scientific failure or success. In other words, we would still have to establish whether a theory is scientific independently, and the only arguable objective criterion for this purpose is, to my knowledge, Popper's falsifiability. The fact that Popper plays a small part in all of this does not mean that Popper is not relevant to linguistics or phonology but that, sadly, large parts of what is called phonology is not relevant to science.

To sum up, I hope to have shown in this section that one of the more famous phonologists, i.e., John Goldsmith, confuses sociological/psychological "scientific" *practice* with logically based, i.e., Popperian, *science* and sets up a set of four criteria or key aspects of science all four of which are pseudoscientific and consist mostly of terminological variants of the metatheoretically flawed versions of simplicity and non-arbitrariness that are the main topics of this paper.

Let me finally also point out that this section was *not* intended as a personal attack on John Goldsmith but rather as a deconstruction of his views which, apart from providing a comparison between his opinions and mine, is rather an invitation to further discussions of metatheory and its relationship to linguistics or phonology.

Summary and some final academico-political thoughts

I have shown in this paper that the most important hypotheses which have been supported by phonologists over the last (three, four) decades are not scientific ones. Two of the most influential methods out of the bag of tricks of pseudoscientists are flawed versions of the criteria simplicity and non-arbitrariness. Unfortunately, in the case of the academico-politically and socially most successful theories, just as it is the case with astrology, we are dealing with something that resembles quackery quite closely. The fact that the supporters of these pseudotheories may well be doctors and professors and that their so-called insights are called science by many, does not alter the degree of metatheoretical (un)soundness of their theories. As pointed out above: science is not democracy, and there is nothing that would guarantee in any way that that which is regarded as science by the majority (or by the world-dominating circles at UCLA, MIT and the University of Chicago) is not completely unfounded. (Remember Stirner!)

The good news is that it is possible to form an objective opinion on the basis of Popper's proposal of testability as criterion of demarcation between science and pseudoscience, i.e., an opinion independent of socio-politics, prestige, titles, jobs, money, one's peers and of whether it will play a role or be "important" or considered essential reading. I should add that "objective" does not entail that it is possible to know 100% or some other specifiable percentage of the truth of some matter; objective knowledge can only be corroborated, which is done by a comparison of different theories such that, insofar as they are commensurable and testable, one of them is established by destructive (not confirming, "constructive") tests to be less false and thus closer to the truth than the others (cf. Popper 1934, 1972, 1973, 1983).

This also makes it all too apparent that those who insist on what they call "constructive criticism" (e.g., *Glot International*, cf. note 27), and who will refuse to publish what they consider negative and destructive, have not quite understood how objective knowledge is created. The result is the censorship of knowledge-creating deconstructions (because they are too negative, harsh and unpleasant) in combination with the marginalisation of their authors—and the publication of pleasant summaries with advice that is always pleasant enough and can never be out to destroy and thus never be out to create scientific knowledge, and the support of their authors. Where pleasantry and humanitarianism rule, the humanitarian "anything goes" is just a motto (again, read Stirner! or Ploch 1999a): anything will just "go" as long as you are pleasant and humanitarian, of course.

Abbreviations

DOH	Domain-final-obstruents-in-German-are-voiceless Hypothesis
GP	Government Phonology
LP	Lexical Phonology
OR	Occam's Razor
OT	Optimality Theory
PH	Phonetic Hypothesis
SCVA	strict CV approach
SGP	Standard Government Phonology

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