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## ON MAŃCZAK'S FREQUENCY AND ANALOGY

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### Abstract

Witold Mańczak's oeuvre comprises various topics of historical linguistics. This article attempts to explain why some aspects of his theory are hardly accepted, yet his work still deserves interest and serious discussion.

### 1. Introductory remarks

There are about one thousand items in Witold Mańczak's bibliography – an impressive number, indeed. The diversity of discussed problems is likewise impressive. It would of course be unfair to reduce all these studies to one topic only and, then, to assess it negatively. And this is what actually happens nowadays as a rule. Frequency (or, in Mańczak's original wording: "irregular sound change due to frequency", see e.g. Mańczak 2010) has become, as it seems, a sort of a trademark of Mańczak's linguistic thought. After these remarks one might expect me just to show, in the remainder of this article, what else should be always borne in mind when talking of Mańczak. However, this is not what I am going to do, even if this aspect of his bibliography cannot be entirely ignored.

Two problems are connected with the assessment of Mańczak's work. One of them is mentioned above: reduction to one topic and rejection. The other concerns the fact that his opinions are more often than not just ignored or, at best, mentioned without a deeper discussion. This is, to be quite frank, not always the case. An example of a substantive discussion can be found in Best 1973: 82–107, a book concerned with analogy rather than frequency but also containing a discussion of Mańczak's interpretation of frequency. In addition, four articles published in the 67<sup>th</sup> volume of

the *Biuletyn Polskiego Towarzystwa Językoznawczego* (2011), i.e. two years after his “jubilee” article (Mańczak 2009), can readily be viewed as a discussion, all the more so because two of them defend Mańczak’s results (A. Bochnakowa & Z. Szkutnik<sup>1</sup>; I. Kraska-Szlenk), two criticize them (A. Bogusławski; A. Bańkowski), and Mańczak published a rejoinder the next year (Mańczak 2012).

I do not think that any linguist denies the existence and impact of frequency on the lives of languages. In what follows, I would like to present my opinion on some of Mańczak’s studies. Besides, I will try to make clear why Mańczak’s views are not especially vividly discussed (even at home, the Jagiellonian University in Cracow) despite the fact that he is definitely one of the most interesting linguists in Poland today.

## 2. Universality and hierarchy of methods and results

It was more than twenty years ago that I attempted to explain the curious fact that the declensional stem of the interrogative pronoun *tuox* ‘what?’ has in Yakut, a Turkic agglutinative (!) language, four phonetic variants (*tuox(-)*, *tuoy-*, *toy-*, *tug-*), as well as that three cases display double forms: dat. *tuoxxa* ~ *toyo*, acc. *tuoyu* ~ *tugu*, instr. *tuoyunan* ~ *tugunan*. My explanation was based on Yakut historical phonology and diachronic parallels. The main points of my reasoning were as follows:

1. The original long *\*ō* > modern Yak. *uo* in closed syllables, but > *o* in open ones, thus: *\*tōx* ‘what?’ > modern Yak. *tuox* (nominative and oblique cases with suffixes beginning with a consonant) ~ *toy+V* (dat., acc., instr.)<sup>2</sup>;
2. If the case suffix was *-u* (acc.) or began with *-u...* (instr. *-unan*) the regularly developed *-y-* found itself between two vowels: *\*oyu-*. This phonetic sequence is admittedly not totally absent from Yakut (cf. *olox* ‘life [nom.]’ → *oloyu* ‘life [acc.]’) but it will probably have appeared more seldom than *-ugu-* ~ *-ügü-*,<sup>3</sup> as in Yak. *uygu+güt* ‘your (pl.) richness’, *üñkü+güt* ‘your (pl.) dance’ (Stachowski 1990: 117). It was probably analogy that triggered the *\*oyu* > *ugu* change,<sup>4</sup> i.e. *\*tōx+u* (acc.) > *\*toyū* > modern Yak. *tugu*. This was the first case of analogy in the history of this Yakut pronoun.
3. The system resulting from the phonetic evolution was not really simple: five case suffixes had the stem *tuox(-)* (nominative, partitive, ablative, comitative, comparative), two had *tug-* (accusative, instrumental) and one had *toy-* (dative). It is not surprising, then, that the Yakut language tended towards simplification by analogy. Since Yakut oblique cases are produced by adding suffixes to the nominative form of nouns accusative, instrumental and dative were also made subject

<sup>1</sup> Z. Szkutnik, being a statistician, fully accepts Mańczak’s results.

<sup>2</sup> The consonant *y* results from intervocalic sonorization of *x*.

<sup>3</sup> Vowel length does not play any role in this process, so that variants like *-ugu-* and *-ügü-* are possible too.

<sup>4</sup> The consonant *y* is only found after low vowels in Yakut.

to this rule in the next phase of the evolutionary process, and thus secondary forms came into being: acc. *tuoyu* (< \**tuox+u*), instr. *tuoyunan* (< \**tuox+unan*), dat. *tuoxxa* (< \**tuox+xa*). This was the second case of analogy in our story.

The result of these processes was that new secondary forms (*tuoyu*, *tuoyunan*, *tuoxxa*) were longer than the older ones (*tugu*, *tugunan*, *toyo*).<sup>5</sup> This fact did not match Mańczak's opinion that shorter forms are newer and result from irregular shortening due to frequency. This discrepancy was an impetus for him to publish a critical article (Mańczak 1998) in which my explanation was rejected.

We have now reached the main point of the problem of hierarchy. Mańczak did not find any faults in my presentation. Nevertheless, he did reject the explanation of the facts of Yakut because it did not match the facts he knew from Romance and some other European languages (actually, their presentation takes up a greater part of his article). But the reason for his doing so was neither virulence nor unfounded stubbornness. No one who knows Mańczak personally (as I do) can believe that. Instead, it was his deep belief and strong conviction that his way of "operating analogy and frequency" provides results of universal validity. This universality manifests itself in two aspects: (a) the results are valid in an unchanged form in all languages of the world; (b) if they conflict with historical phonology, their witness is more significant than that of historical phonology.

My own opinion is very different. Analogical changes are caused by psychological processes and mental associations (cf. already Paul [1898: 96]: "Wie schon (...) hervorgehoben worden ist, attrahieren sich die einzelnen Wörter in der Seele [...]"). Different words are associated and different words are used especially frequently in various languages. These facts make a mechanical transfer of associations and frequentative hierarchy from one language to another impossible.

Moreover, languages display various grammatical categories. Mańczak's opinion that singular forms are more archaic and stable than those of other numbers (Mańczak 1983: 44 sq.) can be understood as the result of higher frequency of singular forms. This may easily be correct if the singular is opposed to the dual or the plural.<sup>6</sup> However, I am less sure that this rule is valid for the Basque language that, apart from singular and plural, has a number, called transnumeral, used to denote nouns whose belonging to singular or plural does not, in the given context, matter (cf. *a rose is a flower ~ roses are flowers*). At any rate, one has to reckon with quite different numeric proportions between singular and other numbers in Basque and, say, in French. The situation can be further complicated if a language does not always treat singular as the basic form of nouns, as is the case with the Tanoan family which expects some nouns to usually be used in the singular (e.g. 'man') and some in the plural (e.g. 'lips').<sup>7</sup> A noun does not possess any number marker if it is used in the expected number; otherwise

<sup>5</sup> On the terms "longer/shorter forms" see section 3 below.

<sup>6</sup> Nevertheless, even then paired organs like eyes and ears probably appear principally in the plural (or in the dual, if a language has it) and only sporadically in the singular.

<sup>7</sup> This presentation is admittedly somewhat simplified, all the same correct, even if not really complete.

it receives a suffix *-sh* that inverses the typical number into an untypical one, e.g. *vééla* ‘man’ – *véélash* ‘men’; *tyáha* ‘lips’ – *tyáhash* ‘lip’. In addition, nouns in the singular class (like ‘man’) refer “mostly to animates, including all kinship terms, persons, members of ethnic groups, officials, animals, birds and insects” (Mithun 1999: 81). Even if this class is rather large, the total number of all other nouns (that do not belong to the singular class) is doubtless higher. A dominant position of the singular is, thus, an illusion in the Tanoan family which means that universality of the “singular–plural/non-singular rule” is illusive as well.

These examples also show that individual features of specific languages can override frequency/analogy rules. This is valid for individual historical phonology as well. Frequency is an important constituent of analogy, and analogy has to explain irregular phonetic changes, rather than to dominate regular ones.

### 3. Longer and shorter forms

Even if Mańczak does not exactly define what he means while speaking of longer and shorter word forms the situation is generally not as bad as K.-H. Best suggests:

Ein weiterer terminologischer Fehler, auf den schon Kuryłowicz hingewiesen hat, besteht darin, daß Mańczak bei der Einstufung einer linguistischen Einheit als «länger» oder «kürzer» kein Kriterium angibt, nachdem [!, pro: nach dem] man «Länge» und «Kürze» objektiv messen könnte. Es wäre beispielsweise möglich, die «Länge» oder «Kürze» eines Wortes in Hinblick auf die Zahl seiner Phoneme, Silben oder Morpheme zu messen. Diesen Bezug stellt der Autor aber nicht her. (Best 1973: 105).

I do not possess a chronologically ordered index of what Mańczak wrote in each year so that I cannot say for sure if Best’s reproach was justified in 1973. In any event, it is not today. Mańczak does not explicitly define what sound sequences are considered longer, it is true, but he explains them as having “bigger or smaller volume” (Mańczak 1983: 18). This formulation, if enriched with his examples<sup>8</sup> enables a linguist to understand the idea of “longer” and “shorter” quite well. Considering this explanation no linguist will hesitate to accept that, for instance, Pol. *mruk* ‘taciturn person, grump’ is longer than Pol. *bruk* ‘cobbles, cobbled road’ because of the lack of nasality in the latter word.

Let us have a look at some Yakut verbs and their protoforms now. A general rule is that Yakut verbal stems end either with a consonant (e.g. Yak. *simnat-* ‘soften up, make soft’; *alāriy-* ‘be wide and flat’) or with a long low vowel (e.g. Yak. *simnā-* ‘soften, get soft’). Two verbs, however, step out of line: *diä-* ‘say, speak’ and *siä-* ‘eat’. Even those who have never had contact with Yakut can easily conjecture that these verbs, so frequently used as they are, probably display more archaic forms, as is the case with Germanic strong verbs, too.

<sup>8</sup> For instance, *\*jest*’ is longer than modern Polish *jest* because of the palatalization; *\*predɔ* is longer than *\*prɔdɔ* (> modern Polish *przed* ‘before’) because of non-reduced pronunciation of *\*e*, and so on.

Indeed, the original velar and palatal long vowels in the stem-final position developed differently:  $*\bar{a} > \text{Yak. } -\bar{a}$ ;  $*\bar{ä} > \text{Yak. } -i\bar{ä}$ ;  $*\bar{o} > \text{Yak. } -\bar{o}$ ;  $*\bar{ö} > \text{Yak. } -i\bar{ö}$  (for further details see Stachowski 1993: 52). This fact led to a new and more complex system of stem-final vowels. Less wonder, then, that the Yakut language tended to a simplification of the system in that it partially reversed the diphthongization:  $*\bar{ä} > -i\bar{ä}$  (dial.)  $-i\bar{ä} \sim$  (lit.)  $-\bar{ä}$ , and so on. Thus, Proto-Turkic  $*\bar{all}\bar{ä}$ - ( $< *äl$  'hand')  $>$  Old Yak.  $*\bar{alli}\bar{ä}$ -  $>$  modern Yak.  $\bar{all}\bar{ä}$ - ( $\sim$  dial.  $\bar{alli}\bar{ä}$ -) 'hit, strike' (Stachowski 1993: 73).

The problem is what is longer here: a long monophthong or a diphthong? The former has the length of two identical vowels belonging to one syllable:  $V_1V_1$ ; the latter is equally long but consists of two different vowels:  $V_2V_1$ . Thus, the whole sequence is longer if the sum of distinctive features of both vowels is greater than that in the other sequence, but this case is even more complex. There are four diphthongs in Yakut, and they are all rising:  $i\bar{a}$ ,  $i\bar{ä}$ ,  $uo$ ,  $i\bar{ö}$ . This means that their initial element is less prominent than the closing one. However, in phonetic terms only. Phonologically, the initial vowel is sometimes more prominent as it is what, for example, settles the vowel harmony (esp. vowel attraction). Words with  $-uo-$  behave as if they had  $-u-$  (vowel length does not affect vowel harmony), e.g. Yak. *ostuol* 'table'  $\rightarrow$  *ostuol-a* 'his table', just like *dolbūr* 'shelf'  $\rightarrow$  *dolbūr-a* 'his shelf', unlike *oton* 'berry'  $\rightarrow$  *oton-o* (not *\*oton-a*) 'his berry'. Simultaneously, it is the closing vowel that decides whether the whole will be followed by a  $\gamma$  or a  $g$ , as in the aforementioned pair *tuoyu*  $\sim$  *tugu*. Should, then, the first component of the Yakut diphthong be treated phonetically (= consonant) or phonologically (= vowel)? The decision will probably influence the proportion between diphthongs and long monophthongs in Yakut and, by the same token, the answer to the question of which is longer in such cases. An intuitive solution is, I fear, no longer possible in sound sequences that are more complex than Pol. *mruk* and *bruk*.

#### 4. Conclusions

Mańczak's views on analogy and frequency are not readily acceptable in their canonical form; still, they doubtless give rise to discussion. Why, then, is there nearly no discussion of Mańczak's theses in today's linguistics? Are they really worthless? Why, then, did Kuryłowicz enter into discussion with Mańczak? Why did Best 1973 devote two chapters<sup>9</sup> to Mańczak's work as well as to his discussion with Kuryłowicz? It is true that Best's attitude towards Mańczak's views and methodology is clearly critical but I would rather emphasize two other aspects: First, by no means do every word and every critical opinion in Best's book deserve to be uncritically accepted. Second, Mańczak can be criticized but not ignored with impunity – he was, in Best's eyes, the only linguist worth discussing along with Kuryłowicz and the only Polish linguist who compelled Kuryłowicz to serious (and not very simple) discussion.

<sup>9</sup> These are: Chapter 6: "Die Bemühung von Kuryłowicz und Mańczak um die «Analogiegesetze»" (Best 1973: 61–81) and Chapter 7: "Überprüfung der Untersuchungsmethode Mańczaks" (Best 1973: 82–107).

But those who wish to fair-mindedly assess Mańczak's œuvre have to master all the languages and language families he discusses, or else they cannot make an unaided decision about his examples and reasoning. That would be anything but simple. That has never been simple. Nowadays, that is probably even more complex than it was some thirty years ago because most of today's students of philology consider diachrony and comparative linguistics far too exhausting and prefer instead to work on less fatiguing monolingual synchrony based on cultural history and free of phonological and morphological nuisances. In this situation, it is much easier to join a unanimous chorus of critics who never read Mańczak's works because reading them requires command of French, English and German. In addition, we all remember the polemics between Mańczak and Kuryłowicz. Defending Mańczak involves criticizing Kuryłowicz; criticizing Mańczak does not involve anything. This is the power of chorus.

When talking of analogy and frequency one easily forgets that these are, as a matter of fact, two discrete topics, partially interwoven, true, yet still discrete. Another aspect usually overlooked today is that Mańczak has published a large number of studies on other topics, such as the original homeland of the Slavs and etymology.

Mańczak's views on the former problem are strongly criticized by archaeologists in Cracow, far less so in Poznań and abroad, cf., for instance, a collection of studies edited by Przemysław Urbańczyk (2006).<sup>10</sup>

Mańczak's etymological works cannot be discussed in detail here – they are too many. At least one etymology should be mentioned, however.

In 1982, Mańczak published his first article on the origin of German *Akt* 'nude [art]' (fifteen years later rediscussed in French in Mańczak 1997). In the 1989 edition of F. Kluge's etymological dictionary of German its modern editor, Elmar Seebold was apparently still unaware of Mańczak's explanation and, thus, made no distinction at all between the German *Akt* 'act, action' and *Akt* 'nude'. In the 2011 edition of this dictionary Seebold knew the first article by Mańczak but his way of presenting it was highly unconventional:

Diese letzte Bedeutung ['nude' – M. S.] ist nur deutsch; nach Mańczak – ohne Nachweis – ist sie anderer Herkunft (falsche Ablösung aus *nackt*); nach Bammesberger entstanden durch falsche Ablösung bei *Nacktmödel*. (Kluge 2011: 25b).

A reader will probably understand this formulation to the effect that Bammesberger's explanation is better than that of Mańczak, presumably because Bammesberger did supply proof (German *Nachweis*) that was missing from Mańczak's study ("ohne Nachweis"). In reality, Bammesberger did not supply any proof and, moreover, he generally accepted Mańczak's etymology. An informative summary of this situation was given by Wolter Seuntjens:

Witold Mańczak offers a purely linguistic solution: 'Akt' could have come into use (1) through 'Nackt', as a calque or loan translation of 'le nu' (French) [...] or 'l'ignudo' (Italian) [...], and then (2) by a process of apocope the word 'Nackt' and

<sup>10</sup> For a general overview of these problems see Mańczak 2001.

its article lost its pronounced 'N' and its written 'c': 'ein Nackt' ['ain 'nakt] became 'ein Akt' ['ain 'akt]. [...] Alfred Bammesberger, another linguist, though mostly in agreement with Mańczak, offers a further hypothesis: the change from 'Nackt' to 'Akt' was made in German because of prudishness. [...] This hypothesis has two problems: (a) it assumes that the German public, the German artists, and/or the German academics were more prudish than their English, French, Dutch counterparts. This assumption is hardly convincing. And (b) how to explain the further expansion – an example of linguistic pejoration [...] – of the meaning of 'Akt' into coitus, short for 'Geschlechtsakt'? [...] Following this meaning of the word the only true 'Akt' would be the depiction of the 'act of congress' as, for example, in Rembrandt's *The Bed* [...]. [...] If the Germans indeed were or are so prudish that they collectively changed 'Nackt' into 'Akt', should they not meanwhile have changed the term for 'the artistic depiction of a naked human body' back into 'Nackt' or rather have changed it further into a new innocuous term as an example of euphemism treadmill? (Seuntjens 2012: 5a).

Besides, a junctural metanalysis of *nackt* and that of *Nacktmodell* is always the same: *ein Nackt(modell) > ein Akt(modell)*. Bammesberger (2005) had no way of making his point any different than what Mańczak (1982) had proposed more than twenty years earlier.

Indeed, there exists today no better explanation of German *Akt* 'nude' than that by Mańczak.<sup>11</sup>

Let us come back to analogy and frequency again. Karl-Heinz Best was very critical of Witold Mańczak's views and methodology. This is why the ending of his Chapter 7 is of exceptionally great importance, being an opinion of a critical reader rather than an apologist, and is, thus, suited for the last paragraph of this study, as well:

Wir wollen aber trotz dieser Kritik nicht hervorzuheben vergessen, daß Mańczak es war, der offenbar als erster den Weg gezeigt hat, daß und auch wie man mit Hilfe der Statistik die Analogieforschung auf gesicherten Boden stellen und dadurch zu besseren Ergebnissen führen kann. (Best 1973: 107).

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<sup>11</sup> Seuntjens's (2012: 5b) conjecture is hardly acceptable but worth adducing: "Taking everything together, I hypothesize that a German-speaking academic (theoretician or historian of art?) deliberately introduced the change of 'Nackt' into 'Akt', which was then adopted by the wider academic and artistic world. [...] As to the motives of the intentional change from 'Nackt' into 'Akt', even I dare not speculate".

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