ARGUMENTS AND NON-ARGUMENTS FOR NATURALNESS IN PHONOLOGY:
ON THE USE OF EXTERNAL EVIDENCE

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§1.0 The concept of naturalness has become a major concern for many phonologists. In my view, the concept of naturalness should be best regarded as a basic principle of a phonological theory and should be tested by the judicious use of external (or substantive) evidence.

As to the relationship of naturalness to psychological reality, my point is that a natural phonological analysis of a phenomenon claims psychological reality for its concepts and constructs. However, not all psychologically real constructs in a phonological analysis need to be phonologically natural. E.g., a phonological process (henceforth PR) posited by the linguist may refer to constructs of natural morphology (cp. Mayerthaler to appear), especially in case of so-called morphonological rules (cp. Dressler, 1977a).

§1.1 In the theory of Natural Phonology (henceforth NatPhon), as proposed by Stampe since 1968 (see now Donegan and Stampe to appear) and 'Polycentristic Phonology' (Dressler 1977a), naturalness occupies a central place. Phonological systems are phonetically (and I add, psychologically and, to a lesser degree: sociologically, historically) motivated. The basic constructs of Natural Processes in the sense of "mental substitutions which systematically but subconsciously adapt our phonological intentions to our phonetic capacities" (Donegan and Stampe to appear, §1, including its perceptive converse) are substantive universals.

§1.2 Similar to adherents of NatPhon, S. Schane and M. Chen (see Sommerstein 1977, 230, 233) have claimed that particular languages select PRs from a fixed universal set of natural processes and may impose constraints on their applicability. In the best of cases a PR forms a subset of a universal process (as characterized by the theory) and any restrictions vis-a-vis the general form of the respective universal process can be derived from the hierarchies of the universal process and from a fairly small number of principles of restrictions.

But what if a PR is not such a regular subset of a universal process? In this case NatPhon (or at least Polycentristic Phonology) cannot appeal to frequency or intuitive plausibility, but
must explain why the given PR is not a regular subset of a universal process. Several avenues are open: 1) Modification of the universal process. 2) The deviation is due to language acquisition; in this case well-motivated linguistic and psychological concepts must explain the deviancy. 3) The deviation is due to historical circumstances (including sociological factors). 4) The PR is not totally (phonologically) natural, a possibility avoided in NatPhon (but cp. Dressler 1977a; Sommerstein 1977, 235f). Since such PRs (diachronically) must go back to totally natural PRs, explanation 4) includes explanations 3) and 2).

§1.3 Thus, it becomes clear that external evidence, at least from language acquisition, diachrony, and sociolinguistics is not external for NatPhon, but forms an integral part of the area it has to cover. Moreover, there is no theoretical or methodological principle which should exclude other dimensions of external evidence from investigation:

§1.3.1 Take sociophonology: The restriction to the investigation of only one level of formal, maximally differentiated speech as practised in most of generative phonology and almost all of structural phonology is an undue limitation of interest and of access to natural speech, whose variation is apt to give important insights even to formal principles of rule application (cp. Dressler 1975). However, any detailed and theoretically sound work on casual vs. formal speech presupposes the inclusion of both, psychological/psycholinguistic theory (cp. Vanecek and Dressler 1977) and sociological/sociolinguistic theory (cp. Wodak and Dressler 1978).

§1.3.2 Or: The differential (and always non-random!) breakdown of phonology in aphasia gives important insights into the structure of phonology. However, studies so far have not completed the desirable integration of all disciplines relevant to aphasia, e.g. the brilliant thesis of Keller (1975) neglects all recent phonological theories, whereas the present writer’s studies (Dressler 1977b; 1978) have not yet integrated neuropsychology. For other types of external evidence, see Linell (1974), Fischer-Jürgensen (1975, 22ff), Zwicky (1975), Skousen (1975).

§2. Non-arguments for naturalness

In the literature we find certain non-arguments/fallacies:

§2.1 "Facts about the real working of the brain are most important". Anttila (1977, 221) believes to have found direct evidence, as opposed to indirect neuro-linguistic evidence, against generative grammar, when he cites the biologist W. Wieser about the brain not working exactly, often blundering and correcting itself, not proceeding logically, but according to similarities, being extremely redundant, etc. However, Wieser has informed me that these phenomena at the micro-level do not preclude precise rules at the macro-level (which is the level of interest for linguistics), just as Heisenberg’s indeterminacy relation does not vitiate the precise working of laws of classical physics in macrophysics. Here we might speak of a micro-anatomic fallacy.

§2.2 There is a similar fallacy which one might call the macro-anatomic fallacy or mistaken equation of phonology and phonetics, which is an exaggeration of the Physical/Phonetic Basis Condition (Botha 1978 II, 16ff) of phonology. This line of argument neglects the interaction between phonological and morphological or phonological and lexical naturalness (cp. Dressler 1977a) and of what Hyman (1977) has called phonologization (which in my view starts with allophonic PRs producing extrinsic instead of intrinsic allophones).

§2.3 Still more common is the false equation of naturalness with concreteness, since as a result of refusing the abstractness involved in standard generative phonology, many phonologists have regarded concreteness as a virtue in itself. However, phonological concreteness has often been achieved at the expense of morphology for which very few 'concrete phonologists' (e.g. Skousen 1974) have cared to provide a theoretical framework. More important still, concreteness has been defined (if at all) as restrictions on the relationship between underlying phonological and surface phonetic representations. In my opinion it is possible to define the naturalness of processes and of representations (be it as structural symmetries as found by phonemicists or natural asymmetries as derived from processes, see Stampe (1973)), but not the naturalness of relationships between representations. Notice both the failure of Kenstowicz and Kisseberth (1977) to find universal formal constraints on the distance between phonological and phonetic representations. In my opinion it is possible to define the naturalness of processes and of representations (be it as structural symmetries as found by phonemicists or natural asymmetries as derived from processes, see Stampe (1973)), but not the naturalness of relationships between representations. Notice both the failure of Kenstowicz and Kisseberth (1977) to find universal formal constraints on the distance between phonological and phonetic representations (cp. Gussman 1978, 154, 167f; Sommerstein 1977, 237 n. 47), and the undesirable results of the much more rigorous restrictions of Natural Generative Phonology (see Hooper (1976) and...
its critique by Gussman (1978, chapter 1) and Donegan and Stampe to appear, §3.1., §4).

As an example I simply want to refer to the abstract analysis of German [ŋ] as underlying /ng/ (discussed in detail in Dressler to appear; cp. Dressler (1977a, 51)). For the much debated PR g + O/— — (except before non-centralized vowel), I have found external evidence, e.g. in loan-word integration and sociophonological variation (e.g. ['anga] vs. ['anga] 'Angela'), in child language (Mandarine 'tangerine' — [manga:ri:] vs. [manga:ri:] and aphasia (see Stark 1974). Thus, multiple external evidence has been found in support of the psychological reality of this PR, although this analysis implies a very abstract underlying representation (cp. Kenstowicz and Kisseberth (1977, 7f, 51), Gussman (1978, 168), see below §3.4).§2.4 Often natural is falsely equated with productive. This equation (Fischer-Jørgensen (1975, 228f); Skousen (1975); Linell (1976), etc.) might hold most of the time, but not always (Dressler * (1977a, 1977c)). §2.5 Still weaker and never explicitly justified is the equation of natural and (e.g. typologically) frequent. Frequency might be a first indicator for the phonologist looking for universals, but what counts is explanation in the sense of causal argumentation. §3. Counterarguments against external evidence §3.1 "External evidence is unnecessary, internal evidence suffices". This 'Nonnecessity Thesis' has been proved by Botha (1978 II) to be incompatible with empirical mentalism. Formal, 'pure' linguistics cannot alone do the job of vouching for psychological reality. Due to the serious underdetermination of standard data (internal evidence), various sources of external evidence must be adduced (cp. §1.3 and Botha (1978 II, III §5.3)). §3.2 "External evidence is too unclear". However, internal evidence based on intuitions as utilized in generative phonology is unclear itself in many respects as Ringen (1975) has shown. Moreover, it must be noted that evidence from diachrony and loan-word integration should be accepted by many who shun other external evidence. Unfortunately, the use of both types of evidence has been grossly simplified by most generative phonologists; for loan-words see Fischer-Jørgensen (1975, 229), Kiparsky (1973, 112ff), Dressler (1977a, 35ff). As to diachrony, both structuralists and generativists have limited themselves far too often to nomological explanations (e.g. symmetry, rule simplification), while neglecting the all-important genetic explanation, e.g. by confusing sound change with sound correspondences; thus context-free processes have been liberally adduced as evidence, although they are, I believe, always the final result of generalizing context-sensitive sound change.

§3.3 External evidence shows "what in fact counts as internal evidence" (Kenstowicz and Kisseberth 1977, 3). Does this mean that e.g. English loan-words in Japanese might be used to demonstrate the necessity of morpheme structure constraints or redundancy rules within phonological theory, but not for corroborating their specific forms in Japanese itself? §3.4 "Internal evidence is more important than external evidence", a view held by many (called the Nonprivileged Status Thesis by Botha (1978 II, 12f)). However, quite apart from its theoretical shakiness (cp. §1), there are counterexamples: E.g. the abstract analysis of English [ŋ] as /ng/ rests on exceptional (and thus suspect) alternations like [OAD], [OOGA:ES], whereas the normal, productive superlatives are e.g. [OOG:EST], [WHIN:EST]; but external evidence for the abstract analysis is excellent (starting with Fromkin (1973, 223)). Even more extreme is the German situation, where in most varieties internal evidence is restricted to distributional evidence (Vennemann 1970), which generativists usually esteem much less than evidence from alternations, alternations in this case exist only in external evidence (see above §2.3). §3.5 Botha (1970, 130ff) has deplored the 'qualitative type jump' from internal to external evidence and the lack of criteria of adequacy. Since then he has revised his standpoint and has demanded the construction of 'bridge theories' mediating between linguistics and other disciplines relevant for the given type of external evidence (Botha 1978 III, 27ff). But 'hyphenated' disciplines, such as psycholinguistics, sociolinguistics, neurolinguistics have strived just for that since many years! §3.6 "External evidence is often divergent and incoherent" (Gussman (1978, 167f) happily cites Dressler (1977d, 224), where higher standards in the use of external evidence are demanded). Here Botha (1978 II, 30, 27ff) correctly states "that the relative weight of a given kind of external evidence is a function of the
adequacy of a particular bridge theory". In other cases conflicting external evidence may force us to revise phonological theory (e.g. in the case of introducing Korhonen's concept of 'quasi-phonemes' in Dressler (1977a, 52ff). §3.7 In connection with §3.6 I want to discuss a problem which seems to strike a heavy blow to the theory espoused here: I have linked naturalness firmly with the universality of natural processes. However, processes actually studied, show different hierarchies, both typologically and in external evidence (cp. Drachman 1977; Drachman 1977), although hierarchies have been claimed to be an integral part of the universal processes constructed by NatPhon. Whereas Atomic Phonology has found a purely formal solution (criticized by Donegan and Stampe (1977)) to this problem, I want to come back to §1.2. The reactions of an individual to innate physiological and psychological restrictions are determined both by maturation and social environment. In this way I agree neither with (rather mystical) strong claims about innate universals (as in certain quarters of TG), nor with the arbitrariness of the outcome of societal constraints (as implied in marxist critiques of TG). Therefore (in Dressler 1977a) I have spoken only of universal tendencies (one type being universal processes) which necessarily conflict and must be compromised by the language learner: Thus, certain universal processes are suppressed either in the language as a whole or in certain domains of external evidence; or they are restricted in ways allowing different process hierarchies. Moreover, a typology of phonological processes must consider advances made in the theory of typology: e.g. ordering typologies may be multilinear (with branchings).

References


