THOUGHTS ON ANALOGY AND SOME PROBLEMS IN INTERPRETING
PHONOLOGICAL EXPERIMENTS[1]

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Abstract: An inconclusive pilot study in Danish phonology gives
         rise to questions about testible differences between
         explanation by analogical algorithm versus rules.
         The literature on analogy shows its resistance to
         valid limitations on its operation in terms of marked-
         ness, similarity, or on the basis of purely conceptual
         or grammatical considerations. It is argued that
         being the less constrained mechanism, it is inferior
         as a working hypothesis to rules. It is suggested
         that the convincing instances of synchronic analogy
         are special cases where a speaker resorts to a more
         basic cognitive strategy as ill-defined and hence
         powerful as our ability to recognize similar aspects
         of nonidentical complexes.

1. Introduction

These remarks begin with the description of a pilot study
undertaken to test the status of consonant gradation regularities
in native speakers' grammar of modern Danish. Out of this very
restricted experiment rose a number of questions about the possi-
bilities of interpreting such data in terms of analogy as opposed
to generative rules. The focal point of this paper lies in the
discussion of these questions. If I devote some space to the
discussion of the pilot study, which for a number of reasons to
be mentioned later cannot be said to contribute substantially to
our understanding of consonant gradation in Danish, it is because
I believe the confrontation with such an experiment can increase
our awareness of some of the problems involved in research on
the "psychological reality" of phonological alternations.

1) This paper was made ready for ARIPUC in the summer of 1976
    during the author's stay at the Institute. [Editors' note.]

No attempt has been made to cover all the relevant literature
on analogy, but the positions discussed do, I think, represent
the crucial viewpoints in the history of this long and involved
debate. In any case, the remarkable lack of progress on this
topic since the Neogrammarians suggests to me that the problem
may lie in the formulation of the question itself. It is hoped
that the synoptic view presented in the following pages, while
not new in the particulars, will nevertheless be of help to those
who run across similar problems in the design and interpretation
of grammatical experiments.

The term consonant gradation as applied to Danish (Rischel
1970) comprises several morphophonemic alternations, which as
Hans Basbøll (Basbøll 1974) has pointed out, all have in common
the weakening of a consonant in syllable-final position. "Accord-
ing to this principle the phonemes /p,t,k,d,g,r/ are manifested
as [p^h,t^s,k^h,d,g,ʁ]^1 in syllable initial position and as [b,d,g,
ð,ɣ,ɒ] in syllable final position ..." (Basbøll 1974a, p. 43).
This pilot study was specifically concerned with the alternation
between [ɣ/g] and [ð/d], as for example in Danish <u>røde</u> - <u>rødt</u>
'red' [ʁœ:ðə - ʁœd] or <u>søge</u> - <u>søgt</u> 'to seek - sought' [sø:ɣə -
søgd]. Considering only surface forms it is clear that these
alternations are not automatic, since there are surface [d]'s
and [g]'s that do not alternate even though the phonetic environ-
ment is present. In the relatively well leveled morphology and
phonology of modern Danish these alternations are in fact re-
stricted in native vocabulary to a few morphological positions.
They do not generally occur in adjectives in [-ɣ], but do show
up in weak verbs with infinitives of the form [-ɣə], e.g. <u>søge</u>,
<u>smage</u>, <u>bage</u>, <u>koge</u>, <u>bruge</u>, when a [d] (participial ending) is
added. (With stems in [-ð] it is not possible to interpret the
forms unambiguously because the [ð] disappears before [d] as for
example in the verb [mø:ðə - mød].)

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1) In the text the letters [b,d,g] represent [b̥,d̥,g̊]. The
    neuter singular adjective ending and the participle ending
-t are written [d], i.e. [d̥], even though [t^h] occurs.

The pros and cons of several variant generative descriptions of gradation have been discussed in the literature (Rischel 1970, Austin 1971, Basbøll 1974a,b). The form of the rule is not at issue here but rather the status of the regularities described in the grammar of native speakers. Formulating the question in terms of "psychological reality" hardly offers any advantage in specificity and hence testibility. This concept or bundle of concepts is not sufficiently well defined at the moment. I would like rather to begin with a more specific question: will Danes extend these given regularities to words which they believe are Danish, but which they have never encountered before, i.e. nonsense words which fill accidental gaps in the vocabulary of modern standard Danish (Rigsmål)? If under these conditions consonant gradation appears, we can conclude that speakers have not merely memorized independent forms showing the change, but have at some level learned the regularity as such, and that the latter is productive in the sense of the experiment. The question of what model best describes the mechanism involved will remain open.

2. The pilot study

2.1 Method

Using work on Danish syllable structure (Spang-Hanssen 1959, Vestergård 1967, Basbøll 1973, 1974a) it was possible to construct a list of nonsense monosyllables with one of the four vowels i̲,ø̲,u̲,a̲ as the syllable peak flanked by one two or three consonants. Adjustments were made for the distribution of vowel allophones, length restrictions $(^{*}V{:}_\eta, {}^{*}V{:}\begin{Bmatrix} b \\ d \\ g \end{Bmatrix}^{1})$, and to assure that

adding the ending [d] would not produce a malformed cluster.

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1) The sequences V:g and V:d do not generally occur in adjective and verb stems. Stems of the form V:b do occur but are frequently subject to diphthongization and in consultation with Jørgen Rischel it was decided to exclude the entire class $V{:}\begin{Bmatrix} b \\ d \\ g \end{Bmatrix}$

The monosyllables were then screened with Politikens Rimordbog ('Rhyming Dictionary') to eliminate already existing forms. The nonsense words were also checked by a native Dane and trained linguist, Jørgen Rischel, who is of course not responsible for any errors in the list. An attempt was made to weed out those forms which might cause problems in audibility, seemed marked as childish, vaguely obscene sounding, or onomatopoetic - even if the criteria for these judgements will not be made explicit. Stød was assigned wherever the phonetic basis was fulfilled (Basbøll 1972) and it was compatible with the morphological patterns of Danish.

The entire material was recorded on tape by Jørgen Rischel. The subjects saw nothing but the scales to be marked in the fifth section of the test (see below). The first section began with a statement that the goal of the study was a description of Danish sentence patterns and their intonation.[1] The subjects were warned that many of the adjectives and verbs they would hear might sound a little strange because they were not a part of everyday Danish vocabulary and because the subjects would hear a number of them during the course of the experiment. It was suggested that the words could be found in older Danish literature, handicraft and agricultural language, and modern nonsense poetry. The subjects were instructed to give the answer that sounded best regardless of whether they were familiar with the word in question.

The first section consisted of 26 short sentences with nonsense verbs in the present tense. The participants were asked, after having heard each sentence twice (5-6 sec.), to record the sentence changing it to present perfect. For example: "De [tʁɑlgɒ] gennem junglen" 'They (nonsense verb) through the jungle' → "De har [tʁɑlgd] gennem junglen". Two examples were

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1) The linguists were well aware of the real point of the study, while the non-linguists had no prior knowledge of its purpose. This was considered acceptable in a pilot study and is taken into account in the discussion of the results.

given using nonsense verbs two and three syllables long and in-
capable of a change in the stem consonant. This section had a
dual purpose: first to see which weak ending (d/ə$\left\{\begin{smallmatrix}ð\\d\end{smallmatrix}\right\}$) the sub-
jects would use and whether this was predictable from the struc-
ture of the stem. The forms represented most of the possible
stem types according to final consonantism. Among them were two
stems in [ð] and three in [ɣ][1] to see if subjects would harden
these before the weak participial [d]. Hardening would not be
expected before the other weak participial ending [ə$\left\{\begin{smallmatrix}d\\ð\end{smallmatrix}\right\}$]. The
timing was identical in sections 1-4. In each of the sections
the crucial forms, i.e. those stems capable of showing the alter-
nation [ɣ/g] or [ð/d], were mixed with other stems in order to
conceal the focus of the study and reduce any learning effect
within each section. The ratio of crucial to dummy forms was 1:3.

Directions for the third section were identical to those
for section one, except that 16 sentences with nonsense verbs in
the present perfect tense were to be changed to present tense:
"Hun har [kvi:'ld] tøjet i en stor gryde" 'She has (nonsense verb)
the clothes in a big pot' → "Hun [kvi:'ɪɒ] tøjet ...". Among the
participles were four in [gd] and two in [d] to see if Danes would
soften the [g] or reintroduce the [ð]. The same example sentences
were used as in the first section just going in the opposite di-
rection - present perfect to present.

In the second section the subjects were given sixteen sen-
tences with a neuter predicate adjective, i.e. one with a [d]
ending. The cue sentences had the form: "Det her (neuter noun)
er (neuter adj.)", 'This _____ is _____.'. Subjects were asked
to change each sentence so that it had the form: "De fleste
(noun's plural form) er (adjective + plural [ə])", 'Most _____
are _____.'. Among the nonsense adjectives were four stems in
[gd] which could be interpreted as alternating with [gə] or [ɣə].
For example: "Han har [vøgd] ..", 'He has (nons. vb.)..' could be-
come in the present tense "Han [vøgɒ].." or "Han [vø:ɣɒ]..".
There were also two stems in [d].

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1) This paper distinguishes categorially between stop "[g]"
    and continuant "[ɣ]", the latter symbol being used irre-
spective of weakening to semivowel or zero in actual forms.

Section number four required subjects to change a sentence of the form: "Mange (plural noun) er (adjective + plural [ə])." 'Many ____ are ____.' to one of the form: "Men det her (singular noun) er utrolig (adj. + neuter [d])." 'But this ____ is unbelievably ____.'. Examples were the same as in part two just going in the opposite direction - plural predicate adj. to neuter singular. The crucial forms, those which could show an alternation in the final stem consonant, were identical except for the initial consonant(s) within sections I/IV and sections II/III. The difference between corresponding crucial forms was thus for all practical purposes limited to morphological and semantic information.

The fifth section had a different character from the preceding four. The subjects were told that, as they perhaps had guessed, not all of the new adjectives they had heard existed in Danish, but that these might appear some day in a nonsense poem or as a brand name. They were asked to listen to a list of these words and rate their relative "Danishness" on the basis of their first impression. A scale from 1 (normal Danish) to 10 (not at all Danish sounding) was printed on a piece of paper so that they could record their impression of each item.[1] It was suggested that the ratings need not be equally distributed over the whole scale. The words read were simply the crucial forms in each section. Subjects were informed of the morphological category from which each of the forms came, whether present tense verb, neuter adjective, etc. Half the participants heard the list in the same order they had heard the crucial items during the course of section I - IV; the other half heard the same order of sections but with each list read from bottom to top. Each form was read twice (2-3 sec.) and the subject had 4-5 sec. to mark an answer.

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1) The scale was patterned after an 11 point scale used by Greenberg and Jenkins (1964) in a very similar rating test in English. Several of the linguist-participants in the present study felt the scale was approximately twice as broad as advisable.

There were 9 subjects in the pilot study, all native Danes and Rigsmål speakers. Five were linguists connected to the Institute of Phonetics, while four had little or no training in linguistics. The cue sentences were played back to each subject over headphones and responses were recorded by means of a desk microphone and a second tape recorder. Each subject was alone during the test.

2.2 Results

The following paragraphs contain a synopsis of the results for the crucial forms according to final stem consonant and morphological category.

2.2.1 Verbs $-\gamma \upsilon \twoheadrightarrow -\begin{Bmatrix} \gamma \\ g \end{Bmatrix} d$

Of the 234 responses only 5 first responses showed the weak participial ending [d]; all others ended in $\vartheta \begin{Bmatrix} \delta \\ d \end{Bmatrix}$ regardless of the structure of the stem. Three of the five variant responses were given by subject number 2 in sentences 1 and 2, who then revised her answer to sentence 2 using the [əð] ending and continued to use this ending (with one exception) throughout the rest of the section. Because of this strong preference for the [əð] ending none of the final [d] or [g] stem consonants came into a position where we would expect hardening. Thus only the potential changes $gd \twoheadrightarrow \begin{Bmatrix} \gamma \upsilon \\ g\upsilon \end{Bmatrix}$ will be of interest in the verbs (see table 1, cues 43, 48, 49, 55). As mentioned before, the change in [ð] is not subject to clear interpretation because of possible assimilation or deletion before [d]. The two [ð] stems in the first section were included for the sake of balance and curiosity.

2.2.2 Adjectives $-\delta \vartheta \twoheadrightarrow -\begin{Bmatrix} \delta \\ d \end{Bmatrix} d$

The surface [ð] to [d] change between plural and neuter respectively occurred in 7 of the 10 responses by linguists but only in 1 of 5 by non-linguists (some of the non-linguists' re-

Table 1

Presence or absence of alternation in the crucial forms

O	No alternation ɣ/g or ð/d
X	Alternation present
	Response not usable
P	Interpretation of cue as Past Participle

Verbs

	Cue	Response		Subject								
			1	2	3	4	5	6	7	8	9	
3	kriːðɒ	krid	O	O	O	O	O	O	O	O	O	
15	vuːðɒ	vud	O	O	O	O	X	O	O	O	O	
43	sløgd	slø(ː)ɣɒ	X	O	X	X	X	X	O	X	O	Linguists
48	kvagd	kva(ː)ɣɒ	X	X	X	X	X	O	O	O	O	
49	migd	mi(ː)ɣɒ	X	X	XO	X	O	O	O	O	O	Non-Linguists
55	vøgd	vø(ː)ɣɒ	X	O	X	X	X	O	X	O	O	

Adjectives

	Cue	Response		Subject								
			1	2	3	4	5	6	7	8	9	
68	bluːða	blud	O	X	X	X	O	O	O	O	X	
70	fiːða	fid	O	X	X	X	X	O	O	O		
67	paːɣe	pagd	X	X	O	O	O	O	O	O	O	
65	toːɣe	togd	X	O	O	X	O	O	O	O	O	
61	fliːɣe	fligd	X	O	X	O	O	X	O	O	O	
27	vrigd	vri(ː)ɣe	P	X	P	X	O	O	O	X	O	Linguists
30	fnagd	fna(ː)ɣe	P	O	P	O	O	P	O	O	P	
39	pøgd	pø(ː)ɣe	P	O	P	O	O	PP	O	O	P	Non-Linguists
32	kvad	kva(ː)ðe	O	X	O	O	O	O	O	XO	O	
42	kred	kre(ː)ðe	O	X	O	O	O	X	O	O	O	

sponses could not be used because one merely repeated the plural form and another used a different stem altogether; see table 1, cues 68, 70). The remaining responses showed [ðd], confirming Basbøll's contention that this is the productive strategy (Basbøll 1974b, p. 64).

2.2.3 Adjectives $-\gamma\partial \twoheadrightarrow -\left\{\begin{matrix}\gamma\\g\end{matrix}\right\}d$

Here 4 out of 15 linguist responses showed [gd]. Only 2 of 9 non-linguist responses evidence the alternation. (See table 1, cues 61, 65, 67.) These data are difficult to explain as adjectives do not normally participate in this change.

2.2.4 Verbs $-gd \twoheadrightarrow -\left\{\begin{matrix}\gamma\\g\end{matrix}\right\}d$

12 out of 20 linguist responses interpreted the final stem consonant as a [ɣ] and this with some consistency. The non-linguists behaved differently. For the first two crucial examples (43, 48) none of the subjects 6, 7, 8, or 9 softened the [g]. Then on items 49 and 55 (of which 55 has the same vowel as 43) two of the non-linguist subjects softened the [g] to [ɣ]. (See table 1, cues 43, 48, 49, 55.)

2.2.5 Adjectives $-gd \twoheadrightarrow -\left\{\begin{matrix}\gamma\\g\end{matrix}\right\}\partial$

Considering the same change in adjectives the picture is even more complex. Among the linguists' 15 responses, 6 had the alternation. One subject consistently interpreted the [gd] form as a past participle and merely added [ə] in the plural. This strategy makes it impossible to get any evidence for or against the existence of the alternation in these cases. None of the non-linguists' responses showed the softening. The change $d \twoheadrightarrow \left\{\begin{matrix}\eth\\d\end{matrix}\right\}\partial$ in adjectives was found in 4 out of 10 responses by linguists and once (quickly revised) among the 8 answers from non-linguists.

The fairly consistent difference between the linguists' and the non-linguists' responses in sections I - IV also appears in part V. The item averages for the former ranged from 1 to 2.4 while the latter's item averages ranged from 4 to 7.8. Conversations with non-linguists after the test indicate clearly that they were not aware that they were working with nonsense forms during the course of the first four sections. Several reported that they were upset that so few (in fact none) of these words, which they felt they ought to know, were familiar to them.

2.3   Problems of interpretation

In order to be able to generalize any conclusions to Danish about the subjects' treatment of nonsense syllables we must be reasonably certain that they treated the constructed verbs and adjectives as though they were Danish. The data support this conclusion. The low ratings given by the linguists in part V attest to the well-formedness of the nonsense stems. While the higher ratings given by the non-linguists could be used as an argument against this conclusion, I believe that these subjects' informal comments to the effect that they felt the words were genuine Danish should be taken into consideration. The higher ratings may be due to the fact that they did not have such ready access to information on well-formedness as linguists, who had no doubt dealt with the problems of accidental versus systematic gaps. But as Per Linell (1974) points out, we always run the risk that the linguistic knowledge we want to test "is being modified and manipulated in various ways through the very process of investigation (the testing or introspective analysis). It may happen that test subjects organize or make conscious their linguistic knowledge in an artificial manner during the test. In addition, various linguistically irrelevant factors may influence the output of the test or analysis." (p. 139). The fact that both groups may have been reacting to the semantic features which the carrier sentences suggested for the nonsense forms, if

they could in fact remember them, rather than to the structure of the items themselves, should not have skewed one group's responses in relation to the other.

The overwhelming majority of responses, especially among non-linguists, can be described in terms of invariant stems and the addition or subtraction of invariant endings in a given morpho-syntactic environment. However, the fact that the alternations were extended by non-linguists suggests that the subjects have not merely memorized the independent forms which show the alternation, but have at some level grasped the relationship between the alternating forms. The extension common to both groups occurred in adjectives [γə - gd] and verbs [gd - γɒ]. Only linguists softened the [g] in adjectives.

We can proceed a little further and ask on what data the extension is based. It cannot in any case be in analogy to or induction based on the other cue forms in the list, since none of these allows a change in the final stem consonant. If there is any rule to be induced from the majority of items on the list, it is to use invariant stems and endings. Could the extensions be based on analogy to forms of similar phonological, semantic, and morpho-syntactic make-up already existing in the speaker's lexicon? This cannot be the case with the adjective stems in [γə] which showed hardening in two responses from non-linguists, unless subjects ignored morphological categories suggested by the data. Adjectives in [γ] do not show the alternation; a few verbs do. It may be objected that the two responses were flukes of the test situation and statistically insignificant. The pilot study is clearly to be viewed with a critical eye as far as the generalization of the conclusions is concerned. But flukes or not, the two items under discussion are nevertheless responses, and the only pattern available is in the verbs.

Here the fairly consistent surface pattern is [γɒ ~ gd], and subjects might have been expected to produce new forms which fit this pattern. However, not surprisingly, only a few non-

linguist responses (4 of 16) showed the alternation and these
occur in items 49 and 55, not in 43 and 48 which differ from the
former only in the initial consonants. Again small numbers make
interpretation precarious, but if subjects were using model verbs
which rhymed (à la Ohala 1973), then 49 should have been less
likely to evoke the alternation since there are virtually no
verbs in [iɣ] which have hardening in the participle. And why
soften in 55 [vøgd] if not in 43 [sløgd]? This difference can
hardly be based on phonological cues.[1]

3. The appeal to analogy

3.1 Ohala and Hsieh

    This is as far as this particular pilot study can bring us
in the issues discussed so far. The really interesting question
of whether a classical independent phonological rule or an ana-
logical rule mechanism is the best explanation for these data
cannot be resolved here. But the difficulty in deciding this
question may not lie so much in the experimental design as in
the form of the question and its tacit assumptions about the
relationship between analogical and independent rules, as for
example Ohala (1973) has outlined them. What are the testible
differences between these two types of rules and what are the
ramifications of adding an analogical algorithm to the theory?
While in attempting to answer such questions one risks repeating
part of a long, well-known debate. I believe there is ample
justification to do so. First of all, the assumption that the
debate is a part of every linguist's basic education may not be

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1) Jørgen Rischel has suggested that there may be pressure against
    responding to [sløgd] with [sløgʋ] because apparently
only a few verbs which take the [d] weak ending have stems ending
in [d,g]. This fact is confirmed by my own visual search of
Gyldendals Dansk-Engelsk Ordbog (Danish-English Dictionary).
Speakers would then have to choose between two unusual things, -
a stem showing the alternation and a new stem type in the [d]
class of weak verbs. (There is precedent for such a type in the
form of such verbs with stems in [b].)

true for the upcoming generation of linguists. Second, and more importantly, an increasing number of scholars (Hsieh 1970, Ohala 1973, Vennemann 1972) are rejecting significant parts of the transformational model and invoking "analogy" as the best explanation for certain synchronic and diachronic data. Rare is the case in which more than passing mention is made of the impressive difficulties involved in defining what we mean by "analogy", even operationally, or of the implications for theory testing of adding such an unrestrained, powerful mechanism to a model of language. The work of John Ohala and Hsin-I Hsieh in synchronic linguistics is particularly interesting in this connection.

John Ohala in an unpublished paper "On the design of phonological experiments" and in "Experimental Historical Phonology" (1973) argues against the existence of vowel shift in English as an independent phonological rule and for the explanation of the behaviour of the subjects in question by an analogical phonological rule. According to Ohala, analogical phonological rules are ones "which for their application require not only information about the given phonological item but also information culled from the lexicon as a whole." (Ohala 1974, p. 25). Linguistically naive speakers of English were asked in one part of the experiment to fabricate new derived forms. They were primed with a form which really existed. Hearing the pair detain - detention as an example and cued with obtain, 18 out of 26 (all those who changed the stem at all) responded with [ʌb tʰɛnʃən]. Given explain - explanatory as an example and the same cue, most of the subjects left the stem unchanged, but 10 changed it to [ʌb tʰænətɔri]. 9 of these 10 were among the 18 who gave [ʌb tʰɛnʃən].

Ohala (1973) concludes as follows: "This shows among other things that the assumption by generative phonologists of unique underlying forms is not supported, because they would apparently posit a different underlying form for those words showing the [ej-ɛ] alternation versus those showing the [ej-æ] alternation (Chomsky and Halle 1968). But here we have the same word showing

both alternations in the speech of some subjects. So these derivations cannot be based on a single underlying form or perhaps, ... as I will suggest below, on any abstract underlying form. It also shows that the particular form of the derivations, contrary to the assumption of generative phonology, does depend on other words or pairs of words in the lexicon of the speaker. Having found, or in the present case, having been provided with suitable existing models, the speaker can pattern new derivations after them, that is, he can analogize."

The conclusion is carefully stated; the use of unique underlying forms and an independent phonological rule is not supported. But neither is their existence called into question except in this one special instance discussed by Ohala. He has shown that given an example, speakers, using something akin to the ability to recognize and reproduce rhyme, can solve an analogical proportion a:b::a':x and x = b'. Having specified three of the quantities, the predictability of x is hardly surprising. The interesting question in instances of analogy is, given a' and some information about x, what a and b will the speaker choose? Given bring and the fact that x must be the past tense form of the same verb, what factors affect the choice of which, if any, models to produce bringed, brang, or brought?

As for the distinction between analogical and independent rules with regard to their dependence on information from the lexicon, it has perhaps been made misleadingly clear. Generative phonological rules are in fact based on other forms in the lexicon, only in a much more explicit way than Ohala's analogical algorithm. An independent phonological rule could be described as a more or less permanent (from speech act to speech act) analogical rule stripped of all but the structural similarities of the set of alternating morphemes specified as relevant for triggering the rule. In most versions of the formalism and evaluation metric such an independent rule is constrained by the goal of encompassing maximally natural classes of forms. The analogical

rule is based on a subset of those data that underlie the independent rule, - a subset with one or a pair of members. The analogical rule refers to a specific form while the independent one represents an abstraction from this piece of data and others. The relatively unconstrained nature of an analogical rule makes it impossible to find an individual output for which an independent rule can be shown to be necessary and sufficient. It appears that an analogical rule can produce any individual output an independent rule can, but as Ohala's experiment shows, the reverse is not true. In fact, any distinction must lie in the predictions the two types of mechanisms make about the relatedness of various outputs. As Kiparsky (1972a, p. 280) points out, propositional analogical equations are not systematically related as independent rules may be (stress mine). The former cannot represent overall aspects of morphological organization but rather only relations between individual morphemes. The exact nature of the relation is left open.

Hsieh's experiments provide a broader argument for invoking an analogical mechanism than Ohala's, but the nature of the mechanism is equally vague. In brief, Hsieh (1975) tested five tone shift rules in Taiwanese by confronting native speakers with new forms (partially nonsense syllables), which were eligible to undergo tone sandhi, an exceptionless process among native morphemes in the correct environment. He presents a good deal of evidence that at least in the test situation speakers use a lexicon with surface form and handle new material by associating it with familiar forms and proceeding as though the new material followed the pattern of the familiar model.

Hsieh concludes (p. 132) "The experimental results, some of which have puzzled us earlier, can now be satisfactorily explained in terms of the power of association". Consistently more frequent application of two of the rules is "explained" by the larger number of forms in these tone categories. Differences in the applicability of a given rule depending on whether it is used

forward (base to sandhi form) or backward can be attributed to different "powers of association" for words in their base forms as opposed to their sandhi forms. Variation among different test items governed by the same rule stems supposedly from a different "degree of associability" based on phonetic, syntactic, and semantic factors (p. 130-132).

This sort of explanation does not solve the problems involved but rather restates them in terms of a more general cognitive process, one which is unfortunately not at all well understood despite decades of study. Like Ohala, Hsieh brings experimental evidence for the existence of some sort of analogical strategy, but both leave us hanging as far as the details of how analogy works and its relation to present linguistic models.

3.2 Traditional analogy

Unfortunately the background literature on this problem, as extensive as it is, offers a bewildering mass of data but no gain in specific explanation. Hermann Paul (1920, p. 109) for example agrees with Ohala and Hsieh on the importance of analogy in synchronic linguistic behaviour. He writes: "Man wird diesem Faktor des Sprachlebens (analogy) nicht gerecht, wenn man ihn erst da zu beachten anfängt, wo er eine Veränderung im Sprachusus hervorruft". While Paul allows for relatively unrestricted association of forms on the basis of partial identity in semantic, phonetic, and morpho-syntactic traits, he restricts the operation of the analogical equation as follows (p. 117): "Es muss ein jeder (Glied) mit dem andern irgendwie vergleichbar sein, d.h. in diesem Falle, es muss mit dem einen im stofflichen, mit dem andern im formalen Elemente eine Übereinstimmung zeigen. So lässt sich z.B. im Lat. eine Gleichung ansetzen animus:animi = senatus:x, aber nicht animus:animi=mensa:x". (A stofflich group would be all the case forms of one noun; a formal group - all datives, all causatives, etc.)

Paul provides no real arguments for requiring so much over-
lap. In the case of the example, animus:animi = senatus:x =
mensa:x, it is apparently not enough that _animus_ and _mensa_ are
both nouns, singular, and nominative. They must be of the same
declension. This requirement seems to contradict other propor-
tions mentioned by Paul such as gebe:gab = kann:konnte = bin:war
= lebe:lebte or processes like elision in French or the [c - x]
alternation in German, which he believes are undoubtedly ana-
logical. As a result he is forced to admit a large class of
exceptions which look like instances of analogical extension
but according to Paul lack the required degree of partial identi-
ty. These examples include the extension of an inflectional
ending or set of inflectional endings from one subclass of forms
to other subclasses, e.g. the extension of the -s noun plural in
English or the -s genitive in Danish from one subclass of nouns
to almost the whole class. In each of these instances the terms
have some common traits, as is almost trivially true of any two
forms in the lexicon. Hermann Paul believes in the central role
of analogy but is no good source of principled restrictions on
its operation.

The only other limitations placed on analogy by Paul or
later by Eduard Hermann (1931) are statements to the effect that
the association of forms or proportions is facilitated by maximal
overlap in sound and meaning, by the strength of the form in the
memory, and syntagmatic associational links. These are very
fragmentary and speculative observations. Deese's (1966) in-
vestigations into the interaction of grammatical class and asso-
ciations in English demonstrate that the situation is much more
complex than occasional casual references to associativity as
the basis for analogy would indicate. He found that both common
and rare nouns tend to elicit paradigmatic associates. Associ-
ates to common adjectives are more likely to be paradigmatic than
those to uncommon ones. Unlike other classes, syntagmatic re-
sponses to nouns define paradigms of meaning and do not necessarily

reflect the context in which the stimulus word appears. Most paradigmatic associates to adjectives are polar opposites or synonyms. For common adjectives they are overwhelmingly antonyms. The correlation of frequency of usage (Thorndike-Lorge) with the appearance of an antonym is .889 (p. 110). Facts like these must be taken into account in discussions of the structure of the lexicon, which must in turn be considered in determining how a speaker would search his lexicon for an analogical model.

Another attempt at limiting analogy so that it allows the historically attested examples which linguists want to call analogy but does not predict implausible or even absurd formations, has been made by Kuryłowicz (1945-49) and Manczak (1958). These restrictions come under the heading of marking. Nigel Vincent (1974, p. 430) summarizes their principal hypotheses in three groups as follows:

(i) morphological markedness - i.e. certain syntactic categories are unmarked - e.g. indicative mood, masculine gender, etc., and these are of relevance in establishing the direction of change.

(ii) length/strength of exponents - i.e. there is a tendency to form regular exponents of morphophonemic categories with elimination of zeros and retention of longer rather than shorter morphological endings.

(iii) elimination of redundancy and reduction of allomorphic variation, i.e. as in the previous case, changes operate to establish more regular correspondences between categories and exponents - this time by elimination of redundant variation.

The views of analogy presented so far are representative of the best thinking on this subject by traditional and modern grammarians, and yet the picture of analogy one gets is a very blurred one. Markedness studies deal in tendencies which allow many exceptions. They tell us mostly about the generalizations which apply to historical analogical realignment and only very indirectly about the process itself. Association studies have not yet been exploited as a source of information about the lexicon.

We are left with an intuitively grounded requirement of similarity between the terms of a proportional equation but no principled measure of the degree of overlap.

3.3 Generative criticism

Generative grammarians have had little to say about analogy beyond a rejection of it in both synchronic and diachronic models. In Cartesian Linguistics Chomsky (1966, p. 109) writes:

"To attribute the creative aspect of language use to "analogy" or "grammatical pattern" is to use these terms in a completely metaphorical way with no clear sense and with no relation to the technical usage of linguistic theory. A description in these terms is incorrect if the terms have anything like their technical meanings and highly misleading otherwise, in so far as it suggests that the capacities in question can somehow be accounted for as just a "more complicated case" of something reasonably well understood."

Regrettably, Chomsky does not reveal "their technical meanings" so it is difficult to judge the truth of his assertions. It is debatable too why we cannot consider as creative "the decision to exclude some concrete actually occurring factors as irrelevant while retaining others as central to a perceptual unity" - the essence of analogy according to Dinneen (1968, p. 102).

In diachronic studies analogy has been rejected as "often terminologically empty", a catchall "for irregularities in the operation of regular sound laws" (King 1969, p. 127). Analogy was reinterpreted as grammar change. The generative criticism of traditional analogy is summarized in King (1969) and Kiparsky (1974). It is threefold. If one requires only that there be some similarity between the items of the proportion then the mechanism is asserted to be not only too strong but also too weak. Because almost every lexical item has some trait(s) in common with every other, proportions can be set up and presumably

solved producing results which are unattested historically and
seem intuitively highly unlikely to occur in the future.
Ear:hear = eye:x x= heye. John knows Mary: Mary who John
knows = John knows Bill and Mary:x x = Mary who John knows
Bill and. Kiparsky suggests the proportion must correspond to
an actual or potential rule of the language and not violate
other constraints. Thus there is no rule in the case of ear:hear
and the rule in the second example violates the coordinate
structure constraint (Kiparsky 1974, p. 259). It is a fact,
however, that such formations occur especially in the speech
of children and in folk etymology, cf. four airplanes:formation
= three airplanes:"threemation" (Anttila 1972, p. 274).

King (1969) and Kiparsky (1974) argue analogy is too weak
by pointing out some cases which they claim cannot fit a pro-
portional equation. Kiparsky cites double plurals such as
mices, feets, mens, and changes in isolated non-derived forms
as for example the assimilation of loan words to native vocabulary
in stress and other characteristics.

But the irregular plurals mice, feet, men must be memorized
anyway. Why not explain the double plurals by saying that the
speaker chose the correct irregular plural stem but for some
reason (habit?) went ahead and formed the productive plural, by
analogy if one will, just as in the case of a normal stem. As
for examples like garáge > gárage I can see no way to make them
fit a four membered proportion. (However, Dinneen (1968) men-
tions other forms of analogy such as a:b=b:c or the basic
similarity relation a:b.)

Finally King (1969, p. 132) writes: "To show that Old
English caru 'care' gave up its plural cara for cares by pro-
portional analogy, one must produce an a-stem noun agreeing with
caru in some way. And we must do the same for dæd 'deed', tunge
'tongue', and a host of other nouns that did not originally take
a plural in -s. This simply cannot be done since, for one thing,
a-stem nouns ended only in consonants in Old English".

This is one of the many examples which even Paul recognized as not fitting the requirement of Übereinstimmung. But this depends on how much and what kind of overlap one requires. If you do not require that nouns belong to the same declension, then there are proportions available. a (noun sing.): a+s (noun pl.) = cara (noun sing.): x (noun pl.) x = cara+s. It seems to me one can complain in these cases that the mechanism is much too strong and that the direction of change is not predictable from the proportional model, but not that the mechanism is too weak.

The third and final objection to the traditional proportion model is undoubtedly the most serious. Because analogy applies to a form at a time, changes by analogy, it is argued, should proceed item by item. "But in reality it is only morphological analogy which is typically sporadic; in the case of syntactic phenomena on the one hand and purely phonological phenomena on the other, analogical change proceeds typically (though not always) across the board" (Kiparsky 1974, p. 260). Traditional analogy is then seen as one manifestation of "a universal process of simplification that ultimately goes back to the child's acquisition of grammar" (King 1969, p. 130). Then the asymmetry of across the board versus item by item spread could be a reflection of the generalization of morphological rules, which apply to designated individual lexical items, versus syntactic and phonological rules, which are more general (Kiparsky 1974, p. 262).

However, rule simplification will not explain it all, as King himself points out. (See also Vincent 1974, Skousen 1974). Kiparsky (1971, p. 590-4, 1974, p. 265) and Ohala (1974) give evidence that speakers do not necessarily learn the generalizations linguists deem simplest and hence optimal. Kiparsky (1974, p. 261) concludes as follows: "Therefore, the class of possible analogical changes that a language can undergo cannot be characterized on the basis of grammar alone, any more than it can be

characterized on the basis of surface structure alone. The
failure of the first two approaches brings us by default to
the third, in which reference is made to both grammar and sur-
face structure". This third approach is generally called
functional.

3.4 Functional explanations

Although there is some debate as to the status of functional
explanations within the theory, there is considerable agreement
on the tentative functional guidelines arrived at by various
scholars. Kiparsky (1972, p. 222) cited three substantive
targets imposed by performance on language change and hence
analogy. The first is maximal distinctiveness of categories,
which he relates to perceptual needs. The second is maximal
paradigm coherence, which he presumes to be mostly a matter of
ease of acquisition. The third is optimalization of phonotactic
structure to avoid overly complex articulations. For this last
one one might want to substitute preferred structures as the
target. In 1974 Kiparsky reiterates the importance of "learna-
bility", "perceptibility", and "producibility".

Already in 1969 Karl Heinz Wagner went beyond the generative
equation of analogical change with grammar change by suggesting,
a little too strongly perhaps, the automatic elimination of a
rule with low functional yield, reordering of rules if the order
has a low functional yield, and change in the abstract phono-
logical status of morphemes in the case of phonological ambiguity.

Vennemann (1972) like Wagner interprets analogy in terms
of grammar change, but he distinguishes two types of analogy on
the basis of motivation for each type. On the one hand, phonetic
analogy, or grammar simplification (when the formalism has been
adjusted to mirror various sorts of simplification), has as its
goal preferred phonological structure, but may lead to diversity
in the output, the linguistic sign, by introducing paradigmatic
variation. Conceptual analogy works toward uniformity of the

linguistic sign but may result in marked phonological structures.
Vennemann has dubbed this "innate principle of linguistic change".
Humboldt's Universal: "Suppletion is undesirable, uniformity of
linguistic symbolization is desirable: Both roots and grammatical
markers should be unique and constant" (Vennemann 1972, p. 184).
This corresponds quite well with Kiparsky's formulation (minus
the phonotactic constraints).

Nigel Vincent (1974) has pointed out the similarity of
these principles to those of Kuryłowicz (1945-49) and Manczak
(1958) and has tried to relate them for further explanatory
adequacy to Bever and Langendoen's (1972) work with perceptual
strategies. Strong unique, invariant grammatical markers would
facilitate the perceptual strategies' first guess as to the
morphemic structure. The tendency to maximize ease of percep-
tion would be countered by a tendency to maximize ease of pre-
diction (ease of learning). "The postulation of such a pair of
forces in mutual opposition will help us explain the continual
interplay along the time dimension of phonetic change and ana-
logical reformation, in a way that King's unidirectional prin-
ciple of grammar simplification cannot hope to do" (Vincent
1974, p. 435). These functional proposals provide more explana-
tion than merely attributing the change to grammar simplifica-
tion without showing why simplification takes place, i.e. in
what sense the system becomes simpler. But there is a good deal
less specificity as to how the analogical adjustment takes place
and no suggestions for getting at this question empirically.

4. Conclusion

On the one hand we have been confronted with data which
seem to require an analogical mechanism of the proportional type.
On the other we have seen that across-the-board leveling pheno-
mena, and one could add synchronic notions such as "unmarked

ending" or "productive process", can hardly be accounted for by the pairwise relations on which analogy is based. Because it has up to now been impossible to pin down the factors which determine the choice of an analogical model, rules, as one way of formalizing an empirical regularity as powerful as they are often lamented to be, remain the more constrained hypothesis and are thus the initial alternative which must be demonstrated to be implausible before one turns to analogy as an explanation. This is only sensible methodology. The instances for which analogy seems most plausible as an explanation are in fact special and have something in common. I refer to examples of analogy documented in language acquisition and to the behaviour of subjects more or less forced to cope with unfamiliar linguistic material. In the first case, the speaker has a smaller set of model forms and less experience with the statistically dominant regularities of the language. In the second, the speaker also apparently lacks an appropriate generalization and so must, if he does not merely balk at the task, use a more basic cognitive strategy. This ability to recognize individual elements in a larger complex and to factor out common similarities or differences goes by the name of analogy - perhaps in its elementary form not a predominantly linguistic ability at all. The data from association studies cited indicate that while linguistic usage accounts for some associative cohesion among our linguistic symbols, psychologists and with them linguists have only just begun to understand the extra-linguistic moment. This is, I think, important to consider if analogy looks like the bandwagon of the future.

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