

## IN FAVOUR OF THE ARCHIPHONEME

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Halle's famous argument against an autonomous phonemic level was originally constructed in order to decide between two possible conditions on a phonological description. Since, according to Halle - and I believe most linguists would agree on this statement - phonological segments are theoretical constructs, they must be appropriately related to observable data. The weakest form of condition for such a phonological description reads as follows in Halle's (1959, 21) setting:

(1) A phonological description must provide a method for inferring (deriving) from every phonological representation the utterance symbolized, without recourse to information not contained in the phonological representation.

There are, however, many cases where distinctively different utterances will be pronounced in the same way. The Russian expressions mok by 'were (he) getting wet' and mog by 'could (he)' would be pronounced identically: ['mogbɨ]. Similarly, there is no way to decide directly from the phonetic data that the nasal in Swedish imbecill 'imbecile' and inbilsk 'conceited' represents phonologically distinct segments. Obviously Halle is right when he states that "it must be possible to read phonological representations regardless of whether or not their meaning, grammatical structure, etc., is known to the reader". Given quite natural assimilation rules and phonemic representations of the form /mok bi/, /mog bi/, /imbesil:/ and /inbilsk/ condition (1) will be satisfied. But many, not to say most, traditional phonologists would not accept these representations, since they are impossible to arrive at from the utterance alone. The only kind of data, available to the adherent of pregenerative linguistics, was speech forms in the Bloomfieldian sense, i.e. actual physical utterances. Every conscious appellation to introspection was, accordingly, held to be unscientific. Therefore one would ask for a decisive method to derive the underlying form directly from the utterance itself. Halle formulates the condition necessary to satisfy this demand in the following way:

(2) A phonological description must include instructions for inferring (deriving) the proper phonological representation of any speech event, without recourse to information not contained in the physical signal.

The most obvious way to arrive at a proper phonological representation, given condition (2), is to use one symbol for every sound one is able to detect in the physical signal. But since, actually, the number of possible sounds is unlimited, one must restrict the number of symbols used so as to represent only distinctive sound types and leave "allophonic" variations to be accounted for by phonological rules. The fact that such a restricted application of condition (2) requires that we logically ought to represent English hang with the same symbol for both the initial and the final segment of the word, i.e. give the sequence in question one of the phonological representations /ŋæŋ/ or /hæh/, has mostly been rejected because it is intuitively absurd. However, phonological representations are still theoretical constructs which need no psychologically motivated support. Therefore I cannot understand why we should reject an abstract representation /hæh/ as psychologically disturbing, when we just have decided to prefer the psychologically suspect phonological representation /mog bi/ for mok by to the psychologically motivated representation /mok bi/. Accordingly, accepting the restricted version of condition (2) entails our giving up all psychological or intuitive motivations for constructing phonological representations, and it also forces us to accept the principle 'once a phoneme, always a phoneme'. If we do not, our conception of the phoneme will be quite empty and arbitrary.

Given an arrangement of grammar based on condition (2) - which must be the only reasonable one, if we want to avoid psychological motivations - Halle's argument against the phonemic level is quite devastating. It runs as follows: in Russian all obstruent phonemes occur in voiced-voiceless pairs which often alternate morphophonemically. Thus we have alternations such as /gorət/ 'city' - /gorədə/ (g.sg. of the same word), /mosk/ 'brain' - /mozgə/. The three obstruents /c/, /č/ and /x/, however, do not possess voiced cognates. Followed by a voiced obstruent they are nevertheless voiced. This means that we have to write the same obstruent assimilation rule twice in the grammar, first for obstruents which are paired with regard to voice and then for the unpaired. The only way to save condition (2) without increasing the complexity of the grammar is to show that the argument given by Halle is somehow invalid.

There have been many attempts to invalidate Halle's argument. I fail to see, however, that anyone of those counterarguments I know of are really successful. Johns (1969, 375) tries to get rid of the argument in this way:

"The fact is that the voicing assimilation rule is always phonetic, since voicing is never distinctive in obstruents in the position before another obstruent. In other words, if we have a way of representing the concept of neutralization, which is surely not incompatible with the notion of a phonemic level, Halle's problem becomes totally spurious."

As a matter of fact, Halle demonstrates how to represent neutralization in such a way, viz. the concept of archiphoneme. But given condition (2) there is obviously no way of making the concept of archiphoneme compatible with the notion of an autonomous phonemic level. What one cannot detect in the physical signal has, according to this view, no relevance for a linguistic description, and, certainly, one does not pick up archiphonemes without reference to data not contained in the physical signal. The embarrassing fact is that we all know, whatever our attitude to the question of the existence of independent phonemics as opposed to morphophonemics may be, that the voicing assimilation in Russian actually is a phonetic phenomenon. But as adherents of condition (2) we are not supposed to know this. Thus when Johns wants a phonological description powerful enough to account for neutralization he actually argues in favour of condition (1), i.e. the phonemic level which Johns claims to be compatible with the concept of neutralization is, in fact, a different phonemic level from the one Halle and his generativist followers have rejected. Johns' phonemic level is identical to Halle's morphophonemic level.

Derwing (1973, 186) following Householder (1967) rejects Halle's argument by the same means. But unlike Johns, he explicitly states: "The proper morphophonemic rule involved in such a neutralization is therefore as follows: 'all obstruents are unspecified for voicing before other obstruents' and the proper phonetic rule is precisely the generalization which Chomsky and Halle suggest: 'all obstruents are voiced before voiced obstruents' (or, more generally, 'all obstruents assimilate in voicing to the final obstruent of an obstruent cluster')." Thus Derwing agrees with Halle's original argument against the phonemic level. His proposal could have been a counterargument against Halle, if there had been empirical evidence that absolutely all obstruents are unspecified for voicing before other obstruents. But Derwing's morphophonemic rule is ad hoc constructed in order to save condition (2). That is to say that Derwing commits himself to the same kind of axiomization that he accuses Chomsky and Halle of. For consider what his proposal means: Russian mog in the sequence mog by should be morphophonemically

represented as /MOK/, a representation that, certainly, no Russian native speaker could have incorporated in his grammar. There is no doubt about the fact that Halle's own proposal to separate /c/, /č/ and /x/ from other obstruents and assign their voicelessness by way of a special rule is the sounder solution. Observe that this does not force us to specify other obstruents for voice in position before an obstruent. The difference between Derwing and Halle is this: while Derwing insists that all obstruent morphophonemes in the given contexts are unspecified as to voice, Halle says that the three morphophonemes /c/, /č/ and /x/ are always unspecified whereas the other obstruents need not be specified in neutralizing contexts, i.e. their specification for voice is redundant (cf. Halle, 1959, 61, 63f.).

Linell (1974, 105) recapitulates the arguments of Johns and Derwing, but he goes on to postulate that there is "no clear empirical support" for the principle 'once a phoneme, always a phoneme'. Since it is exactly this principle that is at stake here, there would be no empirical evidence for Halle's argument either. But given condition (2) I cannot see that Linell's assertion holds. Obviously it can be empirically proven that the segment [g] in ['mogbɪ] is, in a certain sense, the same sound as [g] in [got], which is the phonetic output of Russian god 'year' and stands in phonemic contrast to kot 'male cat'.

Moreover, Linell cannot be convinced of the relevance of Halle's argument until he finds a case, where in spite of a neutralization rule morphophonemes could appear, which do not participate in the rule. Such a situation would, according to Linell, turn up in a case where X and Z are bundles of features and have the same feature specification except for one feature and the distinction X - Z is phonemic and, furthermore, we have two kinds of morphemes: (i) morphemes which are X in a context C\_\_D as opposed to A\_\_B and are subject to the neutralization rule in the context A\_\_B, and (ii) morphemes that do not participate in the alternation and are X in the context A\_\_B. As a matter of fact, Halle's original argument constitutes such a case, if we define the archisegment as a not fully specified segment in the underlying form. But since Linell seems to be sceptical about the psychological relevance of this concept, I shall present a case where such a rule really seems to exist.

In the indigenous system of Czech there is no phoneme /g/, old [g] having developed into [h]. Czech has the same obstruent assimilation and final devoicing rules as Russian. Consequently words like gde 'where', kdo 'who'

and k Brnu 'to Brno' ought to be phonemically represented as /kde/, /kdo/ and /kbrnu/. (Undersived prepositions in Czech form one phonological word together with the following word.) Now, words beginning with a vowel are optionally preceded by a glottal stop [ʔ]. Czech Amerika can thus be pronounced [ʔamɛrika] or [amɛrika]. If the voiced preposition v 'in' stands in front of a word with initial vowel, the preposition will be devoiced to [f] as a consequence of the obstruent assimilation rule; but, N.B.!, this will happen irrespective of whether or not there is a glottal stop between the preposition and the vowel; v Americe 'in America' is therefore pronounced either [fʔamɛricɛ] or [famɛricɛ]. That the unvoiced preposition k 'to, till' remains unvoiced in the same position is not surprising: k otci 'to father' is pronounced [kʔɔci] or [kɔci]. These two variants are both commonplace in Standard Czech (Prague) pronunciation. In certain other Czech dialects the preposition v is not devoiced before a vowel; the pronunciation [vamɛricɛ] is thus also possible. But in such dialects where v does not devoice, the preposition k tends to become voiced; for k otci one would here say [gɔci], as if otci were immediately preceded by a voiced obstruent. Observe that the segment [g] never occurs before a vowel, liquid or nasal in any dialect other than in this sole case. As a consequence we find in Czech just that situation Linell asks for: though there is no phoneme /g/ and [g] regularly replaces [k] before voiced obstruents, we may nevertheless establish minimal pairs such as [kɔlɛji] - [gɔlɛji], corresponding to koleji (dat.sg. of kolej 'boarding-house') and k oleji 'to the oil' (formed from olej 'oil') respectively.

Now, if I were a classical phonologist and thus a true adherent of condition (2), I would say either that there must be some sort of boundary phoneme that could account for both the devoicing of v and the voicing of k, or I could quite straightforwardly give the Czech /g/ the status of an independent phoneme, since one can in any case state the existence of minimal pairs such as [kɔlɛji] - [gɔlɛji] and, furthermore, /g/ in Czech clearly has phonemic status amongst foreign words, giving rise to minimal pairs such as gáže 'salary' - káže 'preaches' and grog (phonetically [grɔk]) 'grog' - krok 'step'. Furthermore, as an argument against an autonomous phonemic level my example suffers from the fact that I must suppose an underlying segment - a voiced glottal stop - which it is impossible to realize at surface level. Such animals simply do not exist.

Now that I have given the fortress up, I may as well continue to play

the role of the devil's advocate. The opponents of Halle's proposal referred to so far have tried to escape its consequences by invalidating condition (2), an approach which is doomed to failure. Whatever the value of Chomsky's (1964) analysis may be, it has at least clearly shown that the only possible position that can be taken by the strictly data-oriented phonologist is to faithfully stick to the principle of invariant biuniqueness. And there is no possibility to do this and simultaneously adhere to the principle of complementary distribution. In English /k/ is in complementary distribution with lax /a/. This means that both socked and Scot should be phonemically represented as /skkt/. The only solution is to give up the principle of complementary distribution and add as a prerequisite of a proper data-oriented description of the phonemic system of a given language, that it gives an exhaustive account of which natural phonological classes the phonemes of the language in question can be divided into. English has at least the two main classes of vowels and consonants. Since /a/ and /k/ can be shown to belong to separate classes, defineable both articulatorily and acoustically, the proper phonemic representations of socked and Scot will be /sakt/ and /skat/ respectively. For the same reason hang shall be represented as /hæŋ/, since /h/ belongs to the class of glides and /ŋ/ belongs to the class of nasals.

Halle's argument hinges on the fact that unless one does not give up the notion of an autonomous phonemic level, one is forced to divide the natural class of obstruents into two independent classes. However, Halle has not shown that the phonemes /c/, /ç/ and /x/ really have the phonemic status within the Russian system that he ascribes to them. I shall now show good grounds for questioning his assumption.

It is true that the segment [ç] in certain contexts has a distribution which allows us to determine it as being an allophone of /x/. But this is not the whole truth. Firstly, [ç] is in these contexts a free variant of /x/. Russian v gorách by 'were it in the mountains' can be pronounced both [vgə'raxbɨ] and [vgə'raxbɨ], whereas mok by obligatorily is pronounced ['mogbɨ]. Furthermore, it is not clear that /x/ shall be classified as an obstruent at all. The voiced counterpart is in any event to be characterized as a glide. Henning Mørk has brought to my attention the fact that modern Greek shows up with a similar distribution: here the non-palatal and palatal obstruents /x/ and /x, / have as voiced cognates the glides /ç/ and /j/. Even on articulatory grounds it is reasonable to place Greek /x/

and /x,/ in the class of glides. They are, actually, not clearcut obstruents but rather approximants. This also holds for Russian /x/. It is a constant complaint of authors of handbooks on Russian phonetics that the sound corresponding to the cyrillic letter X is symbolized [x] and not [h].

There is another peculiarity about [ɣ] which Halle leaves totally out of account. The sound in question is generally a very rare one, but when it does occur it is far more often a variant of /g/ than of /x/. In elderly style a word such as kogda 'when' is pronounced [kɑɣ'da]. The exclamation gospodi! 'Lord!' is almost invariably pronounced [ˈgɔspəd,ɪ]. Oblique forms of bog 'god' are pronounced either ['bɔɣə], ['bɔɣu] or ['bɔgə], ['bogu]. To my knowledge it has not been observed that these pronunciations reflect a semantic difference. Pronounced with the fricative the forms refer to the Christian god, whereas the stop implies a reference to a pagan god or the Christian god equalled with pagan gods. Observe that the plural forms bógi, bogóv etc. are always pronounced with the stop. Finally, the word buchgalter 'book-keeper' is, according to the literary norm as given by Avanesov & Ožegov (1960), always pronounced [bu'ɣalt,ɪr].

Thus, faithful to condition (2) we can state both that it is doubtful whether /x/ might be classified as an obstruent at all and whether /x/ has no voiced cognate /ɣ/ and whether [ɣ] should properly be regarded as an allophone of /x/ and not /g/.

What regards the affricates /c/ and /č/, these sounds are clear obstruents. But Halle's argument turns out to be completely vacuous, once it is considered that these sounds could equally well be analyzed as the phoneme sequences /ts/ and /tš/. When the dental stop and a sibilant come close but a morpheme boundary separates the two sounds as in détstvo 'childhood' (formed from the stem det and the suffix stvo) the combination is pronounced in exactly the same way as the affricate [c]. The reason for treating the affricates as single phonemes is that within morphology they alternate regularly with single stops: pekú 'I bake' alternates with pečúš' 'you bake'. A similar kind of morphological alternation occurs in Swedish, where the verb skära 'cut', phonemically /šæ:ra/, has the preterite skar. Nobody would suppose the sequence /sk/ to be a single phoneme, simply because it alternates with /š/. The standard argument against the biphonemic analysis of the Russian affricates, however, rests on their distribution. In certain cases a so-called unstable vowel is inserted before the last consonant of a word-final consonant cluster. When such a form ends in an affricate the un-

stable vowel is inserted before the affricate, not between the two segments constituting the sound; cf. otéc 'father' (<sup>\*</sup>ottes) and its genitive otcá. The reason for this peculiarity is again the fact that the affricates go back to old single segments (Old Slavonic otĭkŭ). Something of the same principle also works for the combination stop - sibilant but the other way round. From the verb mstit' 'revenge' you form the deverbal noun mest' with the same type of unstable vowel before the sibilant. The sequence /st/ of this verb - and there is a host of equivalent cases to be found in modern Russian - corresponds to the simple phoneme /š,/: mšču, /mš,u/, 'I revenge' contrasts with mstiš' 'you revenge'. Thus, if you take /ts/ and /tš/ to be single phonemes, you must do the same thing with at least /st/ and /sk/. And that would be absurd. Consequently, the only sound phonemic analysis of the Russian affricates is the biphonemic one.

If my analysis of the Russian affricates and any of my assumptions on the voiced velar [ɣ] are accepted, then the main argument against the taxonomic phonemic level is definitively invalidated. It is true that Halle's argument has been repeatedly reformulated on material from other languages, for example by Bach (1964, 128) on German data, and Bach's argument has subsequently been reformulated on Norwegian data by Hovdhaugen (1971). But in both cases it is obstruents and the /r/-phoneme that are compared. Since /r/ in both German and Norwegian must be classified as a liquid, and liquids per se belong to a separate class, Bach's and Hovdhaugen's argument is not valid.

Another argument against an autonomous phonemic level was given by Kiparsky (1965, 4f.). It concerns the Old High German umlaut of a, which resulted in a fronted and non-raised vowel if the umlauted vowel was long but a fronted and raised vowel if the umlauted vowel was short. According to Kiparsky it is possible to formulate this phenomenon in one rule:

$$\begin{array}{ccc} \text{-consonantal} & \longrightarrow & \text{-grave} \\ \langle \text{long} \rangle & & \langle \text{compact} \rangle / \text{---} C_n i \end{array}$$

Thus there is no longer any reason to believe that umlaut first created fronted allophones of /a/ and raising afterwards incorporated the fronted allophones in the phoneme /e/. Vennemann (1972), however, has convincingly demonstrated that it is probably incorrect to handle the raising as part of the umlaut rule. Rather there was one phonological rule proper, i.e. the rule fronting back vowels before [i]. In addition to this rule there was



a general constraint - as in Modern Standard German - with the effect of prohibiting short low front vowels at all levels of the grammar. Thus the old opinion that /a/ and /e/ (the result of raised [æ]) were distinct phonemes because they contrasted in environments other than before syllables containing [i] can be maintained.

The arguments against an autonomous phonemic level can doubtlessly be multiplied, but I strongly suspect that it will always be possible for a phonologist who strictly holds to condition (2) to refute them. It must be so, because he always has sturdy and substantial data to refer to. To take such a position is therefore a safe way of going about linguistics. Nothing can dislodge you and you are always rescued from the plague of thinking. Now I deliberately confess that I am greatly impressed by some recent highly critical studies of the metatheoretical basis for generative phonology. I have in mind books such as those by Botha (1971), Derwing (1973), Linell (1974) and Karlsson (1974). I also admit that I have very little to object to in the criticism that has been directed against generative grammar in general as in the books by Itkonen (1974) and Hiorth (1974), though the conclusions I would like to draw myself might diverge from those made by the authors mentioned. Finally, I declare that never have I been fully convinced of the viability of the phonology practised by Chomsky and by Halle from the early sixties, a thing which developed into the remarkable kind of morphological phonology exhibited in Lightner (1966, 1967) and above all in Chomsky & Halle (1968). But even the very fact that orthodox generative phonology has wallowed in the mire of exorbitant abstractions far from rhyme and reason and that a strict and calm data-oriented linguistics can obviously tell us much more about reality than the excessive speculations of generative phonologists ever could, cannot convince me that the neotaxonomic orientation of Derwing and Linell is the right way to set about it all.

As a matter of fact, there is very little of substance that divides us from one another, but the points in which I disagree are important in principle. In taking the standpoint of Derwing and Linell you do actually adhere faithfully to Halle's condition (2). But in spite of all that can be said against condition (1) it will remain my position. It must be possible to supply the phonological description you have constructed on the basis of physical data with relevant information provided through the use of introspective evidence.

Derwing (1973, 188ff.) discusses Spanish e-epenthesis and Russian pala-

talization. In Spanish there is a general phonotactic constraint prohibiting a word from beginning with s + stop. Words, inherited or borrowed from Latin, originally beginning with such a cluster, are in Spanish rendered: España, estudiante etc. But this also happens to new borrowings: English scar takes the form eskar, stop will be rendered estap. We could formulate this phenomenon as a phonological rule:

$$\emptyset \rightarrow e / \_ \# sC$$

where C is a voiceless stop

Now, Derwing asks, does the formulation of this phonological rule make it plausible that the underlying form of España is /span,a/? Of course not. The Spanish epenthesis is still a general phonotactic constraint, something similar to Vennemann's [æ]-raising rule of Old High German. Derwing's position prevents him from realizing the difference between phonotactic constraints and phonological rules proper. Most assimilation rules are of the latter type. Therefore it is not possible to place Spanish España on a par with Russian sledovat 'follow', where the stem sled throughout the conjugation has the form [s,l,ɛd]. Why then do we not join Derwing in proposing the underlying form /s,l,edovat,/? The Russian palatalization is a phonological rule with the effect of making all consonants of a consonantal cluster - with some well defineable exceptions - conform in palatality with the last consonant of the cluster. Thus from the noun mesto, [m,ɛstɔ], 'place' we get the locative v meste [v,m,es,tɨ]. Accordingly, in other environments palatalization is of that typical phonetic character which we would have liked to have called allophonic, had palatalized consonants not been established in phonemic contrast with non-palatalized. But now, observe a peculiarity about the Russian palatalizing vowels. Whenever a palatalizing vowel in Russian is not preceded by a consonant, it is realized as [jɛ], [ja], [ju], [jo]. The only exception is the high front vowel [i]. As an adherent of condition (1) I can without exaggeration suppose that there really are no palatalized phonemes in Russian but a palatalizing segment, which I shall define as a high front syllabic or non-syllabic vocoid. In agreement with old taxonomic principles of economy my solution will reduce the number of consonant phonemes in Russian by half. Chomsky and Halle have been accused of making a phonology of English with the spelling as a model, correctly I think. If Derwing cannot accept my postulated underlying palatalizing segment I am inclined to accuse Derwing of writing a phonology for Russian

with the spelling in mind. Accordingly, I propose that the underlying form of sledovat' is /sljedovatj/.

Both Derwing and Linell are remarkably anxious to admit that neutralization could be of allophonic character. Thus I fail to see why German Bund could not have an underlying form /bund/, why its phonological structuring necessarily must be /bunt/. As Karlsson (1974, 53, note 74) points out final devoicing is a rule which is natural, productive, and exceptionless. Karlsson also points to the fact that this is a rule that carries over when learning a foreign language. In Derwing's theory the last point, of course, is no argument, since his phonological component is build up of articulatory habits and does not contain rules. And it is, presumably no argument for Linell either, since it is not possible for Bund to occur on the articulatory plan with a voiced final stop. But is it really quite certain that this is also true of the perceptual structuring of the word? I shall not deny that Linell may be right that the identification of [t] in Bund with [d] in Bunde could be more a matter of morphology than of actual perceptual structuring. Nevertheless it is strange that German, Russian, Polish, Czech and a host of other languages, which have this rule in common, spell the devoiced obstruent with the symbol for the voiced sound. In Serbo-Croatian, which has an almost phonemic spelling, one writes the word for 'sparrow' vrapac and its genitive vrapca. This spelling, which obviously is in accordance with Linell's perceptual structurings, causes the Serbo-Croatian school children great difficulties. They spell intuitively vrapca and must be trained to achieve the phonemic and phonetic spelling vrapca. If they really hear nothing but an unvoiced stop in this word, and, certainly, they cannot produce anything but [vrapca], why should it then be so disturbing to write down the internalized phonological structuring directly without checking the morphological paradigm? My assumption is - and this assumption is supported by evidence from native speakers of Slavonic languages - that the children actually hear [b], though they can reasonably only perceive [p], exactly in the same way as I hear [n] in inbilsk, though I should be able to perceive nothing but [m].

In his famous article on Russian conjugation (1948) Jakobson develops a procedure of how to arrive at base forms (to be differentiated from phonemic forms) which are very much the same as the archiphonemic representations I shall propose her. In Russian all unstressed vowels except /u/ are neutralized into [ʌ], [ɨ] or [ə]. These three reduced vowels are in complementary distribution and could, accordingly, in classic taxonomic phonology be re-

presented by the same vowel symbol, say /ə/. The noun nogá 'leg, foot' is pronounced [nʌ'ga]. But since the first syllable receives stress in the accusative: nógu and in the plural nominative: nógi, Jakobson would establish the base form of the stem as being /nog-/. In the loan-word kovbój 'cow-boy', however, the first syllable is never stressed, and here Jakobson would have chosen the representation /kavbój/, whereas Troubetzkoy would have preferred to classify the unstressed vowel as the archiphoneme /A/, which gives the representation /kAvboj/. I shall in cases of uncertainty stick to this type of mixed morphoarchiphonemic-phonemic representation. In cases where I feel I have good reason to believe that a neutralization really only reflects the operation of a phonological rule, I shall take the morphophonemic representation to be identical with the phonemic. Thus I shall suppose that the underlying form of Serbo-Croatian vrapca is /vrabca/ and of German Bund is /bund/. This is an assumption that goes against Troubetzkoy, who would have preferred the representation /vraPca/ and /bunT/ respectively. Observe that I say feel and believe, which means that I have no other data for my decision than what I believe I have understood correctly from native Serbo-Croatian and German speakers' statements about their intuitions.

What then with a case such as Russian nogá? Would it not be a sound decision to take its underlying (base) form to be /nogá/ as Jakobson would? There are actually certain facts that are against such a solution. Morphophonemic change per se cannot be allowed to decide the question for me. As a matter of fact, Russian school children have great difficulties in learning the spelling of the Russian unstressed vowels. A Russian child who is not fully trained in spelling would therefore write noga as well as naga. In Eyelo-Russian orthography the full consequence of this difficulty has been taken into account and the a-writing has been generalized for all non-stressed non-front non-high vowels. It looks, furthermore, as if the a-spelling is more natural, whereas non-correct o-spelling seems rather to be a case of hypercorrection. It could thus be so that what we have here is a kind of paradigmatic vowel alternation, very much the same as the German morphologized umlaut. Since I, nevertheless, cannot be sure that this is the case, I prefer, until convincing evidence in either direction has come to light, to assume that the vowel in question is a not fully specified segment, i.e. the archisegment /A/, not specified with respect to the features back and low. Notice, however, that I do not exclude the possibility that

either one of the phonological representations /nogá/ or /nagá/ could be equally correct. Observe that the representation /nagá/ implies that I take the a-o-alternation to be morphological as opposed to morphophonemic. The really embarrassing failure of orthodox generative phonology was that this distinction was never made.

Thus the Russian a-kan'e phenomenon can in a certain sense be equated with the problem of the proper specification of a stop following an [s] in English and Swedish. Observe that none of the variants of unstressed Russian o or a can be phonetically identified with their stressed counterparts. The unstressed variants all share certain features with both [o] and [a]. But Russian native speakers tend to identify the unstressed allophones with [a] rather than with [o]. In the same way, an English native speaker is said to identify the second segment of spin with [p] rather than [b]. Stampe (1972, 34ff.) argues in favour of the [p] solution because "in general stops after /s/ are phonologically voiceless". As evidence for this statement we have orthographical tradition, slips of the tongue like [hwipsr] for whisper, babytalk [phæk] for spank, the fact that intensifying [s] occurs in pairs such as mash/smash and trample/strample but never occurs before a voiced stop. Stampe also refers to pre-school children's spontaneous spelling, such as SCICHTAP for Scotch tape and SKEEIG for skiing. (I have myself on the other hand registered spellings such as SGA for ska 'shall, will' and SBARA for spara 'spare, collect'.) All these facts and Stampe's own theory of the so-called "natural phonological processes" - for a printed summary of his theory see Stampe (1969) - tell us that every language possesses an inventory of phonemes "which is at least indirectly accessible to it's speakers' consciousness". Stampe means that this assumption is critical to an understanding of how alphabets are used and devised. I can follow Stampe this far, and as yet his theory is not incompatible with my own ideas of the impact of the concept of archiphoneme. But Stampe, furthermore, takes the fact that archisegmental theories of underlying representations are unable to identify the inventory of phonemes as a criterion of the fallibility of such theories. As far as I can tell, this cannot be a valid argument at all. There is not the slightest reason why alphabets should be able to represent archiphonemes, i.e. segments which in underlying form and/or at surface level are not fully specified. What is really at stake here is the simple fact that the voicelessness of the second segment of spin is of higher value than the features the segment

in question shares with [b], i.e. its being unaspirated and lenis. For this reason all those peculiarities occur which could indicate an identification of [ph] and [p]. But this is nothing that per se could invalidate the concept of archiphoneme. For it is so that our identification of sounds is one thing and the factual specification of them quite another. Therefore I can argue that the last segment of Bund is /d/, whereas the second segment of spin is the archisegment /P/, i.e. a segment unspecified with respect to the feature of voice.

It would suffice to take the argument the other way round. In that variety of Swedish that I speak I make a systematic difference, phonemic and phonetic, between the vowels in fem 'five' and hem 'home'. I pronounce these words [fɛm] and [hem] respectively. However, until the age of about 30, when I first started studying linguistics, I had not the faintest idea that I made such a difference. As a Sturm und Drang poet of twenty I would consider these words to be perfect rhymes. Nevertheless it is selfevident that these words in my grammar are represented as /fɛm/ and /hem/. Therefore I mean that it is a very unwise thing to determine phonological representations from phonetic or introspective data only. A reliable phonological representation cannot be achieved without recourse to both physical and psychological information.

## References

- Avanesov, R.I. & Ožegov, S.I. (eds), Russkoe literaturnoe proiznošenie i udarenie, Gos. izd. inostrannykh i nacional'nykh slovarej, Moskva 1960
- Bach, E., An Introduction to Transformational Grammars, Holt, Rinehart & Winston, New York etc. 1964
- Botha, R.P., Methodological Aspects of Transformational Generative Phonology, Mouton, The Hague & Paris 1971
- Chomsky, N., "Current Issues in Linguistic Theory", Fodor, J.A. & Katz, J.J. (eds), The Structure of Language. Readings in the Philosophy of Language, Prentice-Hall, Englewood Cliffs, New Jersey, 1964
- Chomsky, N. & Halle, M., The Sound Pattern of English, Harper & Row, New York etc. 1968
- Derwing, B.L., Transformational Grammar as a Theory of Language Acquisition, Cambridge University Press, 1973
- Halle, M., The Sound Pattern of Russian, Mouton, 's-Gravenhage 1959
- Hiorth, F., Noam Chomsky. Linguistics and Philosophy, Universitetsforlaget, Oslo etc. 1974
- Householder, F.W., "Distinctive Features and Phonetic Features", To Honor Roman Jakobson, Volume II, Mouton, The Hague & Paris 1967, pp. 941-944
- Hovdhaugen, E., Transformasjonell generativ grammatikk, 2. ed., Universitetsforlaget, Oslo 1971
- Itkonen, E., Linguistics and Metascience (= Studia Philosophica Turkuensis II), Kokemäki 1974
- Jakobson, R., "Russian Conjugation", Word 4 (1948), pp. 155-167. Reprinted in Jakobson, R., Selected Writings, II, Mouton, The Hague & Paris 1971, pp. 119-129
- Johns, D.A., "Phonemics and Generative Phonology", Papers from the Fifth Regional Meeting of the Chicago Linguistic Society, Department of Linguistics, University of Chicago, Chicago, Illinois, 1969, pp. 374-381
- Karlsson, F., Phonology, Morphology, and Morphophonemics (= Gothenburg Papers in Theoretical Linguistics 23), Göteborg 1974

- Kiparsky, P., Phonological Change, M.I.T. Dissertation 1965. Reproduced by the Indiana University Linguistic Club 1971
- Lightner, T.M., "Ob alternacii e-o v sovremennom russkom literaturnom jazyke", Voprosy jazykoznanija XV:5, 1966, pp. 64-80
- Lightner, T.M., "On the Phonology of Russian Conjugation", Linguistics 35 (1967), pp. 35-55
- Linell, P., Problems of Psychological Reality in Generative Phonology, RUUL (= Reports from Uppsala University, Department of Linguistics), no. 4, 1974
- Stampe, D., "The Acquisition of Phonetic Representation", Papers from the Fifth Regional Meeting of the Chicago Linguistic Society, Department of Linguistics, University of Chicago, Chicago, Illinois, 1969, pp. 443-454
- Stampe, D., How I Spent my Summer Vacation, Unpublished University of Chicago Dissertation 1972
- Vennemann, Th., "Phonetic Detail in Assimilation: Problems in Germanic Phonology", Language 48:4 (1972), pp. 863-892