

What is wrong with linguistics?*

Stefan Ploch

ploch@linguistics.wits.ac.za

1 Introduction

My answer to the question raised in the title of this paper is simply that Linguistics is, traditionally, one of the Humanities, and that thus its explanations suffer from a certain lack of precision and lack of testability which is not unusual in the Humanities.

In order to make this point clearer, I will first explain why inductive reasoning, which is very wide-spread in linguistics, is flawed. (An example of inductive reasoning would be a case in which someone tries to provide evidence for a proposal by pointing to examples where the proposal "works" but not to where it does not work.) I will then clarify in what way Popper's demand that a hypothesis must be testable is epistemologically sound.

After this preparation, I will illustrate why a number of widely-held and supported linguistic hypotheses/theories are untestable or falsified, and therefore flawed. The set of problematic views discussed will include the (1) Phonetic Hypothesis, i.e., the claim that phonological phenomena are phonetically motivated, (2) Lexical Phonology, (3) Optimality Theory, the (4) Grammaticality Hypothesis, i.e., the communis-opinio view that there exists a Language Acquisition Device on the basis of which anyone (or, possibly, any child) acquiring a language knows (albeit unconsciously) about any form it is presented with whether that form is grammatical or not, and (5) Descriptive Linguistics.

Subsequently, I will sum up the main strategies employed in Linguistics in order to maintain problematic theories.

2 The problem of inductive reasoning

Popper elaborated on the problem of inductive reasoning in a number of seminal works (1934, 1959, 1972). In short, a case of inductive reasoning occurs when we think that we can infer from statements about which we do have experience (e.g., *The sun rose in the morning on the last 124,485 days*) statements about which we have no experience (e.g., *The sun will rise tomorrow morning* or *The sun rises every morning*.)

As Ploch (2003c) has shown, inductive reasoning typically occurs when researchers try to avoid arbitrariness and end up building a flawed version of non-arbitrariness into their theories. So in order to understand the problem of inductive reasoning, let us approach this topic via an explanation of why arbitrariness is scientifically bad and what unsound non-arbitrariness as a misguided attempt to avoid arbitrariness looks like.

Ad-hoc-ness/arbitrariness in scientific research arises when untestable assumptions¹ that prevent the falsification of another assumption are employed in order to *immunise* this other assumption *against refutation* ("immunisation against refutation", Popper's

* This paper started out as a talk presented by the author as invited speaker at the LSSA/SAALA joint conference RAU, Johannesburg, on 30 June 2003. It is a revised version of Ploch (2003c), has been partially extended, and draws on findings argued for in Ploch (2001, 2002, 2003a,c,d).

¹ An assumption is testable only via its (existential!) predictions, not by itself, i.e., not without such predictions, cf. section 3.

phrase, cf., e.g., 1934, 1959). This point should become clearer by looking at the following two examples.²

(1) Examples of unscientificity via arbitrariness (*ad-hoc-ness*)

a. *Biorhythms*:³

"...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 scientist's 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."

Generally, we can say that "whenever the theory does not seem to work, the contrary evidence is systematically discounted" (Carroll, "ad hoc hypothesis"). Note that an *ad hoc* hypothesis often consists of a reference to unknown influences. It goes without saying that the terminological (labeling without testable predictions) of such influences does not make them 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 (1) and end up getting the flawed version of non-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 from generalisations provides evidence for these generalisations. As a result, this method consists in pointing to data accounted for by some firm or verified by some data or accounts for data. This is a justificationist⁴ approach to science, which, for logical reasons, does not work.

It is important to demonstrate that the notion "confirming evidence" is methodologically problematic and 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 probably any assumptions. So for example, no matter how many phonological phenomena the "Phonetic Hypothesis" can account for, it does not work.

² Cf. "ad hoc hypothesis", "biorhythms", "astrology" in R. T. Carroll's *Skeptic's Dictionary*. The examples quoted in (1) are taken from there too.

³ The precise details as regards the claims made by biorhythm theory are not relevant here. What matters is that falsifying data is systematically discounted in this pseudoscientific theory. However, those interested may want to 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 their life which these cycles cater for (their physical, emotional life, etc., are such areas).

⁴ Regarding justificationism, cf. the index of Popper (1983: 414).

is", i.e., the doctrine that phonological phenomena are motivated phonetically (cf. Kaye 1989; Ploch 1997, 1999, 2003a; Piggott 1999), accounts for, none of them and no number of them provides evidence for the Phonetic Hypothesis; in addition, no matter how many more pieces of data that the Phonetic Hypothesis can account for will be found, not one piece of data will make this hypothesis more likely to be true.

The basis for my (or, really, Popper's) claim that explanations cannot be proven or probabilified is based on logic and mathematics. Consider the equations in (2):

(2) Two intuitively sound equations

a. **Error!**

b. **Error!**

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 to rise again. The more often we observe something happen the more likely we feel it is to happen again. When we think like that, we 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*, *On 6 November...*, etc.). Since a (strictly) 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 generally 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. So 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 (2b) 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 (because we have observed a higher percentage of all the cases that are "out there" and could possibly be observed).

This induction is, however, not logically valid, as Hume had already found out.⁵ It is neither the case that a universal statement can be logically inferred from any number of existential statements nor can its truth be probabilified by an ever increasing number of

⁵ 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).

"confirming" evidence. The reason for this can be made apparent by the equation in (3), which in opposition to (2b) is valid:

- (3) The definition of the two symbols $+∞$ and $-∞$ (excerpt from Apostol 1974: 14, section 1.20, definition 1.24)
 By the extended real number system \mathbf{R}^* we shall mean the set of real numbers \mathbf{R} together with two symbols $+∞$ and $-∞$ which satisfy the following properties:

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

Error!

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). 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; in both scenarios the relation number of observations divided by the number of all possible observations exhibits the same result: 0. As will become clearer in section 3, this fact has been the main reason for the 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 belief that data can confirm assumptions.

Let me at this point of our discussion counter one argument someone may want to raise against my (or, rather Hume's and Popper's) claim that (strictly) universal statements cannot be deduced or probabilified by any number of existential statements. Someone could say that above, only a specific type of universal statement, i.e., *strictly universal statements*, were referred to above; instead, we could make use of *numerically, not strictly, universal statements*, which would render our claim that (strictly) universal statements are not provable, confirmable or probabilifiable pointless.

The problem is this: While strictly universal statements refer to an *infinite* set of arguments in some proposition, numerically universal sets refer to a *finite* set. For example, the universal statement *All sharks have teeth* can be interpreted strictly (referring to an infinite set of sharks) or numerically (referring to a finite set of sharks). Which interpretation is used has important meta-theoretical consequences: If a numerical interpretation is chosen, as in *All sharks (that have been observed in recorded history) have teeth*, the set of sharks referred to is finite, and thus, any number of observations of sharks with teeth is out of a *finite* number of possibly observable sharks and *not* out of an infinite set. Therefore, the equations in (2b) and (3) are not applicable, while the equation in (2a) is. So if we compare a situation in which x number of sharks with teeth have been observed with one in which it was $x+y$ number of sharks (with both x and $y \in \mathbf{R}$, and > 0), then the relation of the number of observations divided by the number of all possible observations *does not* exhibit the same result and *does not* equal 0 in both scenarios, and corresponds

to a higher number in the second scenario (in which a higher number of sharks has been observed).

Importantly, there is no flaw in the logic of this argument once we accept that we could simply make use of the numerical interpretation useless for the following reason: Remember how an argument works. It is necessary to assume a universal statement (*All sharks have teeth*) and an existential statement (the initial condition, e.g., *The object that has joined me in the water five minutes ago is a shark*). If one interprets the universal statement strictly (referring to an infinite set), then it is perfectly logical to deduce from these two assumptions that any shark one may encounter must have teeth. However, it does not follow from the numerical universal statement *All sharks (that have been observed in recorded history) have teeth* that any shark one may encounter must have teeth too. Actually, *absolutely nothing* follows from the assumption of a numerically universal statement and an existential statement. The reason for this is that each numerically universal statement can be expressed as a number of existential statements (*Shark 1 that has been observed in recorded history had teeth, Shark 2 ... etc.*), and since nothing ever follows from any finite number of existential statements, nothing ever follows from a numerically universal statement (because it refers to a finite set of propositional arguments). No *strictly* universal statement, no prediction; no potential falsifiers; no universal falsifiers, *no explanation*. Therefore, the attempt to counter my claim that universal statements cannot be deduced or probabilified by any number of existential statements by trying to make use of the numerical interpretation of a universal statement fails.

What I would like to show in the following (section 4) is that, in order for their explanations not to be falsified but to appear *non-ad hoc*, three of the most successful philosophical paradigms of the last few decades, i.e., the Phonetic Hypothesis, Lexical Phonology and Optimality Theory, employ arbitrariness in tandem with a meta-theoretically flawed version of non-arbitrariness. Before that, let me however explain how a scientific argument is set up.

3 A way out: testability and deductive reasoning

Each scientific argument contains an assumed generalisation, i.e., a strictly universal statement referring to an infinite set of entities (e.g., *Sharks have teeth*), an assumed initial condition, i.e., an existential statement (e.g., *The object that has joined me in the water five minutes ago is a shark*), and a consequence/theorem (existential statement) that is predictable via logical derivation from the assumptions (e.g., *The object that has joined me in the water five minutes ago has teeth*).

An assumed strictly universal statement can be testable in that each affirmative universal statement corresponds to a negative existential statement. For example, *Sharks have teeth* corresponds to *There is no shark that does not have teeth*. If it is possible to observe a case in which the corresponding affirmative existential statement is true (*There is a shark that does not have teeth*), 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 other words: (the assumption of) the truth of the affirmative existential statement proves wrong the negative existential statement *and thus the affirmative universal statement* (and with that, the proposed explanation). Thus, the existence of certain entities can disprove the truth of certain universals/explanations (cf. Popper 1934, 1959).

We note: an assumption is testable not by itself but only via the *existential statements it predicts*.

Finally, we see that there is an asymmetry between verification and falsification. Even though, as was shown in section 2, explanations *qua* strictly universal statements cannot be verified (and not even be probabilified), they can be falsified, or rather, their falsity can be logically *deduced* from the assumption of an existential statement which matches an observational statement) which is in conflict with the negative existential statement that the assumed strictly universal statement is a transformation of. This is why an approach that makes use of this fact is a *deductive* approach.

What follows from this is Popper's method of trial-and-error: Compare different hypotheses as to how badly they are doing when you severely test them (i.e., try to show that they are wrong); the one that comes out best (i.e., least wrong) must be closest to the truth, i.e., has the highest amount of *verisimilitude* and the highest degree of *corroboration*. Popper sums up his method of trial-and-error in the following way:

"In any stage of your researches be as clear as you can about // your problem, and watch the way it changes and becomes more definite. Be as clear as you can about the various theories you hold, and be aware that we all hold theories unconsciously, or take them for granted, although most of them are almost certain to be false. Try again and again to formulate the theories which you are holding and to criticize them. And try to construct alternative theories — alternatives even to those theories which appear to you inescapable: for only in this way will you understand the theories you hold. Whenever a theory appears to you as the only possible one, take this as a sign that you have neither understood the theory nor the problem which it was intended to solve. And look upon your experiments always as tests of a theory — as attempts to find faults in it, and to overthrow it. If an experiment or observation seems to support a theory, remember that what it really does is to weaken some alternative theory — perhaps one which you have not thought of before. And let it be your ambition to refute and replace your own theories: this is better than defending them, and leaving it to others to refute them. But remember also that a good defence of a theory against criticism is a necessary part of any fruitful discussion since only by defending it can we find out its strength, and the strength of the criticism directed against it. There is no point in discussing or criticizing a theory unless we try all the time to put it in its strongest form, and to argue against it only in that form" (Popper 1973: 265–266).

4 Examples of problematic linguistic theories and approaches

4.1 The Phonetic Hypothesis

The first problematic linguistic view I would like to discuss is the Phonetic Hypothesis (PH). Kaye (1989) deconstructed the PH and concluded that it was wrong. His argument has not, however, been widely adopted in the literature.

Kaye's (1989) main arguments against the PH are: (1) The PH predicts the whole-sale phonetic and thus phonological convergence of the languages of the world. **But:** no such convergence can be observed: we see that *the PH is falsified* (2) The PH predicts that the input of a synchronic or diachronic phonological change in one language should not be the result of a process in another. **But:** Latin *kt* and *pt* changed to Italian *t* [t], avoiding the supposedly articulatorily too costly sequences *kt* and *pt*. Classical Arabic *katāba* 'he wrote' changed in Moroccan Arabic to *kīb*, thus creating the supposedly articulatorily too costly sequence *kt*. Obviously, *the PH is falsified*.

Kaye (1989) also discusses a number of potential counterarguments and shows why they are flawed: Counterargument (1): A given process is not necessarily equally costly to speakers of different languages. However, (a) since any human can learn any human

language natively without marked differences, this assumption is baseless. (b) Furthermore, Kaye states that no-one has been able to successfully formulate a theory of language-specific scales of muscular effort. Let me add here that I have never come across a theory that can explain in what way it should be possible for the human articulatory system to be subject to various language-specific scales of muscular effort.

Counterargument (2): The predicted phonetic-phonological convergence has not happened yet but will happen, or it happens so slowly that linguists have not been around long enough to study them, or the observable phonological phenomena are not as drastic in their convergence as the ones to come. However, (a) this counterargument requires the scientist to have faith in the PH since there is no evidence. (b) Since there *are* phonological phenomena one would still expect them to support the claim of wholesale convergence. (c) There are limits to what phonological phenomena look like. It is questionable why future processes should be different from the ones we can see now, begging why future happens quite rapidly (rapid as in the Romance languages evolving from Latin in less than 2000 years). Romance languages should show signs of phonetic-phonological convergence. But they did not converge, they diverged.

Counterargument (3): To answer Kaye's argument that the input of a diachronic or synchronic change in one language should not be the output of another, one could refer to examples of backtracking in biological evolution (e.g., loss of flight). However, phonological phenomena are not adaptive to the environment, so biological backtracking does not support the PH.

Counterargument (4): The PH can be maintained at an *individual* level. However, this would predict correlations between the quantity (and, let me add, even quality) of phonological phenomena and the physical state of the individual. There is no evidence for such a correlation.

Finally, note also that an acoustic version of the PH (e.g., in Stevens & Blumstein 1978) again predicts the phonological convergence of the languages of this world (albeit according to acoustically motivated parameters), which cannot be observed (cf. Ploch 1999: 24).

4.2 Lexical Phonology

One of the main characteristics of Lexical Phonology (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 to specific levels. As an example, let us look at velar softening in English (/k/ → [s] / _/V).⁶ Important for our purposes is that, even though velar softening takes place before the suffix *-ing*, it does not do so before, say, *-ing*: e.g., *electric*[k], *electric*[s]ing; **electric*[k]ing; but *kick* [k^hk], *kick*[s]ing; **kick*[s]ing (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:

⁶ It has no bearing on the following discussion whether one assumes /k/ → [s] / _/V or /k/ → [s] / _/V.

(4) Velar softening à la Lexical Phonology

Stratum x	-ty	velar softening	<i>electric</i>	<i>kick</i>
Stratum x + 1	-ing		<i>electric s ly</i>	<i>k k ing</i>
Result			<i>electric s ly</i>	<i>k k ing</i>

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

So where is the problem? It 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. It appears that this problem is not recognised as such because many phonologists believe that a derivation makes the generalisation it is based on less "redundant" (which is a case of inductive, i.e., logically flawed, reasoning). In some way, only operative at a specific stratum is supposedly "simpler" than assuming a human brain which has to remember each form ending in *-ty* as it is. In other words, one tries to derive whatever one can derive, and one does this because one is convinced that storage in the human brain is most limited (cf. Bromberger and Halle 1989: 56, to whom it is "at a premium"⁷ 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.

The reluctance to investigate the possibility of mental storage of relatively unlimited size is, metatheoretically speaking, unscientific.⁸ How small the storage capacity of the brain is in relation to the needs of human language cannot be decided upon *a priori*. So if the only way to avoid a mnemonic (lexicalisation-based) explanation consists of assuming untestable *ad hoc* strata (only so that something appears as derived or is described in terms of derivation), then it is better to assume that phenomena which would otherwise have to be considered to be accounted for by *ad hoc*-levels are not derived and are understood, in this way, as undervived, completely lexicalised forms (which does not exclude the possibility that humans can recognise patterns/regularities within the stored data after the data has become attested or with the data becoming attested, as opposed to some generative rules generating the data in question and, in this way, generating and deriving the patterns). The central tenet of Lexical Phonology, i.e., that phonological processes and suffixes are limited to specific strata, is not supported by testable evidence. All that Lexical Phonology does is account for data, which, as I have explained above, provides no evidence for whatever account. Furthermore, it does not matter whether the Lexical Pho-

⁷ I fully agree with the following statement from Kaye (1995: 319–320): "I only wish to suggest that B&H's [Bromberger and Halle's 1989] assumptions... are not a priori true. Since they are unaccompanied by any form of argumentation I feel justified in dismissing them."

⁸ Cf. S. Jensen 2000, especially chapter 1, for a more detailed discussion.

nology view of what is described in terms of derivation is intended as derivation in the usual sense or as some "lexical" and thus "undervived but ordered" phenomenon.⁹

Importantly, the methodological tool on the basis of which the fallacy of ordered levels is achieved within Lexical Phonology is the arbitrarily established *ad hoc* arrangement of levels and the *ad hoc* assignment of affixes and processes to these levels, without independent evidence. An example of independent evidence would be found if, say, all independent affixes are dealt with at a stratum prior to the level at which derivational affixes are dealt with. In light of this it can be said that it is metatheoretically most problematic to regard Lexical Phonology as a scientific theory only because some data are derivable from some premises in this theory. No degree of abstraction of this sort and no amount of data accounted for can make Lexical Phonology analyses any less *ad hoc*. Only independent evidence for each of the assumed levels could, and this is precisely where we find Lexical Phonology's weak point.

4.3 Optimality Theory

This paper does not provide enough space to discuss the methodology of Optimality Theory 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 optimality-theoretical analysis of nasals and nasal harmony (Ploch, ms.).

The case of unscientificity to which I would now like to direct the reader's attention is the untestable nature of each violable constraint and thus, in some fundamental manner, of each Optimality-theoretical analysis.¹⁰ To pick an analysis of a set of data reasonably well known in the literature, let us look at Turkish vowel harmony and how it is treated within Krämer's (ms.) optimality-theoretical account. Consider the *tableau* in (5) and the constraints in (6):¹¹

⁹ The last statement is also important in relation to the discussion in section 4.3.4.3 of Optimality Theory, a framework that is conceptually non-derivational but still distinguishes inputs and outputs, i.e., a "before" and an "after". The important point to remember here is that it is irrelevant in what way the definitions/concepts "derivation" versus "non-derivation" generation differ, only propositions matter. For a similar view on the inductive character of Optimality Theory as regards derivation in opposition to its claim that it is a non-derivational approach, cf. Mohanan (2000).

¹⁰ I am not saying that all Optimality Theory analyses are completely untestable. Given that the number of constraints proposed as part of Universal Grammar must be limited because the human brain as a physical entity must be limited (too), there are, even under factorial ranking, also only a limited number of constraint rankings (and, in this way, language types). This makes it possible, in theory, to come up with language types that are not covered by any specific constraint ranking. In other words, there can be language types that would prove a proposed set of constraints wrong. Since this set is always identical to the full universal set because all constraints, no matter how low ranking and unobservable, are operative in all languages), and since within that set, none of the constraints is itself testable, any improvement to the whole universal set of constraints can only be achieved by abandoning the whole (universal) set. Unfortunately, even if someone were to find a set that, even though falsifiable, cannot be falsified, this set would still exclusively contain untestable constraints, i.e., constraints that no evidence can be provided for.

¹¹ The tableau in (5) is from Krämer (ms.: 22). The page numbers on which the constraints in (6) can be found are: the Ident(F) constraint family: 16 (from McCarthy and Prince 1995: 264); *LORO: 22; S-Ident (back) and S-Ident (round): 21. In (6), I have not followed Krämer's rules for capitalisation, paragraphing, italicisation and setting-of-quotes.

(1997, 1999) notion "sympathy", and at Ploch's (2001) argumentation against Optimality Theory in terms of its unscientific handling of non-monotonic logic (i.e., logic dealing with situations in which theorems derivable from new assumptions can take precedence over old theorems).

4.4 The Grammaticality Hypothesis

The Grammaticality Hypothesis is, in Jensen's (2003) terms, "the belief that there exists a faculty in the human mind that is able to take any well-formed phonological form as input and state whether that form is *grammatical* or not." There is not much space here to go into much detail; the interested reader is referred to Jensen (*ibid.*) and Ploch (2001). It suffices to say here that native speakers' intuitions range over more judgements than "grammatical" and "non-grammatical" or "acceptable" and "unacceptable", and therefore all in-between judgements have to be systematically reanalysed as one of the obligatory judgements or allowed for as "foreign data" *that need not be accounted for* by the "grammar", thus rendering the Grammaticality Hypothesis untestable. An alternative explanation for speakers' judgements is provided in Jensen (*ibid.*) and Ploch (*ibid.*).

4.5 Descriptive Linguistics

I have often come across the view expressed by descriptive linguists that "only the data counts", and that theories are secondary. Such a view of data would however only be sensible if it was possible to observe objectively, i.e., without theory-ladenness. This is not possible though: all observations are theory-laden (cf. Popper 1959, 1972, 1973; Ploch, forthcoming). Therefore a scientist chooses their assumptions, and different sets thereof, and tests them, to find out which one comes out best.

Generally, any approach to linguistics that is dismissive of theories and "only interested in the data" is inductivist (cf. section 2) and subscribes to the view that based on supposedly (but not really) "objective" observation, some supposedly relevant (but really quite arbitrary) corpus of data can be collected, on the basis of which some generalisations are made, which unfortunately only apply to this arbitrary corpus of data. To point out to examples where the generalisation holds (and not, in a testability-based manner, where it *does not hold*), i.e., to examples within their corpus of data, which is a justificationist mistake that goes hand-in-hand with inductivism. For more details on this, cf. Popper (1934, 1959, 1973) and Ploch (2001).

So, descriptivism cannot exist without theory: one can only observe *with* theory (theory in a wide sense, i.e., including all expectations, no matter how precise or imprecise).

5 Problematic strategies to maintain unscientific theories

The most prominent unscientific methodological strategies that are used in linguistics are (cf. Ploch 1997, 1999, 2003a): (1) justificationism or verificationism; (2) denial; (3) flexibility of applicability; (4) the causality fallacy, i.e., falling into the so-called whodunnit trap; and (5) flawed reductionism.

5.1 Justificationism (or verificationism) suffers from the mistaken belief that evidence for a justificationist (or more specifically, a strictly universal statement) can be provided by a general statement "in favour" of it, i.e., by finding instances in which a predicted situation (finding evidence "in favour" of it, i.e., by finding instances in which a predicted situation (existential statement) can be matched up with an observation. Note: this is logically flawed. As Hume [1739] (1888) and [1777] (1966) showed, it follows from no finite number of sunrise that the sun will rise tomorrow. In other words, no matter how many instances one has seen in which a prediction of a strictly universal statement can be matched up with an observation, this prediction (or no matter how many such predictions) is (are) always in relation to an *infinite* number of instances that the strictly universal statement is (are) always in relation to. Since any finite number divided by plus or minus infinity ($\pm\infty$) equals 0, no explanation (which must at least contain the assumption of a strictly universal statement referring to some entities x with some property y plus the assumption of an existential statement about an instance of x) can be proven to be true nor can be probabilified (cf. section 2).

Clearly, explanations cannot be proven or probabilified. For more details about this, cf. Popper (1934, 1959) [1963] (1972), [1972] (1973).

Unfortunately, justificationism in phonology is quite standard and wide-spread. As a student of phonology one is always presented with cases where some phonological phenomenon is described in phonetic terms, and this provides the "evidence" for the claim that the phonetics involved motivates the phonology. Obviously, this is not logical and a case of justificationism: instances where the PII works, i.e., where some phonological data can be described in phonetic terms and thus be "accounted for" by the PII, are taken to be evidence for the PII. The question why most "phonetically motivated" phonological phenomena do not occur in most languages (so how most speakers can resist most "phonetically motivated" urges to increase ease of articulation) is not even asked. Testability is often not relevant in mainstream phonology (at least as far as the acceptance of the PII is concerned; cf. section 4.1).

Also, one of the key words of justificationists is "account for": the PII accounts for phonological facts, and so it actually does. But this provides, as Popper has shown and Ploch has extensively written about in relation to phonology, absolutely no evidence for the PII.

Importantly, astrology works in exactly the same manner: If we look long enough we can find enough meticulous virgos "confirming" the universal claim *Virgos are meticulous*. However, not a single meticulous virgo nor any number thereof can support this universal claim. Claims must be *corroborated* by evidence, i.e., one must come up with different *testable* hypotheses, try as hard as one can to find counter-evidence and then see which hypothesis comes out best; in this way, one hones in on the truth (cf. the references to Popper's work). The astrological hypothesis accounts for meticulous virgos, but this provides no evidence for astrology. The relevant question here is: Is it possible to prove the statement *Virgos are meticulous* wrong, i.e., are there virgos that are not meticulous? Or, are there languages whose speakers can resist the natural phonetic urge to increase ease of articulation by vowel harmony? The answer to both questions is: Yes, this planet is full of unmeticulous virgos and unharmonic languages.

In summary, the PH and the astrological hypothesis (i.e., that the character traits and deeds of people are influenced by a number of planets) are maintained by the same strategy: justificationism; only "confirming" evidence matters.

5.2 The strategy of denial

The strategy of denial¹⁴ (Ploch 1997, 1999, 2003a) is closely linked to justificationism. It is the refusal to look at counterexamples. I have been in conversations with PH supporters for a number of years now. Commonly, the PH is subscribed to without counterexamples sent "evidence" for the "phonetic foundation" of phonology which pre-examples.

5.3 The strategy of flexibility of applicability

When this strategy is applied, one explicitly immunises some universal claim, e.g., the PH, against refutation. This is probably the most widely used method on the part of the supporters of the PH. The PH is not applied whenever it would otherwise be proven wrong. I have found three versions of this strategy:

1. Parameterisation of "phonetically motivated" phonological phenomena: a universal claim, e.g., assimilation being caused by the attempt on the part of speakers to increase ease of articulation, is parameterised; when something does not happen in some language, the parameter is "off" in that language. So when a spoon held up by a magnet does not fall down, gravity is 'off' for the spoon? (Example adapted from Mohanan 2000.)
2. Lexical phonology: a universal claim (e.g., a constraint) is only universal at a certain level/stratum. Other constraints operate at other levels. If a constraint (which in order to explain anything *must* be a strictly universal statement) is not applicable to certain affixes, then the constraint is said to operate at a level prior to the level at which the non-undergoing affixes are dealt with by the grammar. In this way, counterexamples to any universal claim can be ordered *after* the claim they are counterexamples too, and in this way, the universal claim is immunised against refutation. Is a spoon held up by a magnet dealt with after gravity has applied, and so the spoon is just not subject to gravity?
3. Optimality Theory: Violable constraints (note: a violable constraint is an explicitly violable, i.e., immunised, constraint) are ranked. Actual output forms are said to be those candidates that violate the highest ranking constraint least. Of course, similarly to Lexical phonology, any form that would prove a constraint wrong can be accounted for by some other higher ranking constraint. In this way, no OT constraint is testable; that is to say that all OT constraints are unscientific.

¹⁴ Note, in Ploch (1997, 1999, 2003a), I called the strategy of denial what in this paper is referred to as justificationism. For example, in Kenstowicz (1994) and Archangeli & Pulleyblank (1989) there is a large number of phonological phenomena accounted for in these works, but counter-evidence is not discussed; this is an example of justificationism or of what I used to call 'strategy of denial'. In this paper, the strategy of denial is used by those who insist on not taking into account counter-evidence.

5.4 The whodunit trap and the causality fallacy

One could try to counter my arguments against the maintenance of the PH by saying that all phonological phenomena are phonetically "natural" or "neutral" towards phonetics, but that there are no phonological phenomena that are "counter-natural". One could try to claim that since the PH makes the prediction that there are no counter-natural phenomena, it is testable, and since there are no counter-natural phenomena, it has proved its claim that since there are no counter-natural phenomena, it is testable, and since there are no counter-natural phenomena, it is testable, more specifically.

There is unfortunately a logical flaw in this kind of counterargument. More specifically, a flaw that I would like to call the "causality fallacy" (and which I have before referred to as "falling into the whodunit trap"). The fallacy consists in the conflation of the notions "relation" and "motivation/causation". Note that only some relationships between, say, *A* and *B*, are of the kind that *A* motivates/causes *B*. So the problem with the above counterargument is that it conflates a relational link between phonetics and phonology with a causal link *from* phonetics *to* phonology. The causal situation may as well be different. It may be the phonology that causes both phonetics and phonology (mediated link).

There may be a third factor that causes both phonetics and phonology (mediated link). I used to refer to this logical mistake as "falling into the whodunit trap" because one can find the same mistake in countless whodunit movies or novels, in which the police, when it arrests the wrong guy, think that some guy *caused* the murder just because they can link him to the murder. As we have seen again and again in such movies, it does not follow from such a link that the guy really committed the murder, nor does it follow from the fact that the hypothesis that they guy committed the murder *accounts for* the data that there is something to that hypothesis.

5.5 Flawed reductionism

As Popper (1973) has pointed out, there is nothing wrong with trying to reduce one science to another, e.g., with the attempt to reduce all chemistry to physics. However, one must do this in the right way. What one must avoid at all costs, is that one looks at chemistry from a physicist's perspective (through physicist's glasses, so to speak) and then reduces everything one "sees" this way to physics. — Of course, this is always possible. There is no such thing as objective observation, and looking at chemistry from the perspective of a physical theory is in itself a reduction to physics that makes sure that one can only see what is reducible to physics. A genuine reduction of chemistry to physics would contain a *chemical* analysis of chemistry, a *physical* analysis of physics, and then with chemistry and physics established by themselves and in this way independently of each other, one could try to see whether either physics or chemistry (each viewed by itself or from its own perspective) is reducible to the other, and if so to what extent.

Similarly, the attempt to reduce phonology to phonetics (to bring phonology and phonetics "closer together" as the modern jargon goes, or to encode "phonetic reality" into the phonology) can only be valid if (and only if!) the phonology in question is established *phonologically* (i.e., it must under no circumstances be a theory that cares about phonetic details) and the phonetics in question is established *phonetically*. (I still have not seen any phonetics that does not have to refer to phonologically, i.e., non-phonetically, established units; I therefore doubt that phonetics is an independent discipline, but this is another matter.) So the attempt to reduce phonology to phonetics can only be done in a scientific manner if the very thing that is actually attempted is abandoned: bringing phonetics and

phonology' closer together. Keep them separate, establish them independently of each other, hypothesise non-phonetically motivated phonological theories; otherwise any reduction to phonetics is problematic.

6 Some conclusions

1. There is no evidence for any phonetic motivation of phonology.
2. Phonetics is not an independent discipline. Without phonology there is no phonetics, but without phonetics as a scientific discipline, there is phonology. In other words, the importance of phonetics in phonology is greatly overrated.
3. Those who want to reduce phonology to phonetics should value non-phonetically motivated phonological frameworks, even though they do generally not.
4. It is not surprising that automated speech recognition that is based on phonetically motivated frameworks of phonology is still not working properly.
5. Lexical phonology is not a scientific framework.
6. Optimality Theory is not a scientific framework.
7. The Grammaticality Hypothesis should be dropped.
8. Descriptive Linguistics is suffering from a lack of (scientific) theory and thus of explanatory power.
9. The philosophy of science should be studied as part of Linguistics. I particularly steer clear of objectivity-denying philosophies through which we could be seen to be subjectivist, irrealist, skepticist, pragmatist, nihilist or such-like.

References
 Dianna and Douglas Palleyblank
 Archangele, Yonba vowel harmony. *Linguistic Inquiry* 20.2: 173–217.

- 1989 Laura Walsh Dickey and Suzanne Urbanczyk (eds)
Papers in Optimality Theory. (University of Massachusetts Occasional Papers 18.) Amherst: GLSA.
- Bromberger, Sylvain and Morris Halle
 1989 Why phonology is different. *Linguistic Inquiry* 20: 51–71.
- Carroll, Robert Todd
The Skeptic's Dictionary. skeptic.com.
- Charate, Monk and Ash Göksel
 1996 Switching and vowel harmony in Turkic languages. In: Kardela and Szymarek (1996: 29–55). First published in: Cobb and S. Jensen [1994: 31–52].
- Charate, Monk and Ash Göksel
 1998 Licensing constraints and vowel harmony in Turkic languages. In: Cyran (1998: 65–88). First published in: Ploch and Simpson [1996: 1–25].
- Chomsky, Noam
The Minimalist Program. Cambridge, MA: The MIT Press.
- 1995 *The Minimalist Program*. Cambridge, MA: The MIT Press.
- Cobb, Margaret and Sean Jensen (eds.)
 1994 *SOAS Working Papers in Linguistics and Phonetics, volume 4*. School of Oriental and African Studies (University of London).
- Cobb, Margaret and Yan Jiang (eds.)
 1993 *SOAS Working Papers in Linguistics and Phonetics, volume 3*. School of Oriental and African Studies (University of London).
- Cyran, Eugeniusz (ed.)
 1998 *Structure and Interpretation. Studies in Phonology*. (PASE Studies and Monographs 4.) Lublin: Foliun.
- Durand, Jacques and Francis Karamba (eds.)
 1995 *Frontiers of Phonology: Atoms, Structures, Derivations*. London/New York: Longman.
- Hines, Terence M.
 1998 Comprehensive review of biorhythm theory. *Psychological Reports* 83: 19–64.
- Hume, David
 1888 *A Treatise on Human Nature*. Edition L.A. Selby-Bigge. Oxford: Clarendon Press. First published [1739].
- Hume, David
 1966 *Enquiry Concerning Human Understanding and Concerning the Principles of Morals*, 2nd edition. Edition L. A. Selby-Bigge. Oxford: Clarendon Press. First published [1777].
- Jensen, Sean
 2000 A computational approach to the phonology of connected speech. Ph.D. thesis, Department of Linguistics, School of Oriental and African Studies (University of London).
- Jensen, Sean
 2003 Meta-phonological speculations. In: Ploch (2003b: 131–148).
- Kardela, Henrik and Bogdan Szymarek (eds.)
 1996 *A Festschrift for Edmund Gussmann from his Friends and Colleagues*. The University Press of the Catholic University of Lublin.
- Kave, Jonathan
 1989 *Phonology: a Cognitive View*. (Tutorial Essays in Cognitive Science.) Hillsdale, NJ, et al.: Lawrence Erlbaum Associates.

- Kaye, Jonathan
1995 Derivations and interfaces. In: Durand and Kalamba (1995: 289–332).
First published in: Cobb and Jiang [1993: 90–126].
- Kenstowicz, Michael
1994 *Phonology in Generative Grammar*. Cambridge, MA/Oxford, UK: Blackwell.
- Kiparsky, Paul
1982 From cyclic phonology to lexical phonology. In: Hulst and Norval Smith (1982a: 131–177).
- Kirchner, Robert
1993 Turkish vowel harmony and disharmony: An Optimality theoretic account. Presented at Rutgers Optimality Workshop I (ROW-I), October 22, 1993.
- Krämer, Martin
ms. A correspondence approach to vowel harmony and disharmony. ROA-293.
- Lass, Roger
1984 *Phonology: An introduction to basic concepts*. (Cambridge Textbooks in Linguistics). Cambridge, UK, New York: Cambridge University Press.
- Lewis, Geoffrey
1967 *Turkish Grammar*. Oxford: Oxford University Press.
- McCarthy, John J.
1997 Sympathy and phonological opacity. Paper presented at the Hopkins Optimality Theory Workshop, University of Maryland Mayfest 1997, Johns Hopkins University.
- McCarthy, John J.
1999 Sympathy and phonological opacity. *Phonology* 16: 331–399.
- McCarthy, John and Alan Prince
1995 Faithfulness and reduplicative identity. In: Beckman, Walsh Dickey, and Urbanczyk (1995: 249–384).
- Mohanan, K.P.
1997 Falsifiability in OT. *Linguist List* 8: 575.
linguistlist.org/issues/8/8-575.html.
- Mohanan, K.P.
2000 The theoretical substance of the optimality formalism. *The Linguistic Review* 17: 143–166.
- Piggott, Glyne L.
1999 How phonological is the phonology of nasality? Paper presented at the 4th HIL Phonology Conference (HILP4), Holland Institute of Generative Linguistics, Leiden.
- Ploch, Stefan
1997 The nasal fallacy. In: Ploch and Swinburne (1997: 221–273).
- Ploch, Stefan
1998 Non-switch harmony and Yawelmani (and Turkish and Sakha). In: Ploch and Scott (1998: 209–238).
- Ploch, Stefan
1999 Nasals on my mind. The phonetic and the cognitive approach to the phonology of nasality. Ph.D. thesis, Department of Linguistics, School of Oriental and African Studies (University of London).
- Ploch, Stefan
2001 Link Phonology: a functional explanation of non-monotonicity in phonology. *CLS* 37.2: 123–138.

- Ploch, Stefan
2002 Evidence-based phonology—the elimination of ghosts. Paper held at the 9th International Phonology Meeting, Vienna, November 2002. To be published in the selected proceedings.
- Ploch, Stefan
2003a Can 'phonological' nasality be derived from phonetic nasality? In: van de Weijer, van Heuven, and van der Hulst (2003: 73–116).
- Ploch, Stefan (ed.)
2003b *Living on the Edge. 28 Papers in Honour of Jonathan Kaye*. (Studies in Generative Grammar 62.) Berlin/New York: Mouton de Gruyter.
- Ploch, Stefan
2003c Metatheoretical problems in phonology with Occam's Razor and non-ad-hoc-ness. In: Ploch (2003b: 149–201).
- Ploch, Stefan
2003d Why the study of phonology is a cognitive (not a phonetic) discipline. Paper held at the University of Natal (Durban), May 2003.
- Ploch, Stefan
forthc. Evidence-based phonology—the elimination of ghosts. Paper presented at the 9th International Phonology Meeting, Vienna, November 2002. Under review for the selected proceedings.
- Ploch, Stefan
ms. Review of Rachel Walker's "Nasalization, Neutral Segments, and Opacity Effects". Cf. languages.wits.ac.za/~stefan/.
- Ploch, Stefan and Gary-John Scott (eds.)
1998 *SOAS Working Papers in Linguistics and Phonetics, volume 8*. School of Oriental and African Studies (University of London).
- Ploch, Stefan and Andrew Simpson (eds.)
1996 *SOAS Working Papers in Linguistics and Phonetics, volume 6*. School of Oriental and African Studies (University of London).
- Ploch, Stefan and David Swinburne (eds.)
1997 *SOAS Working Papers in Linguistics and Phonetics, volume 7*. School of Oriental and African Studies (University of London).
- Popper, Karl R.
1934 *Logik der Forschung*. Wien: Julius Springer Verlag. With the imprint [1953].
- Popper, Karl R.
1959 *The Logic of Scientific Discovery*. London: Hutchinson. English translation of Popper (1934).
- Popper, Karl R.
1972 *Confutations and Refutations: the Growth of Scientific Knowledge*. 4th edition. London/Henley: Routledge & Kegan Paul. First published [1963].
- Popper, Karl R.
1973 *Objective Knowledge: an Evolutionary Approach*. Reprint (with corrections). Oxford: Oxford at the Clarendon Press. First published [1972].
- Popper, Karl R.
1983 *Realism and the Aim of Science. From the "Postscript to the Logic of Scientific Discovery", volume 1*, edited by W.W. Bartley, III. New edition. London/New York: Routledge. First copyrighted [1956].
- Remison, John R.
2000 OT and TO. On the status of OT as a theory and a formalism. *The Linguistic Review* 17.2–4: 135–142.
- Sampson, Geoffrey
1999 Educating Eve: the 'Language Instinct' Debate. New York: Cassell.

- Stevens, Kenneth N. and Sheila E. Blumstein
1978 Invariant acoustic cues for place of articulation in stop consonants. *Journal of the Acoustic Society of America* 64: 1358–1368.
- Weijer, Jeroen van de, Vincent J. van Heuven and Harry van der Hulst (eds.)
2003 *The Phonological Spectrum, volume I: Segmental structure*. (Current Issues in Linguistic Theory 233.) Amsterdam/Philadelphia: John Benjamins.